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 A DECISIVE FACTOR IN THE  
 ASSESSMENT OF EVIDENCE<sup>1</sup>

BY G. N. M. TYRRELL

memory

THE purpose of the following argument is to show that in the collection of evidence in psychical research one very important factor has been largely ignored. The primary aim of the psychical researcher is to collect evidence and to carry out experiment, making both as water-tight as possible. But he begins to do this without previously criticizing the situation in which he proceeds to act. He regards the public as being divided into a credulous section on the one hand and a sceptical section on the other, with a tiny minority of truly balanced individuals in the middle, of which he is of course one. But a little reflection is sufficient to show that the situation is not as simple as that ; for the person who is careful and sceptical in psychical research accepts evidence of a lax and hearsay type in everyday occurrences and, strangely enough, this lapse into credulity is justified on the whole. Again, determination on the part of the investigator to be wholly objective in his methods can lead to the worst kind of subjectivity, not only in psychical research, but in every research of a far-flung kind : for while the conscious attention is projected into the outside world, the internal working of the mind attempts to force upon the facts an interpretation that is suited to its own structure. The lesson that this teaches is that in any such far-reaching subject as psychical research we should keep an eye on the workings of our own minds.

One curious feature, visible in the highly sceptical critic, is that he reveals his attitude far more by his *behaviour* than by what he directly says. This applies not only to the critic of psychical

<sup>1</sup> At the Editor's request, Mr Tyrrell kindly prepared this abbreviated version of the paper which he read at a meeting of the Society on 21 September 1950.

research but to human beings at large. For example, some philosophers who have a certain acquaintance with the *Proceedings* and *Journal* of the S.P.R., when writing about such a thing as the scope of knowledge, assume that the physical sense-organs provide all the information we have and, if they mention extra-sensory perception at all, they skate lightly over it as if it were an incidental triviality and not a fact of central importance, which it must be if it exists at all. Something other than pure reason is obviously at work deflecting their arguments towards a pre-determined end. It is *behaviour* rather than overt logic which reveals their true attitude.

If we look back over the past history of psychical research, we cannot help asking ourselves why the subject has made such slow progress in comparison with other subjects—why, after seventy years of careful research, there is still no more than a handful of people at work on it. Why do its funds remain so painfully small, when many other projects of less importance in an ultimate sense have made outstanding progress? The answer seems to be that the great majority of people by-pass the subject, impelled by some reluctance or repugnance in themselves, and turn to other things. There may be—in fact there are—cogent arguments which show that the paranormal is of first importance for human knowledge; but these arguments do not make any impression, and it becomes clear that it is *behaviour* and not rational argument which is the decisive factor in the general attitude towards psychical research. In other words, there is an innate instinct which determines peoples' behaviour; so that when the paranormal is broached the general attitude to it is not properly rational. When instinct is in the saddle, we cannot be surprised if the mind tends to be impelled towards foregone conclusions, and can only be weaned from these conclusions slowly and painfully by continuous repetition of the same evidence and the same experimental results.

Another thing which brings to light this sub-rational influence is the light and casual way in which highly intellectual people deal with this subject. Professor A. D. Ritchie, for example, when treating of psychical research, wrote as follows:

... that a number of very queer and obscure phenomena have been observed that do not fit in well with orthodox theories about bodies and minds and their relations. These phenomena can be interpreted in terms of a theory of 'spirits' but they could equally well be interpreted otherwise and with a saving of gratuitous hypotheses. It seems that one must accept either telepathy or clairvoyance as a fact and most probably both as independent facts. Well, granted telepathy and clairvoyance and granted too the possibility of a certain amount of distortion of the temporal sequence of events, so that what is in the

future for one person's experience is not always in the future for another's, it seems possible to account for all alleged 'spirit' communications. It can, perhaps, be done by means of telepathy and clairvoyance without temporal distortion, or by telepathy and temporal distortion without clairvoyance. The point is that the 'spirits' have never reported anything which has not been already known to some living person, or about to be known in the near future or available in written documents, or by means of some already existing material evidence.<sup>1</sup>

The interesting thing about this quotation is not the kind of theory that Professor Ritchie supports but the light-hearted casualness with which he deals with telepathy, clairvoyance, precognition, etc. If these functions of the mind are facts, then they are central to our knowledge of the mind and should not be injected into arguments until they have been made the central theme of inquiry. Imagine the criticism that would be aroused if any 'normal' subject were treated in this casual manner or brought into serious argument without careful study. One can almost hear an inward voice saying: 'This is only 'paranormal' stuff: it won't enhance your reputation if you take it seriously!

From the rational standpoint, what *reason* is there for lightly dismissing the paranormal? There is surely no valid reason; on the contrary, there is every reason for scrutinizing it with the utmost care; for if the evidence is valid, it is of the greatest consequence for science, philosophy, and human knowledge in general: in fact, it throws a vivid light on the entire human situation, and throws it into a perspective which is quite different from that commonly accepted today. One would think that this would act as a spur to every thinking person and make him wish to probe the paranormal to the bottom. But the fact is that while the normal stimulates curiosity the paranormal kills it. It does even more than kill curiosity: it engenders a *resistance* to serious consideration of it. When Dr Rhine, and others in this country, carried out quantitative experiments in extrasensory perception, in which the odds against chance rose to astronomical proportions, the general reaction was to find a way of escape from the conclusion to which the evidence pointed, not an enthusiastic desire to repeat the experiments all over the world and so to install a new piece of knowledge of the highest importance.

If we take a wide survey of the subject, it is surely as plain as a pikestaff that the common reaction towards evidence for the paranormal is not prompted by the purely rational faculty of the mind but is prompted by something *below* the rational level—something

<sup>1</sup> 'Theories of Immortality', *Philosophy*, vol. 17, no. 66 (April 1942) p. 119.

that is very nearly akin to instinct. Many people would be terribly shocked at the suggestion that men of science could have the balance of their minds upset by deep-seated instincts of whose existence they were unaware. It would be objected that if there are instincts of this kind, psychologists would have discovered them ; and also that science could not have achieved its enormous successes if instinct had the power to interfere with reason. The answer to this is highly illuminating if we take tight hold of ourselves and refuse to push it away. Psychologists have failed to realize the presence of this instinct because they are as much under its influence as anyone else. It is not like the individual complexes with which psychiatrists deal : it is not even a group-characteristic belonging to scientists (although it is true that science has reinforced it) : it is a racial characteristic belonging to the whole of mankind—a product of mental evolution. For evolution did not stop short when it had adapted our bodies to their physical surroundings : it adapted our minds to their surroundings as well. But the latter process was far more subtle than the former : it was part of the instinctual suggestions themselves that they should be concealed from consciousness. As for the argument that science could not have succeeded if reason had been influenced by instinct, the answer to that is that science succeeded precisely because instinct did interfere in the processes of the mind. It was because the mind had been pre-adapted to deal with the external world that science was so successful in every practical direction.

A question that inevitably inserts itself at this point is : How, if this instinctive suggestion is universal and invisible, can anyone become aware of it? It is a fundamentally important question ; but for the moment it must be regarded as a side-issue, for there is no space in which to deal with it. Very briefly the answer is that the human mind is not a monad but is more akin to a spectrum. It can attain to different levels of awareness. Intuition and inspiration are examples of this. Only the practical level of the mind is wholly dominated by adaptive instinct ; yet the latter rises into and influences higher levels—that of intellectual thought, for example—and deflects its reasoning towards foregone conclusions.

To go into the nature of the adaptive instincts which enter into the structure of the mind, or even to follow the work of psychologists, such as C. G. Jung, who has taken steps that should have led him in this direction, is impossible here. But there is one feature of this instinct which bears on psychical research, as well as on many other things. This is the instinctive resistance put up by the mind to anything which comes from beyond the province of the senses. Why nature should have instilled this resistance into us

would entail a lengthy explanation ; but the main fact is that the sense-world *had* to be accepted as all-inclusive, otherwise the world would not have appeared to be simple enough for the primitive mind to grow and to act in. It is because of the pre-adaptation of the human mind to the physical world that every question that arises in practical life is capable of being clearly and finally answered.

The effects of mental adaptation can be traced in science, in a great part of philosophy, and in every department of practical life ; but we are here concerned with their influence on psychical research. Throughout the history of the S.P.R. there have always been those who were cautious about evidence and those who went further and tried to explain it away ; and within recent years, the latter tendency has become more pronounced. There are at least three ways in which the tendency to explain away shows itself in the records of this Society ; and these are to be found in external criticism as well. One of these is to inflate the explanatory powers of chance so that chance in relation to the paranormal is held to be capable of accounting for a great deal more than it is in the field of the normal. Even statistical figures do not dispose of this inflationary tendency. The position is quite irrational ; but instinct, when it causes the mind to act irrationally, can at the same time blind the mind to the fact that it is doing so. The same tendency comes out in the criticism of evidence when the weaker features are brought into high prominence and the stronger features thrust into the background. Again, a suggestion is sometimes found running through a criticism of paranormal evidence which is lacking in definiteness yet sullies it through innuendo, so that the reader is left with the feeling that something is wrong somewhere, though it is not clear where. For example, if careful tests were made with a physical medium, which pointed strongly to some of the phenomena being genuine, while a single case with the same medium pointed to fraud, this would be worked into a suggestion that it undermined all that the medium had produced. It is, of course, perfectly right that in psychical research the evidence should be as good as possible ; but there is a point, not at all easy to detect, at which caution lapses into specious argument.

It may be said that it is perfectly rational to demand a higher standard of evidence in psychical research than in any other subject because the paranormal is much more improbable than the normal. This brings us to the heart of the matter. Is the paranormal more improbable than the normal? Many people would say that it obviously is ; for normal things happen all around us and we understand them and know their laws ; but the paranormal only happens occasionally ; and when it does it seems to break the laws of the normal. But there is no cogency in this

argument unless we make the assumption that nothing can act into the world from beyond it. If things did act into it, they would very likely be occasional and might disrupt or modify physical laws. Thus the paranormal is not improbable unless we have strong proof that our senses reveal, in principle, the entire universe. We have no proof of this at all, *but we have a strong, instinctive urge to take it for granted.*

When assessing evidence in psychical research the principle which is surely rational is that the more improbable the alleged facts are the stronger should be the evidence needed to prove them. The critical person, who nevertheless accepts flimsy evidence about things in ordinary life, would probably defend his action by saying that there was nothing improbable in that for which he accepted weak evidence; therefore weak evidence was enough. The principle is that the strength of the evidence should increase in proportion to the improbability of that which it supports.

But when we turn to psychical research, or to any subject which deals with what is held to be improbable, and adopt this principle, we are at once faced with a difficulty. *How* improbable are the alleged facts? We must surely know this before we can decide on the standard of evidence required: and there is the added complication that we cannot fix a standard for the whole of the paranormal. Some paranormal features may be more improbable than others, so we must vary our standards of evidence throughout the subject. Many people would be likely, for instance, to regard telepathy as more probable than communications from the dead; and the phenomena of the seance-room have their own degree of improbability again. But these differences in probability are not the main difficulty. The main difficulty is that people are not agreed as to *how* improbable any of these paranormal features are. This inserts a subjective element into the problem and prevents us from deciding beforehand how strong the evidence need be; for the improbability of anything depends on general agreement about it.

Let us ask this question: How improbable is telepathy? Some people regard telepathy as a matter of course; others regard it as not very unlikely, while others, again, react towards it as if they regarded it as an impossibility, though they probably do not say so in so many words. How then are we to decide *how* improbable telepathy is? If we cannot decide, how can we fix on the standard of evidence required to prove it? Should we agree with the first class? If so, hear-say evidence would be sufficient as it is for countless things in everyday life. Or should we agree with the middle class? In that case the quantitative experiments would long ago have convinced us. Or should we agree with the third class?



In that case, why not say outright that telepathy is impossible and that no amount of evidence can prove it?

This brings us to the heart of the problem. There is no *reason* why the universe should not extend beyond the scope of our senses, and consequently no *reason* why things of an utterly strange and incomprehensible kind, such as telepathy and precognition, should not occur there and should not give rise to sporadic effects within the sense-world. But an ingrained instinct of a racial type has been instilled into us by nature in order to keep our minds focused upon the world of our senses; and this urges us to reject the possibility of any further extension of the world.

The problem of psychical research is thus not how to obtain evidence which satisfies the rational mind (supposed to be in a state of perfect balance) but how to obtain evidence which will vanquish our racial instinct, which continually attempts to make our reason explain the evidence away. This is why psychical research makes such slow progress, why so few treat it seriously, and why it does not invoke a general enthusiasm for further inquiry.

There are more complications. The power of this racial instinct varies in different individuals because the human mind is graded, and elevation towards the intuitive level weakens the hold of instinct. Also, those who are usually termed credulous placate this instinct by drawing the paranormal into this world and 'normalising' it.

To sum up the present argument: an essential condition for further progress in psychical research is that those engaged in it should recognize the existence of this racial instinct and the subtle part it plays.

## DISPERSION OF SCORES IN ESP EXPERIMENTS

BY D. J. WEST

LAST September arrangements were made to test five friends for ESP after they had taken a drug which was supposed to induce a euphoric mood. The experiment did not fulfil its intended purpose, since in no case did the drug produce the desired effect. The results were nevertheless interesting.

The ESP experiment took the form of a clairvoyance test. Packs of ESP cards (five of each symbol) were shuffled by hand and the card order recorded before each session by Miss Elizabeth McMahan of the Parapsychology Laboratory at Duke University who was in London at the time. During the session the target

pack was enclosed in a small wooden box. The subject was asked to call out his guesses 'down through' the pack while the experimenter wrote down the calls. After each run of twenty-five calls the cards were taken out of the box and the calls checked in the subject's presence. One evening session was devoted to each subject. At each session twenty-five runs were carried out excepting at the third session, which was cut short after twenty runs. The results are summarised in Table I.

TABLE I

<i>Subject</i>	<i>Sex</i>	<i>No. of Runs</i>	<i>Deviation</i>	<i>Critical Ratio</i>
J. N.	M	25	+18	1.80
L. D.	M	25	-25	2.50
J. C.	F	20	+13	1.45
Y. A.	F	25	-13	1.30
B. H.	F	25	-14	1.40

$$\Sigma(C.R.)^2 = 15.25 \text{ (5 degrees of freedom). } P = .01$$

Table I shows a significant dispersion of subjects' scores from chance expectation, suggesting that some of them used ESP to score positively while others had a tendency to use their ESP to avoid the target and produce negative scores.

This avoidance of the target, when the declared aim is to score as much as possible, might be described as a disguised response. Other disguised responses have been reported, such as *Displacement* (that is, hitting a target ahead or behind instead of the one aimed at), *Salience* (that is, heaping up of hits at the beginning and end of a run with a deficiency in the middle), and *Consistent Missing* (that is, naming a particular symbol wrongly, such as calling cross whenever circle is the target). These 'effects' may all be disguised responses. It is as if the subjects were like criminals, guilty of revealing this knowledge of the targets, yet unable to prevent this knowledge influencing their calls in devious ways. Of course, this is as yet only speculation.

Alternate positive and negative scoring is a situation which has been encountered in the investigations of Dr Betty Humphrey and Professor Gertrude Schmeidler. By the use of psychological tests predictive of the likely direction of each subject's score, these experimenters have succeeded in subdividing seemingly null data into significantly different positive and negative scoring groups. It occurred to me that if a number of subjects were tested for ESP under constant and favourable conditions, the positive and

negative scoring tendencies of the individual subjects might be sufficiently consistent to produce a significant dispersion of their scores. If the number of trials per subject were too few, as in most of the American work, the effect would not have a chance to display itself. On the other hand, if the subjects were made to do too many runs, the proverbial decline phenomenon might set in and the initial effect be diluted to insignificance by continuous null results. The effect obtained in the small experiment with five friends might have been due to a combination of favourable psychological conditions and choice of a more or less optimum number of trials.

In order that this possibility could be further explored, Mr J. Fraser Nicol and Mr Edward Osborn kindly lent me some ESP data which they collected in 1948-9.<sup>1</sup> Their experiments took the form of card-calling tests with individual subjects under GESP conditions. The investigators were alive to the desirability of providing an encouraging, unstrained atmosphere. The

TABLE II  
NICOL-OSBORN DATA

Subject	Sex	No. of Runs Completed	FIRST 16 RUNS		SUBSEQUENT RUNS	
			Deviation	Critical Ratio	Deviation	Critical Ratio
E. F.	F	58	- 4	0.500	+ 11	0.85
A. K.	M	80	- 10	1.250	- 3	0.19
W. J.	M	16	+ 11	1.375	—	—
A. N.	F	102	- 18	2.250	- 45	2.42
M. B.	F	77	+ 10	1.250	+ 11	0.70
D. W.	M	16	- 10	1.250	—	—
E. N.	F	58	+ 20	2.500	- 10	0.77
L. N.	F	30	+ 14	1.750	+ 6	0.80
J. N.	M	26	- 6	0.750	+ 8	1.28
M. T.	M	27	- 7	0.875	- 6	0.90

Total dev. = 0

Total dev. = - 28

$\Sigma(CR)^2 = 22.53$  with 10 degrees of freedom ( $P = \text{approx. } .01$ ).

$\Sigma(CR)^2 = 10.79$ , with 8 degrees of freedom (*insignificant*).

<sup>1</sup> The data also included some obtained by Mr Nicol alone. These are the experiments which were referred to in the Society's Annual Reports for 1948 and 1949.

experimental precautions were adequate. In most of the tests agent and subject were in separate rooms communicating by means of a signal light. The target sequence was determined by random numbers. The total result for all subjects showed no significant deviation. (In a total of 15,769 trials the deviation was only 39.2).

Before seeing this data I decided upon sixteen runs as an arbitrary figure likely to be somewhere near the optimum number of runs per subject. Table II shows the results obtained by those of the Nicol-Osborn subjects who completed sixteen or more runs. The table is divided into two sections, the first sixteen runs and subsequent runs.

Table II shows that in spite of the null result in the total, and in spite of the poor performance in later trials, the subjects do display to a significant extent the same dispersion of scores as was seen in Table I. A more accurate method of demonstrating the differing performances of the subjects is the analysis of variance. Each subject's scores are arranged in a column of figures with sixteen entries (one for each run) in chronological order. When the ten subjects' scores are put together a table results with 160 cells. The subsequent course of the analysis is shown in Table III.

TABLE III  
FIRST 16 RUNS OF NICOL-OSBORN SUBJECTS

Source of Variance	Degrees of Freedom	Sum of Squares	Mean Square	Variance Ratio, F	P
Between Subjects (Columns)	9	90.1	10.0	2.60	.01 <i>approx.</i>
Within Subjects (Rows)	15	61.2	4.08	1.06	( <i>insignificant</i> )
Residual	135	520.7	3.85		
TOTAL	159	672.0			

The variance ratio in Table III indicates a clearly significant tendency for the scores of the different subjects to vary, some above chance, some below.<sup>1</sup> The table also shows that there was

<sup>1</sup> The scores used in this analysis form a discontinuous variable with a limited theoretical range from 0 to 25. It is therefore appropriate to use in place of the raw scores the converted value  $\theta$ , where  $\theta = \sin^{-1} \sqrt{x/25}$ . An analysis of variance was performed on the first 16 runs of each of the 5 subjects in the drug experiment. Using the raw score the variance ratio between subjects was found to be 3.11,  $P = .02$ . Using the angular transformation  $F = 3.21$ ,  $P = .02$ . The indication is that angular transformation would make no substantial difference.

no significant variation within subjects, that is no consistent variation common to all subjects with lapse of time. In other words, in the Nicol-Osborn work, as well as in the first experiment with five subjects, some people were using their ESP to score substantially above chance, while others were giving a sort of disguised response, using their ESP to avoid the target and score below chance.

Over the past year a large mass of data from home-testing experiments has been collected by Mr G. W. Fisk. The participants were advised to carry out the tests under GESP conditions and to employ hand-shuffled packs. Since there were no investigators supervising the tests, and the experimental precautions were not rigid, the results, whatever their nature, could never be regarded as more than suggestive. Any subject who achieved an initially promising score was encouraged to continue. There were 177 subjects in all, of whom 134 carried out 8 or more runs and 75 completed 16 or more runs. The results, so far as direct hits were concerned, were null (see p. 369).

TABLE IV

HOME-TESTING EXPERIMENTS. FIRST 16 RUNS. 75 SUBJECTS.  
ANALYSIS OF VARIANCE

<i>Source of Variance</i>	D.F.	<i>Mean Square</i>	F	P
Between Subjects	134	4.80	1.19	0.1 <i>approx.</i>
Within Subjects	15	3.03	<i>insignificant</i>	
Residual	1110	4.02		

An analysis of variance was performed on the direct hit scores of the 75 of these subjects who completed 16 runs. Subsequent runs were not included. The result, shown in Table IV, gives no indication of a dispersal of scores as was found in Tables I and II.

The Fisk data were interesting on account of the significant displacement effects. It is important to emphasise, however, that although the total score was statistically significant the scoring average was only very slightly different from chance expectation. The average deviation per run on forward displacement was  $-0.12$ . This tends to confirm the view that one cannot expect a large yield from card-calling experiments using unselected subjects without attempting to introduce strong motivation in each of the participants. In this connection Dr Rhine recently wrote advising a psychologist who was about to embark upon ESP

tests to the following effect: It is important to remember that ESP tests are no mere passive measurement such as you are accustomed to making. It is likely to help most subjects to challenge them to get as many as possible of the symbols correct. Unless the subject is strongly motivated himself, the tests call for the liveliest methods and strongest pressure the experimenter can properly bring to bear.

#### SUMMARY

Other investigators have demonstrated positive and negative scoring trends in groups of subjects selected by psychological testing. A small ESP experiments with five friends showed a significant dispersion of subjects' scores. This led to the suggestion that, given the right conditions and an optimum number of runs, the positive and negative scoring trends of each subject might reveal themselves as a significant dispersion of scores without any psychological test having been applied. This idea received some confirmation from an examination of the data collected by Nicol and Osborn. No such effect was found in the large body of data from home-testing experiments. The matter would probably repay further investigation by experimenters who have suitable data in their possession.

### AN EXPERIMENT IN PRECOGNITION

REPORTED BY G. N. M. TYRRELL

Mrs V. M. Austin, a member of the Society, asked me some time ago if I could suggest an interesting experiment in precognition. I replied that it would be a good thing, I thought, to introduce some process of randomisation between a prediction and its fulfilment in order to see whether this would prevent the fulfilment from taking place. The following is an epitome of the experiment. Mrs Austin took a mascot, a small black cat, to Mrs Methven, a medium, and asked her to write down impressions as to what would happen to it in the future.<sup>1</sup> These she sealed up in an envelope without looking at them. This was done on 27 September 1946. One or two nights later, Mrs Austin wrote the names of thirty acquaintances on slips of paper and mixed them up in a hat. She then got a friend to draw one of them, and the name

<sup>1</sup> Mrs Austin tried the experiment on three occasions. The first two, which were not with Mrs Methven, were failures.

on it was found to be Miss Graham (pseudonym). The mascot was at once sent to her, and Mrs Austin opened and read the medium's statements and filed the paper away. Miss Graham sailed for a foreign country on 25 March 1949. She was a civil servant in the employ of a foreign government, and she went to that country partly in order to arrange her retirement from her post. In May 1949 she returned to England. She had intended to travel on the outward journey in a freight steamer alone, but on hearing that her passage would be paid, she went by liner, where she met a friend and travelled with her.

Mrs Austin then sent questions based on Mrs Methven's statements to the other twenty-nine persons whose names had been in the hat in order to find out to what extent these statements applied to them. Mrs Methven's statement is given in full below (A). It is followed by list B, which contains the medium's statement as put to Miss Graham by Mrs Austin, with Miss Graham's annotations shown in italics. List C contains the questions put to the remaining twenty-nine persons.

- A. 'The article in question will be carried to a foreign country. Will bring good luck. Two people are travelling together. A great deal of activity. Many new links will be made. I feel will return to this country again perhaps for retirement. I feel you must take care of your health, nothing to be anxious about, chest seems a little difficult at times, able to overcome much with right thought.'
- B. '(1) The article in question will be carried to a foreign country. *Wrong.* (2) Two people are travelling together. *Correct.* (3) A great deal of activity. *Yes.* (4) Many new links will be made. *Yes.* (5) Will return to this country again. *Correct.* (6) Perhaps for retirement. *Yes.*'
- C. '(1) Did you travel to a foreign country between September 1946 and June 1949? (2) If so, did you travel with a single companion? (3) If so, did you return to England within this period? (4) If so, did your journey bring marked good luck? (5) If so, did you have an exceptionally active time abroad? (6) If so, did you make many new links during this period? (7) If so, was the journey connected with your professional retirement? (8) Even if you did not go abroad, during this period, did you retire or make arrangements for your retirement during this period?'

The chance question was roughly tested by sending list C to the other 29 persons whose names had been in the hat. The gist of the result was that in 45 cases statements of the medium applied,

while in 158 they did not. The whole of the eight statements did not apply to any one of the 29. One of the 29 did, however, make a journey which was connected with retirement. Three out of the 29 had arranged to retire during this period but had made no journey in connection with it.

It has been suggested that the experiment contained the following defects in design : (i) that list B given to Miss Graham to annotate did not correspond exactly with the statement (A) made by Mrs Methven in that it omitted the references to good luck and health ; and (ii) that while Mrs Austin divided list B into six parts, the list (C) given to the twenty-nine persons for annotation was divided into eight parts and omitted the point about health. It has been suggested that as a result the applicability of Mrs Methven's statement to Miss Graham on the one hand and to the twenty-nine persons on the other cannot satisfactorily be compared.

While Miss Graham did, in a later letter, make clear that she had had good luck, it was admittedly a fault that the medium's statement about health was not circulated. This statement did not apply to Miss Graham and was in fact the only mis-statement made by the medium. For the first item in list B, which was there marked wrong, was afterwards found to be right. Miss Graham had inadvertently taken the mascot abroad with her in an out-of-the-way pocket of a bag and only discovered this after her return. As regards the first part of point (ii) : the two additional questions in list C were (4) and (8). The former, which concerned good luck, is dealt with above ; (8) would not have been applicable to Miss Graham as it was known that she had gone abroad.

Perhaps it should be added that Mrs Austin told me in a letter that when Mrs Methven was presented with the cat-mascot, she first made statements about the former environment of Mrs Austin, who had, up to then, been its owner. These statements were correct. Also, during the winter which followed the presentation of the mascot to Mrs Methven, Mrs Austin had a long illness of combined bronchitis and influenza, to which the statement about health might quite well refer.

I think Mrs Austin is to be congratulated on the achievement of this experiment and it is to be hoped that, despite the doubt between chance and the paranormal in this case, more work on precognition along these lines will be done.



## HOME-TESTING ESP EXPERIMENTS

A PRELIMINARY REPORT BY G. W. FISK

SINCE quantitative experiments in extrasensory perception started in Great Britain, a mere handful of subjects has been found who have been able to maintain a consistently high rate of scoring. It was in an effort to discover promising subjects that in January 1950 arrangements were made for the carrying out of simple ESP tests by members of the Society in their own homes. An instruction sheet was drawn up, and this, with numbered scoring sheets and packs of ESP cards, was sent to every group of two or more people who were willing to give each other and their friends card-calling tests. It was recommended that Agent and Percipient should be separated by a screen measuring at least three feet square or by its equivalent, that the Experimenter should signal by tapping when the Agent looked at each card, and that 200 calls should be made at each session. It was suggested that any subject scoring ninety-two or more (or sixty-eight or less) in 400 guesses should be regarded as promising, and that further tests with him should be carried out. As the experiments were not supervised, it is, of course, legitimate only to regard the results as suggestive, however significant they may appear statistically.

	<i>Backward Displacement (-1)</i>	<i>Target Hits (0)</i>	<i>Forward Displacement (+1)</i>
Total Hits	13116	14026	13081
Expectation	13416	13975	13416
Deviation	-300	+51	-335
Critical Ratio	2.89	0.48	3.24
P	.004	.32	.001

Up to the end of October 1950, 177 subjects had been tested, and the total number of runs (twenty-five calls to each run) was 2,795. No individual subject was found whose scores were consistently significant either on the target or in either displacement direction. Taking the results as a whole, however, there were significant negative scores on both +1 and -1 displacement. An examination of a substantial sample of the data for +2 and -2 displacement indicated that a negative trend, similar to that discovered in the +1 and -1 scores, was present, though not to a significant degree.

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Statistical tests have not yet been applied, but by inspection it appears that the negative deviations on displacement were well distributed throughout the data and were not due to the anomalous performances of a few subjects. It must be emphasised that 'closed' packs were used and that the scores in the direct target and displacement positions are not statistically independent of each other. The close conformity of the direct hits with chance expectation is interesting. In experiments conducted by different volunteers and in a variety of conditions, one might have supposed that spurious deviations would be likely to arise from such normal causes as lack of care in recording, sensory leakage, etc.

The search for promising subjects still goes on. I hope that any readers of this *Journal*—whether members of the Society or not—who have not taken part in these experiments but who wish to do so will get in touch with me (6 Ditton Grange Close, Ditton Hill, Surrey).

### THE FRAUDULENT MEDIUMS BILL

ON Friday, 1 December, this Bill was read a second time in the House of Commons. Proposed by Mr Walter Monslow (Labour, Barrow-in-Furness) and seconded by Mr T. J. Brooks (Labour, Normanton), the Bill, in the words of the preamble, is designed to 'Repeal the Witchcraft Act, 1735, and to make, in substitution for certain provisions of section four of the Vagrancy Act, 1824, express provision for the punishment of persons who fraudulently purport to act as spiritualistic mediums or to exercise powers of telepathy, clairvoyance or other similar powers'. It contains the following clauses:

1.—(1) Subject to the provisions of this section, any person who—

(a) with intent to deceive purports to act as a spiritualistic medium or to exercise any powers of telepathy, clairvoyance or other similar powers, or

(b) in purporting to act as a spiritualistic medium or to exercise such powers as aforesaid, uses any fraudulent device,

shall be guilty of an offence.

(2) The foregoing subsection shall apply only where a person acts for reward; and for the purposes of this section a person shall be deemed to act for reward if any payment is made in respect of what he does, whether to him or to any other person.

(3) A person guilty of an offence under this section shall be liable on summary conviction to a fine not exceeding fifty pounds or to imprisonment for a term not exceeding four months, or to both such fine and such imprisonment, or on conviction on indictment to a fine not exceeding five hundred pounds or to imprisonment for a term not exceeding two years or to both such fine and such imprisonment.

(4) No proceedings for an offence under this section shall be brought in England or Wales except by or with the consent of the Director of Public Prosecutions.

(5) Nothing in subsection (1) of this section shall apply to anything done solely for the purpose of entertainment.

2. The following enactments are hereby repealed, that is to say—

(a) the Witchcraft Act, 1735, so far as still in force, and

(b) section four of the Vagrancy Act, 1824, so far as it extends to persons purporting to act as spiritualistic mediums or to exercise any powers of telepathy, clairvoyance or other similar powers, or to persons who, in purporting so to act or to exercise such powers, use fraudulent devices.

3.—(1) This Act may be cited as the Fraudulent Mediums Act, 1950.

(2) This Act shall not extend to Northern Ireland.

Members who spoke in favour of the Motion were Mr Arthur Colegate (Con.), Mr George Deer (Lab.), Mr C. W. Gibson (Lab.), Mr Leslie Hale (Lab.), Mr Douglas Houghton (Lab.), Mr James Hudson (Lab.), Mr B. Janner (Lab.), the Rev. Gordon Lang (Lab.), Mr L. M. Lever (Lab.), Lt-Col M. Lipton (Lab.), Mr R. J. Mellish (Lab.), Mr T. C. Pannell (Lab.), Mr George Rogers (Lab.), Mr William Ross (Lab.), Mr H. N. Smith (Lab.), Lt-Cdr R. H. M. Thompson (Con.), Mr S. P. Viant (Lab.), Mr David Weitzman (Lab.), Col G. E. C. Wigg (Lab.), the Rev. G. S. Woods (Lab.). The Home Secretary, Mr Chuter Ede, stated that the Bill would be left to free discussion and a free vote. He hoped 'that in some form or other we may through this measure be able to release some of our fellow citizens from an indignity to which at present they feel they are subjected'.

The Council of the Society considered the Bill at their meeting on 7 December. They expressed their entire concurrence in the declared objects of the Bill, namely, to protect honest mediums from prosecution under out-dated Statutes which are in their terms offensive to the whole profession, and at the same time firmly to check fraudulent mediumship.

The Society has for its object the examination in an impartial spirit of various real or supposed faculties usually called paranormal, with a view to bringing within the domain of scientific inquiry those aspects of human personality left unexplored by other branches of psychology. To carry out this difficult and important enterprise, the psychical researcher must be granted the same freedom to pursue his investigations unmolested as is enjoyed by other scientific workers. The mediumistic situation is not as simple as the debate in the House would suggest, and presents no clear-cut alternative between genuine spirit communication and

fraud. The psychical researcher must feel himself secure to state without fear or favour just what his opinion is of any of the incidents under his examination. This, however, necessarily involves making statements that are critical of the powers or alleged powers of mediums. Since it has hitherto been assumed that the existence of the Acts now sought to be repealed has been a bar to the bringing by mediums of libel actions against researchers, the Council felt that the repeal of these Acts, without the provision of some safeguard in this respect, might make it difficult for a researcher to publish the results of his investigations and his comments thereon without running the risk of an action in the Courts. As psychical research is a highly specialised subject, and the proper examination of alleged paranormal phenomena requires a lifetime of study, it was suggested that an ordinary Law Court might be an unsatisfactory tribunal for the decision of such questions. The Council therefore consulted Mr K. E. Shelley, K.C., himself a member of the Society, on this matter and he advises as follows:

While it is undoubtedly of the greatest importance that psychical researchers should have full liberty to publish the results of their investigations and to state their conclusions and even their conjectures with the utmost freedom, in my opinion the ordinary law of libel affords them sufficient protection for this purpose. As the law stands, no criticism can make its author liable to damages provided it is fair and honest, that it is made without malice, and that it contains no misstatement of fact. The circumstance that the criticism reflects the personal beliefs (or disbeliefs) and the prejudices of the author does not matter. All the many reports of investigations contained in the various publications of the Society that I have read fulfil these requirements and, quite apart from the Acts now proposed to be repealed, no successful action for libel could have been brought in respect of any of them. As the Society certainly intends to maintain its existing policy in regard to criticism, I see no reason for it to seek any special protection. Of course, nothing can prevent any person who thinks he has a grievance from instituting legal proceedings, but in my opinion this is an advantage rather than the reverse. Mediums will know that they have the same rights as any other person and can protect themselves from unfair and malicious attacks, and therefore will be the more ready to submit themselves to impartial investigation. Any action brought in respect of a proper criticism is bound to fail (it is surprising how our Courts can master technical details of every kind when these are properly explained) and an unjustified action that deservedly fails may ultimately do so much good by informing the public of the pitfalls and difficulties in this branch of knowledge that the trouble and expense of defending the action would be a valuable investment.

In view of the above advice, it is not felt that there are grounds for making representations in regard to the Bill.

## REVIEWS

SPUK : IRRGLAUBE ODER WAHRGLAUBE? Vol. I. By F. Moser. Foreword by C. G. Jung. Baden bei Zurich, Gyr-Verlag, 1950. 342 pp. 9 plates.

This book is the first of two volumes on the poltergeist by Dr Fanny Hoppe Moser, a Swiss biologist, who became interested in occult phenomena in 1914 when she assisted at a sitting for table-turning and levitation with the Berlin medium, Mrs Fischer. In her earlier book, *Der Okkultismus* (Munich, 1935), Dr Moser gave a general survey of the whole field, and although she tried to be as objective as possible, it was clear that she was of the opinion that many so-called psychic phenomena were facts in Nature, and could be investigated by the methods of science.

In her present work Dr Moser has set herself the task of inquiring into the evidence for poltergeist activity, and for this purpose she has examined afresh the records of some cases dating from 1663 to 1946. The accounts of most of these cases are in German, and among them is the very curious story of the phenomena observed by Professor C. G. Jung during his week-ends in a haunted farmhouse in Buckinghamshire in 1920. It is a pity that these events were not reported to the Society for Psychical Research at the time of their occurrence, as, from the notes now printed thirty years later, it is clear that Dr Jung made no effectual attempts to inquire into the objectivity of the sounds, whilst his colleague Dr X. went to bed with a loaded gun as his companion.

As an opening to her survey, Dr Moser has chosen the famous Joller case which began in Stans, a small place south of Lucerne, in 1860 and was recorded by the lawyer, Melchior Joller, in a pamphlet, now very rare, which was issued in Zurich in 1863.<sup>1</sup>

The phenomena began with raps, as is so often the case. Joller thought little of them, but when one day in 1861 his small son was found in a fainting condition his father was disturbed to hear that the fit was brought on by raps followed by the alarming appearance of a white misshapen form. Gradually the phenomena increased in number and complexity. The raps on the floor were now as loud as if they had been caused by a heavy mallet, and

<sup>1</sup> A second pamphlet, apparently in reply to normal explanations which had been advanced, was issued the same year. The authorship (attributed by Daumer to Joller himself) is still in dispute. Dr Moser is at pains to reject Daumer's attribution; she also discusses the authorship of the preface to Joller's own pamphlet. In my own copy of the work, which came from the library of Baron Carl Du Prel, the preface is assigned to Perty, which seems to me very probable and which may have been written in by Du Prel himself.

furniture jumped after every blow. Grey shapes were now and then observed flitting about, and soon the case became a sensation. In view of the public clamour a kind of committee of three persons was appointed, including the chief of the local police. On investigation little occurred, but when the Joller family was requested to withdraw from the house the phenomena immediately ceased, only to recommence the moment it returned and actually to increase in violence. A massive walnut table was overturned and an object like a three-tailed cloth appeared and disappeared. Vague shadows as of waving hands were observed, and greyish forms were occasionally seen gliding about. Many pieces of furniture were knocked over; an apple danced about on the floor, and apparitions were seen by the children both in the house and garden. Finally, the Joller family moved elsewhere and the house was locked up.

Dr Moser has taken a great deal of trouble to supplement Joller's account with other contemporary records and the recollections of those still living. Although her industry is to be commended, it does not seem that much was gained from the scientific point of view. Indeed, the publication of these cases is mainly of comparative, academic, and historical interest, and like so many other writers Dr Moser is inclined to be overwhelmed by the enormous weight of testimony to poltergeist activity. She now and then seems to take the attitude of the 'ten thousand-people-can't-be-wrong' school without fully appreciating the basis on which others maintain their attitude of scepticism. Moreover, I suspect that Dr Moser has had but little practical experience in the field. Had she had such experience she would know how utterly different are the accounts of untrained observers from those furnished by skilled investigators, and how the most transparent and simple little deceptions may appear as major phenomena which, at first sight, seems quite inexplicable. When Dr Jung maintains, as he does on p. 260, that parapsychology would do well to make use of the psychology of the unconscious, he is preaching to the long-since-converted. Indeed, could the Jungian psychologists be persuaded to listen to and learn from the parapsychologist, they would sometimes refrain from claiming as examples of the paranormal incidents in the consulting-room that they cannot explain.

It is this inability to explain that lies at the root of Dr Moser's general attitude, and it is connected with her failure to realise the meaning and relevance of much of the material with which she is dealing. The very complexity of poltergeist cases make them very difficult to appraise when all we have are written records from sources which are often suspect in themselves. For, apart

altogether from conscious fraud (which may also be present), we have to contend with the misinterpretation of normal events, which are then fitted into what is supposed to be a supernormal framework, and thus add false notes to the overall picture.

Dr Moser's book, therefore, is of value not so much from the point of view of the practical observer as from that of the historian whose work in this field is also to be esteemed as it enables the student to compare what has been recorded in the past with what is being observed in the present. From the case histories here collected, the reader will be able to learn something of the less spectacular poltergeist records from house and stable and note how, in many instances, those favoured failed even to make the most simple kind of inquiry into the origin of the manifestations. Thus we have the case of the Bavarian Professor of Physics, who, for six months, noted the racketings of a poltergeist in the attic of the house in which he lived, and did not even take the trouble to investigate upstairs. Then we have the case of the student of chemistry in whose apartment a poltergeist raged for some time. Yet, when one day she discovered that over one and a half pints of 'milk' had been apported and spilt in one of her rooms, she did not even go to the point of having the liquid analysed! Can it be that many of these events are to be explained by assuming that, *at the time*, they did not strike those participating in them in the same light as they did *later* when describing them to collectors of ghost stories? It is for the reader of Dr Moser's book to judge for himself, and in this he will not be wasting his time. We shall look forward with much interest to the second volume where the authoress has promised to provide a commentary and discussion of the fact recorded in the first.

E. J. DINGWALL

THE ILLUSION OF IMMORTALITY. By Corliss Lamont. New York, Philosophical Library, 1950. Second Edition. xvii, 316 pp. 4 plates. \$3.95.

This is a learned and well-argued defence of the thesis that man is mortal and that there is no life beyond the grave. Most of the arguments are along familiar lines but they are well presented, and generally the case on the other side is fairly considered. Having read Dr Lamont's book, I remain unconvinced by his thesis but I do not think this is due to any defect in the presentation of his case. Its weakness lies not in its arguments but in the system of assumptions on which the arguments rest. If the universe is merely a physical system in which human bodies have evolved from unicellular organisms by a blind operation of the laws of

natural selection, it is indeed unlikely that somewhere in that process an immortal soul was added to man's body, and that in some corner of this naturalistic universe there are to be found a spiritual world and a God who on rare occasions interferes with the natural order.

There is, however, another possibility: that God and the spiritual world are primary and important, and that the natural world is a creation by God for the purpose of attaining spiritual ends. From such a point of view it is not unreasonable to suppose that man is now and eternally a part of that spiritual world, or even that the purpose of evolution was to provide a fit instrument for the human spirit. Such a possibility is not considered by Dr Lamont. Our choice between the possibilities as to a naturalistic or a religious view of the universe must be made on the evidence; the question cannot be decided merely by looking at it from one of the alternative points of view.

The decision is, of course, difficult; that is why there is much difference of opinion on the matter. The assessment of evidence depends on one's valuation of revealed religion and on one's opinion of the finality of the scientific point of view, as well as on one's opinion of the kind of evidence obtained through mediums. Dr Lamont seems to abandon his attempt to be fair in considering the latter kind of evidence. There are obvious difficulties in judging how far the results of seances support a belief in a future life, but consideration of this evidence demands more than reference to 'dear departed grandfather's indulging himself in table-rapping, playing weird tunes on cheap musical instruments or telling the secrets of his past to strange women mediums'.

Even less happy is his reference to experimental psychical research. Dr Lamont repeats criticisms made by Professor Jastrow fourteen years ago of the work carried out at Duke University which were of doubtful validity when they were made and are certainly inapplicable to the work that has been carried out there since that time. He also states that Rhine's successes in ESP have not been repeated in other countries, ignoring the contributions of Soal, Tyrrell, and Whately Carington in this country alone.

R. H. THOULESS

THE MESSAGE OF FATIMA. By C. C. Martindale, S.J. London, Burns Oates & Washbourne, 1950. vii, 183 pp. 10s. 6d.

Fr Martindale's study of the events of 1917, which transformed Fatima from an obscure Portuguese village to a centre of pilgrimage, is a quiet, scholarly, scrupulously accurate piece of research which



should be of considerable value to those interested in the psychical phenomena involved.

He distinguishes carefully between the content of the message given to the three children, calling the peoples of the world to prayer and repentance, and the forms through which they received it. He points out that the human mind is bound to clothe its intellectual perceptions in whatever imagery is most familiar to it—and that that of a Portuguese peasant accustomed to baroque churches is sure to differ from that of (say) an American, or an Indian, or a Greek. (The same truth may be exemplified in a more familiar instance by considering how differently an Englishman and a Chinese will 'see' and paint an identical objective landscape—witness Chiang Yee's delightful pictures of various Oxford scenes.)

Fr Martindale has also carefully separated the evidence given in contemporary documents—the repeated cross-examinations of the children, the reports of their parents, the newspaper accounts of how enormous crowds saw the sun 'spinning' in the sky, and emitting a series of coloured lights, ending with a 'very ugly yellow'—from the later, adult writings and reflections of Lucia, the nun who was the only one of the three who lived to grow up.

None of the original oddities is smoothed away—the suddenness of first vision, with the flash of lightning directing the children's attention to a young girl standing on top of a small tree; the fact that the little Jacinta wanted to offer her some bread and cheese; the sound heard at one later vision by a woman, and at another, later still, by a man, neither of whom saw or heard anything else that seemed unusual, like 'the buzzing of a bee' or 'a horsefly in a bottle'; and the fact that on the 13th October, when most of a crowd assessed at 70,000 strong were awed and terrified by the unexpected apparent movements of the sun, and 'unbelievers' such as the editor of a freethinking newspaper, and a stolid, irritated English domestic brought by her employers saw all the phenomena, several devout persons in the assemblage perceived nothing at all. This, by the way, seems to be an argument against Fr Martindale's tentative hypothesis that the appearances might be explained by 'natural causes'; just as the fact that they were observed from two places some distance away seems to argue against the theory of a collective hallucination brought about by suggestion from one person to another.

This seems likely to remain the standard English work on the subject. Its original sources are quoted (in translation) at considerable length. No attempt is made to attach any disproportionate importance to the form of the visions or the locutions, or to the solar 'miracle'; and the Preface provides a close and interesting analysis of the process through which the mind receives,

symbolises or projects, rationalises, 'tidies' and may even occasionally misinterpret in detail a spiritual experience. It is fascinating to see how closely this description corresponds with what is thought to occur with psychical experience.

Readers should realize, of course, that the book is not written as a piece of psychical research, but in a spirit of deep devotion which holds the service of exact and detailed truth to be an integral part of religion.

RENÉE HAYNES

JOURNAL OF PARAPSYCHOLOGY. Durham, N.C., Duke University Press. \$1.25.

Vol. 14, No. 2, June 1950

In an Editorial on 'Parapsychology and Biology', Professor Rhine discusses the problems raised for parapsychology by such performances as migration and homing. He also considers the bearing of parapsychological findings on basic biological theory.

J. G. Pratt and Esther B. Foster contribute 'A further study of ESP displacement in relation to hits and misses'. They find further evidence that a subject may show a characteristic reaction to the fact of success in an ESP experiment although he is not told of his success. The reaction described is a tendency to produce a pair of misses of the target card, particularly after a direct hit.

There is an account by R. Rose of a preliminary PK experiment in which a positive deviation was obtained which was not significant. The experimenter intends to carry on further with an improved experimental design.

A criticism by C. C. Stevens of Rhine's book *The Reach of the Mind* is followed by an interesting correspondence between Professor Rhine and Mr Stevens in which each tried to clarify his point of view.

M. Skibinsky has tried out the suggestion that ESP subjects will score more highly on targets that are personally significant to them. His results do not support this expectation. The score of his subjects on family names was significantly lower than their score on the symbols in the ordinary ESP pack.

Vol. 14, No. 3, September 1950.

The Editorial deals with the shifting scene in parapsychology. Professor Rhine here discusses the present field of interest in experimental parapsychology and also the further problems for which fruitful methods of experimental attack have not yet been devised.

An article by Dr Soal and Mr Bateman gives an account of experiments done with Mrs Stewart as subject when agents were working in opposition and in conjunction. In both cases it appeared that Mrs Stewart took her responses from one agent, and was not hindered by another agent trying to communicate a different card, or helped by other agents trying to communicate the same card.

Dr Humphrey has carried a stage further her work on separating successful and unsuccessful subjects by the use of tests of personality. By adding to the results of an expansion-compression test and the mid-range and extreme scorers on Stuart's Interest Inventory a further measure derived from items of the Interest Inventory, she finds that she can make a better separation of high and low ESP scorers than the two previous measures gave in combination.

Dr Focht gives an account of the opinion of Ehrenfels on prophecy. It is that future events are known to the over-souls of human groups and that the knowledge of these over-souls is sometimes tapped by individuals. This is obviously one of many possibilities; it is not easy to see, however, in what way this hypothesis could be subjected to experimental test.

The number ends with an acute review reprinted from *Mind* by Professor Price of Rhine's book, *The Reach of the Mind*.

R. H. T.

JOURNAL OF THE AMERICAN SOCIETY FOR PSYCHICAL RESEARCH.

Vol. 44, No. 4, October 1950. New York, A.S.P.R., \$1.50.

George H. Hyslop gives an interesting account of the philosophy of James H. Hyslop and of his contribution to psychical research.

A history of the S.P.R. by W. H. Salter is based on an address he gave to the American S.P.R. in April 1950.

There is an important and fascinating article by Dr C. M. Cooper, M.D., entitled 'An Inherited Baffling Perception and its Uncovering'. This is an admirable little study of the process of unconscious cerebration and the problem of 'hunches' which must be read by everybody interested in paranormal cognition.

D. P.

## CORRESPONDENCE

'RETROACTIVE PK' AND THE  
CLASSIFICATION OF PHENOMENA

SIR,—Apart from its direct aim, Mr Mundle's paper in *Proceedings* (XLIX, pp. 61-78) is valuable in indicating the difficulties that beset psychical researchers in using current systems of classification, in which the concepts of precognition and PK play a prominent part. Not only is the main theme of his paper the problem of deciding in practice whether both precognition and PK are basic irreducible concepts, or whether apparent cases of one can always be reduced to cases of the other, but also he shows in passing (p. 62) that the term 'precognition' at least is unfortunate, implying as it does that the phenomenon is a form of knowing, an implication which seems not to be justified by our present knowledge of the facts.

The general problem is by no means new. Throughout its history scientific psychical research has been faced with the difficulty of classifying phenomena which tend most perversely to overlap, to masquerade as that which they are not, and to be shy of discovery in isolation. Two main schemes of classification have been used, and neither has been entirely satisfactory. The first, that adopted by F. W. H. Myers in *Human Personality*, is a scheme dictated by the phenomena themselves and using mainly psychological concepts. This becomes inadequate as soon as it is realized that while the surface manifestations, e.g. of trance mediumship or psychometry, may have a close family likeness, the sources upon which they draw vary enormously, perhaps from mere hypermnesia to the activity of discarnate spirits. The second scheme, that more commonly used or at least assumed today, is based partly on experimental experience and partly on our common *a priori* notions of time, causation, cognition, matter, and mind. This scheme encounters difficulties of the type brought out by Mr Mundle, and I would question here whether we are right to accept these *a priori* factors so readily. It is sufficiently established that the world of the psychical is a very queer one—how queer we have as yet no means of telling. The danger of using our everyday *a priori* framework is that it may lead us to ignore certain possibilities which if followed up would give us a better understanding of what we have to deal with. That this particular framework is not necessary to our thinking is shown by its partial abandonment in modern physics.

Theory must not run too far ahead of the facts. But it is idle to pretend that we can ever wholly divorce fact from theory, or

refrain from building hypotheses until we have had a good look at the bare data. We have to view the facts in the light of some theory, but we can at least realize that it is a theory and make it as explicit as possible, hoping by so doing to reveal its weaknesses and its potentialities.

My aim here is to show that by extending the scheme followed by Mr Mundle we find room for a new type of phenomenon not so far considered. If such a phenomenon is to be found, it should not be very difficult to produce *prima facie* evidence for its existence. On the other hand, if such *prima facie* evidence is discovered, it will increase the problems of using this type of scheme at all. Difficulties of distinguishing between phenomena will be multiplied, and it may be necessary to abandon the scheme altogether.

As things are, Mr Mundle recognises two main groups of phenomena, one cognitive and the other conative. The cognitive group consists of mental events related to other events, mental or physical, in the past (retrocognition), the present, and the future (precognition). In the conative group we have an apparently different type of mental events, bearing a relationship of a different kind to physical events (PK) or mental events (telepathic suggestion) in the present or near future. (Mr Mundle does not mention telepathic suggestion, but it fits in naturally here.)

I am being purposely vague in my description of these two groups. An attempt at better definition would only lead to distortion. It will be seen that as they stand the two groups are not on all fours. While the cognitive group covers past, present and, paradoxically, future, the conative group, in accordance with normal experience, covers only present and future. But if we admit the possibility of knowing what has not yet happened, we should also be prepared to consider that of causing what has already happened. Or, to put it another way, if a future event can 'cause' a present mental event, why cannot a future mental event 'cause' a present event?

I suggest, therefore, that experiments should be conducted for *prima facie* evidence for what might be called 'retroactive PK'. I cannot find that this has yet been done. In *The Reach of the Mind*, for instance, Rhine says: 'Displacement in ESP tests has become a well-established phenomenon, even though it is not very common. . . . It could not occur in PK in the same way, because of the difference in techniques' (p. 127). Now this is perfectly true of the classical PK experiments. The target is not changed often enough for a displacement of this type to be noticeable—though it is interesting that Rhine mentions indications of a displacement in the other direction, e.g. the 'lag effect' of Pratt

and Woodruff. But experiments with more frequent changes of target would provide an opportunity for such an effect to appear.

If it did appear, however—if, for instance, we found something similar to the -1 displacements of telepathy—we would at once come up against a difficulty similar to those discussed by Mr Mundle. Such an effect could be explained as a combination of straight PK plus precognition of the next target, assuming that the choice of target was not predetermined. (If it were, the alternative explanation would be even simpler—direct telepathic or clairvoyant knowledge of the target.) And I do not see that this difficulty can be entirely overcome. In all types of experiment some alternative explanation would be possible, though not necessarily probable. The most we could do would be to devise experiments to which arguments of the type used by Mr Mundle on pp. 75-6 would apply, i.e. where to explain the effect produced as being due to retroactive PK would be far more simple and plausible than any other. Such experiments are not easy to devise; but we might be lucky enough to find, for example, a subject who scored well on retroactive PK but got no results in direct precognitive experiments.

It is not of much value to consider such possible experiments in detail unless and until we have at least *prima facie* evidence that retroactive PK does in fact occur. It is sufficient for my present purpose to point out that, as in the cases considered by Mr Mundle, here too it would be very difficult to bring about an experimental separation of the various possible factors involved. And this, I suggest, may be due at least in part to the inadequacy of our conceptions of these factors.

It seems to me possible that we are going astray in assuming a complete separation between knowing and willing, cognition and conation. There are other reasons for this besides the experimental problems so far considered. First, while we seem in ordinary life to know very well what we mean by knowing and willing, the attempts of philosophers and psychologists to analyse these concepts have been singularly unsuccessful. Hence we cannot say with any confidence what the essential features of an act of knowing or an act of willing are. This is paralleled on the other side by the fact that it is also very difficult to give an account of what goes on in an 'act of precognition' or an 'act of PK'. We are therefore on very shaky ground in trying to classify these latter as cognitive and conative acts respectively.

Secondly, what little we do know about the psychology of psi-phenomena seems to give some importance to the imagination. And the imagination may be regarded from different angles as either active or passive, as cognitive or conative, or as a neutral

ground between the two. It is true that in laboratory tests for ESP and PK the imagination plays little part, and is normally by-passed for direct motor-action. But outside the laboratory the importance of the imagination is often brought out. One method of achieving success in telepathy seems to be—in rather unsatisfactory metaphorical terms—to make the mind a blank, and then allow images to drift into it; and, on the other hand, to achieve a desired result by supernormal means (i.e. PK, etc.), it sometimes seems to be sufficient to imagine it happening. In both cases, images play an important part, and there is nothing that can be termed an act of cognition in the one case or an act of willing in the other.

All this tends to show that there may be something unsatisfactory for psychical research in adopting without question our everyday categories of cognition and conation. It might be of advantage, at least for a while, to try to forget these divisions, and to make our thought and our language as non-committal as possible. On the analogy of 'psi-faculty' we might talk of alpha-, beta-phenomena, etc., giving a definition of the phenomenon as far as possible in positivist terms. There remains the danger that we would still regard these terms as nothing but new names for our old friends precognition, etc., but the more this danger is recognized and allowed for, the less formidable it will be. We may by this means be able to emancipate ourselves from our earlier ways of thinking, and allow the facts to dictate to us a new and more suitable scheme of reference.

PAMELA M. CLARK

### ERRATA

JOURNAL, NOVEMBER–DECEMBER 1950

IN the review of *Into the Unknown*, a description of the panel of investigators was printed as follows (p. 345): 'Three are members of the Society, Lord Amwell, Dr Bendit, and Mr L. A. G. Strong. The other five would probably disdain for themselves the description of expert in psychical research. . . .' The author of the review points out that he wrote 'disclaim', not 'disdain'.

In the last sentence of the review of *In Search of the Miraculous* (p. 352), 'hypothesis' should read 'hypotheses'.

We apologise for these errors.

## INDEX TO VOLUME XXXV

THE index to Volume XXXV of the *Journal* (January 1949 to December 1950) is being prepared and will be circulated with the issue for March-April.







# JOURNAL

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## SELECTIVITY IN ESP EXPERIMENTS

BY C. W. K. MUNDLE

I AM going to discuss a point raised by Dr Thouless in his contribution to the symposium on a programme for the next ten years of research which appeared in the *Journal of Parapsychology*.<sup>1</sup> Thouless wrote: 'I suggest investigation of the conditions of singularization. Positive results in ESP or PK experiments require that the subject should have reacted to (or acted on) one particular object or set of objects and not others of the same kind. The problem of how it happens that the experimental object is thus picked out seems to have attracted little attention except for the restricted case of what is called "psychometry."' In discussing this problem I shall use the word 'selectivity' instead of 'singularization', partly because it is more familiar, but also because it seems to me more neutral with respect to the question whether the role of the subject is active or passive. (We speak not only of people actively selecting things, but also of machines, e.g. radio sets, being passively selective.) I would state the problem, then, as being to ascertain the conditions which influence selectivity. I shall only consider the problem as it arises in ESP experiments. The problem appears much less conspicuous in PK experiments in which subjects have usually been able to look at the objects they were trying to influence.

Now one—and perhaps the most important—virtue of Carington's theory of telepathy is that it offers us an explanation of the subjects' selectivity. I know of no other theory which does this. Physicalist theories are notoriously incapable of explaining why a subject should respond to a radiation emanating from a certain brain (or pack of cards) rather than that from any other brain (or pack) within the same distance. On the other hand, to invoke

<sup>1</sup> 'A Program of Parapsychology', *J. Parapsychol.*, vol. 12, no. 2 (June 1948) p. 117.

'rapport' between subject and agent as a necessary condition of telepathy is, in the absence of a theory, merely to give a label to one of the facts which requires explanation.

As is well known, Carington explains telepathy by supposing (1) that an idea of the target object becomes associated in the mind of the agent (and/or that of an experimenter) with a second idea; (2) that this second idea (which he calls a K-idea) occurs, as a result of normal processes, in the mind of the subject; (3) that these two events tend to produce jointly a third event—the subject's thinking of the target object. On this theory, the factor which makes the subject select (or selective to) the target is the K-idea in the mind of the agent (or experimenter). It is difficult to understand how an idea could perform this role if it were one which was simultaneously being entertained by people not participating in the experiment; but Carington pointed out that there will always be one idea which will be entertained only by those who are participating, or, if entertained by non-participants, will not be so important to them—namely the idea of 'this experiment'. We must admit that Carington's theory does not take us as far as we would wish, for we still cannot understand how one person's associations could influence another person's experience in the conditions in question—we do not know whether any mediating processes need occur, nor what their nature would be. Nevertheless, our starting point in investigating selectivity ought surely to be an attempt to submit Carington's theory to the test of experiment. To do this I should suggest that we carry out some such experiments as the following:

(i) Using a subject who, like Mrs Stewart, has obtained good results with several agents, we could employ two or more of these agents working in opposition (i.e. attending simultaneously to different target-cards). At the start of each series each agent would be given a different code-name and/or picture to be associated with the ensuing targets, and one of these code-names would be selected at random and given to the subject. We should thus be deliberately providing a K-idea to supplement the idea of 'this experiment'. Precautions would be taken to ensure (a) that neither the subject nor the experimenter controlling the subject received any sensory clues regarding the other code-names in use and the allocation of code-names to agents; and (b) that neither the agents nor the experimenter(s) controlling the agents received any sensory clues regarding the code-name given to the subject. If we found a regular tendency for the subject to get significant scores only on the cards being thought of by the agent with the corresponding code-names, this would support Carington's theory.

(ii) Alternatively, or in addition, we could employ only one

agent, who would work simultaneously with two series of targets, each series being associated by the agent with a different code-name, one of which had been given to the subject.

It will be noticed that the former experiment is very similar in design to one which Dr Soal performed recently with Mrs Stewart. (This is described in § 33 of his Myers Lecture *The Experimental Situation in Psychical Research*.) Soal carried out two series, each of 400 trials, with two agents working in opposition. In the first 200 trials of each series Mrs Stewart was informed that there were two agents, but not that they were working in opposition. Dr Soal tells us that 'the pooled results on the 400 trials show that with neither agent was her score significant'. In the second 200 trials of each series, Mrs Stewart was told the name of one of the two agents and was given to understand that this agent would be working alone. In this case, the pooled results show a highly significant score on the targets of this agent with a chance score on the targets of the other agent. Now these results are just what Carington's theory would predict, but the support which they give to this theory is unnecessarily weak. (We must of course remember that Soal was not trying to test Carington's theory.)

Let us consider Soal's conclusion—that the results of the above experiments 'illustrate the importance of conscious orientation in telepathy experiments'. This statement is somewhat vague, for what is meant by 'conscious orientation'? Presumably Soal meant by this the subject's adjusting herself to the experiment by imagining the agent at work. Conscious orientation, in this sense, would involve her possessing knowledge of two kinds: (a) being acquainted with the person who acts as agent, i.e. having, by normal social intercourse, established a personal relationship with this person, or, at least, being familiar with the appearance of this person;<sup>1</sup> (b) knowing that it is this person (identified by name or other verbal description) who is acting as agent. Was knowledge of either of these kinds necessary or favourable to Mrs Stewart's success? Soal's above-mentioned experiments do not enable us to answer this question.<sup>2</sup> The main interest of Carington's theory,

<sup>1</sup> The subject might of course be *familiar with the appearance* of the person who acts as agent through having seen a photograph, without having seen this person 'in the flesh'. Whether the former is an adequate substitute for the latter is a matter for experiment. To simplify exposition I shall ignore this complication in what follows.

<sup>2</sup> Since this paper was written, a report of some long-distance experiments with Mrs Stewart has been published by Bateman and Soal in the *S.P.R. Journal* (Vol. XXXV, No. 659). The results are highly relevant to the question I am considering here; for in the Cambridge-Richmond series, in which she was not acquainted with the agents, Mrs Stewart failed completely, whereas in the London-Antwerp series, in which both

in this context, is that it suggests that such knowledge may not be relevant to a subject's selectivity. When Soal told Mrs Stewart the name of one of the agents (suppose it was 'John Brown') this name *may* have been functioning, solely as a K-idea, in Carington's sense; that is to say the name 'John Brown' may have made Mrs Stewart select (selective to) the targets being thought of by John Brown solely because these targets were associated in the mind of John Brown with his ideas of himself and of his own name. Mrs Stewart's being acquainted with this person called 'John Brown' and/or her knowing that it was this person who was acting as agent may have been irrelevant to her success.

It seems important to try to ascertain the relevance of the factors to which Soal seems to have been referring when he spoke of 'conscious orientation', for it seems to be commonly taken for granted that these factors are necessary, or at any rate favourable conditions of telepathy. I am not sure that I understand what psychical researchers mean by 'rapport', but I suspect that some, when they use this term, have in mind a theory to explain telepathy—a vague theory which might be expressed by saying that 'rapport' means 'direct psychic contact' between two (or more) persons whose bodies are not manifested to each other's senses. On this view, it would be natural to assume (i) that it is a necessary condition of the establishment of such rapport that the persons in question should be previously acquainted with each other, and/or (ii) that the more intimately they are acquainted the easier it will be to establish such rapport. Adopting Carington's theory would not imply that we should drop the word 'rapport'; we could still say that rapport is a necessary condition of telepathy, but rapport would become a much more precise concept, to be defined not vaguely in terms of 'direct psychic contact'—which, incidentally, suggests that it is a matter of all or none—but rather in terms of two (or more) minds entertaining the same (or similar) ideas, which is a matter of degree. On this view, a high degree of rapport might hold between people who were not acquainted with each other, yet the minimum degree of rapport—two people 'sharing' a single idea—might be a sufficient condition of telepathy, if, for example, the idea in question was important to each of them and was not 'shared' by anyone else. One reason why Carington's theory deserves serious attention is that there are

of the agents were her close friends, Mrs Stewart was highly successful. This strongly suggests that, for Mrs Stewart, being acquainted with the agent is a necessary condition of success. Unfortunately there was another factor which may, the experimenters tell us, have adversely affected the Cambridge-Richmond results, namely inadequate synchronisation of the stop-watches.

records of spontaneous cases in which 'conscious orientation' seems to have played no part whatsoever. I have in mind the type of case where a subject receives an impression of an accident involving no persons with whom the subject was acquainted or connected (e.g. the case discussed by Mr Tyrrell in *Proc. S.P.R.*, Vol. XLVIII, Part 173, pp. 89-91). In such cases I can think of no explanation of the subjects' selectivity except that provided by Carington's theory.

We require, then, to test the relevance to the subject's selectivity of the following factors :

(*x*) the subject being acquainted with the person(s) who act(s) as agent(s) ;

(*y*) the subject knowing (by name or other description) the identity of the person(s) who act(s) as agent(s).

I suggest, therefore, that the experiments which I proposed earlier be performed under each of the following conditions :

(1) with *x* and *y* both fulfilled.

(2) with *x* but not *y* fulfilled.

(3) with neither *x* nor *y* fulfilled.

We might for completeness try variation (4) i.e. with *y* but not *x* fulfilled. It is, however, difficult to see how the name (or a description) of an agent with whom the subject was not acquainted could be relevant *except* as a K-idea, in which case the only difference between conditions (3) and (4) would be that in the former case two K-ideas would be used, in the latter three. (I am counting the idea of 'this experiment' as a K-idea, as well as the code-name.) I should not expect conditions (3) and (4) to yield different results, but if they did it might be worth investigating whether the description of the agent which was given to the subject had to be a unique description (i.e. applicable to no one else) in order to affect the results. (In some cases a person's name is a unique description—Whately Carington's probably was, and this point may be relevant in applying his theory to his own experiments with drawings in which many of the subjects were not acquainted with him.)

One further factor which might with profit be varied in the above experiments is the subject's knowing the location of the agent(s). We already have one experiment whose results suggest that this factor is irrelevant (reported by McMahan and Lauer in the *Journal of Parapsychology*, Vol. 12, No. 1). This result requires confirmation.<sup>1</sup>

<sup>1</sup> Some further confirmation has now been provided in the long-distance experiments with Mrs Stewart. (Bateman and Soal, *op. cit.*, pp. 266-7.)

We may learn from such experiments that for different subjects different sets of conditions are necessary for or favourable to selectivity. If, however, we found any subjects able to select the appropriate target under conditions (3), this would be an important conclusion and one which would call for a thorough testing of Carington's theory in all possible respects. One of the next steps would be to test the sub-laws of the association theory—the laws of recency and frequency. Carington found quantitative evidence that these sub-laws had been fulfilled in some of his experiments with drawings, and this should be checked in other experimental situations. In connexion with the law of frequency, there would of course be two different factors to be investigated :

(a) the effect of repeating the conjunction of the idea of the target and the K-idea in the *same* agent.

(b) the effect of increasing the number of agents in whose minds these ideas are associated.

I shall now consider a possible criticism. It might be said that any adequate theory of selectivity must be applicable to clairvoyance as well as telepathy, whereas Carington's theory applies only to telepathy. This criticism raises two issues. The first is whether it is possible to discriminate between 'pure clairvoyance' and 'pure telepathy'. The second is whether Carington's theory could be applied to explain selectivity in cases of 'pure clairvoyance'. I shall briefly consider each of these issues.

Regarding the first issue, I am inclined to think that in their recent attempts to isolate 'pure telepathy' and 'pure clairvoyance' experimenters may have been pursuing a will-o'-the-wisp. At any rate, there is one difficulty which they do not yet seem to have considered. It is generally (and rightly) agreed that if, in a telepathy experiment, an agent or experimenter at any time mentions *aloud* the target-object or a code from which this might be inferred, this utterance, being a physical event, constitutes a potential object for the subject's clairvoyant faculty. It seems to be taken for granted that if an agent or experimenter merely thinks of the target or a code without mentioning it *aloud*, there is no physical event constituting a potential object for clairvoyance. This assumption seems to be unwarranted in view of the well-known fact that our thinking is usually (some psychologists say always) accompanied by certain physical events, notably the small movements of one's tongue and larynx which occur as one silently talks to oneself, and which systematically resemble the larger movements which occur in these organs when one utters the same words aloud. To admit this is not to adopt the (to my mind unacceptable) view which identifies thinking with sub-vocal



speech. But if we grant that sub-vocal speech movements are normal unconscious concomitants of our thinking, how are we to eliminate these as possible<sup>1</sup> objects of clairvoyance? In the recent 'pure telepathy' experiments in which the agent has employed a code relating target-faces to numbers, the agent has presumably, in the process of memorising the code, run over it several times in unspoken words. This difficulty may be surmountable—e.g. if the agent who uses the code only speaks a language not understood by the subject—but it is not my present purpose to explore such avenues. It is at any rate certain that attempts to isolate telepathy and clairvoyance involve us in some very complicated problems. We need not, however, postpone investigation of selectivity until each of these problems is solved or found to be insoluble. We may be able to advance a long way in understanding selectivity by means of experiments whose design is relatively simple. In the 'telepathy' experiments which I proposed above, I think we should not at present bother about trying to eliminate all possibilities of clairvoyance, but should employ the GESP<sup>2</sup> method.

Regarding the second issue, it does not follow that because Carington's theory was devised to explain telepathy, it could not be used to explain selectivity in cases of clairvoyance. The following questions can be distinguished and may require different answers :

(a) What makes the subject select (selective to) the object which an experimenter has decreed to be the target, e.g. this pack rather than another?

(b) How does a subject succeed to a significant extent in identifying the nature of the target-object, e.g. the order of the target pack?

(These questions could be distinguished, though it does not seem necessary to do so, in dealing with the facts usually attributed to telepathy.) When these questions are distinguished in considering clairvoyance, the former can be answered by saying that the subject

<sup>1</sup> Professor H. H. Price has pointed out to me that lip-reading has to be learned and is difficult to learn, and that the sort of clairvoyant larynx-reading which I am postulating would be much more difficult to learn. This must be granted. All that I am claiming is that since this sort of larynx-reading cannot be ruled out as impossible, this undermines the view that recent experiments have re-established conclusively the occurrence of 'pure telepathy'.

<sup>2</sup> GESP stands for 'General Extrasensory Perception', and is applied to experiments in which the target cards lie successively before the agent's eyes. Thus the subject may get his information either by clairvoyance from the cards themselves or by telepathy from the agent's perception of the cards.

selects the pack in question because it is (most strongly) associated in the mind of an experimenter with his idea of 'this experiment'.<sup>1</sup>

Now obviously we can test the applicability of Carington's theory to the selectivity of subjects under conditions which have usually been called clairvoyant. Let us not bother at this stage about excluding all possibilities of telepathy, but employ a method like DT<sup>2</sup> calling. We can arrange an experiment in which the experimenter controlling the target (E C T, as we may call him) uses two or three alternative target packs, each associated by him (and only by him) with a certain code-name, while the subject, located elsewhere, is given one of these code-names. If such experiments yielded the predicted results, there are several factors which it would be interesting to vary one at a time :

- (1) the subject being acquainted with E C T.
- (2) the subject knowing by description the identity of E C T.
- (3) the subject knowing the location of E C T.
- (4) E C T knowing the location of the target pack corresponding to each code-name.

Since the above was written, an important article by Soal and Bateman has appeared in the *Journal of Parapsychology* (September 1950) giving a detailed report of all the experiments performed with Mrs Stewart up to July 1949 in which two or more agents have been used in opposition or in conjunction. Much of this material has not been previously published, and the results display some new and interesting features, notably :

(i) When Mrs Stewart has been working successfully with agent No. 1, and agent No. 2 is introduced acting in opposition to No. 1, Mrs Stewart continues to work with No. 1 and 'ignores' No. 2. This feature recurred consistently in a series of experiments in which Mrs Stewart was fully informed as to the identity of the agents and their roles and in which agent No. 2 was a person with whom Mrs Stewart had previously worked very successfully.

<sup>1</sup> Professor Price has pointed out to me that what I say here would apply only in the experimental situation, for in a case of spontaneous clairvoyance there would be no experimenter decreeing that a certain object is the target. I should, however, recommend that in examining spontaneous cases of apparently pure clairvoyance we should look for a person who might be performing this function of the experimenter, i.e. a person in whose mind the clairvoyantly perceived object might be associated with a K-idea. Could we be certain in any given case that no such person existed?

<sup>2</sup> DT stands for 'Down Through', and is applied to experiments in which the subject guesses the order of the pack from top to bottom, the order not being observed by anyone until the subject's guesses are completed. This method does not eliminate precognitive telepathy.

(ii) It appears that when several agents are used in conjunction (i.e. each attending simultaneously to the same target), Mrs Stewart works with only one of these agents and 'ignores' the others. This very interesting conclusion is an inference based on two types of experiments :

(a) in which a single agent is used for the first 200 calls and a second agent acts in conjunction with the former for the second 200 calls. There were three experiments of this design. In two of them Mrs Stewart got a significant score for the first 200 calls, and the conjunction score for the other 200 was at approximately the same level. In the third case, the person who was used as agent No. 1 appears to have been a bad agent (independent evidence of this was obtained). The scoring for the first 200 calls was at chance-level, but was significant for the second 200.

(b) in which the same two (or three) agents are used throughout a block of 400 calls, their roles alternating every 50 calls between opposition and conjunction. If we take the first four experiments of this type, we find that when the agents were in opposition Mrs Stewart obtained in each case a significant score with one agent and chance scores with the other(s). If we take the pooled results of these four experiments, we find that the conjunction score and the highest opposition score are both highly significant. The conjunction score is in fact the lower (209/800 against 231/800)—*but there is no significant difference between these scores* ( $P=0.16$ ).

One cannot do justice in such a brief summary to the material contained in this article. However, these latest results do not, so far as the writer can see, provide any grounds for choosing between the two theories of 'rapport' which have been discussed earlier—the theory of 'conscious orientation' on the part of the subject, and the theory suggested by Carington. The disclosure of these new facts does, however, accentuate the problem discussed in this paper, and provides a further challenge to us to try to ascertain the factors which influence selectivity in psi-phenomena.

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PERROTT STUDENTSHIP IN PSYCHICAL  
RESEARCH AT TRINITY COLLEGE,  
CAMBRIDGE

The following communication has been received from the Electors to the Perrott Studentship in Psychical Research :

The Electors to the Perrott Studentship are prepared to receive applications from candidates.

Psychical Research is defined, for the purpose of the Studentship, as 'the investigation of mental or physical phenomena which seem *prima facie* to suggest (a) the existence of supernormal powers of cognition or action in human beings in their present life, or (b) the persistence of the human mind after bodily death'.

The Studentship is open to any person who shall have completed his or her twenty-first year at the time when the election takes place. A Student may be re-elected once, but not more than once.

The Studentship is tenable for one year, and the Student will be required to devote a substantial part of the period of his tenure to investigating, in consultation with a Supervisor to be appointed by the Electors, some problem in Psychical Research. The Student shall not, during his tenure of the Studentship, engage in any other occupation to such an extent as would in the opinion of the Electors interfere with his course of research. Residence in Cambridge is not required.

The Studentship will be of such value, not exceeding £300, as the Electors may award after considering the nature of the research which the candidate proposes to undertake.

Applications from candidates should be sent to THE SECRETARY, PERROTT STUDENTSHIP ELECTORS, TRINITY COLLEGE, CAMBRIDGE, not later than 30 April 1951. Intending candidates should write to the Secretary for further details before applying.

The election to the Studentship will take place in the Easter Term of 1951, and, if a candidate be elected, his tenure will begin at Michaelmas following the election.

The Perrott Studentship was established in 1940 out of a bequest left to Trinity College, Cambridge, for that purpose by Frank Duerdin Perrott as a memorial to Frederic W. H. Myers. Myers, like Edmund Gurney and Henry Sidgwick, also founders of the Society, was a Fellow of Trinity.

The Studentship was first held by the late Whately Carington, who began his tenure in the Michaelmas Term of 1940 and who was engaged in experiments on the paranormal cognition of drawings. The Studentship was next held by Dr S. G. Soal for a year from Michaelmas 1948, during which time he carried out ESP research with his subject Mrs Gloria Stewart.

## REVIEWS

PSYCHICAL RESEARCH, ETHICS AND LOGIC. Supplementary Volume XXIV to *Proceedings* of the Aristotelian Society. London, Harrison & Sons, 1950. 231 pp. 21s.

At the annual joint meeting of the Mind Association and the Aristotelian Society, held at Bristol in July 1950, there was a symposium on the relevance of psychical research to philosophy. The three contributors to it were Mrs M. Kneale, Mr R. Robinson, and Mr C. W. K. Mundle. Their papers will be found on pp. 173-231 of the present volume. Before discussing them, it may be well to point out that during the last twenty years there has been a change in the attitude of professional philosophers towards their own subject. For good or ill, many of them now think that the traditional problems of philosophy are in the end problems about language, and are to be solved (or else shown to be nonsensical) by methods of linguistic analysis. This linguistic conception of philosophy has not yet spread very far beyond professional circles, and may not be familiar to all readers of the *Journal*.

In the first paper, Mrs Kneale argues that even if we accept the linguistic conception of philosophy, the facts discovered by psychical researchers are still relevant to philosophical problems; what they show is that common sense and scientific terminology are not, in all respects, adequate for the description and explanation of all known empirical facts (p. 176). Thus the physical phenomena of psychical research call for some revision of the terminology of physics, as the phenomena of magnetism did long ago. Again, the mental phenomena show that a purely Behaviouristic terminology is inadequate for describing all occurrences in human beings. When we consider telepathy, we can see that an 'inner life' terminology is needed as well. PK shows us that the old problem of the relation of mind and body is not so dead as Behaviourists think; it compels us to enquire into the relation between the 'inner life' terminology and the 'material object' terminology. It follows that the hypothesis of survival is not meaningless, as an over-indulgence in Behaviouristic terminology might lead us to suppose. But in all these cases, Mrs Kneale thinks, the chief service of psychical research to philosophy is to save us from 'forgetting the obvious'. Thus, it ought to have been obvious to anyone who reflects on mental images, or hallucinations, or eidetic images, or voluntary action, that an inner life terminology is needed in any case. Psychical research mainly

provides an *a fortiori* argument for something we ought to have known already.

I think that Mrs Kneale, in her laudable endeavour to conciliate the Linguistic Analyst, has over-stated her case. Can this be the only lesson which the phenomena of clairvoyance have for philosophers? Perhaps—if clairvoyance were at all analogous to ordinary sense-perception. But surely it is not. (Mrs Kneale merely mentions clairvoyance in passing and does not discuss it.) And what of precognition? Here again Mrs Kneale takes the same over-modest line. She points out that precognition is not foreknowledge, but only fore-imaging. She thinks, or at least hopes, that no very radical revision of the conceptions of Time and Causation will be needed; and she is sure that the phenomena of precognition give no support to mystical doctrines or to theories about 'the nothingness of Time'. Even so, can it really be maintained that precognition merely provides us with an *a fortiori* argument for accepting a terminology which we ought to have accepted in any case? On the contrary, it seems to break the rules of *all* the existing terminologies, physical or psychological, introspective or Behaviouristic. It is not altogether surprising that Mr Robinson accuses Mrs Kneale of 'damning the importance of psychical research for philosophy with faint praise'.

Mr Robinson himself, however, thinks that even this faint praise is unjustified. If the linguistic conception of philosophy is accepted, he can find only one thing in the literature of psychical research which could instruct a philosopher; and that is the terminology used by psychical researchers themselves. For example, when one calls a coincidence a case of telepathy one is not explaining it; one is denying the possibility of explaining it. According to Mr Robinson, the whole point of a phrase like 'ESP' is to denote a process in which information is acquired by no *means* at all. Again, Mrs Kneale does not explain precognition by calling it 'fore-imaging'; she is only telling us that there is *no* explanation of the fact that Mr A. had an image resembling X before X actually occurred. And supposing we did some day discover the means by which telepathy occurs, most psychical researchers would cease to be interested in telepathy. For it is only 'meansless' cognition which is relevant to the metaphysical and theological theories which most of them wish to maintain.

But what if we reject the purely linguistic conception of philosophy? (Mr Robinson, himself, I think does wish to reject it.) Then he is willing to admit that psychical research is relevant to philosophy in two important ways. First, if supernormal cognition does really occur, we shall have to conclude that Empiricism is false. Empiricism tells us that no synthetic proposition is to

be accepted unless it is supported by the evidence of the senses ; and in ESP, if it does really occur, we are acquiring factual information *without* the evidence of the senses. (As Mr Mundle points out later, it would be very odd if Empiricism could be refuted by empirical facts. Mr Robinson's definition of Empiricism is surely too narrow. In any case, the classical Empiricists also laid great stress on the evidence of introspection. Mr Robinson seems to have forgotten the importance which Hume attached to 'impression of reflection', i.e. introspective data.)

Secondly, Mr Robinson also admits that psychical research is relevant to what he calls 'the theory of man'. In his view, we have to choose between two philosophical theories of human nature: on the one hand, the Platonic theory, i.e. the dualistic interaction theory, which regards mind and body as two distinct entities ; on the other, the Aristotelian theory ('the soul is the form of the body') whose modern form is philosophical Behaviourism. By the end of the nineteenth century, it seemed that the question was settled for good and all. The biological sciences had apparently shown that Plato was wrong and Aristotle right, to the confusion of the theologians and other religious-minded persons. But then it occurred to the Platonic party that the scientists might be defeated by their own weapons—by appeal to empirical facts. Their plan was to re-establish the Platonic theory of human nature, not by theological or metaphysical arguments, but by scientific ones. And that, according to Mr Robinson, is what psychical research consists of. Accordingly, he is not surprised to find that psychical researchers attach so much importance to 'the attempt to communicate with men who are not at the time attached to any specimen of *homo sapiens*' (p. 202).

Mr Robinson admits that the psychical researchers have succeeded in collecting a number of 'queer coincidences'. But he still adheres to the Aristotelian theory of human nature. Is this irrational? He claims that it is not. No one can be expected to explain all the queer coincidences which happen from time to time. And the psychical researchers themselves cannot explain them ; to label them 'telepathy', 'precognition', etc., is not to explain them. If one accepted the Platonic theory of human nature on such grounds, one would be falling into the fallacy called the argument from ignorance—'this proposition must be true because we do not know for certain that it is false'.

I cannot but think that here Mr Robinson proves too much. If he were right, no theory could ever be refuted, or even weakened, by adverse empirical evidence. The adverse evidence could always be written off as just a 'queer coincidence'—inexplicable but needing no explanation. It does not follow either that if the

'Aristotelian' theory were refuted by empirical evidence, the 'Platonic' theory must be true. There may be other alternatives. For example, the Buddhist analysis of human nature is neither Platonic nor Aristotelian.

In the last page or two of his paper, Mr Robinson takes back his admissions that the coincidences collected by psychical researchers are at any rate queer. On the contrary, the emotional aura which they have for him is 'boredom and banality'. An alleged communication from Henry Sidgwick about the relation of mind and body strikes him as 'here and there ridiculous but otherwise a dead bore' (p. 204). To him, the reports of psychical researchers are like dead clichés or pointless schoolboy jokes. He cannot go to five tea parties without hearing at least one 'super-normal' story—fully authenticated, of course. In his view the trouble with the data of psychical research is not that they are too strange and queer to be believed; on the contrary, they are so insufferably tedious that one can hardly give one's attention to them.

I think that this sort of emotional reaction is not at all uncommon, especially among highly-educated people; and psychical researchers should give more thought to it than they have, if they want to understand and remove the 'resistance' which their data often meet with. Indeed, they would do well to read the whole of Mr Robinson's paper with care. It is a very good statement of views which are widely held, but not nowadays often expressed.

Mr Mundle's paper is very interesting indeed. But it is also very long and cannot be adequately summarised in a paragraph or two. I shall confine myself to four of the most important points in it.

First, when we ask whether psychical research is relevant to philosophy, what do we mean by 'psychical research'? According to Mr Mundle, we may mean (1) the observational data, or (2) the primary hypotheses, or (3) the speculative theories which have been suggested for co-ordinating the primary hypotheses. If we mean the observational data, he thinks that these *can* be described in Behaviouristic terminology, and indeed, must be. The experimenter's data consist in 'the percipient's overt behaviour and the written record produced thereby' (p. 209). And even in spontaneous cases, the private experiences of the percipient are not evidence, unless they result in some kind of overt action which can be confirmed by witnesses or documents. 'Psychical researchers accept as evidence only what Behaviourists can acknowledge as hard facts' (p. 209). I should have thought, on the contrary, that in the spontaneous cases the percipient's



overt actions are only of interest because they tend to show that the *private* experiences did, in fact, occur at or about the time when the percipient says they did.

Secondly, with regard to the relation of mind and body, some philosophers think that psi-phenomena, if they prove nothing else, do at least prove that Epiphenomenalism is false. Mr Mundle does not agree. For example, what we take to be telepathy may be a causal transaction between a *brain* event in the agent and a mental event in the percipient (it would then be a sort of clairvoyance). And surely this account of the matter is compatible with Epiphenomenalism? I think that it is only compatible with a new and extended version of Epiphenomenalism. For in ordinary Epiphenomenalism the cause of a mental event in Mr A. is an event in Mr A.'s own brain, not in someone else's. In any case, it is hard to see how precognition could be compatible with any form of Epiphenomenalism. And clairvoyance of the ordinary kind, where the 'object' is not a brain event but some non-physiological entity like a Zener card or a sealed-up letter, would not be compatible with it either.

Thirdly, Mr Mundle asks us to consider a new way of classifying psi-phenomena, though he does not ask us to accept it. In the new classification we abandon the terms 'mental' and 'physical', and replace them by 'human' and 'non-human' respectively. Then we have (1) non-chance relations between human events and other human events in the same or in different persons; (2) non-chance relations between human events and non-human events. The first head would cover 'inter-personal' PK, as well as telepathy. The second would cover ordinary PK, clairvoyance, and poltergeists.

This new classification would no doubt save us from begging questions about the relations between mind and body, and would please the Behaviourists whom Mr Mundle is so anxious to conciliate. But there are disadvantages in it too. Precognition would fall partly under the first head and partly under the second; and the most important thing about it—namely, its reference to the future—would tend to escape our notice. Again, the Survivalist explanations of mediumistic communications would be ruled out by definition, unless events in discarnate minds are counted as 'human events'; and if they were, the advantage Mr Mundle hopes to gain, of conciliating Behaviourists and other 'tough-minded' thinkers, would be lost. Moreover, there is some evidence that ESP occurs occasionally in non-human animals; and this would be ruled out also.

Fourthly, Mr Mundle distinguishes between two ways of interpreting psi-phenomena of the 'mental' kind (to return to the

traditional classification). In the one, the fundamental concept is *causation*; in the other, it is *cognition*. If we accept the causal interpretation, he thinks we shall have to re-define the term 'causal connection'. At any rate, we must *not* so define it as to make spatio-temporal contiguity part of its meaning, nor in such a way that the proposition 'a cause precedes its effect' is analytic. Moreover, 'is happening' must not be so defined that it is equivalent to 'is coming into existence'. For in precognition (on the causal interpretation) a future event has effects. It must therefore be in existence, though it is not yet happening.

Mr Mundle then points out that psychological researchers *have* produced explanations of the queer occurrences which they study, despite Mr Robinson's denials. They have done it, as other scientists do it, by postulating 'interphenomena' to make the observed phenomena intelligible. Thus they try to explain telepathy by the theory of a Common Unconscious, i.e. by postulating mental interphenomena; and they conceive the Common Unconscious as an inter-personally effective store of traces or engrams. Mr Mundle does not think much of this explanation. He objects that there is no reason to call this inter-personal, trace-bearing medium 'mental'. Perhaps not. But if it is physical, it will not do what is wanted, unless we go back to a Radiation Theory of telepathy, which Mr Mundle has already rejected (pp. 221-2). I think he has ignored another relevant point. It may be argued that an unconscious of some kind has to be postulated in any case, in order to explain the data of abnormal psychology; and we call it 'mental', *faute de mieux*, because our explanation will only work if processes analogous to conscious planning are supposed to go on in it. If so, it is economical to make use of the postulated unconscious for explaining psi-phenomena too (since we have to put up with an unconscious in any case); and we can do this by supposing that processes in the unconscious are inter-personal. Moreover, the Freudian mechanisms of repression and symbolic distortion are ready to our hands for explaining the queer and devious ways in which psi-contents manifest themselves in consciousness and in behaviour.

However this may be, Mr Mundle thinks we may do better if we interpret psi-phenomena cognitively instead of causally. If cognitive relations really do span spatial and temporal gaps, as philosophers suppose, will not this give us just what we want? The cognitions we should have to postulate would be themselves uncognized (i.e. subliminal); so they too would be 'interphenomena'. I am not sure that this cognitive explanation really differs so much from the causal explanation which Mr Mundle has rejected. Once we descend below the conscious level—as we

have to, on both theories alike—the distinction between ‘ being an awareness of ’ and ‘ being an effect of ’ seems to lose much of its sharpness ; and the lower we descend (if this spatial metaphor is appropriate) the less sharp the distinction becomes. Moreover, if we apply the ‘ cognitive ’ theory to telepathy, we shall have to allow that there is such a thing as unconscious *inspection* of the contents of another mind ; and this surely is something for which we can find no analogy in the sphere of conscious cognition.

Mr Mundle leaves it to the audience to decide whether these issues are among the ones which philosophers ought to discuss. But he himself clearly thinks that the answer is ‘ yes ’ ; and he is clearly right.

H. H. PRICE

HARRY PRICE : THE BIOGRAPHY OF A GHOST-HUNTER. By Paul Tabori. London, Athenaeum Press, 1950. viii, 316 pp. 11 plates. 15s.

Harry Price’s long and varied career as an investigator of the physical phenomena of psychical research deserved a biographical record, and the choice of Mr Tabori to compile it was a happy one. In his skilled hands a great number and variety of incidents are described in an interesting way. He lays proper stress on Harry Price’s wide reading, unflagging energy, mechanical skill, and knowledge of methods of deception, but he makes it clear that he is not blind to some peculiarities of temperament which brought Harry Price into conflict not only with persons whose views of psychic phenomena were opposed to his own, but with others who shared in general his opinions and interests and were anxious to co-operate with him. In fact, for the official biography of a man recently dead, the book is remarkably and commendably frank.

The book is very largely based on ‘ the vast correspondence he [Harry Price] carried on, much of it unpublished and unpublishable ’, but for an informed judgment on some of the controversial matters dealt with by Mr Tabori there are other documents that the reader would be well advised to consult. For example, as to Harry Price’s proposals for the amalgamation of the self-styled ‘ National Laboratory of Psychical Research ’ with the S.P.R., he should read our *Journal*, vol. XXVII, p. 25, and for the much debated ‘ exposure ’ of Rudi Schneider he should put side by side the letters to Rudi printed on pp. 113 and 116 of this book with the facts stated by Lord Charles Hope in S.P.R. *Proceedings*, Vol. XLI at pp. 284–91. We regret that Mr Tabori appears to endorse Harry Price’s preposterous claims to have contributed by

his researches to the foundation of the Perrott Studentship at Trinity College, Cambridge, and the acceptance by New College, Oxford, of the reversion of the Blennerhassett Trust.

Mr Tabori pays a graceful tribute to the help he received in preparing the book from Mrs Goldney and Dr Dingwall, who are familiar with many of the events related in the book. On some of these, particularly the Borley case, a more complete report based on a wider range of evidence than seems to have been accessible to Mr Tabori would be welcome. May we hope that they will see their way to provide it?

W. H. S.

WILLIAM JAMES: A SELECTION FROM HIS WRITINGS. Edited with a commentary by Margaret Knight. Harmondsworth, Penguin Books, 1950. 248 pp. 1s. 6d.

Members of the Society for Psychical Research will be interested to read this book on our distinguished former president, Professor William James. James was a vivid and self-revealing writer and the greater part of the book is composed of judiciously chosen extracts from his own writings. These are preceded by a biographical account which helps one to understand the family situation that made James the intellectual adventurer that he was. While the extracts are mostly from his psychological writings and not from any contributions he made to psychical research, the biographical sketch and the writings make clear why he was attracted to psychical research. Born into an argumentative family, he was always impatient of accepted orthodoxies and had a quick eye for the unexpected fact. That psychical research did not fit into the scheme of the biological psychology which he himself taught was for him a reason for being interested in it rather than ignoring it as a more conventionally minded man might have done. As in his study of religion in his Gifford Lectures, he brought to bear on psychical research an inquiring and open mind with a tendency to scepticism which led him to question the dogmas of orthodox science no less than any new and unexpected phenomena.

R. H. THOULESS

ENTHUSIASM: A Chapter in the History of Religion with special reference to the XVII and XVIII Centuries. By R. A. Knox. Oxford, Clarendon Press, 1950. viii, 622 pp. 30s.

'When strong currents of spiritual emotion, aroused by some religious crisis, sweep through a multitude of human hearts,

physical reactions of an abnormal kind are liable to occur as their byproduct' writes Mgr Knox in this learned and stimulating study of fanatical sectarianism over eighteen centuries. The physical reactions displayed in some of the revivalist movements chronicled are, indeed, odd enough; men and women are shown expressing their sentiments by leaping, shaking, rolling, trembling, talking gibberish, swelling up, and (my own favourite) 'barking demurely' at those who preached to them.

As bizarre, and of greater interest to psychical research, are the borderline phenomena which sometimes accompanied these manifestations. The most notable are perhaps those seen in the prophesying of the Camisard Huguenots after the Revocation of the Edict of Nantes; and those of the Jansenist convulsionaries of Saint-Médard, of which Dr Dingwall has written a short account.<sup>1</sup>

A particularly fascinating point about the earlier movement is that its 'prophets' were apparently taught some sort of psychophysical technique. 'M. du Serre . . . collected fifteen children of either sex from the peasantry of the Vivarais, and trained them in the art of prophecy. His school was at Mont Peyra in the Dauphiné . . . he initiated his pupils, exercised by two or three days' fasting, into the four grades of prophecy' which were known as 'l'Avertissement, le Souffle, la Prophétie and le Don'. Those interested in the natural history of contemplation may find it significant that a pupil who had reached the stage of 'le Don' frequently abandoned his former preaching activities; activities whose normal pattern ran thus: 'The prophet beat his head with his hands for some time, then fell down on his back' (sometimes accompanied by his more impressionable congregations) 'his stomach and throat swelled up, and he remained speechless' for some time, then 'broke out into utterance.'

The five or six hundred children between three and fifteen who were trained at this school seem, however, to have shewn more interesting results than this; there is evidence that 'some drove knives into themselves', exposed themselves to fire, and fell from rocks twelve feet high without being injured, while others shewed that they possessed extrasensory powers, narrating events which were taking place at a distance, and accurately foretelling the future. The whole episode, with its apparently deliberate organization and training of children below the age of puberty, is curiously reminiscent of what is said about the psychical techniques of the East.

The Saint-Médard affair has a more spontaneous ring. It began quite simply with the cure of a number of sick people at the tomb of the Jansenist ascetic, François de Pâris. Then, after a convul-

<sup>1</sup> *Some Human Oddities*. London, Home & Van Thal, 1947.

sion had accompanied the cure of a paralytic girl, convulsions became the general rule. Mgr Knox points out that they were at first regarded as the concomitants of healing ; but that quite soon their occurrence became an end in itself, as an evidence of the miraculous. Later still came the *secours* ; this was at first regarded as a counter-irritant to the pain of the healing convulsion ; then, and with greater reason, it too was held as proof that the supernatural was at work. The *secours*, for which the convulsionaries might crave as a source of relief, took a variety of forms. The sufferer might ask to be trodden on ; to be subjected to the pressure of heavy weights ; to be beaten ; to be prodded by sharp objects, such as swords or spits (which, curiously enough, very often pierced the clothes, but never the skin) ; or to be crucified.

The evidence for these occurrences is very good, in so far it was collected and recorded by persons who would much rather have believed them to be faked ; for the logical justification of these 'miracles' was to provide support for the Jansenist teachings, and to protest against the enforcement of the Bull '*Unigenitus*' which condemned these ; and they were chronicled by those who held the opposite view. Here again, some of the actual phenomena—the rotation of sword blades within the sockets of a sufferer's eye, the prolonged 'whirling' of other sufferers, the insensibility to pain—recall stories of Eastern fakirs, and of the dancing dervishes of North Africa. It may also be remarked that, as in the case of Rasputin, some of the healings later performed had little to do with the personal sanctity of the healer ; thus, for instance, the numbers of cures of ulcers and abscesses attributed to one particular woman shewed no diminution when she took to sexual irregularity. Healing is obviously a less demanding occupation than lion-taming, for which chastity is essential.

RENÉE HAYNES

HYPNOTISM AND THE POWER WITHIN. By S. J. van Pelt, M.B., B.S. London, Skeffington, 1950. 208 pp. 18 plates. 18s.

Dr van Pelt, President of the British Society of Medical Hypnotists, has written an account, clearly intended for the general reader, of hypnotism and its use in the treatment of neurotic ailments. He has also taken the opportunity to deliver an attack on psycho-analysis, prefrontal leucotomy, electric convulsion therapy and other methods of shock treatment, Christian Science, and stage hypnotism. Except to express some surprise at the violence of his offensive—especially on the founders of psycho-analytical theory, to whom he denies credit for any contribution to our knowledge—we are not concerned with these matters here.

Dr van Pelt devotes several pages to the argument that the mediumistic trance is identical with the hypnotic trance. Although the two states have many features in common, it would be premature to say that they are identical. Some evidence pointing to a difference between them has been provided by the electroencephalograph. In the hypnotic trance, the E.E.G. shows the same pattern as the waking state. Experiments carried out by J. L. Franke, a neurologist of Haarlem (see *Tijdschrift voor Parapsychologie*, 1938, 10, 111-17), led him to the conclusion that the brain appears to be 'asleep' in the mediumistic trance. It is also of interest that Thomson, Forbes, and Bolles, with a subject who was able voluntarily to induce light trance states in himself, found an E.E.G. which was characteristic of light sleep (see *Amer. J. Psychiat.*, 1937, 93, 1313-14). The E.E.G. was, however, then in an early stage of development, and there is need to repeat this work with modern and more reliable models. The present writer has been unable to trace any more recent attempt to obtain the objective evidence of the E.E.G. on this point.

Intending readers should be warned that the book is exasperatingly repetitive, and that (in common with so many other works published by the firm of Hutchinson and its associated companies) there is no index.

E. O.

DIANETICS: THE MODERN SCIENCE OF MENTAL HEALTH. By L. Ron Hubbard. New York, Hermitage House, 1950. xxvii, 452 pp. \$5.00.

This book is certainly a work of genius. The author claims to have discovered, by following the Scientific Method, not only how the mind works in health but also the one true cause of all psychological illness. He presents a new method of psychotherapy which he guarantees will cure all cases of such illness without fail, and will further transform normal man into superman.

The genius of the author lies, however, not in the painstaking and accurate manner in which he has accumulated, sorted, and presented his evidence, for there is none—not one scrap; nor in his powers of invention and salesmanship, magnificent though they are (his self-assurance and arrogance are superb); but it is in the fact that he has been able to produce a system of psychology based on the most obvious untruths which has nevertheless, in America, competed with and to a large extent defeated all rival systems of redemption. The author must have realized that people in these times will listen to any Messiah who claims complete authority in the name of Science and will relieve them of the

burden of being morally responsible for their actions. Mr Hubbard's system has the further advantages of undercutting its rivals in cost, and in the fact that anyone can practise it. The sole qualification seems to be the purchase of Mr Hubbard's book ; this of course saves the tiresome business of an expensive medical training.

Dianetics consists of : (i) rewriting a lot of platitudes in brand new jargon ('Aberree' is a good example) ; (ii) large quantities of Freud rewritten in a mixture of current teen-age American and electronic terminology ; (iii) the crib of the engram.

The basic idea in dianetics is that the mind consists of two sections : (a) the Analytic Mind which works as a perfect computing machine ; and (b) the Reactive Mind, a jazzed-up version of that well-worn vehicle, the unconscious, which causes all our troubles by interfering with (a). It appears that whenever we are unconscious, asleep, injured, drugged, or *in utero*, the 'reactive' mind is wide awake, picks up all painful stimuli, and stores them as impressions on the actual protoplasm of the brain cells. These impressions are the engrams, and cause the bulk of our troubles from ulcers to war. Dianetic therapy exorcises them, and superman, the perfect analytical machine, is produced. The patient is relieved of all responsibility for his actions since he can either blame the engrams or function happily in the iron determinism of a perfect machine.

The technique of Dianetic Therapy, though this fact is heavily disguised, consists of powerful suggestion applied during a light hypnotic trance to a gullible subject. The subject, having had the theory of engrams explained to him, obligingly 'recalls' infantile or pre-natal experiences, with cathartic accompaniment.

Mr Hubbard's dismissal of ESP as an explanation of the alleged ability of dianetic therapy to recall pre-natal experiences is, of course, otiose. Indeed, his statement that one 'should not accept telepathy any more than he would accept ESP' (p. 321) to account for the 'phenomena' is a measure of the half-knowledge and confused thinking which pervades the book.

Dianetics is the Mumbojumbology of Dr Rhubarb come to life. Mr Hubbard might well have taken his cue from the following extract from Beachcomber's column published in the *Daily Express* a few months ago :

One or two people have asked what is meant by Mumbojumbology, of which Dr Rhubarb is the foremost living exponent. It means idiotic veneration. The Abracadabrists, led by Bottelburg, discovered that any drivel uttered repeatedly, and in an authoritative tone, will be accepted today with the fervour formerly accorded to the worship of an African idol. But Dr Rhubarb widened the appeal of the new philo-



sophy by relying less on completely meaningless sentences than Abracadabraism had done. He evolved a system of half-meanings uttered in a jargon of bastard Latin and Greek words. He also concentrated on clothing obvious lies in pseudo-scientific gibberish.

The depressing aspect of the whole shameful affair is that half a million Americans, some of whom are sincere and intelligent people, have fallen for it.

J. R. SMYTHIES

JOURNAL OF THE AMERICAN SOCIETY FOR PSYCHICAL RESEARCH.

Vol. 45, No. 1, January 1951. New York, A.S.P.R., \$1.50.

Mrs Lydia Allison contributes an obituary of Mrs Leonora Piper, the well-known 'mental' medium who was at the height of her powers in the eighteen-nineties. Dorothy A. Berg in a useful review compares and contrasts various theories that have been put forward concerning the *modus operandi* of trance communication.

#### WATER DIVINING

The names L. A. Dale and Gardner Murphy at the head of a report guarantee a carefully designed research project. They have collaborated with R. M. Greene, W. Miles, J. M. Trefethen (Professor of Geology, University of Maine), and M. Ullman, M.D., in 'Dowsing: A Field Experiment in Water Divining' (pp. 3-16). It is, as far as I know, the first adequately planned and supervised field experiment in which dowsers and professional geologists have been matched on equal terms. It was *not* the purpose of the experimenters 'to present an evaluation of the evidence for and against the general claims of water diviners or of the conclusions reached by other investigators. . . . We should not wish to be interpreted as believing that we have contributed any findings which radically change the overall research situation'.

The problem which they set themselves was simply: 'Can water diviners find water under conditions in which the professional geologist is unable to do so?' It was necessary to choose a level terrain without surface water or wells which would betray the presence of underground water, and in which digging or drilling could be used without great expense to check the results. A search led to the selection of a reasonably satisfactory tract of land near Liberty, Maine, and an advertisement was inserted in four Maine newspapers.

WATER DIVINERS—Dowsers, those who can find water with forked stick, are invited to take part in search for water in open country near Liberty, Maine, on August 4th, 5th, or 6th [1949] for the benefit of scientists

interested in this power. Travel expenses and \$12 for the working day available; also prize for outstanding success. Those interested write to: Gardner Murphy, Ashland, N.H.

Twenty-seven dowsers were finally selected (22 men, 4 women, 1 adolescent girl). Details of the life history and of the dowsing history of each were noted. At the close of the interview the diviner was conducted to a spot near but out of sight of the test ground, where he was blindfolded. Most of the diviners brought their own sticks or rods with them, and it is interesting to note that not one of them subscribed to a psychic or psychokinetic theory of dowsing; all believed it to be an as yet unexplained *physical* phenomenon.

Each dowser was led over the site first with blindfold and then without. Records were taken of his method of working, and of the number, intensity, and form of the rod-movements. As a rule, there were several points with blindfold and several without at which the rod turned and an attempt was made to get the dowser to specify depth and rate of flow at each point; most were very reluctant to specify rate of flow except vaguely as 'good' or 'poor'. The dowser was asked to name his 'best' spot chosen with blindfold and his 'best' without blindfold, and for these two spots a quantitative estimate of rate of flow and depth was insisted on.

Subsequently Trefethen, the Professor of Geology, and a local water engineer made their own estimates of the depth and rate of flow at sixteen pre-arranged points. The experts quite properly made use of their knowledge of the existence of a nearby well: '... knowing that the water in soft soil must be nearly level, they could properly apply their information. The dowsers, thinking and working in terms of "veins" of water (which did not of course exist) could hardly make use of the fact of the well even had they known about it'.

Then pipes were driven down to determine the depth, and rate of flow was estimated by pumping timed with a stop-watch. This was done (with certain unavoidable exceptions) at each indicated point. The results are illustrated graphically, as 'scatter' diagrams. In the case of the dowsers there was no general correspondence between the estimates and the actual facts. Both depth and rate of flow were greatly overestimated. For example, a blindfolded dowser estimated 75 gallons per minute when the actual flow was 1 gallon per minute, while another gave 25 ft. where the depth was 4 ft. The two 'experts' on the other hand estimated quite closely the over-all depth of the water table and the depth at specific points, and also agreed with each other. The correlations were mathematically significant. As to rates of

flow, 'the water engineer did well in over-all estimate of *rate of flow*, but did not appropriately vary his estimates from point to point within the terrain; the geologist's estimates of rate of flow were too low and likewise unrelated to the actual point-by-point variations'.

There was some discussion with the dowzers about the 'artificiality' of the conditions, since it was clear that nobody was planning a house or well on that site. But if a test of this kind is not a valid test of dowsing then we may as well throw up the sponge and declare that the phenomenon, as claimed, is untestable. If the conditions are accepted, and *if* the results of Dale *et al.* are confirmed, we may, rather reluctantly, have to label dowsing as a spurious phenomenon.

Meanwhile, the report concludes: 'But two major issues remain open: (1) Does water divining ever occur by virtue of a paranormal process? and (2) Is the motion of the stick invariably the result of the diviner's own muscular contractions? The present writers do not wish to commit themselves, . . . believing that far more research is needed before they can be answered.'

D. P.

## CORRESPONDENCE

### THE STATISTICAL EVALUATION OF GROUP EXPERIMENTS

SIR,—Dr Schmeidler's interpretation of her interesting and important work reported in the November–December *Journal* seems to be marred by a statistical error. She appears to have treated group experiments, in which several percipients guess at the same target, by the same methods as she would treat individual experiments, in which every percipient has his own run of targets.

The mistake is important and it has occurred before. What Dr Schmeidler has overlooked is that the statistics of card-guessing must be based on the question: given the guesses, how likely is it that the targets would have corresponded with them at least as closely as they do? and not, given the *targets*, how likely is it that the *guesses*, etc.? This is because we know the targets to be random, but we do not know about the randomness of the guesses. It happens that if there is only one percipient per target the statistics are the same whichever question we ask, and that is perhaps why this point has often been overlooked. But where several percipients guess at the same target it can make a great deal of difference.

A more succinct but less instructive way of putting essentially the same criticism is that we cannot evaluate the results of several subjects independently since their results are not in fact independent; but this does not bring out the essential point that, since the statistics must treat the *targets* as variable and the *guesses* as given, we cannot expect to get more significant results by using more percipients to guess at the same targets. From this fact it is clear that the only advantage to be expected of group experiments is that they should be more representative and perhaps more consistent.

How can group experiments be evaluated? In theory by the use of the elaborate multiple matching methods developed by Greville and others, but in practice more simply by the almost equivalent method of getting a group opinion—e.g. a majority opinion—on each guess. Thus, if the most popular guess for a given exposure of a target is Star, the 'group-guess' may be taken as Star, and the group is then treated statistically as an individual. The group opinion can be determined either at the time of the guess or after the session. Both methods demand extra time and work, though much of this can be saved by a resourceful experimenter.

What about Dr Schmeidler's experiments? I understand that the 1943-45 series (sheep *v.* goats) was individual and the rest (sheep *v.* goats with Rorschach studies) group testing. The former were therefore correctly evaluated, and since the results were significant it seems reasonable to take at their face value at least the sheep *v.* goats part of the group studies. To clinch the matter all we need to know is: in how many runs the mean sheep-score exceeded the mean goat-score and in how many the mean goat-score exceeded the mean sheep-score; and similarly for good and poor adjustment, etc. It seems likely that the results would still be significant. I wonder if Dr Schmeidler can give them to us.

I would like to express the hope, too, that in any future reports Dr Schmeidler will state explicitly whether each result comes from individual or group experiments, and give rather more complete accounts of the procedure used for statistical evaluation.

CHRISTOPHER SCOTT

SIR,—Mr Scott's criticism is technically justified. In the article to which he refers, probabilities were calculated from *t*-scores both for the individually tested subjects and for the group tests in which all members of a class guessed at the same targets. As he points out, this is not theoretically correct.

But does it matter, in practice? I should like to raise two points which indicate that it does not ; and that the method which was used leads to the same conclusions as the correct but more time-consuming one devised by Dr Greville.

The first is empirical. In several other series which were similar in procedure and in the nature of the target, both the Greville and other methods were applied, and no appreciable differences were found. Dr Betty M. Humphrey states in relation to this :

In every case the CR of the difference obtained [by the Greville method] differed from that obtained by the binomial formula by only a few hundredths of a point. (For example, for Series S<sub>1</sub>, S<sub>2</sub>, and S<sub>3</sub>, CR's of the difference obtained by the binomial formula were 2.79, 2.81, 2.29. By Dr Greville's method the comparable CR's of the difference were 2.76, 2.75, 2.26.)

Because of the almost prohibitive amount of work involved in carrying out the extensive analyses according to the Greville method, and especially because of the fact that no appreciable difference has ever been found in connection with any of the series of this report between the results obtained by the Greville method and the simple binomial formula, it was deemed unnecessary to apply the Greville method to the series of the present report where the CR's obtained were not of *borderline* significance. (*J. Parapsychol.*, 1949, 13, p. 157, footnote.)

It will be remembered that the two probability figures quoted in my article were  $P = .000001$  for the difference between mean ESP scores for well-adjusted 'sheep' and well-adjusted 'goats' ; and  $P = .0003$  for the difference between mean ESP scores of the sheep whose Rorschach protocols were free of seven 'signs' and of goats whose protocols were free of the same signs. Since neither of these figures would ordinarily be considered *borderline*, I followed Dr Humphrey's precedent in evaluating the data. Even allowing for some margin or error because of the statistical method used, we can, I think, take it that differences between the groups were demonstrated by the research. If this is so, spending many hours on further evaluation would not contribute to our understanding.

My second point relates to theory. The reason for applying a correction to multiple calls on a target is the possibility that many members of a group have a preferred pattern of symbol choice, which (if it exists) may be similar to, or different from the symbol pattern of the target list. This can, of course, affect the data when an experiment uses only one target list. In our case, however, 27 groups guessed at 9 lists each, making 243 lists in all. It seems obvious that the importance of multiple calling is minimized when so many sets of multiple calls are used.

In this connection, Dr Greville has written to me, 'Experience

has shown that similar stimulus preferences among the members of a group are an important factor only when (1) the number of possible stimuli from which the target is selected is very small, and (2) the number of calls in an experimental session is also very small.'

Neither of these conditions was fulfilled in the research to which Mr Scott refers; thus his criticism would seem to be inappropriate.

There is one comment of Mr Scott's which should not go unchallenged. In his third paragraph he writes, 'We cannot evaluate the results of several subjects independently since their results *are not in fact* independent.' (The italics are mine.) Here he assumes a point which he might find very hard to demonstrate. The argument against using *t*-scores for multiple calling of a target is based on the assumption that members of a group have similar stimulus preferences; but this is only an assumption. Examination of the data shows that any such similarity is so small that it is not perceptible to ordinary observation. This is also demonstrated by the fact that Dr Humphrey found such small differences between CRs obtained by the Greville method and the binomial formula.

The general problem is one to which many of us in psychical research are especially sensitized. Often, in every field but particularly in ours, critics point out, correctly, possible loopholes in the procedure of some research. These criticisms are valuable for later tightening of the procedure and proper evaluation of the data. But if the critic assumes that what *might* have affected the results did 'in fact' affect them, he will be making the same sort of error as the experimenter who assumed that what might have affected the results did not affect them at all. Sometimes, as a result of such irresponsible criticism, worthwhile or provocative research is dismissed too readily as worthless. (This generalization is not intended to apply to the present case, nor to Mr Scott's courteous letter.)

GERTRUDE SCHMEIDLER

#### 'RETROACTIVE PK' AND THE CLASSIFICATION OF PHENOMENA

SIR,—I should like to make two comments on the interesting letter from Miss Pamela Clark which appeared in the *Journal* (January–February 1951) and in which she referred to my paper in Part 178 of *Proceedings*.

When Miss Clark says that I recognize 'two main groups of

phenomena, one cognitive and the other conative', she attributes to me a view which I have never wished to defend. Miss Clark mentions my criticism of the term 'precognition' on the grounds that it begs (or seems to beg) the question whether the facts in question are attributable to cognitive acts. The term 'extrasensory perception' is, of course, open to the same objection. I agree with Miss Clark's statement that 'there may be something unsatisfactory for psychical research in adopting without question our everyday categories of cognition and conation'. Indeed, I intended to underline this point when I argued (on p. 72 of my paper) that we are not entitled to regard the introspectible difference between 'guessing' and 'willing', as they appear to the subject, as a reliable criterion for classifying psi-phenomena.

My second point concerns Miss Clark's suggestion that we should define phenomena 'as far as possible in positivist terms'. It may be worth mentioning that this policy, if rigorously applied, would, I think, render it *meaningless* to distinguish 'retroactive PK' from 'retrocognitive clairvoyance'. The distinction between these concepts surely hinges on our interpreting causal connection between a pair of events, *A* and *B*, as a relation which holds in a certain direction (as an asymmetrical relation), so that there is a difference in meaning between saying '*A* causes *B*' and '*B* causes *A*'. The direction of causal influence between two events is not, however, something which can be observed by the senses; hence a positivist should deny that this is a legitimate or useful feature of our concept of causation. The common-sense belief that causes must precede their effects would be attributed by most positivists to an arbitrary linguistic convention concerning our use of the word 'cause'. (That this is not the whole story is illustrated in the discussion by Professors C. D. Broad and H. H. Price of 'The Philosophical Implications of Foreknowledge' in *Proceedings of the Aristotelian Society*, Supplementary Volume XVI.)

It does appear tempting, in view of the temporal displacement effects found in psi-phenomena, to abandon the conception of causation as an asymmetrical relation. The main attraction of this course is that it seems to dispose of the great theoretical difficulties involved in supposing that events may be directly influenced by events which have not yet happened—difficulties which would apply equally to cases of 'retroactive PK' and cases of 'precognition', and which seem to be due primarily to our taking causal connection to be an asymmetrical relation. If this course were adopted, it would require some radical changes in the classification of psi-phenomena. It would, for example, imply (as I argued on p. 63 of my paper) that PK and precognition are not independent

concepts. I wonder whether Miss Clark is prepared to be a consistent positivist?

C. W. K. MUNDLE

*ERRATUM*

JOURNAL, JANUARY-FEBRUARY 1951

In Table IV on page 365 the figure 134 should be 74.



# Society for Psychical Research

31 Tavistock Square · London · WC1

SUPPLEMENT

TO

JOURNAL

March-April 1951, Vol. XXXVI, No. 663

FOR MEMBERS AND ASSOCIATES

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## ANNUAL REPORT OF THE COUNCIL FOR 1950

### I. AWARDS FOR RESEARCH

THE Society during the past year continued to make good progress notwithstanding the difficulties that affected it in common with many other Societies, namely, the general rise in costs and the decreasing leisure which members are able to give to the Society's work. In these circumstances the Council have thought that research could best be promoted by offering monetary awards for original investigations which the Council regarded as reaching a sufficiently high standard. It was their hope that in this way younger workers might be attracted to specified pieces of research in any branch of our subject, whether in the form of quantitative experiment or otherwise. They decided that such awards should be open to all investigators, whether members of the Society or not, and irrespective of nationality. If persons intending to carry out research or experiments will get in touch with the Society before beginning their investigations, they will be encouraged to inquire into problems which have hitherto received insufficient attention, and will obtain information as to the methods which appear particularly promising.

The first award was made to P. H. Marsh, of Gainsborough, for experimental work carried out by him in connexion with extrasensory perception. A note on this work will be printed in the *Journal*.

A piece of major research being carried out with the approval of the Council is a critical analysis of PK data by J. Fraser Nicol, which is likely to occupy him for a considerable time.

### 2. PROFESSOR J. B. RHINE'S VISIT

An important event of the year was the visit to this country of Professor J. B. Rhine, Director of the Parapsychology Laboratory at

Duke University, who had been invited to deliver the Tenth Myers Memorial Lecture. He gave the lecture, which was entitled 'Telepathy and Human Personality', on 10 May 1950 at the Caxton Hall at a better-attended meeting than any organized by the Society for several years. Many of our members also had the advantage of meeting him and Mrs Rhine at a Reception and discussing some problems of psychical research with them. Professor Rhine also addressed well-attended meetings at several University towns in this country and abroad, and undoubtedly gave a great impetus to public interest in our subject. His lecture was published on 25 January 1951 and has been circulated to members.

### 3. OTHER VISITS

In June Professor Gardner Murphy passed through London on his way from America to India, where he was conducting an investigation under the auspices of Unesco, and several of our members much enjoyed the opportunity of meeting him again.

As mentioned in the last Annual Report, Dr Betty Humphrey, a member of Professor Rhine's staff at Duke University, accepted an invitation to visit London and to conduct research work here, the Society contributing to the expense of her visit. The pleasure that her visit gave was increased by the fact that she was accompanied by Miss Elizabeth McMahan, another member of Professor Rhine's staff. They stayed at 31 Tavistock Square for about two months from the middle of August to the middle of October. Of their visit the Hon. Research Officer writes :

I know that I am expressing the feelings of each one of the small number of British experimenters in parapsychology in recording how much we appreciated the help and encouragement given by these workers from Duke. It was of the utmost value to us to be able to discuss problems with two people who spend their whole working life experimenting. Their unfailing enthusiasm and willingness, even after a long day's work at their own experiments, to continue talks late into the night, was an example to us all. The tie of common aims and interests that has been growing between Duke and the British workers was greatly strengthened by this visit.

In addition to addressing a meeting of the Society (12 September), Dr Humphrey was able to visit and lecture at both Oxford and Cambridge. The chief piece of experimental research carried out in London was a follow-up of the work of Professor Urban of Innsbruck. Using patients in a mental hospital as his subjects, Professor Urban reported striking improvement in their performance in ESP tests after they had been given electro-convulsive therapy and other forms of physical treatment. Dr Humphrey concentrated on testing patients before and after electro-convulsive therapy. In addition to this work, Dr Humphrey conducted a series of ESP tests at 31 Tavistock Square in which interested members were able to take part as subjects.

The data from both these series required a considerable amount of statistical analysis, which Dr Humphrey undertook to carry out after her return to the United States. We look forward with interest to receiving her report when the statistical work has been completed.

Dr S. G. Soal has been awarded a travel grant for the purpose of visiting Duke University by the United States Educational Commission in the United Kingdom under the Fulbright Program. There he hopes to co-operate with Dr J. G. Pratt in a detailed examination of the negative displacement effects noted by F. Bateman and himself in the Stewart records. Dr Soal, who has received an official invitation under the Woodward Foundation to lecture on parapsychology at Yale University, hopes through the kindness of Professor Gardner Murphy to spend the first fortnight of his visit in New York and to make the acquaintance of the workers in parapsychology at the City College of New York. He has also received an invitation to lecture at Pittsburg University.

Dr and Mrs Soal will be away from England from 22 March to 29 May.

#### 4. RESEARCH

In the early part of the year Dr West completed a series of experiments in which fifty subjects were tested for ESP in individual sessions. Both cards and drawings were used as targets. Dr Humphrey and Dr West both rated the subjects' drawings according to the psychological characteristics of 'expansion' and 'compression'. The aim was to find out if there were differences in the ESP performance of the two types of subjects. The results, which were reported in the *Journal* for September-October 1950, were inconclusive, but some interesting points were brought out about the expansion-compression test.

Later in the year Dr West re-examined the drawings obtained from the subjects who took part in the 'mass' ESP experiment conducted by himself on 7 February 1947. (See S.P.R. *Journal*, May 1947.) Mrs C. H. Gay rated all the subjects who sent in a drawing in accordance with the expansion-compression test. An independent judge had 'blind matched' the subjects' drawings against two possible targets. The results showed that there was no significant difference between the matching assessment scores of the expansive and compressive subjects, both being close to chance expectation.

Later in the year further interesting results were obtained. In some ESP tests with five subjects Dr West noticed a significant dispersion of scores, some of the subjects scoring above chance, others below. The same effect was found in some data collected by J. Fraser Nicol and Edward Osborn in experiments conducted in 1948-9. The results were given in detail in the report in the *Journal* for January-February 1951.

The large series of home-testing ESP experiments organized by G. W. Fisk yielded some interesting displacement scores. (See the preliminary report in the *Journal* for January-February 1951.) Mr

Fisk and Dr West have since carried out further tests under improved conditions.

Mrs K. M. Goldney and Dr E. J. Dingwall are engaged on a detailed investigation of the reported haunting of Borley Rectory, and have been allowed to inspect many records and correspondence regarding it left by the late Harry Price. A great deal of preliminary work has been done, and several witnesses have been interviewed personally. It is, however, expected that the investigation will require considerable time to complete, as there is a very large mass of material to be examined. It is hoped that a report of this inquiry will be published in due course.

Mrs Gay has for some time past been supervising an experiment in cross-correspondences in which several automatists who are members of the Society have taken part. In the course of her investigation she has carried out an experiment designed to ascertain whether the connexions between a large number of scripts which have come under her scrutiny are to be considered fortuitous or call for some other explanation. This is a work of considerable intricacy, in which an attempt to produce an early report might spoil the result.

Mrs Goldney has taken part in the conduct of a series of proxy sittings at which interesting results have been obtained. Further lines of investigation are under discussion.

Denys Parsons, one of the Hon. Secretaries, followed up a report of a telepathy act recounted in the *New Statesman* of 27 August 1949. The writer referred to two airmen he had known during the war who performed a thought-reading act which he was convinced was genuine. It took until April 1950 to track down one of the airmen, who at once admitted that the act was based on a code. This *dénouement* was reported in the *New Statesman* of 9 December 1950.

Mr Parsons also initiated other investigations which originated in Press reports. In the *Evening Standard* of 27 June 1950, for example, appeared the headlines 'Dog is "Thought-reader": Mongrel does 3-card trick—and knows about money'. The cutting described the reporter's visit to Mr Percy Harvey of Norwich, owner of a bitch named 'Sally' who could indicate with her paw which card had been chosen by a member of the audience, could tell a two-shilling piece from a penny when both coins were held in closed fists, and so on. The case was kindly followed up by a member, Dr O. C. de C. Ellis, assisted by a friend, Peter G. Bailey, who is a member of the Magic Circle. Both wrote admirable reports. Unfortunately, there was no feature in the case of interest to the physical researcher and very little of interest to the magician.

As a result of a short letter from Mr Parsons which appeared in the *Leader*, a letter was received from the father of a Norfolk boy who claims to perform psychic diagnosis and healing. This case was ably followed up by R. W. K. Hill, a member of the Society who lives in Norfolk, but his report showed that the case rested on slender foundations.

The Council are also indebted to Miss Irene Herzberg and Mrs Kenneth Richmond for the trouble they have taken in the investigation

of a case of 'inspired drawing', and to L. R. A. Baker for investigating an interesting poltergeist case at the home of another member, Dr Hilda de Peyer.

A large number of cases of various kinds of apparently supernormal activity are constantly being reported to the Society. All are followed up as far as practicable, but notwithstanding the great amount of labour so entailed it is only rarely that a case can be brought to a point justifying a published report. The Council wish to thank all members of the Society who have been so good as to report cases, and to assure them that their action is much appreciated by the Officers of the Society. Even when no report can be printed, insight may be gained into some of the obscure problems involved.

## 5. FILM ON PSYCHICAL RESEARCH

In November the Society was invited to co-operate in the making of a film on psychical research. The inquiry came from Rayant Pictures Ltd., a company associated with Twentieth Century Fox Ltd., and the film is intended to form one of the 'Spotlight' Series of twenty-minute films to be shown in public cinemas.

Two members of the Council, Denys Parsons (whose professional work is the production of scientific films), and Edward Osborn (Hon. Editor of the *S.P.R. Journal*), were appointed to assist in the preparation of the script and to arrange facilities for some of the shooting. There was some difficulty in reconciling the film company's desire for dramatic presentation with the need to give a strictly accurate picture of psychical research as carried out in practice. After several conferences and revisions a script satisfactory to both parties was prepared, though it proved impossible to incorporate in the space allotted all the 'safeguards' and qualifications in the dialogue and commentary which the S.P.R. representatives would have wished.

The film should be released in April or May 1951.

## 6. FINANCE

The Society's accounts for 1949 showed an excess of expenditure over income of some £576 (excluding the Myers Memorial Fund, which is self-contained). As will be seen from the financial statement on pp. vii and viii, the Society finished the year 1950 with an excess of income over expenditure of some £648 (again excluding the Myers Memorial Fund). While this more than counterbalances the deficit for the previous year, a word of explanation is necessary.

Expenditure in 1950 was reduced mainly as a result of (a) the transfer of the Research Officer from a full-time to an honorary basis, (b) the cessation of her pension on the death, in February 1950, of Miss Isabel Newton, and (c) the printing of only one Part of *Proceedings*. It must also be borne in mind that the 1949 accounts included a non-recurring item of £265 for the cost of producing Part III of the Combined Index

to *Proceedings* and *Journal* (1913-46). Income during 1950 included the gratifying total of £317 from the voluntary increases which many members have most generously made in their subscriptions, and an increase of some £71 in the sale of publications as compared with the previous year. While the greatest economy will be exercised, rising costs must be expected to affect almost every item of the Society's expenditure. The Council desire to see an increase in the volume of research initiated by the Society, but such an increase must be in proportion to funds available after the budget has been balanced.

Non-recurring items of income, such as legacies and donations, are not included in the figure of £648 mentioned above. These reached the excellent total of £307 during the year. The Council have felt it advisable that such items should be kept separate from the Society's ordinary revenue, and should form a fund which may be drawn upon for specific pieces of research distinct from such investigations as are carried out in the routine work of the Society.

#### 7. THE PRESIDENCY

Dr S. G. Soal was elected President of the Society for the year. His Presidential Address, which was delivered at Manson House, Portland Place, W. 1, on 3 November 1950, will shortly be published in *Proceedings*.

#### 8. MEMBERSHIP OF THE COUNCIL

In March, to the great regret of her colleagues, Miss Theodora Bosanquet announced that for reasons of health she felt compelled to resign her position as a member of Council, and Professor F. J. M. Stratton, D.S.O., F.R.S., previously a co-opted member, was appointed an elected member of Council to fill the vacancy so caused.

In October the Hon. Mrs C. H. Gay was co-opted a member of Council.

#### 9. JOURNAL

One of the objects which the Council had in view when lifting the restriction on circulation in September 1949 was to make the work of the Society available to a wider audience. This hope is beginning to be realized. As a result of careful circularization, and with the co-operation of the F. W. Faxon Company, the Society's agents in the United States, nearly 200 non-members, half of whom are American, are now regularly receiving the *Journal* on a subscription basis. The *Journal* is also helping to attract new members, and some subscribers have already transferred to membership of the Society.

# INCOME AND EXPENDITURE ACCOUNTS FOR THE YEAR ENDED 31 DECEMBER 1950

Cr.

## FREDERIC W. H. MYERS MEMORIAL FUND

	EXPENDITURE		INCOME	
1949	To Tenth Myers Memorial Lecture :		By Interest on Investments (gross) -	£31 5 0
	Lecturer's Fee -	£26 5 0	Income Tax Recovered and Recoverable -	-
	Hire of Hall -	8 8 0	Interest on Deposit Account -	0 4 0
	Hire of Sound Amplification Equipment -	4 4 0	Sale of Lectures -	10 4 8
	Balance, Excess of Income over Expenditure for the year	2 16 8		
40		<u>£41 13 8</u>		
				<u>£41 13 8</u>

## BLENNERHASSETT RESEARCH FUND

£30	To Grant to Research Endowment Fund -		By Interest on Investments (net) -	£24 18 6
	Postages -	0 4 0	Income Tax recoverable -	20 7 10
	Balance, Excess of Income over Expenditure for the year	45 2 4	Balance, Excess of Expenditure over Income for the year	
		<u>£45 6 4</u>		
				<u>£45 6 4</u>

## RESEARCH ENDOWMENT FUND

£312	To Salaries :		By Interest on Investments -	£413 11 8
262	Research Officer -		Income Tax recovered and recoverable -	110 2 0
265	Secretarial Assistance -		Grant from Blennerhassett Fund -	-
53	<i>Combined Index, Part III</i> -		Balance, Excess of Expenditure over Income for the year	
	Travelling and General Research Expenses -	£46 6 2		
	Grant for Dr Betty Humphrey's visit -	150 0 0		
	Award to P. H. Marsh for E.S.P. Experiments -	15 0 0		
	Contribution towards General Fund Rent (Research Officer's flat) -	125 0 0		
21	Sittings with Mediums -	3 8 10		
	Apparatus -	0 10 6		
54	Sundry Expenses -	183 8 2		
	Balance, Excess of Income over Expenditure for the year	<u>£523 13 8</u>		
				<u>£523 13 8</u>

# INCOME AND EXPENDITURE ACCOUNTS FOR THE YEAR ENDED 31 DECEMBER 1950—(cont'd)

## GENERAL FUND

<i>EXPENDITURE</i>			<i>INCOME</i>		
1949	To Office Expenses:		1949	By Subscriptions:	
	Rent	£240 0 0		Members	
£240	Rates	67 2 3		1948	£2 0 9
69	Insurance	17 16 8		1949	191 2 1
27	Fuel and Lighting	108 18 5		1950	1,433 18 6
78	Salaries:				1,627 1 4
	Secretary	£325 0 0		Associates	
275	Organizing Secretary	208 0 0		1948	£2 16 5
	(Part-time)			1949	29 11 7
				1950	91 0 5
			123 8 5		
	Clerical Assistance	533 0 0			
37	Cleaning and Caretaker's Wages and Uniform	74 8 8		Proportion of Life Members' Subscriptions	£1,750 9 9
243	National Insurance	196 12 6		Interest on Investments	152 5 0
38	Stationery and General Printing	20 11 2		Income Tax recovered and recoverable	226 7 7
230	Telephone	24 15 0		Contributions towards Rent from Research Endowment Fund	53 12 11
22	Postages	149 13 8		Sublet Rent	125 0 0
152	Repairs	142 12 6			100 0 0
293	Sundry Expenses	27 9 3			
14			<u>£1,761 14 1</u>		
	" Printing and Binding Publications and Purchase of Books:				
	<i>Proceedings</i>	£179 19 4			
	<i>Less</i> Contributed by T. E. Wood towards Part	15 0 0			
£307					
	<i>Journal</i>	164 19 4			
366	Pamphlets	489 11 8			
23	Other Books	117 8 7			
10		8 19 8			
	<i>Less</i> Proceeds of Sales	£780 19 3			
£264		335 19 8			
			444 19 7		
	" Expenses of Meetings				
£442	Pension—Miss I. Newton	41 6 10			
67	Audit Fee	26 16 0			
175	Library	21 0 0			
21	Legal Expenses	48 19 11			
143	Travelling Expenses	14 19 0			
20	Transfer to Income Reserve Fund, being amount of Donations, Legacies, and Piper Fund <i>per contra</i>	12 16 1			
	By Balance, Excess of Income over Expenditure for the year	633 6 3			
		419 9 4			
			<u>£3,425 7 1</u>		
	" Legacies:				
	G. D. Newman deceased excluding Post War Credits	£175 0 4			
	Miss I. Newton deceased	50 0 0			
	Proceeds of Piper Fund				
	Repayment of deposit for purchase of Library Books				
	Sale of Books and Waste Paper				
	Balance, Excess of Expenditure over Income for the year				
			317 6 0		
			82 13 5		
	<i>Total Ordinary Expenditure</i>				
			<u>£3,425 7 1</u>		
	<i>Total Ordinary Income</i>				
			<u>£2,586</u>		

*(See list on p. xi)*



# BALANCE SHEETS AS AT 31 DECEMBER 1950

## FREDERIC W. H. MYERS MEMORIAL FUND

	1949			ASSETS
<i>LIABILITIES</i>				
Balance at 1 January 1950	£1,205	2	0	£101
Add Excess of Income over Expenditure	2	16	8	1,106
	<u>£1,205</u>	<u>18</u>	<u>8</u>	<u>1,207</u>
				<u>18</u>
				<u>8</u>

	1948			ASSETS
Cash at Bank	£93			
Income Tax Recoverable	5			
Investments at Book Value as per Schedule	1,107			
				<u>1,207</u>
				<u>18</u>
				<u>8</u>

## BLENNERHASSETT RESEARCH FUND

Balance at 1 January 1950	£1,548	12	9	£38
Add Excess of Income over Expenditure	45	2	4	10
				<u>35</u>
	<u>£1,548</u>	<u>15</u>	<u>1</u>	<u>1,510</u>
				<u>0</u>
				<u>0</u>

## RESEARCH ENDOWMENT FUND

Balance at 1 January 1950	£18,270	18	2	£791
Add Excess of Income over Expenditure	183	8	2	54
	<u>£18,454</u>	<u>6</u>	<u>4</u>	<u>17,643</u>
				<u>10</u>
				<u>4</u>
Creditor	-	-	-	-
				<u>£18,489</u>
				<u>6</u>
				<u>4</u>

# BALANCE SHEET AS AT 31 DECEMBER 1950

## GENERAL FUND

1949	<b>LIABILITIES</b>			<b>CURRENT ASSETS</b>	
£141	Annual Subscriptions and Special Appeal Donations received in advance	£91 18 7		Cash at Bank and in hand	£1,347 2 1
556	Sundry Creditors	11 18 4		Amount not yet received for Sales of Publications	
	Life Membership Account:			Income Tax Recoverable	25 9 1
	Balance at 1 January 1950	£1,202 5 0		Payments in Advance	33 17 3
	Add Life Subscriptions received during year to date	126 0 0		Investments at Book Value, as per Schedule	9,585 12 10
		<u>£1,328 5 0</u>			<u>£11,004 8 3</u>
	Less Transfer to Income and Expenditure Account	152 5 0		<b>FIXED ASSETS</b>	
1,202	Balance being 50% of existing Life Members' Subscriptions	£1,176 0 0	1,176 0 0	Office Furniture, etc. at cost less Sales (Purchases prior to 31 December 1946 remaining unvalued).	£129 7 0
	Special Donations:			Additions to Furniture during year	20 6 3
	Unexpended balances at 1 January 1950	£99 10 0		Less Sale	£149 13 3
100	Add T. E. Wood for Special Test	85 0 0	184 10 0		<u>5 0 0</u>
	General Fund:			<b>LIBRARY BOOKS, EXPERIMENTAL AND TECHNICAL APPARATUS</b>	<i>Not valued</i>
	Balance at 1 January 1950	£8,631 19 0			
8,632	Add Excess of Income over Expenditure	419 9 4	9,051 8 4		
	Income Reserve Fund:		633 6 3		
	Note.—No account has been taken of post-war credits for Income Tax amounting to £2 15 0.				
		<u>£11,149 1 6</u>	<u>£10,631</u>		<u>£11,149 1 6</u>

### REPORT OF THE AUDITORS TO THE MEMBERS OF THE INCORPORATED SOCIETY FOR PSYCHICAL RESEARCH

We have obtained all the information and explanations which to the best of our knowledge and belief were necessary for the purposes of our audit. In our opinion proper books of account have been kept by the Society so far as appears from our examination of those books. We have examined the above Balance Sheet and annexed Income and Expenditure Accounts which are in agreement with the books of account. In our opinion and to the best of our information and according to the explanations given us the said Accounts give the information required by the Companies Act 1948 in the manner so required and the Balance Sheet gives a true and fair view of the state of the Society's affairs at 31st December 1950 and the Income and Expenditure Accounts give a true and fair view of the Society's revenue transactions during the year ended on that date. We have also verified the investments of the General, Research Endowment, Myers Memorial and Blennerhassett Funds.

MIALI, HARPER & Co.  
Chartered Accountants,  
9 Idol Lane, Eastcheap, E. C. 3

## SCHEDULE OF INVESTMENTS

	Nominal	Book Value	Value 31 December 1950
<b>MYERS MEMORIAL FUND</b>			
3½% Conversion Stock 1961 - - -	£250 0 0	£287 10 0	£240 9 4
3% Savings Bonds 1960/70 - - -	750 0 0	819 7 6	739 13 9
		<u>£1,106 17 6</u>	<u>£980 3 1</u>
<b>BLANNERHASSETT RESEARCH FUND</b>			
British Transport 3% Guaranteed Stock 1978/88 - - - - -	1,510 10 10	<u>£1,510 0 0</u>	<u>£1,379 6 4</u>
<b>RESEARCH ENDOWMENT FUND</b>			
4% Consolidated Stock - - - -	1,460 0 0	£1,699 1 6	£1,494 13 6
3% Funding Stock 1959/69 - - -	2,300 0 0	2,504 2 6	2,269 16 3
3½% War Stock 1952 or after - - -	800 0 0	864 0 0	753 15 0
British Transport 3% Guaranteed Stock 1978/88 - - - - -	6,626 1 5	6,472 17 11	6,050 8 8
British Transport 3% Guaranteed Stock 1967/72 - - - - -	514 0 0	539 14 0	485 14 10
3% Defence Bonds - - - - -	600 0 0	670 5 0	617 12 6
3% Savings Bonds 1960/70 - - -	3,157 9 2	3,449 9 5	3,114 1 7
Australia 2½% Stock 1967/71 - - -	995 2 9	1,040 10 0	924 4 6
2½% Consolidated Stock - - - -	504 3 8	403 10 0	356 11 0
		<u>£17,643 10 4</u>	<u>£16,066 17 10</u>
<b>GENERAL FUND</b>			
4% Consolidated Stock - - - -	86 11 11	£100 15 4	£88 0 10
York 3% Redeemable Stock 1955/65 - -	800 0 0	848 0 0	792 0 0
Australia 3% Stock 1955/58 - - -	250 0 0	262 10 0	251 17 6
Nigeria 3% Stock 1955 - - - -	1,200 0 0	1,272 0 0	1,236 0 0
East India Railway Deferred Annuity Class D - - - - -	23 8 0	590 17 0	526 10 0
3% Savings Bonds 1960/70 - - -	3,000 0 0	3,240 2 3	2,958 15 0
3% Savings Bonds 1955/65 - - -	1,664 12 4	1,703 10 3	1,683 6 11
Australia 3½% Registered Stock 1965/69	1500 0 0	1,567 18 0	1,485 0 0
		<u>£9,585 12 10</u>	<u>£9,021 10 3</u>

## SCHEDULE OF DONATIONS

Anonymous Donations - - - - -	£57 7 0
Mrs A. T. Cox - - - - -	3 4 5
James Langham - - - - -	2 2 0
A. E. Rigg - - - - -	1 0 0
Miss D. E. Warren - - - - -	2 0 0
Mrs E. Wenberg - - - - -	17 0 0
	<u>£82 13 5</u>

## 10. THE LIBRARY

During the year 93 books have been added to the Library, of which 14 were presented by the authors or given by members of the Society.

The number of books borrowed by Members and Associates during the year was 669. Eighty-one books were borrowed by the National Central Library.

The Council have presented Volumes 38 to 48 of *Proceedings* to the Institut für Grenzgebiete der Psychologie und Psychohygiene, Freiburg, Germany, to bring its collection up to date. The Principal, Dr Hans Bender, has expressed his grateful thanks for the gift.

## 11. OBITUARY

On 18 February the Society suffered a severe loss through the death of Miss Isabel Newton, an Hon. Member of the Society. Miss Newton's services, first as Assistant Secretary, then as Secretary, and later as a member of Council, extended over a period of forty years, and were distinguished in the highest degree by loyalty, efficiency, and sympathy for all with whom she came in contact. A tribute to her memory has been printed in Part 178 of *Proceedings*.

Among other losses by death may be mentioned those of Miss May Walker, who took an active part in many investigations of physical phenomena and who left the Society a legacy by her Will; Mrs Lloyd Tuckey, whose husband was a pioneer in the investigation of hypnotism and a member of the Council; and Mr C. J. P. Cave, one of the oldest members of the Society.

## 12. MEMBERSHIP OF THE SOCIETY

During the year 81 new Members, 11 Student-Associates, and one Corresponding Member were elected. The total loss in membership from deaths, resignations, etc., was 64 Members and 7 Associates, leaving a net increase of 22 in the total membership which now, including Hon. and Corresponding Members (14) and Hon. Associates (13), stands at 1002.

## 13. PUBLICATIONS

One Part of *Proceedings*, Part 178, was published during the year, and six numbers of the *Journal* and three Supplements were issued. In May was published a pamphlet by W. H. Salter entitled *Trance Mediumship: An Introductory Study of Mrs Piper and Mrs Leonard*, and in July the pamphlet *Hints on Sitzings with Mediums* was revised and made available to the public.

The Secretary's sales to the general public amounted to £231 15s. 1d. and to members of the Society £89 3s. 11d.; the sales (1949) in the United States to members and the general public were £15 os. 8d.

The total of £335 19s. 8d. is an increase of £70 19s. 10d. over the sales recorded in the last Annual Report.

#### 14. DATE OF ANNUAL GENERAL MEETING

An alteration to Article 37 of the Society's Articles of Association was made by a Special Resolution, unanimously passed at an Extraordinary General Meeting held on 14 December, extending from 28 February to 30 April the date by which the Annual General Meeting must be held. This alteration, by allowing more time for the auditing of the Accounts, will make it easier for the Annual Report and Financial Statement to be circulated to members in advance of the Meeting.

#### 15. MEETINGS

##### *General Meetings*

- 10 May. Tenth Myers Memorial Lecture by Professor J. B. Rhine.  
3 Nov. Presidential Address by Dr S. G. Soal.

##### *Private Meetings*

- 12 Jan. 'The Experience and the Object in Science and Psychological Research' by C. C. L. Gregory.  
26 Jan. 'Perceptivity and the Psyche' by Dr L. J. Bendit.  
11 Feb. 'Precognition' by C. W. K. Mundle.  
28 Feb. Annual General Meeting of the Society.  
28 Feb. 'The Technique of a Certain Clairvoyant' by Eric Cuddon.  
9 Mar. 'Psychical Research and Occultism' by James Leigh.  
23 Mar. 'The Psi Faculty: A Personal Record' by Dr S. G. Soal.  
19 April 'Stage Telepathy' by Jack Salvin.  
17 May Reception for Professor J. B. Rhine.  
25 May 'The Electro-encephalograph' by Dr C. C. Evans.  
7 June 'The Design of Experiments in Psychological Research' by Dr R. H. Thouless.  
17 June 'The Measurement of Personality' by Dr H. J. Eysenck.  
12 Sept. 'Recent Research on the Relationship between Personality and E.S.P.' by Dr Betty M. Humphrey.  
21 Sept. 'The Assessment of Evidence in Spontaneous Cases' by G. N. M. Tyrrell.  
5 Oct. 'A Physical Basis for Parapsychology' by Dr J. R. Smythies.  
21 Oct. 'Searching for Missing Persons by Divination' by L. J. Latham.<sup>1</sup>  
16 Nov. 'Some Famous "Talking" Animals' by Denys Parsons.  
30 Nov. 'Some Aspects of Psycho-kinesis' by Mrs Martha Kneale.  
14 Dec. 'Psychical Research and Modern Science' by C. C. Stevens.

The Council wish to record their appreciation of the great amount of time and effort which Denys Parsons has devoted to the task of arranging the Society's programme of Meetings.

<sup>1</sup>The case described in Mr Latham's talk was fully reported in *Radio-perception* for September 1950, published by the British Society of Dowsers.

## MEETINGS OF THE COUNCIL

Meetings of the Council were held as follows :

460th	21 Sept. 1950	Chairman : The President, Dr S. G. Soal.
461st	31 Oct. 1950	Chairman : The President, Dr S. G. Soal.
462nd	7 Dec. 1950	Chairman : Admiral the Hon. A. C. Strutt.
463rd	29 Jan. 1951	Chairman : The President, Dr S. G. Soal.

At a meeting of the Council held on 31 October 1950, the Hon. Mrs C. H. Gay was co-opted as a member of Council for the current year.

## EXTRAORDINARY GENERAL MEETING

An Extraordinary General Meeting of the Society was held on Thursday, 14 December 1950, Mr W. H. Salter in the Chair. The following Special Resolution was passed unanimously :

That for the words ' January or February ' in Article 37 of the Articles of Association of the Incorporated Society for Psychological Research be substituted the words ' March or April '.

## NEW MEMBERS

## MEMBERS

*(Elected 21 September 1950)*

- BRYAN, T. S., 2 West Park Avenuc, Kew Gardens, Surrey.  
 DAVIDOVICH, P., Rua Paissandu 186, Apt. 203, Rio de Janeiro, Brazil.  
 GIBBARD, R. F., Colmore Housc, Victoria Street, Dargaville, Northland, New Zealand.  
 HALDANE, MISS CHARLOTTE, 25 Empire House, Thurloe Place, London, S.W. 7.  
 HOBKIRK, MRS N. L., 99 Eaton Place, London, S.W. 1.  
 LEMON, C. G., D.Sc., Tidmarsh Grange, nr Pangbourne, Berks.  
 LLOYD-OWEN, CAPT. R. E., 18 Devas Road, Wimbledon, London, S.W. 20.  
 MUSEL, R., 30 Bouverie Street, London, E.C. 4.  
 PAL, R. D., M.B., B.S., 16 Queen's Gate Place, London, S.W. 7.  
 SCHOFIELD, A., 12 Beresford Drive, Southport, Lancs.  
 UNDERWOOD, V. P., Ph.D., Bridget's Cottage, Riding Mill, Northumberland.  
 WARREN, MISS D. E., 30 Oakley Street, Chelsea, London, S.W. 3.

*Student-Associate*

- Bellamy, F., 13 James Street, Sth. Hiendley, nr Barnsley, Yorks.

MEMBERS

(Elected 31 October 1950)

- BAKER, MRS H. L. S., Carrowduff House, Ballymacurley, Co. Roscommon, Eire.
- BOOTY, G. H., 4 Kings Gardens, West End Lane, London, N.W. 6.
- CLYNE, M. B., M.D., 150 Lady Margaret Road, Southall, Middx.
- COTTON, MRS CECIL, 19 Shooters Hill Road, Blackheath, London, S.E. 3.
- JONAS, GROUP CAPTAIN R. CARTER, R.A.F., Colville, The Avenue, Camberley, Surrey.
- LAMON, S. B., 2000 Miramar Street, Los Angeles 4, Calif., U.S.A.
- MAUNSELL, G. A., Jersey Farm, Hildenborough, Kent.
- MORAN, G. L., Jr., 700 Fairway Avenue, Visalia, Calif., U.S.A.
- MULCKHUYSE, J. J., 44 R. Vinkelskade, Amsterdam Z-1, Netherlands.
- NEWLAND, MISS J. E., 2 Savington Street, Paddington, London, W. 9.
- NICOLLS, MRS E., Cockatoo, Victoria, Australia.
- ODUTOLA, O. A., 37 Norfolk Road, Shirley, Southampton.
- ROBBINS, DR E. W., 503 Reed Street, Phila. 47, Pa., U.S.A.
- SCHMEIDLER, PROF. GERTRUDE R., Ph.D., 229 West 97 Street, New York 25, N.Y., U.S.A.
- SINNETT, KENNETH, 16 Kew Gardens Road, Kew, Surrey.
- SINNETT, MRS K., 16 Kew Gardens Road, Kew, Surrey.
- SMITH, REV. ALSON J., 150 E. Superior, Chicago 11, Ill., U.S.A.
- VAUGHAN-JONES, T. G. C., P.O. Box 72, Lusaka, Northern Rhodesia.
- WILSON, DR KATHARINE, 3 Wythfield Road, London, S.E. 9.
- ZUCKER, E. P., 46 Highcliffe Gardens, Ilford, Esscx.

*Student-Associates*

- Chatelenat, R. P., Douglas Hall, McGill University, Montreal, Canada.
- Coote, D. R., 22 Wimbledon Park Road, London, S.W. 18.
- Pullan, P. W., 17 St Mark's Road, Enfield, Middx.

MEMBERS

(Elected 7 December 1950)

- ADDISON, BARTON, 62 Margaret Street, Sydney, New South Wales, Australia.
- GROVER, Mrs S. R., The Bridge Hotel, Chertsey, Surrey.
- KNEALE, MRS MARTHA, M.A., 11 Holywell Street, Oxford.
- LUTZ, CHARLES B., 906 Braniff Building, Oklahoma City, U.S.A.
- MILLAR, G.W., 473 Collins Avenue, Whitehall, Dublin, Eire.
- UPRIGHT, MISS S., 40 Dorset Street, London, W. 1.
- VEAL, MISS IRENE, 31 Albemarle, Wimbledon Common, London, S.W. 19.

## MEMBERS

*(Elected 29 January 1951)*

- BAILHACHE, V. J., Constantia Lodge, Samarès, Jersey, C. I.  
 BURKE, C. J., 41 Gloucester Place, London, W. 1.  
 COLLINS, G. E., M.Sc., Bridge End Lane, Prestbury, Macclesfield, Cheshire.  
 HALL, A. R., 95 Woodcock Hill, Kenton, Harrow, Middx.  
 HOARE, COLONEL R. R., D.S.O., M.C., 24 Buckingham Palace Mansion, Buckingham Palace Road, London, S.W. 1.  
 MENON, M. A. P., The Travancore Bank Ltd., Mattancherry P.O., Cochin, S. India.  
 NASH, PROFESSOR CARROLL B., St Joseph's College, Philadelphia 31, Pennsylvania, U.S.A.  
 O'DRISCOLL, REV. C., O.S.A., Austin Friars, Carlisle, Cumberland.  
 STEVENS, C. C., Friars, Herongate, Brentwood, Essex.  
 TAPP, A. J., 164 Matlock Crescent, Cheam, Surrey.  
 WRIGHT, F. S., 19 Paget Road, Wolverhampton, Staffs.

*Student-Associates*

- Butler, J., West Street, Scotter, nr Gainsborough, Lincs.  
 Perry, M. C., 18 Kilwardby Street, Ashby-de-la-Zouch, Leics.  
 Swinburn, W. R., 11 Briarfield Road, Cheadle Hulme, Cheshire.

## BINDING CASES FOR JOURNAL, 1949-50

Binding cases for Volume 35 (January 1949-December 1950) are now available, and may be obtained from the Secretary, S.P.R., 31 Tavistock Square, London, W.C. 1, price 2s. 6d. Members who have their copies bound are recommended to arrange for the four Supplements (September 1949, January, May, and September 1950) to be bound in immediately after the relevant issues of the *Journal*. References to items in the Supplements have been included in the Index to Volume 35, which was circulated with the *Journal* for March-April 1951.

It is regretted that the Society cannot arrange for the binding of members' copies.



JOURNAL  
of the  
Society for Psychical Research  
VOLUME XXXVI No. 664 MAY-JUNE 1951

A CASE OF LEVITATION IN NEPAL

BY E. A. SMYTHIES, C.I.E.

*(late Forest Adviser to the Government of Nepal)*

THE following is a full and detailed account of an extraordinary, and to me inexplicable, incident which I saw in Kathmandu on Friday, 8 August 1941, and on which I made notes at the time. This account is based on those contemporary notes.

Before I describe the actual incident, some preliminary remarks are necessary. I was told that some Nepalis have to do periodic *puja* or sacrifice to their *Bhagwan* or spirit at their home village. If they fail to do this at the right time, the *Bhagwan* comes, wherever they may be, and possesses them. They lose consciousness, dance and shout and behave madly while under control. I had a young orderly called Krishna, aged about eighteen, who had to do this *puja*, but when the time came, he did not go to his home for the ceremony. Instead—or in consequence—he was suddenly possessed at eight o'clock at night in our servants' quarters. I went out and heard him making weird noises and raving. At nine o'clock the seizure passed, and our other orderly, the elder of the two, said he must be taken to his home (eight miles away) at once, or the *Bhagwan* would keep on seizing him all night. My Muhammedan bearer, Azmat, went also to help him home, and when all three came back next day, Azmat had a quite incredible story to tell of what he had seen and what had happened. As I was not there personally, I shall not trouble my readers with an account of it, but I told Azmat that if ever again a *Bhagwan* visited our compound he was to let me know at once. I should mention that Azmat, being a Muhammedan, had originally no knowledge or belief in Hindu *Bhagwans*.

A month or two later we had a little dinner party, and after dinner about nine p.m. we were sitting in the dark in the drawing-room admiring a film of big game shooting in Nepal kindly sent by H.H. the Maharaja, when Azmat came quietly into the room and whispered in my ear, '*Bhagwan phir a gaya*,' i.e. '*Bhagwan* has come again'. I did not want to disturb the party, so I slipped out

quietly, and went to the back of the house where the servants' quarters were. These quarters were a building of brick and tiled roof divided into five rooms in a row, each room about ten feet by seven and eight feet high, with a small extension at the back for cooking food, and a door three and a half by six feet high in front, and well lit with an unshaded electric light bulb. In one of these little bare rooms—there was nothing in the room except a roll of bedding in one corner and a small box in another—my elder orderly, aged about twenty-two and also called Krishna, was squatting quite alone on the bare floor, dressed in a shirt and khaki shorts with bare legs. His attitude was approximately as shown in the accompanying illustration (Figure I),<sup>1</sup> cross-legged with his hands clasped between his legs. His head and body were shaking and quivering, his face appeared wet with sweat, and he was making the most extraordinary noises. He seemed to me obviously unconscious of what he was doing or that a circle of rather frightened servants—and myself—were looking at him through the open door at about eight or ten feet distance.

This went on for about ten minutes or a quarter of an hour, when suddenly (with his legs still crossed and his hands clasped, see Figure II) he rose about two feet in the air, and after about a

*Figure I**Figure II*

E.S.

<sup>1</sup>The illustrations were drawn to Mr Smythies's specifications by Mrs Elizabeth Sulivan in December 1950.—ED.

second bumped down hard on the floor. This happened again twice, exactly the same except that his hands and legs became separated.

One of the servants whispered that the *Bhagwan* was very angry with Krishna and was punishing him by bumping him on the floor in this way, which, I must admit, was just what it looked like. The servants were becoming very frightened and worried, and I was feeling very creepy myself at this inexplicable sight. Then one of the Nepali servants produced a splinter of resinous wood, which he lit and placed the burning end in Krishna's mouth for a moment. The seizure continued unaltered for a brief interval, and then suddenly it passed, and Krishna opened his eyes and relaxed. He sat looking dazed but otherwise normal. Shortly afterwards I left and returned to our party, from which I had been absent for less than half an hour.

That is a detailed description of what I saw, and noted down the next morning. I cannot explain it.

To touch on one or two more points :

(1) I am quite convinced there was no fake ; the whole thing was unpremeditated and unexpected, with the orderly an unwilling, or rather unconscious, medium of an extraordinary manifestation.

(2) There was no possibility of any apparatus, such as a thin wire, having been rigged up to haul the man up and down. There was an excellent light, and I was quite close, and could see the whole of the small room. Nor could he have been pushed up from below, since he was sitting on a bare solid brick floor. When the levitation took place, the seven or eight other spectators, who were all my servants, were outside the room with me, and could not have ' assisted ' in any way.

(3) I was not in the least under the influence of alcohol (my limit is one ' chota peg '), and nor were the servants around me, who saw what I saw.

(4) It is, I think, an impossibility for anyone to *jump* from the position shown in Figure I to that in Figure II. At any rate I have tried it myself and cannot move an inch from the ground.

The Nepalis say that a *Bhagwan* regards the person he takes charge of as his own property, and never leaves him until death. Usually he exerts a beneficial influence, but at times when angry or annoyed, he takes violent control as exemplified above.

#### EDITOR'S COMMENTS

The incident described above was first brought to my notice by Dr J. R. Smythies, a member of the Society. I am greatly

indebted to his father, Mr E. A. Smythies, for agreeing to write an account of it.

The word 'levitation' is usually applied to a phenomenon in which the subject (or object) is alleged to remain suspended in the air for an appreciable length of time. In this sense, the incident described by Mr Smythies should not, strictly speaking, be termed a levitation. It should, therefore, be understood that in the discussion which follows the word is used for want of a more precise and convenient term.

Cases of alleged levitation among non-western peoples are usually reported by hearsay, less usually at second-hand. When there are first-hand accounts, which is rarely, more often than not they concern set performances given by a 'magician' or holy man, often with a subject or 'pupil', and are witnessed at a distance laid down by the demonstrator. The only two cases which I have been able to find in the Society's literature (*Journal*, 29, 275-7) exhibit some or all of these features. In both of them the word 'levitation' is used in its accepted sense, the duration of levitation in one case being given by Lord Halifax as 'half-a-minute or so'.

The account written by Mr Smythies is of interest for several reasons: (1) it is at first-hand; (2) it derives from notes<sup>1</sup> written at the time of the occurrence; (3) the incident was apparently spontaneous—that is, it does not appear to have been 'staged'; (4) the writer was able to observe it at a very short distance and in good light; (5) the subject was entirely alone in the room.

Mr Smythies has supplied the following further information at my request: (1) the story told by Azmat (referred to in the second paragraph of the report) concerned the apparent possession of the younger Krishna—and, simultaneously, of an uncle of his—but did not involve levitation; (2) Mr Smythies was not prepared for levitation to take place—'I was certainly *not*', he writes, 'expecting anything like levitation'; (3) the height of about two feet to which the elder Krishna was levitated 'was a sort of average, his feet a few inches lower, his seat about three feet up. That is the impression I have'; (4) Krishna did not immediately drop to the

<sup>1</sup> Mr Smythies informs me, in reply to my inquiry, that these notes were on some sheets of paper. He continues, 'Two or three years later, when my wife was writing her book *Tiger Lady* [not yet published.—ED.], she incorporated my notes into one of her chapters—"Life in Kathmandu". I have hunted through her sheaves of papers, and cannot find now my original notes, but, in case of any use, I enclose three pages of her original typescript. This was typed out years ago in Kathmandu (and is not just a recent addition).' The details of the levitation given in the typescript correspond with those in Mr Smythies's account, but there is no mention of the number of servants present, the distance of the witnesses from the subject of the levitation, or the conditions of lighting.

ground after each levitation—‘ I had the impression of one or two seconds pause at the top—not more.’ ; (5) all the servants who witnessed the incident were aware that Mr Smythies understood their language ; (6) there were no windows in any of the servants’ quarters. To my inquiry whether any of the servants said in so many words that they saw Krishna raised off the ground, Mr Smythies replied as follows :

I remember just after the levitation turning to my bearer (Azmat) who was standing near me, and saying in the vernacular, ‘ What is happening to him (Krishna) ’, and he said (also in the vernacular), ‘ The Bhagwan is very angry with him and is bumping him on the floor.’ This clearly suggested that he saw what I saw, and I never thought further of asking him, or the other servants, what he or they had seen. In fact it never even occurred to me that they could not have seen what I saw, and I had no reason (then) for trying to get corroborative evidence or further witness.

Mr Smythies does not know whether the elder Krishna or the other servants were prepared for levitation to occur. He has no information on the occurrence, or alleged occurrence, of the phenomenon in Nepal, but informs me that the Director General of Forests in Nepal at that time, Sir Kaiser Shumshere, to whom, a few days later, he described what he had seen, said that he himself had not seen or heard of such cases before, though he knew of *Bhagwans*. Mr Smythies is unable to say whether the subject of the levitation was in the ordinary course of events liable to epileptiform seizures, or whether the occurrence of epilepsy is marked in Nepal.

By good fortune, and through the kindness of Mr P. V. Killick, who is at present living in Kathmandu, and whose help I gratefully acknowledge, it has been possible to trace Mr Smythies’s former bearer, Azmat. (In writing to Mr Killick I did not, of course, say what the incident was, explaining that it was ‘ extremely important that if Azmat or any of his fellow-servants were now questioned about the happening, nothing should be said which would suggest a particular answer ’. It should, perhaps, also be said that Mr Killick and Mr Smythies are not known to each other, and that my letter was sent to Mr Killick without Mr Smythies’s knowledge.) It will be best first to reproduce the relevant passages in Mr Killick’s letter from Kathmandu (dated 8 February 1951).

I have examined Azmat and enclose a statement of his account of the incident as he related it to me through the interpreter.

The interesting thing, to me, is that, although he recollected the evening in question quite clearly, he insisted at first that nothing unusual had occurred. I then asked him if something had not occurred in which Krishna was the central figure whereupon he said ‘ Oh that ’

with an expression that implied his surprise and some amusement that anyone should be interested to hear about such an ordinary occurrence all these years later!

He and the interpreter say they have both witnessed many such incidents which they evidently accept as perfectly normal.

There are many stories of strange happenings among these simple people and even among Gurkhas in British regiments—as, for example, the British officer who went to a Gurkha soldier's hut and was saluted smartly by him outside only to enter immediately and see him on his bed undressed in an attitude of meditation.

In a postscript, Mr Killick writes: 'Azmat's account was confirmed by my old ayah who was also a witness.'

The report enclosed with Mr Killick's letter is as follows :

#### REPORT OF INTERVIEW WITH AZMAT

In August 1941, Azmat, the Indian bearer of Mr Smythies (then British Forestry Adviser to the Government of Nepal) went to the quarter of Krishna, Mr Smythies's orderly, and found him sitting cross-legged with arms folded and shivering with bloodshot eyes. Krishna said he was not feeling well. Azmat called Mr Smythies and some other servants who lit a Dhup. A Dhup is a paper container about the size of a cigarette containing incense and is lit and swallowed by someone supposed to be possessed by an evil spirit.

Krishna swallowed the Dhup and suddenly rose about three feet in the air remaining in the same posture with closed eyes and groaning. He rose and descended rapidly about three or four times in quick succession shaking violently.

Mr Smythies's gardener, who was present, is a Jharphuke (a man who specialises in locating and driving away evil spirits) and he then uttered in Nepali a Mantra (a charm) and placed a Tika (a small red circle usually of rice or paper symbolising rice) upon Krishna's forehead promising the evil spirit that Krishna would perform a Pooja (an offering to the gods) the next day. Krishna then slept.

The following day Krishna sacrificed a sheep at a temple and drank of its blood and the evil spirit was thereby placated and driven out of his body.

The reason for his being possessed by the evil spirit was said to be that he had failed to offer the customary annual sacrifice to the gods who were therefore displeased with him.

Both Azmat and the interpreter (an educated Newar) said that there was nothing unusual about this incident. People were frequently possessed by evil spirits when they had done something displeasing to the gods. Sometimes they rose up and down like Krishna. Sometimes they talked a lot of rubbish. Sometimes they recalled past events in which they themselves had never taken part or heard about. A Jharphuke is an ordinary working man who, by study and practice under a teacher, acquires powers of divination and expulsion of spirits. They are commonly used, for example, to name thieves or to detect and expel

evil spirits from a new house and apparently are not motivated by consideration of monetary gain.

Before writing to Mr Killick—which seemed to me to be a very long shot—I had obtained statements from Mrs Smythies and from Lt Col J. D. Ogilvy,<sup>1</sup> First Secretary in the British Legation (as it then was) in Kathmandu at the time, who were present at the showing of the film on the evening of 8 August 1941. Although these statements became less important as evidence after the arrival of the report of the interview with Azmat, they are of interest for the information which they give about Mr Smythies's reactions to his experience.

STATEMENT BY MRS SMYTHIES

5 January 1951

I remember very well how my husband described at our party on 8th August 1941 (the date is recorded in my diary) how he had seen our orderly possessed by a 'Bhagwan', and lifted up into the air by some impossible means. He was quite excited about it, but although he was perfectly sober, I found his account difficult to believe.

(Sgd) OLIVE SMYTHIES

STATEMENT BY LT COL J. D. OGILVY

12 January 1951

Your letter of 9th January [1951] re phenomenon reported by Mr E. A. Smythies on the evening of 8th August 1941 in Khatmandhu.

I am afraid that my memory is rather hazy about the event. I do however remember being present at a film show of big game shooting in Mr Smythies's house in Khatmandhu but do not remember any dates. Mr Smythies was called out of the room and when he came back stated that he had been summoned to deal with his orderly, who was behaving queerly in his quarters. He went on to state that the other servants said that a spirit had entered into the orderly and that he, Mr Smythies, saw the orderly levitated above the ground without any apparent effort or support. After a time the spirit was supposed to have left the orderly who came down to earth again.

I saw the orderly at various times afterwards but did not know him well—he struck me as being rather anaemic.

I did *not* see the phenomenon of levitation nor do I remember any further particulars.

(Sgd) JOHN D. OGILVY

<sup>1</sup> It may be regarded as relevant that Mr Smythies had lost touch with Colonel Ogilvy, and that I had to trace his present whereabouts myself. I need hardly say that no mention of levitation was made in my letter to him.

Dr E. J. Dingwall, to whom I am much indebted for the interest which he has taken in this case, has drawn my attention to a passage in E. T. Dalton's *Descriptive Ethnology of Bengal* (Calcutta, 1872). In discussing ceremonies occurring at the worship of Gansám<sup>1</sup> among the Muásí (or Kúrs or Kúrkus), one of the tribes of the Central Provinces of India, Dalton quotes an account which he obtained from Captain W. S. Samuells, then Assistant Commissioner, Chútia Nágpúr. It was apparently in writing or by word of mouth, for Samuells does not seem to have committed it to print. The account, in which I have italicized those passages which show a particularly close resemblance to the descriptions given by Mr Smythies and Azmat, is as follows:

The Baiga [priest] is always the medium of communication, but he assembles the people to aid him in the invocation. Musical instruments are produced, dancing commences, and the invocation to the spirit is chanted until one or more of the performers manifest possession by wild rolling of the eyes and *involuntary spasmodic action of the muscles*. The affection appears contagious, and old women and others who have not been dancing become influenced by it in a manner that is horrible to contemplate. Captain Samuells, who frequently witnessed the incantation, is confident that no deception whatever is practised. As at revivals where similar scenes are produced by professing Christians, each person seized or exalted loses for a time all self-control, and the body, limbs, and neck are worked in the most exhaustive manner, till the Baiga interposes and relieves the victim.

The affection, says Captain Samuells, comes on *like a fit of ague*, lasting sometimes for *a quarter of an hour*, the patient or possessed person *writhing and trembling with intense violence*, especially at the commencement of the paroxysm. Then he is seen to spring from the ground into the air, and *a succession of leaps* follow, all executed as though he were shot at [possibly a printer's error for 'up'.—ED., S.P.R. *Journal*] by unseen agency. During this stage of the seizure he is supposed to be quite unconscious, and rolls into the fire, if there be one, or under the feet of the dancers without sustaining injury from the heat or the pressure. This lasts for a few minutes only, and is followed by the spasmodic stage. With hands and knees on the ground and hair loosened, the body is convulsed, and the head shakes violently, whilst from the mouth issues a hissing or gurgling noise. The patient next evincing an inclination to stand on his legs, the bystanders assist him and place a stick in his hand, with the aid of which he hops about, the spasmodic action of the body still continuing and the head performing by jerks a violently fatiguing circular movement. This may go on for hours, though Captain Samuells says that no one in his senses could

<sup>1</sup> The Gansám of the Muásí deserves a footnote. He was said to have been formerly a chief who was devoured by a tiger just after his marriage. 'Cut off at such a moment', says Dalton, 'it was unreasonable to suppose that his spirit would rest.' He owed his apotheosis to the feat of having, one year after his death, visited his wife and got her with child.



continue such exertion for many minutes. When the Baiga is appealed to, to cast out the spirit, he must first ascertain whether it is Gansám himself or one of his familiars that has possessed the victim. If it be the great Gansám, the Baiga implores him to desist, meanwhile gently anointing the victim with butter; and if the treatment is successful, the patient gradually and naturally subsides into a state of repose from which he rises into consciousness, and restored to his normal state, feels no fatigue or other ill-effects from the attack. (pp. 232-3).

This is the only eye-witness account of phenomena at all similar to those described by Mr Smythies which I have been able to trace, even with Dr Dingwall's help.

In discussing the evidence in this case, the first points to note are (1) that the possibility of trickery seems remote, (2) that since Mr Smythies was not expecting levitation to occur, the hypothesis of suggestion is unlikely to cover his experience, even if one were to discount Azmat's evidence, (3) that Azmat's testimony corresponds with Mr Smythies's account in all material points as regards the levitation itself (which suggests that the incident must have made a strong impression on his mind at the time).

Mr Smythies says of Krishna that 'suddenly (with his legs still crossed and his hands clasped, see Figure II) he rose about two feet in the air'; Azmat's statement says that he was 'sitting cross-legged with arms folded', and that he 'suddenly rose about three feet in the air remaining in the same posture'. As both observers use the word 'rose', and as neither mentions any leap or spring, it appears that they both received the impression that no effort was involved. If Krishna did indeed rise two feet (or even one foot) clear of the ground by no effort of his own, then he did so by no known means.

It is, however, necessary to consider the possibility that, while appearing to the observers to rise without effort, he did in fact leap upwards. It is mechanically impossible for a person sitting with legs crossed and with hands in lap or arms folded to spring more than an inch or two clear of the ground, and this can only be done by swinging the body forward and then sharply backward in an unmistakable effort. (If the feet are tucked under the thighs, the body can be thrust just clear of the ground; if they are crossed in front of the seat, it cannot be done.) An experienced teacher of ballet very kindly made the attempt in my presence, with the results described. A European practitioner of Yoga was no more successful. (It must be recorded that she was confident of being able to propel herself at least two feet up into the air after a month or two of practice. As, however, she made several other claims—including the ability to make herself completely invisible

—but was not prepared to give a practical demonstration of any of them, this confidence was not impressive.)

Is it possible that Krishna, at the moment of rising into the air, was not sitting cross-legged but that he sprang up from a squatting position and at the same time drew his legs up under him so that he *appeared* to have risen with legs crossed? In this case he would have had to land in a squatting position after the first and second jumps (assuming that there were three in all) while after the third he would presumably have landed with legs crossed. It has been suggested that a very muscular person might be able to perform this feat with practice, but I have not succeeded in finding anyone willing to make the attempt.<sup>1</sup> It would certainly be difficult to do without having the arms free to maintain the balance. We cannot tell whether Krishna could have done it. He does not appear to have been of poor physique, for although he is described by Colonel Ogilvy as 'rather anaemic', Mr Smythies, who presumably knew him better, tells me that he was well built, muscular, and healthy, and puts his height at about 5 feet 8 inches and his weight at about 10½ stone. It seems unlikely that Mr Smythies, after having watched Krishna sitting on the ground with legs crossed for ten minutes, could have failed to notice if he had worked himself into a squatting position. Is it possible that he did notice it at the time, but afterwards overlooked it? Against this one must set the testimony of Mrs Smythies and Colonel Ogilvy. And would Mr Smythies have described himself as 'feeling very creepy at this inexplicable sight' if he had merely witnessed three leaps, however phenomenal?

Many of the details given by Captain Samuells are almost identical with those in Mr Smythies's description, and it is interesting to note that the duration of a quarter of an hour which he gives for the preliminary 'fit of ague' corresponds so closely with the 'ten minutes or a quarter of an hour' mentioned by Mr Smythies as the length of time that Krishna's head and body were shaking and quivering before he rose from the ground. Mr Smythies's comments on Captain Samuells's account (of which he had not previously heard) are as follows :

The 'wild rolling of the eyes and involuntary spasmodic action of the muscles' is very typical of what I saw in Kathmandu in Aug. 1941. Also the 'writhing and trembling with intense violence', the 'spring from the ground into the air . . . *executed as though he were shot up by unseen agency*', is a very good description, but it was hardly a spring.

<sup>1</sup> I should be interested to hear from anyone—ballet dancer, acrobat, or Yogi—who felt confident of being able to demonstrate this. A member of the Council of the Society has kindly offered to make a ciné film of any demonstration which seems likely to throw any light on the question.

It is a great pity that Captain Samuells, despite his insatiable curiosity,<sup>1</sup> did not describe the position of the possessed person at the moment of rising from the ground. Whatever it was, it is quite clear that he regarded him as propelling himself upwards by his own efforts.

Although it is a work which is based largely on hearsay, and should be read with great caution, Mme Alexandra David-Neel's *Mystiques et Magiciens du Thibet* (Paris, Plon, 1929) contains a passage which it may be interesting to quote. It describes the training of runners who are said to travel at great speed over long distances in Tibet in their task of summoning demons to the ceremony of propitiating Shinjed, the god of death. The passage is as follows :

The training consists of breathing and gymnastic exercises practised in complete darkness in a *tshams khang* in strict seclusion for three years, three months, three weeks, and three days.

Among these exercises there is one which enjoys the favour of many self-styled mystics who are not of an especially intellectual type. Not only members of religious orders but laymen also, both men and women, undertake prolonged 'retreats' in order to devote themselves to it. It is as follows :

The student sits cross-legged on a large and thick cushion. He inhales slowly and for a long time [*lentement et longuement*], as if he wanted to inflate himself with air. Then, holding his breath, and without using his hands, he jumps up, still keeping his legs crossed, and falls back without changing his position. Some lamas succeed in jumping to a very great height in that way [*sauter, ainsi, à une très grande hauteur*].

According to Tibetans, those who apply themselves to this method of training for several years become capable of sitting on an ear of corn without bending the stalk, or of standing on the top of a heap of barley without displacing a single grain. In fact, the aim is levitation.<sup>2</sup>

No indication of the height of these jumps is given. Nor are we very much wiser after reading, a few paragraphs later, Mme David-Neel's comment on a claim that after such training it is possible for a person to jump vertically with legs crossed to twice his own height. 'I have heard Tibetans from Kham maintain', she writes, 'that they have witnessed feats of this kind in their country.

<sup>1</sup> At the end of his chapter on the Muási, Dalton drily remarks that 'Captain Samuells was unable to induce a Muási to die, in order that he might, as an eye-witness, describe what then takes place'. (op. cit. p. 234). It may, of course, have been Dalton who omitted to quote Samuells fully as to the posture of the possessed person.

<sup>2</sup> I have translated this from the French edition (pp. 208-9) as the English edition (published in 1931 under the title *With Mystics and Magicians in Tibet*) is not only not an exact rendering but contains remarks which do not appear in the original.

However, those whom I have myself seen jumping have not seemed to me at all capable of such an exploit.' We are given no details of the personal experiences to which she refers.

To judge from the reported remarks of Azmat and the Newar, it would appear that the simple folk of Nepal regard levitation as one of the possible consequences of failure to do *puja*. If, as Mr Smythies's account leads us to suppose, the phenomenon does indeed occur, it is perhaps not beyond the bounds of possibility that its operation may be related to the unquestioning belief of the victim in its reality.

Mr Smythies appears to have witnessed something which Europeans are rarely privileged to see. One can only hope that the publication of this report will lead to the collection of further evidence which may throw some light on this puzzling matter.

E. O.

## VISUAL HALLUCINATION OF THE SELF

BY J. C. FLUGEL

IN a recent number of the *British Medical Journal*<sup>1</sup> the distinguished French neuro-psychiatrist Jean Lhermitte, who has been on a visit to this country, contributes an interesting article on 'autoscopy' or 'visual hallucination of the self', in which he passes in review some of the numerous literary treatments of this theme and shows that clinical evidence provides many close parallels to the descriptions by the gifted authors who have been fascinated by this subject. The record of real or supposedly real cases of seen 'doubles' begins with Aristotle and continues in modern times with reports from a number of psychiatrists of the highest reputation. Goethe is among those who claim to have seen such a 'double' of themselves (though 'when I shook my head at the sight of this delusion it disappeared'). For the most part, however, it seems to appear to those who are suffering from some form of nervous or mental disease, such as epilepsy, general paralysis, encephalitis, post-traumatic disorders, or focal lesions of the brain—and to these mostly, though not invariably, when they are lost in thought or in states of drowsiness; while in the case of epileptics the vision seems liable to occur about the time of their seizures. With regard to these latter, there is a tantalizingly brief statement to the effect that the double has been seen not only by the patient himself but 'by others present during the patient's fit'.

<sup>1</sup> No. 4704, 3 March 1951.

If this is substantiated, we have presumably to deal with cases of collective hallucination; otherwise we are faced only by a particularly interesting and impressive form of individual hallucinatory delusion.

This, however, raises problems enough, of great interest alike to the psychiatrist, the psychologist, and the student of psychical research. Attempted explanations of the phenomenon have been along two main lines, neurological and psychological. It is the former which Professor Lhermitte adumbrates in this article, his suggestions being based chiefly on the work of Henry Head and Gordon Holmes on the one hand and on that of Paul Schilder on the other. The first-named authors drew attention to the sensori-motor elements in the nervous system through which we become aware of our postural attitudes and the relative position of the various parts of our body, while Schilder elaborated these and similar findings into a more general treatment of the 'body-scheme' or 'body-image'. This image can be distorted through various diseases of the nervous system, though the precise nature of the lesions or disturbances involved is still to some extent hypothetical. Other interesting modern accounts will be found in articles by G. Bychowski ('Disorders of the Body Image', *J. Nerv. Ment. Dis.*, 1943, 97, 313) and W. Clifford M. Scott ('Some Embryological, Neurological, Psychiatric and Psycho-analytic Implications of the Body Scheme' (*Int. J. Psycho-Anal.*, 1948, 29, 141). The former, after describing various cases, quotes another authority (Menninger-Lerchenthal) as regarding 'autoscopia as a visual kinesthetic hallucination of the body-image, which becomes split by the action of a mechanism linked with the vestibular organ, the brain stem, and the parieto-occipital cortex'. From further considerations it would appear that the vision may be related to the failure of a 'binding process' connected with the parietal and thalamic regions, in the absence or disturbance of which process stimuli usually integrated with the body-image find a sensory centrifugal discharge and are attributed to the outer world.

The psychological explanation (to which Lhermitte does not here refer, but which of course does not exclude the existence of a neurological basis or predisposition) involves the psycho-analytic mechanism of 'projection', in which 'repressed mental processes, not recognized as being of personal origin, are attributed to the external world and (often) experienced as an outer perception'. The division into 'inner' and 'outer' under these circumstances frequently takes place along the lines of a crude separation between 'good' and 'bad' qualities. Most often it is the 'bad' aspects which are projected, as in paranoia, where the patient's own

unrecognized aggressive or sexual impulses may be attributed to others. This would fit in well with a case described by Lhermitte himself, in which the 'astral body' of a girl (who is described as 'good and pious') was felt by her to be 'in the grip of Satan', who would torture it and inflict upon it sexual outrages. (This patient had, it may be noted, suffered from epidemic encephalitis a few years before these phenomena appeared). Sometimes, however, it may be the 'good' part, the super-ego, which is projected—a process which is represented in certain current advertisements in which a shadowy admonitory self appears, and which occurs in Poe's tale of *William Wilson*.

A more complete understanding of autoscopy would probably involve a knowledge of its relations to certain other phenomena with which it may well have some important elements in common. Within the sphere of psychological research are the materializations produced by certain mediums, in so far as genuine phenomena of this kind may occur (though these differ from autoscopy in that they are seldom, if ever, 'doubles' of an actual sitter). More definitely germane to autoscopy are the appearances (visual or kinesthetic) of parts of a patient's body (as distinct from an apparently complete personality)—as in the feeling of 'phantom limbs' after amputation, or of supernumerary members (a case of a kinesthetically present third arm was, I understand, recently reported by a well-known neurologist), or of the patient mentioned by Bychowski 'who discovered parts of her body in the environment'. These merge into the numerous cases of general lack of appreciation of the spatial relations between various parts of the body or of their relative size or shape, of general disturbances in the judgment of spatial relations ('metamorphopsia' when this is visual), or of seeing objects double or multiple ('polyopia'); while in the last resort we find ourselves involved in the whole problem of distinguishing the self from the outer world.

Apart from hallucinations, and in addition to paranoia to which we have already alluded, we have such varied examples of projection as are involved in the 'imaginary companions' of children, in the process of falling in love (in which some psychologists have recognized as a significant factor the projection of the lover's 'good' aspects or ego-ideal), and in religious and political enthusiasms (in which there is often a 'splitting' along the lines of 'good' and 'bad', the good elements being ascribed to a divine being or human leader, the bad to a devil or to political, racial or national 'enemies'). Processes in some respects the opposite of these (inasmuch as they imply a reduction rather than an extension of the self or body-image) are to be found in the various kinds of depersonalization or of loss of the sense of unity with certain parts

of the body (' my hand doesn't belong to me ') even when sensation in the parts concerned seems to be intact. Allied to these in some ways, though with the significant difference that they are demonstrable in ' normal ' individuals, are the interesting results obtained in such experiments as those described by Werner Wolff in his *Expression of Personality* (1943). Here the subjects were shown photographs of their own hands, profiles, mirror writing, etc., along with three examples of each kind obtained from others. They could not as a rule pick out their own photographs, but, when asked to comment on the whole series, almost invariably displayed greater interest and affect (manifested both in approval and in disapproval or distrust) in their own samples than in those of others. They seem therefore to have had something like unconscious or repressed recognition, in which feelings towards the self played an important part. Finally, there are the phenomena of ' dissociated personality ', in which, too, the cleavage often seems to take place along developmental or quasi-moral lines (a secondary personality being in quite a number of the recorded cases more childish or irresponsible than the primary or ' normal ' one).

A deeper study of the similarities and differences in the physiological and psychological processes underlying these various types of phenomena would hardly fail to cast much further light upon the strange and disturbing experience of autoscopy. It is not without good reason that so many authors have been intrigued by the theme of the *Doppelgänger*, for it raises neurological, psychiatric, psychological, and philosophical issues which take us deep into the very heart of the most intimate of all our problems : that of human personality.

## REVIEWS

'The Theoretical Implications of Telepathy'. By Margaret Knight. Article in *SCIENCE NEWS* 18. Edited by J. L. Crammer. Harmondsworth, Penguin Books, 1950. 1s. 6d.

The account by Dingwall and Parsons of the Shackleton experiments in card-guessing and the picture-guessing experiments of Whately Carington which appeared in *Science News* 9 has now been followed up, in *Science News* 18, by an interesting article by Margaret Knight on the theoretical implications of telepathy. The author, who was at one time on the staff of the National Institute of Industrial Psychology, is the wife of the Professor of Psychology in the University of Aberdeen.

Mrs Knight admits that recent experiments in ESP, such as the Shackleton series, appear to be impervious to criticism and she dismisses the usual objections such as faint sensory ones, faulty statistics, or unwarranted selection of data which have so often been urged by psychologists against the positive results obtained in this field. The most disturbing thing she says about the Shackleton results is that they involve precognition with its apparent implications that causation can work backwards in time. This precognitive aspect of ESP has been thought by many psychical researchers to preclude the possibility of our ever finding any *physical* explanation of telepathy. But Mrs Knight, like Professor J. B. S. Haldane, is not convinced that this argument is a sound one. She thinks it no more incredible that future physical events should influence a present physical event than that future mental events should be able to influence a mental event in the present. The author admits, however, that there is no experimental evidence whatever that a human brain can pick up radiations emanating from another brain, and no reason to suppose that even were this proved to be possible it would result in the owners of the two brains having similar mental experiences.

Mrs Knight is possibly right in saying that in the present state of our knowledge there is little justification for the oft-repeated assertion that 'telepathy is independent of space or distance'. I agree that a vast amount of systematic experiment with the same agent and percipient at varying distances is required before we can conclude that telepathic reception is unaffected by distance. It is also true that a good many long-range tests have produced results which are only of borderline significance. I do not think, however, that the author is factually accurate in her statement that 'the longest distance over which results of undoubted significance have been obtained is some 200 miles—between London and Merksem in Belgium' (p. 14). Rhine himself has reported highly significant results over longer distances in his first book *Extrasensory Perception*.

While Mrs Knight thinks that no hypotheses put forward to account for telepathy and precognition are entirely plausible, she considers that the late H. F. Saltmarsh's theory of the extension of the 'specious present' is the best we have so far achieved with regard to precognition. This theory would naturally appeal to a psychologist, but it seems to me that the theory succeeded by evading the formidable metaphysical difficulties of the problems. It was, however, a brilliant effort. There is no mention of Carington's 'Association Theory' of telepathy, but the author appears to favour the view that at the subconscious level minds are not entirely separate entities. If it is true that electrical and chemical



changes in the brain of A can produce conscious experiences not only in the mind of A but also in the mind of B, then we are driven to the conclusion that the two minds are not entirely distinct. Since no psychologist has been able to explain how a physical event or events in A's brain can give rise to a conscious experience in A's mind, then psychologists who swallow this miracle without any fuss ought to be prepared to accept the further miracle that occasionally physical activity in A's brain may give rise to a conscious experience in B's mind. And why not, if mental experiences are not spatial events?

The author suggests that ESP does not mark a new stage in evolution but may be the vestige of a primitive faculty derived from the lower animals and which is best observed in the behaviour of birds and social insects. Whether we agree or not with the author's views, they are expressed with scrupulous fairness and it is surely significant that a professional psychologist should endorse the opinion of another distinguished psychologist, Dr Thouless, that ESP has now been established beyond reasonable doubt and that it is a waste of time to conduct experiments merely to demonstrate the existence of the faculty. Our efforts should now be directed towards an understanding of the conditions under which it occurs and the practical control of its functioning.

S. G. SOAL

SIXTY YEARS OF PSYCHICAL RESEARCH : Houdini and I among the Spiritualists. By Joseph F. Rinn. New York, Truth Seeker Company, 1950. xviii, 618 pp. \$5.00.

Mr Rinn is a business man who, according to the Introduction to his book, 'began his long public career [i.e. as investigator] in an earnest endeavour to discover scientific evidence of a future life. He realised that once communication was established with the dead and we beheld the spirits of the departed, all doubts would be dispelled concerning a life after death . . .' It was not unnatural that his endeavour to behold the spirits of the departed should have led him into the shady bypaths of the psychic underworld, or that there he should have met nothing but fraud. This, as was also natural, if regrettable, produced a violent reaction against all forms of mediumship, and a bias against all psychic phenomena, mediumistic or not.

To quote the Introduction once more: 'A member of the British Society for Psychical Research, Mr Rinn withdrew from the organisation when he painfully realised that it was more concerned in protecting and pampering mediums, and in covering their tracks, than in exposing them'. Mr Rinn's sole connection with the S.P.R. was that he was an associate of the American

Branch of it for about four years, from 1897, during the period when the Society strictly adhered to its policy of refusing to investigate mediums previously detected in deliberate trickery, a strange way of 'protecting and pampering' them! This was not a very long time in which to form a considered judgment on a subject as complex and obscure as we all know psychical research to be. But then for Mr Rinn there are no complexities, nothing more than fraudulent mediums, a gullible public, and gullible or dishonest investigators. What need for psychological subtleties? Is not Mr Rinn a trained conjurer, and cannot he at a pinch consult Houdini?

How magnificently that oracle disposed of hypnotism as 'a big fake'! Even Mr Rinn was inclined at first to jibe at this sweeping pronouncement, but all his doubts were dispelled when Houdini introduced him to a man who claimed to have been a paid confederate of Charcot. Whether or not there is any truth in the story to which this self-accused confederate confessed, I do not know, but it would in any event have little bearing on the status of hypnotic research. Hypnotism, it is well to remember, was put on a scientific basis as the result of the long labours of many scientists in different countries. For a time the French took the lead, and among the various French schools of the Salpêtrière, Nancy, etc. there was vigorous mutual criticism. Any taking of results on a large scale by one school would quite certainly have been denounced by the others.

But Mr Rinn has a tenderness for the 'confessions' of self-confessed scoundrels implicating alleged confederates who are not in a position to reply, as is shown by his keenness to swallow the 'confession' of Blackburn, who, however, was careless enough not to ascertain that the man against whom he 'confessed' was not dead, as he supposed, but very much alive and kicking: see *S.P.R. Journal*, XV.

Mr Rinn is much too fond of charges of dishonesty against other investigators, which he scatters as freely as he does challenges of thousands of dollars to anyone who will produce genuine psychic phenomena, under conditions, of course, that he approves. The failure to take up these challenges he regards as proof that no genuine phenomena occur, but his own controversial manners have to be taken into account, as in his attack on our former President, Walter Prince (pp. 469-70). It appears from the *New York American* of 19 August 1924, which he quotes, that Prince and Houdini were debating the reality of psychic phenomena in a New York church when 'Joseph F. Rinn entered the discussion with a direct attack on Dr Prince's sincerity'. The meeting broke up in disorder and as the crowd pressed to the doors Mr Rinn

declared, 'That man is a liar because he makes his living out of that nonsense'.

Prince was at that time risking his livelihood by the firm line he was taking as a member of the Committee appointed by the *Scientific American* in criticising 'Margery'. Her supporters at that time dominated the American S.P.R., and Prince lost his position as Research Officer of that Society in consequence, as well as incurring much personal abuse. Houdini, Prince's colleague on the Committee, testifies warmly to his integrity, and so would many still living in America and this country who remember him with honour and affection. But without compunction or apology Mr Rinn twenty-six years later repeats the calumny.

Just what are Mr Rinn's qualifications to pose as a judge of psychical researchers? What sort of investigations has he conducted and with what subjects? The Introduction says, 'The pageant of seance deceptionists passes before the eye in colourful succession', and about two dozen names are given. If the list be analysed, it would appear that in his unregenerate days, while he still had an open mind, he had contacts with two of the Fox sisters and Slade; that later he had a single sitting with Mrs Piper, and another with Eusapia Palladino; and that the rest of the list consists either of mediums with whom he never sat or persons whose importance in psychical research was not equal to their local and transient notoriety. I have not noticed in the book any mention of investigations by Mr Rinn of poltergeists, haunted houses, apparitions, or other spontaneous phenomena, or any experiments by him in telepathy or clairvoyance, apart from the exposure of sundry public performers.

Let us consider whether in his two sittings with Mrs Piper and Eusapia respectively he did anything to advance knowledge of their mediumships. In 1896 Mr Rinn visited a 'temple' in Boston where a medium, Concannon, and his wife produced materializations which he recognized as fraudulent. Shortly after his visit some of his friends made a thorough exposure, grabbing a 'spirit' robe, wig, etc. and leaving a nearly naked Concannon. After quoting a newspaper account of the incident, Mr Rinn continues:

This exposure should have shaken my belief that a genuine medium existed, but the manifestations in Mr Ayer's temple [where the exposure had taken place] continued to be regarded by Spiritualists as undoubtedly genuine, and the members of the Society for Psychical Research did not lose faith. We had been fed with stories of the wonderful performances and psychic power of Mrs Leonora Piper, although a preliminary report had not yet been issued by Dr Richard Hodgson, who had her in charge.

This short paragraph is replete with absurdities. Why should the exposure of Concannon, whose fraud was of a very gross kind, cast doubt on mediumship in general? If the members of the S.P.R. did not 'lose faith' in Concannon, as Mr Rinn's sentence suggests, it was because the majority had never heard of him and had no faith in him to lose. Why bracket him with Mrs Piper, whose phenomena were of quite a different type? As for Dr Hodgson not having produced a preliminary report on her, two long reports on her had already been published in S.P.R. *Proceedings*, one (vol. VI, 436-659) by Myers, Lodge, Leaf, and James, and the other (vol. VIII 1-167) by Hodgson himself.

These reports had established several points concerning the Piper mediumship, such as (a) that it was difficult to take the Controls, e.g. Phinuit, unreservedly at their face value; (b) that sometimes they made incorrect statements and were unable to answer questions correctly; (c) that the medium, when in trance, could and probably did pick up information from the sitters by muscle-reading (see *Proceedings* VI; Lodge's remarks on p. 451, and Leaf's on p. 562),<sup>1</sup> but also (d) that there was no ground for supposing that in her normal state she obtained information as to the sitters, their friends etc. and (e) that, when all allowances had been made for (a), (b) and (c), she had genuine psychic powers. When, therefore, in an interview with Hodgson after his sitting in 1896, Mr Rinn brought up muscle-reading, etc., etc., as a sufficient explanation of Mrs Piper's phenomena, it was not unnatural that Hodgson, with his much longer experience of mediumistic trickery, should have shown impatience. Mr Rinn suggests that this was because Hodgson was 'of the English gentleman type' and unduly touchy. Hodgson was in fact a very unconventional, plain-spoken Australian, well accustomed to the rough and tumble of controversy.

At the time of Mr Rinn's sitting the principal Control and one of the principal Communicators at the Piper sittings was George Pellew, who had died in 1892 and is called in the printed records 'George Pelham' or 'G.P.'. In 1921 Mr Rinn learnt to his 'amazement' (p. 175) 'that documentary evidence existed from the family of "G.P."' that a fictitious story was built about the life of his former friend by Dr Hodgson to justify his change to the spiritistic hypothesis, and that most of the statements made about "G.P." in Dr Hodgson's report [in S.P.R. *Proceedings*, XIII] were *absolute falsehoods*'. (The italics are Mr Rinn's). On p. 180 he says, 'Dr Hodgson lied outrageously on many important points in relation to "G.P." and his family.'

The only evidence Mr Rinn puts forward in support of these

<sup>1</sup> In the later stages of the mediumship this could not occur.

sweeping statements is a letter written in 1918, more than twelve years after Hodgson's death, by 'G.P.'s' brother, Professor Pellew, to Clodd, the Rationalist author. It is a long letter, taking up nearly four pages of print. The first half of the letter is to the effect that the Pellew family refused to accept the 'G.P.' communications as coming from the real George Pellew, mainly because they did not reflect his intellectual standards. This was a matter of opinion, and there is no suggestion of any false statement by Hodgson. The latter part of the letter does accuse him of having in one instance misrepresented the facts, not about 'G.P.', but about a sitter, John Fiske's, opinion of his sitting. Professor Pellew had been shown by his parents 'a curious letter from Hodgson. It was somewhat to this effect'. The substance of the letter, as so recollected, was that after a Piper sitting Fiske had told Hodgson that he was *absolutely convinced* that he had been talking to his old friend, George Pellew. A few weeks later Professor Pellew, as he says in his letter to Clodd, met Fiske, who stigmatised Mrs Piper as 'that old fraud', and denied that he had ever thought that through her he had conversed with George Pellew.

The Fiske story is, of course, the loosest hearsay, depending on Professor Pellew's admittedly vague recollection ('somewhat to this effect') of a letter he had seen at some unspecified previous date and of his memory of a conversation, also of uncertain date, with Fiske. It would not begin to be evidence without having the exact terms of Hodgson's letter, and a first-hand statement by Fiske as to exactly what his comments on the letter were. He should also state when the sitting in question occurred and what report on it, if any, he gave to Hodgson at the time. This might enable some contemporary written record of the sitting and of annotations by Fiske to be traced, and these would of course be evidence worth attention.

The story given in the letter is not a plausible one. By the testimony of all who worked with him in this country or America, whether or not they agreed with his views, Hodgson was a man of honour. But even if they had all been mistaken on this point, he could not have risked misrepresenting things that happened at or in connexion with sittings which he was supervising. If any of the Pellew family, or Fiske, or anyone else had during Hodgson's life reported a single case of misrepresentation by him to any member of the S.P.R. Council in England or to any of his American colleagues, and had been able to substantiate the charges, that would have been the end of Hodgson's career in psychical research.

'Mrs Piper's Confession. Disclaims Contact with the Spirit World.' So runs the heading of one of Mr Rinn's chapters

(p. 195). By this time it will come as no surprise to the reader to learn that Mrs Piper never made a 'Confession' at all. In the summer of 1901 Mrs Piper gave an interview to a journalist, and on 20 October of that year the *New York Herald* published a long statement purporting to be made by her, and in fact based on the interview. The statement contains the following passages: 'I have always maintained that these [psychic] phenomena could be explained in other ways than by the intervention of disembodied spirit forces. The theory of telepathy strongly appeals to me as the most plausible and genuine scientific solution of the problem.' And later on, Mrs Piper says, or at any rate, the journalist reports her as saying: 'I do not believe that spirits of the dead have spoken through me when I have been in a trance state.' On Mrs Piper's attention being drawn to the *New York Herald* article she promptly dictated a statement which appeared in the *Boston Advertiser* of 25 October 1901: 'I did not make any such statement as that published in the *New York Herald* to the effect that spirits of the departed do not control me. . . . Spirits of the departed may have controlled me and they may not. I confess that I do not know.' This latter statement Mr Rinn does not quote. But whether or not the *New York Herald* was justified in reporting Mrs Piper as saying she did not believe the spirits of the dead had spoken through her, the use of the word 'Confession' by Mr Rinn was quite unjustified. For many years prior to this interview two views of the Piper communications had been discussed by psychical researchers. One was that they came from spirits of the departed; and the other was that Mrs Piper's subconscious mind received the substance of them by telepathy, the Controls and Communicators who manifested in the trance being subconscious dramatisations somewhat akin to secondary personalities. The second hypothesis, which Mrs Piper according to the interview preferred, no more implies dishonesty, as the word 'Confession' does, than the first. It appears from the report on the episodes in S.P.R. *Journal* X, pp. 142-3, 150-2, that the *New York Herald* had by way of 'advertising smartness' made an advance announcement of her 'Confession', and that in response to a protest by her, the paper assured her that word would not appear in the actual article, as in fact it did not.

Eusapia was more up Mr Rinn's street than Mrs Piper, and one might have hoped for some new light on her from Mr Rinn's sitting. This took place on 17 April 1910, and is described, together with the preparations for it, on pp. 278-81 of the book. It had at this time been long known and was generally accepted that Eusapia would use whatever trickery the conditions of control permitted. The question was whether she could produce her

phenomena under conditions effectively excluding trickery. The group of highly competent investigators who sat with her at Naples in 1908 believed she could and did: see their report in S.P.R. *Proceedings* XXIII.

In 1909 she came to America and in 1910 gave a series of sittings to a group connected with Columbia University. A member of the group invited Mr Rinn and some friends of his, who were experts in trickery, to attend a sitting. Before this sitting Mr Rinn and his friends arranged an elaborate plan to trap the medium, some, but not all, of the members of the University group being privy to it. Part of the plan was that at a particular point in the sitting and for a prearranged time Mr Rinn's friends, who were acting as controllers of the medium, should deliberately release their control. This plan was put into action, and during the prearranged relaxation of control Eusapia produced phenomena, which she could not do during the part of the sitting when the control was strictly maintained. The result did no more than confirm what had already been established as to her mediumship fifteen years earlier at the Cambridge sittings. The S.P.R. has always maintained that it is possible to test a medium without laying traps, and that complete candour between fellow-investigators is imperative. Departures, such as Mr Rinn's, from the code of mutual confidence between investigators merely open the door to the bogus investigator, who is as much a hindrance to serious research as the bogus medium.

It would be tedious to correct all Mr Rinn's minor inaccuracies: here are a few jotted down as I read the book. Barrett was not 'head of the British S.P.R.' at the time of his American visit in 1885, or anywhere near it (p. 15). J. H. Hyslop is made (p. 294) to speak in 1910 of 'the English branch of our association', which he would certainly not have done, as he had negotiated on the American side the complete separation of the British and American Societies in 1906. Ivor Tuckett was not at any time a 'prominent member' of the S.P.R. (p. 309): in 1911 he was not a member at all. It is correctly stated (p. 599) that in 1938 the ESP cards used by Professor Rhine were unsuited for experimental purposes: see S.P.R. *Journal* for May 1938. Professor Rhine was by that time quite aware of the defect, and was arranging for the use of a better type of card in his later experiments: Mr Rinn does not mention this. Nor does he, after mentioning (pp. 596-7) the negative results of Dr Soal's earlier experiments as tending to disprove Professor Rhine's claims, anywhere refer to the positive results that Dr Soal and Mrs Goldney later obtained through Shackleton (S.P.R. *Proceedings* XLVII). It is not true, as suggested on pp. 291-2 of Mr Rinn's book, that luminous paint is only used by

'crooked mediums'. Until the development of infra-red technique it was the standard method of indicating the situation of persons and objects at sittings held in poor light, and has often in that way been used in our seance-room. Mis-spellings of names familiar in psychical research are common; 'Phenuit', 'Mrs Sedgwick', 'G. B. Door', 'Rev. Charles Tweedle', 'Valentine'. On p. 444 the names of ten members of a Committee to investigate spirit photography are given: five are mis-spelt.

A large part of the book is taken up with one-sided accounts of conversations in which Mr Rinn scores off the other fellow. One's confidence in the accuracy of these accounts is shaken by Mr Rinn's exaggerated bias, and the prevalence of blunders large and small destroys it entirely, beyond hope of restoration by the most copious extracts from the American press, to which he freely resorts.

The pity is that Mr Rinn, whose knowledge and experience of mediumistic trickery would have qualified him to write an interesting book of value to psychical research, if he had confined himself to matters that he understood and that had come under his own observation, has been so unwise as to go outside these limits. Knowledge of methods of deception is needed in psychical research, and the S.P.R. has always been fortunate enough to include members well versed in them. But such knowledge does not by itself make a psychical researcher, without more accuracy, a better idea of evidence, and a greater understanding of human nature, whether in its normal or abnormal states, than Mr Rinn shows in this book. To succeed in business he must have had a fair share of these qualities, but when it comes to psychical research they fail him. No doubt this is because for him the subject has never been one of impartial enquiry. He began with a desire to obtain evidence of a future life, proceeded to take the worst kind of route to that objective, found that it led nowhere, and went violently into reverse for the rest of his life with no better results. Let the sad fate of Mr Rinn stir us all to examine our consciences to see whether we are ourselves free, to quote our Society's inaugural manifesto, 'from prejudice or prepossession of any kind'!

W. H. S.

PSYCHOTIC ART. By Francis Reitman, M.D., D.P.M. London  
Routledge & Kegan Paul, 1950. x, 180 pp. 17 plates. 16s.

Although the artistic products of the mentally unbalanced have long been known, little serious attention was given to them until the nineteenth century when such students as Marcé and Simon in France (1864 and 1876), Lombroso in Italy (1880), and Kiernan



in the United States (1892) began to view them, not only from the standpoint of the art critic, but also from that of the practising psychotherapist. In later years, following the more detailed work of Prinzhorn at the Heidelberg Psychiatric Clinic, came further work from men of such diverse views as Vinchon, Asschaffenburg, Pfeifer, and Schilder.

In the present work Dr Reitman, who has had the advantage of working with Dr E. C. Dax at Netherne House, has attempted to examine the problems inherent in psychotic and in particular schizophrenic art from the point of view of the neuro-psychiatrist, and to show what he calls the biologically determined dynamics of the material at his disposal. He regards the art products of the schizophrenic patient as a kind of active and creative ritual. Due partly to the distorted view of his own and other body-images (body-schemata) and partly to his disordered concept of space-relations, the artist paints in order to adjust himself to his own changed view of reality. His productions are thus in a sense pictorial representations of his own disturbed thinking, and can therefore be evaluated in psycho-physiological terms. It is, Dr Reitman insists, a mistake to appraise them solely from the psychological standpoint, since they contain both motor and sensory phenomena, and must therefore be approached from a biological as well as from a psychological angle. It is here that the author parts company from some of his predecessors, notably Prinzhorn, and from those who, from their psycho-analytic training, tend to find in every schizophrenic painting those signs and symbols which form the basis of much of their psychological interpretation in other fields.

From what Dr Reitman has written it is clear that he regards schizophrenic art as primarily dependent on the patient's own abnormal cognitive condition. From his changed mental position both as regards himself and the external world the schizophrenic is, in one sense at least, maintaining his own existence and adjusting himself to a new situation. Thus any analysis of such paintings must give due allowance to a psychological approach, whatever stress be laid on the purely neuro-psychiatric interpretation.

It is here that the psychical researcher may perhaps be pardoned if he ventures to express an interest in the total lack of any reference in Dr Reitman's book to mediumistic art. Apart altogether from such important and complex cases as that of Thompson-Gifford, it is, to say the least, rather strange that the author has not thought fit to mention the work of such well-known automatists as Mrs W. M. Wilkinson, Gilbert Cooke, F. L. Brown, A. Machner, F. Gentes, or Mrs I. T. Bush. Had he done so, and noted the many striking parallels between schizophrenic art and

that of the automatists, he might have made an effective addition to his discussion of the comparison between the paintings of schizophrenics and those of certain modern artists, while some similarity between these two classes of product is obvious. Dr Reitman is at pains to point out that this is due to the modernistic technique of analysing a system of relations which necessarily leads to fragmentation. On the other hand the same fragmentation is seen in psychotic art, but here it is not due to any system of conscious analysis but rather to the basic disintegration of the personalities of the artists.

From his treatment of psychotic art as seen psychologically it is clear that Dr Reitman has very little patience with the elaborate interpretations of certain of the followers of Freud and Jung, although at the same time he does not seem to distinguish very clearly between the teachings of master and pupil. In this connexion his short treatment of some of Goya's products appears rather unconvincing, and had he chosen Bosch instead of the Spanish artist the reader would have been much more interested in his diagnosis. His discussion is interesting, however, since he shows that, in his opinion, the content of the *Caprichos* can reveal material which is pathognomonic of schizophrenic experience, although how far he would go in a further interpretation remains uncertain.

To sum up, then, Dr Reitman has written a thought-provoking book which should be read by all who are interested in a little understood subject. Although the list of authorities is brief, it contains a representative selection of easily available material, while those who require further references, can turn to the papers of Anastasi and Foley, although these authors, like Dr Reitman, seem to ignore most of the material on mediumistic art.

E. J. DINGWALL

THE MYSTERY OF DREAMS. By William Oliver Stevens. London, Allen & Unwin, 1950. vi, 280 pp. 16s.

Mr Stevens has given us a book of a kind badly needed, one that should put before us a large number and great variety of dreams, should classify them according to their significance for psychical research, and should comment on the points that distinguish one example from another. The scheme of the book appears from the chapter headings. First come The Dream in Literature, Symbolism in Dreams, and The Solution of a Problem (i.e. dreams in which a problem is solved for the sleeper): then follow chapters in which Telepathic, Clairvoyant, Warning and Prophetic, Borderland, Concurrent and Reciprocal dreams are successively passed in review, with, to end, some cases difficult to classify.

In the concluding chapter the author explains the principles on which he has selected his examples. He mentions first readability, that is 'variety in time, place, characters, and especially in type of incident', but adds that primarily the selection was 'based on what seemed to be authenticity'. In all the cases cited he is convinced that the narrators believed what they said, although some of them date from an uncritical age, and for others corroboration could not be obtained. With this caution in mind, the reader can rejoice that the author has cast his net so wide. Cases reported in good faith but falling short of the highest evidential standards can be read 'as Hierome saith' of the Apocrypha 'for instruction—but not to establish any doctrine'. Many of the cases cited are drawn from the *Proceedings* and *Journal* of our own and the American S.P.R. Others are taken from books of various kinds, and many were reported direct to Mr Stevens by correspondents in the United Kingdom and America.

Mr Stevens is impatient (pp. 57, 58) of attempts to explain dreams by bringing in 'the old reliable pack horse, the subconscious mind'. Most people, he truly says, 'are quite hazy as to what the subconscious mind actually is and how it operates'. In the present stage of our knowledge it is well not to attempt too precise a definition of the subconscious mind. Many psychological researchers would not accept the author's description of it as 'a complete record of our lives': 'complete' is too wide, and 'record' suggests passivity. It looks as if he supposed that the champions of the subconscious regarded it as the ultimate source of all paranormal cognition, and not, as most of them do, as the potential vehicle, and in many cases the probable vehicle, for material of external origin.

W. H. S.

THE REVOLT AGAINST REASON. By Arnold Lunn. London, Eyre & Spottiswoode, 1950. xi, 252 pp. 15s.

The author of this lively and stimulating book writes with the enthusiasm of the Catholic convert, and displays an extensive acquaintance with the writings of philosophers and theologians from antiquity to the present day. One wonders, however, whether the abundance of often lengthy quotations is due to a naive desire to show that he is in good company in his opinions, i.e. an appeal to authority, or due to the urge 'to add verisimilitude. . .'. His tale, however, though in many places unconvincing to the non-Catholic, is never bald. Mr Lunn starts from the premise that the Catholic religion is an essentially rational system based on Revelation and the Aquinate 'proofs' for the existence of God, and that faith derived from mystic experience is almost an

unnecessary and not always welcome adjunct. Luther, he maintains, by exalting mysticism and denying the relevance of reason in matters of religion, was the initiator of the 'Flight from Reason' and of the subsequent—according to Mr Lunn, consequent—decline in morality, ethics, and aesthetics which has reached its climax in our day, especially in the dictatorial systems. This theme is developed with much ingenuity.

A large part of the book is taken up with a 'debunking' of Darwinism and a vigorous attack on the 'scientians', a term coined by Mr Lunn to describe those materialists and crypto-materialists who deny the supernatural and affirm the omniscience of natural science. In the Western world at least, this type is fast becoming extinct and thus the author's attacks, which are mostly well-reasoned, apply largely to Victorian and early twentieth century scientists. He makes much of the practice among scientists of often ignoring facts which are inexplicable by, or in contradiction to, a current theory. In this context his examples are drawn mainly from biology and the realm of paranormal phenomena. If Mr Lunn had been an experimental scientist he would have known that in order to construct any theory it is not only permissible but essential to neglect some facts; otherwise—human limitations being what they are—it would be impossible to formulate any theories at all. The successive theories in any subject embrace an ever-increasing number of hitherto ignored facts. His argument, that because the theory of evolution cannot account for certain facts, therefore special creation must be true, shows that he does not comprehend the true role of scientific theory. No theory can be completely adequate to its subject, and a theory has fulfilled its purpose when it is seen to be inadequate and is superseded by a new one. Furthermore, he fails to appreciate that a theory of special creation is heuristically useless.

As far as paranormal phenomena are concerned, the pretext for ignoring them among many scientists was, and still often is, disbelief. The true reason, however, is that they seem irrelevant and cannot be fitted anywhere into the present world picture. It is just as unreasonable to reproach Victorian and later scientists for ignoring paranormal phenomena as it would be to reproach Galileo for ignoring magnetism and electricity. The phenomena available at the time just did not fit in anywhere into the current view of the universe. That is why the present state of psychical research is as good an example as any in the history of science, of the necessity for a working hypothesis before systematic experimental work can be planned.

To the non-Catholic many arguments in this book are difficult to swallow. In particular, in discussing the dichotomy between

materialists and theists, Mr Lunn seems to be unaware that many intellectually honest people are content to be agnostics and to base their morality on intuitive feelings of human dignity.

This is definitely a thought-provoking book.

P. H. PLESCH

JOURNAL OF PARAPSYCHOLOGY, Vol. 14, no. 4, December 1950.  
Durham, N.C., Duke University Press. \$1.25.

Professor Rhine makes a mid-century appraisal of the present position in parapsychology. He lists as solved problems the separate occurrence of ESP and PK, the occurrence of clairvoyant ESP, of precognition, and, as a deduction from these, the non-physical nature of psi. He indicates more problems not yet solved or not fully solved, of which examples are: the experimental demonstration of telepathic ESP, the possibility of conscious control of psi, and the biological nature of psi capacities.

Remi Cadoret and Dr Pratt report a tendency to consistent missing in ESP experiments, that is for the guessing of one face when another was really the target. With the help of Dr Greville they have evolved a method of dealing with the somewhat difficult statistical problem of assessing the presence of such a tendency.

W. A. McElroy and Miss Winifred Brown report an experiment done at Glasgow University on the effect of giving mild electric shocks as punishments for incorrect responses in an ESP experiment. It was hoped that this method would make learning possible. This hope was not fulfilled, since in the series in which shocks were given there was a significant decline effect. The rate of scoring in the series of experiments with shocks showed an over-all significant deviation from mean chance expectation, while a series without shocks showed a much smaller positive deviation.

Olivia Rivers gives a brief account of an experiment designed to test the correlation between psi capacity and the results of a mental health rating. Although her subjects showed significant scores in an ESP test, these were found to be not significantly correlated with their mental health ratings.

An exploratory experiment by J. B. Rhine on two water diviners showed interesting results of which the principal was a strongly significant tendency to score below mean chance expectation. Professor Rhine is inclined to attribute this result to the mental tension connected with the experimental situation.

R. H. THOULESS

## CORRESPONDENCE

## METAPHYSICS AND PARAPSYCHOLOGY

SIR,—Dr Rhine's Myers Lecture *Telepathy and Human Personality* contains a plea that in pursuing parapsychological research we shall free ourselves from 'speculative associations and cultural trappings' (pp. 30-1) and from 'traditions of theological and occult character'. I suggest that it is impossible completely to avoid making metaphysical assumptions in our scientific thinking, and that Dr Rhine's lecture is an illustration of the truth of this.

To begin with, he assumes, as most psychical researchers do, that we can distinguish telepathy from clairvoyance. I do not quarrel with this distinction if it is merely one of scientific method, but I must insist that if we regard it as more than this, we have adopted a metaphysical theory. For there is *one* type of metaphysical theory, that of modern objective idealism, for which this distinction vanishes. On this view, 'object' and 'subject' are abstractions, the concrete ontological unit containing both subject and object, and therefore what we call clairvoyance *is*, in a broad sense, telepathy, for the object perceived in clairvoyance is always known to a superconsciousness, or oversoul, or to the orthodox Christian God who can be regarded both as transcendent and as immanent in finite beings.

Both in Dr Rhine's lecture and in the Thouless-Wiesner four possible interpretations of 'person to person transference of thought impressions' (p. 19) we see the influence of a realist metaphysic which is just as 'speculative' as the idealist. Indeed, Dr Rhine's statement on p. 29 of 'the primary question of psi research' almost suggests that he has not completely freed himself from the influence of the crude materialism which held that the brain secretes thought as the liver secretes bile. He states that the primary task is the discovery 'whether there is an immaterial part of human personality, a spiritual self that might conceivably be considered capable of survival'. Here we see clearly the metaphysical assumption that whatever else may *not* be real, the body as a 'thing-in-itself' indubitably *is*. Now quite apart from the idealist metaphysic, which has been held by thinkers of the calibre of Hegel, Edward Caird, T. H. Green, F. H. Bradley, Bernard Bosanquet, W. T. Stace, and Brand Blanshard, what about the physicists' discovery that the notion of a three-dimensional 'block Universe' in a common time order must be given up and that we must reason *as if* the 'thing-in-itself' were a four-dimensional

continuum which must be measured by 'complex numbers'? What about the discovery that even the electron cannot consistently maintain its objective substantiality but becomes a wave—and a wave of what? It is matter, not the spiritual self, that is doubtful today. For although the four-dimensional continuum, tensors, matrices, and all the rest of the mathematical physicist's stock-in-trade cannot safely be regarded as more than methodological devices to enable observers to adapt themselves to and to control their environment, it is quite impossible to talk and write about physics unless we assume the existence of observers who use the devices—and for that matter who read or listen to our expositions! It is consciousness (using the word not as referring to the 'entity' attacked in Professor Gilbert Ryle's *Concept of Mind* but rather as the general term covering seeing, hearing, thinking, feeling, and so on) that is the indubitable reality. It is 'matter' that is on trial for its life, so to speak.

This last point is important when we try to assess the value of the Thouless-Wiesner fourfold possibilities. One of the possibilities is that the sender's mind may influence directly the body of the receiver; another is that the sender's body may influence directly the mind of the receiver. Now there would be some point in considering these 'possibilities' if the metaphysical doctrine of the body as a 'thing-in-itself' were established, and if the notion of action between a person's 'mind' and his *own* 'body' could be freed from absurdity. But one of the reasons for rejecting the theory that a person is a duality in which a realist or 'thing-in-itself' body is the primal certainty and his consciousness somehow derived or epiphenomenalistic, is the absurdity in which we are landed if we try on this basis to explain the simplest act of perception. What, from this point of view, is supposed to happen when I see a patch of colour? Rays of light, it will be said, are reflected from the patch in straight lines to my eyes, and then microscopic motions, which are different if the patch is red from what they are if it is blue, are propagated along certain nerves to the higher centres of my brain. What happens then? Well, if the motions correspond to red I see red and if they correspond to blue I see blue. But why should I? The redness or blueness has not been conveyed to my brain; all that has been conveyed is motions. The only possible answer, on this set-up, is that I must somehow possess in my mind something analogous to the decyphering tables by which a cyphered message is translated into ordinary language. I translate the motions into the sensuous quality which they symbolise.

Now a person who can believe this is capable of believing anything. It is obviously nonsense. When I see a patch of colour I

see a patch of colour. If I am told that the decyphering is done by my 'subconscious' I must reply that these words convey nothing to me, and in any case are not an explanation. That we are conscious is certain. That there exist material organisms which are things-in-themselves, i.e. which exist out of all relation to any consciousness whatsoever, is a metaphysical hypothesis the truth of which, to put it mildly, is highly speculative. If psychical research is to keep clear of 'speculative associations' it must stick to the 'person to person' account of those phenomena which, as a matter of purely *scientific* method, have in the past been legitimately distinguished as 'telepathic' from 'clairvoyant'.

F. H. CLEOBURY

### PRECOGNITION AND PK

SIR,—Dr Thouless's important and otherwise admirably lucid paper 'A Report on an Experiment in Psycho-kinesis with Dice' (*Proceedings*, Part 179) contains one passage which seems to require clarification. I refer to the two paragraphs (pp. 112-113) on the strength of which Thouless makes the following claim for the method he has employed: 'Success in such a PK experiment can only be explained by precognition if we make the *absurd* assumption that when we are doing a PK experiment, we can precognise with an *incomparably* greater accuracy than when we are doing a precognition experiment.' (My italics.) Whether or not this conclusion is warranted, it does not follow from the preceding argument. His previous paragraph leads one to expect that the rival hypothesis which he is concerned to eliminate is that the subject precognises the falls of the machine-thrown dice and causes the corresponding targets to be selected by some means other than PK. In the argument which follows, however, Thouless discusses only the possibility of the subject's precognising the series of events which determined the target-selection. But in what way could this possibility be relevant? What purpose could such precognition have served, since the subject was in any case informed by his senses of the results of the target-selection before he started to try to influence the machine-thrown dice? Surely no one would claim to explain the extra-chance correspondence between the dice-falls and the targets by supposing that the subject precognised *both* of these series of events!

According to the rival hypothesis which ought to be considered, we must suppose that Thouless (*qua* subject) precognised the faces which would preponderate in the ensuing block of falls from his mechanical thrower. The question is whether it was possible for him to influence the selection of the corresponding targets by any



means other than PK. Now Thouless tells us that he selected blocks of targets by choosing one of seventeen  $6 \times 6$  Latin squares and then permuting its rows and columns several (we are not told how many) times in ways determined by a series of manual dice-throws. Thouless acknowledges that manual dice-throwing is not an adequate test for PK since success may be due to an acquired skill. The rival hypothesis, then, is that Thouless's results were due to precognition plus skill in dice-throwing. We cannot, however, try to apply this hypothesis in detail because we are not told what rules were adopted in permuting the rows and columns of the Latin squares. If, for example, Thouless performed an irregular number of such operations (though I do not imagine he did this) his decisions when to stop might have been influenced by his precognition. But apart from this possibility, skill in hand-throwing might have enabled him to control the arrangement of the targets to an appreciable extent.

To return to Thouless's above-quoted conclusion, surely it is not necessary to suppose that we can only eliminate the PK hypothesis if we assume 'that we can precognise with an *incomparably* greater accuracy than when we are doing a precognition experiment'. In order to explain the results in his machine-thrown series, whose average 'efficiency' was a little below  $1\frac{1}{2}$  per cent, we should not, as Thouless implies, need to postulate precognition which was 100 per cent efficient. It would surely be sufficient to postulate an average efficiency of about 12 per cent for *both* the precognition *and* the manual control of the dice used in target-selection.

The best argument against this counter-hypothesis would be to point out that in the earlier experiments for PK when Thouless used hand-throwing his efficiency was less than 1 per cent. (Thouless indicates, however, that it was appreciably higher in the earlier stages.) We can conclude, then, that the results in his machine-thrown experiments must be attributed to PK, *unless* we suppose *either* that Thouless precognises much more accurately when he is not trying to do this, as in a PK experiment, than he does when he is trying, as in a precognition experiment, *or* that Thouless can control the fall of hand-thrown dice much more efficiently when he is not trying to do this than when he is trying. This consideration does make the counter-hypothesis very un-plausible—but not, I think, *absurd*. For as Thouless emphasises later in his paper (p. 117 and pp. 123-4), it seems to be favourable to a person's exercise of psi-faculties that he should not be trying.

C. W. K. MUNDLE

SIR,—Mr Mundle's suggestion as to how PK results with targets determined by Latin squares might be explained as due to a combination of precognition and skilled throwing is ingenious. I do not think, however, that it is a possible explanation of my results.

He asks what rules I adopted in permuting rows and columns of the Latin square. My method was to throw four dice at a time from a shaker. These were marked A, B, C, and D. Suppose the dice fell A4, B2, C2, D1. I put the row starting with 4 as the top row, that starting with 2 as the second row, and that starting with 1 as the third row (ignoring the fall of C because the row starting with 2 was already placed). I would then throw all four dice again. If they now fell A6, B2, C3, D6, I put the row starting with 6 as the fourth row, and that starting with 3 as the fifth row (again ignoring the falls of B and D because these numbers had fallen earlier). The only row now left is that starting with 5, so that would become the sixth row. The columns were then permuted in the same way. This system of permutations was performed once only to obtain each square. There would be no advantage in repeating the process since every Latin square that can be derived from a particular starting square can be obtained by one system of permutations.

Mr Mundle asks an interesting question when he considers what percentage of success in skilled throwing and precognition would be necessary to obtain a given rate of scoring. The figures he gives are, however, far from correct. In saying that a 12 per cent rate of success in each would give a  $1\frac{1}{2}$  per cent scoring rate he overlooks two facts. First, to get success it would be necessary to have the row position and the column position both right as well as the precognition, so that a 12 per cent success in each of the operations would lead only to hitting the right block of falls  $\cdot 17$  per cent of times (not 1.4 per cent). Even this would, however, not mean that the score was  $\cdot 17$  per cent. Mr Mundle seems to assume that a hit on the right block would give 100 per cent right on that block. This would be true only if the blocks were of single falls. In my experiment, the blocks were of 12 falls, and I find that the average expectation of scoring by the best scoring target face would be 2 over mean chance expectation. So the expectation of scoring, assuming 12 per cent accuracy in dice control and precognition, would be the negligible amount of  $\cdot 03$  per cent (less than one unit in the total score of my experiment IIA).

One could, of course, deal with the question in another way. Assuming that the rows have been permuted, the chance of getting a given column into the best scoring position (assuming 12 pe

cent efficiency of precognition and dice control) is 1.4 per cent. But now we are dealing with bigger blocks, and the mean chance expectation of scoring by putting a column into the best scoring position is only about +3 to +4 (on 72 falls), which leads to a lower expectation than that resulting from assuming control of both rows and columns.

If we ask the question the other way round: what expectation of success in precognition and dice control would be necessary to account for a  $1\frac{1}{2}$  per cent rate of scoring, we find it is slightly less than 50 per cent. A 50 per cent rate of success in each would lead to a total score of 2 per cent. Did I exaggerate in saying that success by precognition assumed an 'incomparably greater accuracy' in precognition than any we have observed in a precognition experiment?

If a rate of precognition of the order of 50 per cent is far greater than any that has been observed, even this would be of no use on Mr Mundle's hypothesis unless it were accompanied by success in dice control of the same order. We have no evidence of any ability to control dice falls by skill, and if such an ability existed at anything approaching this level of success it could not have failed to be observed. The only way I can see in which manner of throwing might effect dice falls is if one used biased dice and found that one way of throwing maximised the bias effect while another way reduced it to a minimum. The first way of throwing would then give a slightly enhanced expectation of scoring on one of the faces favoured by bias. At best the effect would be small and quite insufficient for the present purpose.

There seems no reason for supposing that by skill one can throw a die so that a given face will be more likely to fall uppermost, but an even more fantastic assumption must be made if one is throwing four dice at a time, that one can control which dice will fall which way. If that can be done to any degree at all, I think we should all agree to accept it as evidence for PK and against skill. I must confess that I did not adopt this method in order to meet the point that Mr Mundle has urged but only to save myself trouble.

The most important question is not whether I have succeeded in making an experimental design which eliminates explanation of PK success by precognition, but whether in principle it is possible to do so. Even if one agreed that Mr Mundle's suggested explanation in terms of precognition and skill were a valid criticism of my experiment, it could very easily be met by eliminating any choice (by dice or otherwise) in the selection of the arrangement of target faces. One could simply take the first Latin square in the Fisher and Yates tables and use that unchanged throughout an experiment. My own method for some time past has been to use a

succession of the same five Latin squares in all experiments, repeating the series unchanged after the fifth. This also was originally adopted as a time saving device but it seems adequate to meet Mr Mundle's objection.

R. H. THOULESS

### WATER DIVINING

SIR,—The journal of the Society for Psychological Research for March–April 1951 contains a note by D. P. on the report of what he describes as a ‘carefully designed research project’ which appeared in the Journal of the American Society for Psychological Research for January 1951.

As stated in their report the problem the experimenters set themselves was simply, ‘Can water diviners find water under conditions in which the professional geologist is unable to do so?’

In the light of many years objective experience of dowzers and their craft, I would like to make a few comments on the most obvious fallacies in the rather one-sided contest described in the report.

If the experimenters had known more about the practical results of dowsing they might not have thought it worth while to carry out this experiment, for numerous cases are on record where a dowser has located underground water in an area where a geologist has failed to do so.

In any case the test as arranged was not calculated to solve the problem, as by selecting a site which was practically waterlogged the conditions were such that a geologist would be *certain* of finding water in it.

It is not apparent that any of the twenty-seven dowzers were really possessed of skill and experience and one, ‘an adolescent girl’, could not possibly have been.

The site selected was entirely unsuitable, as water stood a few feet below the surface and no dowser could be expected to work satisfactorily in such an area. This fact alone is enough to damn the whole experiment.

The dowzers were not allowed to work freely. They were blindfolded and manhandled. This sort of personal interference would be enough to disconcert any good dowser. A scientific dowser would get all the information he could from wells in the neighbourhood and in estimating depth and flow would employ a process involving measurements requiring a certain amount of time to carry out.

This test adds one more to a list of many others carried out by uninformed experimenters under unsuitable conditions and from a scientific point of view is misleading and quite without value.

(Colonel) A. H. BELL

President, British Society of Dowsers

SIR,—Your reviewer speaks very highly of the recent American field experiment in dowsing. He concludes that 'if a test of this kind is not a valid test of dowsing then we may as well throw up the sponge and declare that the phenomenon, as claimed, is untestable'. I wish I could agree with your reviewer's estimate of the experiment and with his conclusion, but I cannot. The names of the sponsors of the experiment are a guarantee of a conscientious and sincere piece of work: but that, alas, is not enough. It is surely a rather elementary principle of scientific methodology that when the attempt is made experimentally to reproduce an alleged phenomenon, the conditions of the experiment must be the conditions under which the phenomenon is alleged to occur. There can be an exception to this rule only if these conditions are such as to make verification impossible. Thus, in our field, if a medium claims that certain phenomena will occur in his presence only in the dark and in the absence of control, the investigator is obliged to suspend judgment, and is perhaps even entitled to reject the claim on the ground that it is untestable. Dowsing fortunately is not an alleged phenomenon of this order. Nothing is easier than to put dowsing practically to the test under natural conditions: nothing is easier, but unfortunately nothing in psychical research is more expensive. That is the crux of the matter, that is why after many years of effort I have never been able to organize a proper investigation. The American group have tried hard to do it cheaply, but I fear that they have produced a quite inconclusive report.

THEODORE BESTERMAN

SIR,—The twenty-seven dowsers accepted the conditions and 'in spite of . . . minor difficulties . . . it was apparently the belief of all that the rod responded normally to underground water'. The American team made ultra-modest claims for their experiments, as I was careful to emphasize by extensive quotation.

In *The Divining Rod* by Sir William Barrett and Theodore Besterman (1926) a whole chapter is devoted to experiments with dowsers who were blindfolded and often 'manhandled'. These experiments were for the most part regarded as successful. It is difficult to resist the conclusion that it is only the *unsuccessful* dowsing experiment which evokes criticism.

Dowers and their sponsors could help us by setting down an irrevocable statement of the conditions they will accept for a practicable definitive test. The test should be designed to eliminate or allow for geological knowledge or inference on the part of the dowser.

DENYS PARSONS

#### THE STATISTICAL EVALUATION OF GROUP EXPERIMENTS

SIR,—Dr Schmeidler is perfectly right, and except on the academic issue I was quite wrong. Although it is true that a correct statistical method must always treat the targets as variable and the guesses as given, I was wrong in concluding that 'we cannot expect to get more significant results by using more percipients to guess at the same targets'. We *can* expect to, because a group opinion (guess) will, in general, be better than an individual opinion—random effects cancel out while systematic effects accumulate. The most that can be said is that, however many percipients we use, we cannot on any legitimate statistical method get any result better than would be obtained by a single percipient scoring at the level of 100 per cent success (except conceivably by bringing in further external evidence) and that up to this limit the introduction of more percipients gives diminishing returns. However, this peak is so high and for normal material the returns diminish so slowly that these limitations have almost no practical importance. I therefore accept all that Dr Schmeidler says, apologise for raising a red herring, and thank her for not trouncing me as severely as she would have been quite entitled to do.

CHRISTOPHER SCOTT

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## THE RELATION OF SOME PERSONALITY RATINGS TO ESP SCORES

A REVIEW OF RECENT RESEARCH

BY BETTY M. HUMPHREY

IN recent years a growing interest has developed in the possibility of finding personality characteristics that distinguish the subjects who do well in ESP tests from those who score at the chance level or even below. The discovery of such characteristics would serve a twofold purpose. First, by knowing what kinds of persons succeed or fail in ESP tests, we should gain in understanding the nature of ESP, its possible inhibitors and facilitators. Second, the possession of personality ratings that, with some degree of success, enable us to predict a subject's ESP scoring level should give us increased control over ESP.

We are by no means at the stage of possessing reliable instruments for predicting ESP scores, but the results of the ESP-personality research programme have been encouraging. A brief review of several of the findings may help in judging the progress attained so far, and suggest areas for further research. This review will deal primarily with some of the researches of the Parapsychology Laboratory at Duke University, since readers of this Journal are already familiar with the important work of Dr Gertrude Schmeidler (4).

A great deal of the research has involved the use of two general methods of personality classification. By one method subjects are given personality ratings based on certain characteristics shown in their freehand drawings. The other general method of subject rating involves two different ways of scoring subjects' responses to items in an interest inventory questionnaire. This summary of the findings relating to these personality measures will include all available data, both published and unpublished. Recent unpublished researches involving other personality ratings will be reviewed briefly.

EXPANSION-COMPRESSION AND CLAIRVOYANCE  
DRAWINGS RESULTS

In present-day clinical psychology, projective techniques are being used widely for personality studies. In a projective test the subject is given a relatively unstructured situation to which he is required to respond, and in so doing he reveals certain characteristic ways of seeing life and of reacting to it emotionally. Certain criteria for judging personality characteristics from drawings have been devised, and one of the most useful for ESP research has been that of expansion-compression (1, 2). In applying this criterion a judge considers primarily the *form* quality of a drawing—not *what* was drawn, but *how* it was drawn. The person who is rated 'compressive' usually uses faint, timid lines in making his drawing; he commonly makes tiny little pictures squeezed into a small area of the paper. A subject is rated 'compressive' if the qualities of his drawing indicate that he has been unwilling or unable to express himself freely. An 'expansive' person, on the other hand, makes a drawing with free, bold lines, placing objects on the page with a good sense of proportion and proper relationship with the background. Whereas the compressive subject often draws conventional, stereotyped, or abstract pictures, the expansive person usually draws pictures that show some imagination. In brief, then, the compressive drawing indicates that the subject was inhibited from expressing himself freely, while the expansive drawing reveals no such inhibition on the part of the subject.

One of the classical experimental methods of psychical research requires the subject to draw his impressions as to the content or theme of concealed target pictures. These ESP drawings are quite amenable to the same type of analysis in terms of form qualities as clinical psychologists use in evaluating free drawings. Here the ESP test *is* the projective personality test. By studying the drawings a subject has made in an ESP test, we may get clues as to his attitude towards the test situation: the drawing may indicate whether the subject was entering wholeheartedly into the ESP test or whether he was tense and inhibited.

Up to the present time, a total of twelve clairvoyance<sup>1</sup> series with drawings have been evaluated by the expansion-compression criterion.<sup>2</sup> In all of these series the procedure and scoring methods

<sup>1</sup> Throughout this paper the term 'clairvoyance' is used for convenience in referring to the type of ESP test in which the nature of the target pictures or the order of ESP cards are not known at the time of the test. No attempt was made in any of these 'clairvoyance' series to exclude the possibility of precognitive telepathy.

<sup>2</sup> The first four series were carried out at Stanford University under Dr Stuart's direction (6); five were the work of experimenters at the



devised by the late Dr C. E. Stuart (6) were used. In a typical test session the subject is presented with a sealed opaque envelope and asked to draw his impressions as to the content or theme of the enclosed picture. Ordinarily he makes four drawings to four different target pictures in one session.<sup>1</sup> Later, the four response drawings and the four targets are presented in a random arrangement to an independent judge for scoring. Briefly, the judge's task is to give each response drawing a score of 4, 3, 2 or 1 to indicate the degree of its resemblance to each target picture. The material is scored in this way by two independent judges who do not know the true order of the response drawings or target pictures. From the figures assigned by the judges, it is easy to compute a numerical score for each set of drawings. The figures so obtained may be compared with the known 'chance' score and evaluated by conventional statistical procedures.

The expansion-compression rating is also made by an independent judge (usually myself) who does not know the ESP scores of the set of drawings. The ESP scores of the expansive subjects are then compared with the ESP scores of the compressive subjects.

The average ESP scores of the expansive and compressive groups of subjects in each series are shown graphically in Figure 1. In ten of the twelve series, the expansive subjects as a group obtained a higher average ESP score than did the compressive subjects. When the probabilities associated with the difference in each series are combined (by Fisher's method), we find that, considered in its entirety, the expansive-compressive difference is significant with a probability of approximately .005.

It is interesting to note that the *total* results of these series are insignificant. Without the help of the expansion-compression criterion in separating two different types of subjects, we would probably have rejected these series as being of no value in the search for evidence of ESP or for clues as to its nature. Undoubtedly the time-consuming drawings procedure would have been abandoned in favour of the quicker, more productive card-test measures.

The results shown in Figure 1 provide evidence that ESP was functioning in these series as a whole. We see that generally the

Parapsychology Laboratory of Duke University. One of the series was reported by Nash and Richards (3) in connection with their PK work at Washington College, Maryland. Of the two remaining series (unpublished), one was done by Gladys Gillman, an undergraduate student in philosophy at Skidmore College, and the other by Lillian Ellis, a psychology teacher at Edgewood Park, a junior college in New York State.

<sup>1</sup> In three of the available series, each subject made only two drawings in the clairvoyance test.

group of subjects labelled expansive hit the target pictures much better than the group of compressives. In fact the tendency is for the group of compressives to *avoid* the target pictures to a significant degree. Now it is also of interest to see how many of the individual subjects followed the trend shown by each group. In the twelve series there were 140 expansive subjects. Of these 54 per cent had positive ESP scores, 7 per cent gave the expected chance score, and 39 per cent gave ESP scores below chance expectation. From these figures we see that the majority of expansive subjects followed the group trend and that their group result is not due to merely a few outstanding subjects.

In the compressive group there were 345 subjects. Of these 42 per cent gave positive ESP scores, 6 per cent gave 'chance' scores, and 52 per cent gave negative ESP scores. Here again we have the majority (although a small one) contributing to the total effect.

From a study of the expansion-compression ratings of subjects who have participated in more than one experimental session, it becomes apparent that the rating is indicative of the subject's mood of the moment. A given subject's drawings may be compressive at one session and expansive at the next. Furthermore, analysis reveals that the subject's ESP scores reflect this change from compression to expansion, the score on the latter occasion being higher than on the former. Even within the course of a single session, too, a subject may change from compressive to expansive, or vice versa, and his ESP scores tend to change accordingly.

It at once becomes apparent that a measure dealing with transient moods of the subject would be a valuable adjunct to parapsychological research. The records of most subjects, even the outstanding ones, show fluctuations which, so far, the experimenters have been unable to predict in advance. A measure such as expansion-compression affords an objective basis for separating data obtained from a subject experiencing different moods or attitudes. Undoubtedly there are other psychological measures of mood which may be devised for use in ESP test situations; it would seem worthwhile for parapsychologists to devote some time and thought to the perfecting of additional 'mood-catchers'.

#### EXPANSION-COMPRESSION AND GESP DRAWINGS SCORES

In general ESP tests (GESP), an agent in another room or behind a screen (in group tests) looks at the target picture while the subjects make their drawings. Otherwise the conduct of the tests and the scoring method are the same as for the clairvoyance drawings series.

To date, the expansion-compression criterion has been applied to nine such GESP series. The results, as may be seen in Figure 1, are opposite to those of the clairvoyance series: in GESP tests, the compressives tended to make higher ESP scores than the expansives. The difference between the two types of subjects for all series pooled is statistically significant. Eight of the nine series followed the trend stated.

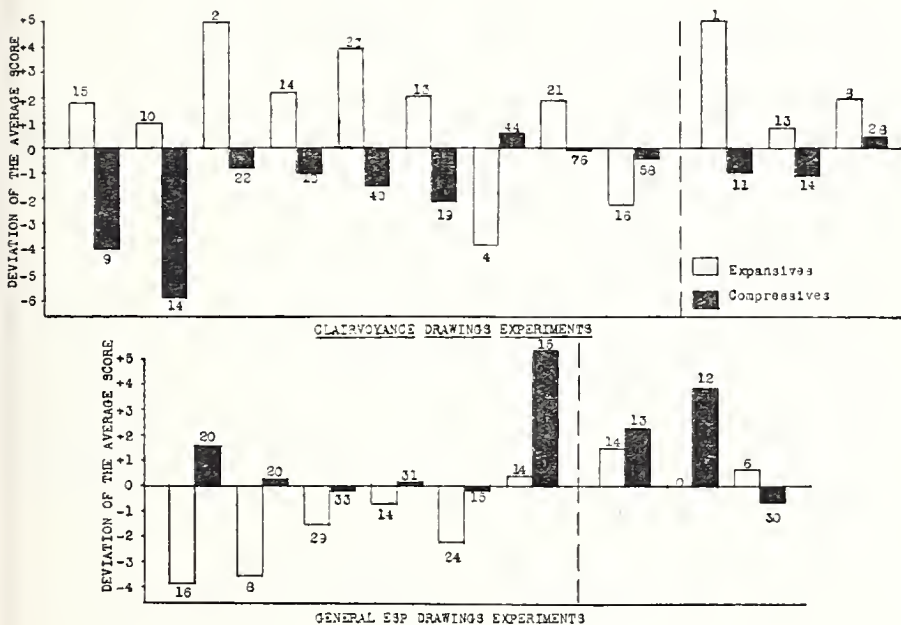


FIGURE 1

Deviations of the average scores of expansive subjects (white bars) and of compressive subjects (black bars) in all available series of ESP drawings tests. In series graphed to the left of the vertical broken line, each subject made a full set of four drawings, while in the series shown after the vertical line, each subject made only two drawings. The numbers given above or below each bar indicate the number of subjects contributing to the deviation indicated.

This consistent difference in clairvoyance and GESP results constitutes one of our most perplexing problems. One may speculate about possible personality differences which might cause compressives, for example, to do poorly in a situation where they are strictly on their own (clairvoyance) and to do well in tests where they have an agent to ' help ' them (GESP). However, the real answer to this problem must wait for further research. This finding does emphasize the need in personality-ESP studies to keep in mind the total psychological situation of the ESP tests. The fact that a certain personality rating works well in separating subjects in a certain type of ESP test is no guarantee that it will

work equally well on data of another type gathered by a different experimenter under different psychological conditions.

#### EXPANSION-COMPRESSION AND ESP CARD SCORES

Because the ESP drawings tests require quite a bit of time, not only in having the subjects make four drawings, but also in getting the material scored by two judges, we tried a number of experiments to see if the expansion-compression rating would distinguish high- and low-scoring subjects in ESP card tests. In such experiments the customary procedure was to have the subjects make one or two drawings aimed at concealed targets and then have them take two or four runs of ESP card tests. The drawings were not scored for ESP but were used only as the basis for the expansion-compression rating.

To date, twenty-nine separate experiments have been completed in which clairvoyance card tests and drawings were given each subject. Of these, twenty-six were done at Duke University and three were carried out by experimenters in other parts of the United States. Details of the experimental safeguards are not necessary here. Suffice it to say that the cards were always screened from the subjects' view and that copies of the subjects' calls were always collected by the experimenter before the true card order was revealed (5).

Psychological conditions in the series vary widely. Some of the experiments were conducted for purposes other than that of testing the effectiveness of the expansion-compression rating; in others, variations in instructions were introduced in order to try to influence the expansion-compression rating. Most series involved testing of groups of subjects, but in a few the subjects were tested individually. In some series the drawings were made before the ESP card runs, and in others they were made afterwards. The expansion-compression rating is based on one drawing per subject in some series; in others it is based on two drawings each; and in three series, four drawings provide the basis for the rating.

The most stringent test of the expansion-compression rating then would be to ignore the variations in psychological conditions and to consider the results of all available data. Doing this, we find that the total results are insignificant; that is, the difference in average run scores of the expansives and compressives is not statistically significant ( $CR = 1.15$ ), although the results do follow the same trend as that shown in the clairvoyance drawings test data. (The 955 runs by expansive subjects gave a deviation of +62, while the 1949 runs by compressives had a deviation of -51.)

A second glance at the total results reveals that the twenty-six

Duke series follow the drawings trend more closely, and that the difference between the two groups of subjects in these series alone has a probability of approximately .024. The non-Duke series, then, show a reversal of the effect which is not statistically significant. The four series in which subjects were tested individually give a much larger difference than that found in the twenty-five group-test series.

Other subdivisions of the data into logical classifications are also possible, but it is not feasible now to go into the details concerning these. In general, the data suggest that the expansion-compression rating is more effective when it is based on two drawings rather than one, and when the drawings are given before the card runs rather than after them. Its effectiveness also seems to be a function not only of the experimenter's personality, but also of other psychological conditions in the test situation.

In general, then, we may sum up the card-test results by saying that there is some tendency for expansive subjects to score positively on clairvoyance card tests and for compressives to score below chance expectation. In the 29 experiments, 17 followed the trend just stated, 9 showed a reversal, and 3 showed no difference at all between the two classifications of subjects. Thus, the expansion-compression measure is by no means as effective in separating high and low scorers in card tests as it was in ESP drawings tests.

#### INTEREST INVENTORY MEASURES AND CARD-TEST RESULTS

*Midrange-Extreme Measure.* In 1946 Dr C. E. Stuart published an interest inventory consisting of a list of sixty items which subjects in ESP tests were asked to check on a five-point scale varying from 'like it very much' to 'dislike it very much'. The list included events, studies, and objects commonly of interest to college students in the United States; for example: tennis, picnics, formal affairs, zoos, history, mathematics, cameras, dogs, and the like. A simple method of scoring the subject's answers was devised by Stuart, and by this method subjects were divided into 'midrange' and 'extreme' groups. Midrange subjects were those whose inventory scores did not differ appreciably from the average score of their group; extreme subjects, on the other hand, were those whose inventory scores were either high or low in relation to the average group score. Testing this method of separation on one series of clairvoyance drawings data, Stuart found that the midrange subjects tended to do well in the ESP test, while the subjects in the extreme group showed a tendency to score below chance expectation.

Following this finding, Stuart went on to try the measure in separating ESP card test data. At the time of his death he had applied the measure to eight series, all of which are also included in the expansion-compression research just discussed. He found a highly significant difference in the clairvoyance card test scores of the two groups of subjects classified according to their interest inventory scores.

I have continued this research and have to date completed the analyses on thirty-two experimental series in all, including the eight Stuart himself analysed. Many of these series have also figured in the expansion-compression research; the experimental conditions were essentially the same as those mentioned briefly in the expansion-compression section. Almost 900 subjects have participated in this research.

In the total of approximately 1700 card runs made by the mid-range subjects, more hits were obtained than expected by chance, while, in the same number of runs, the extreme subjects obtained fewer than the expected number of hits. The difference between the average ESP scores of the two groups is significant with a probability of approximately .0004.

The analyses to determine how many of the subjects in each group followed the group trend have not been completed for all 900 subjects. The results are available, however, for the data of the first 517 subjects; so far they do show that the majority of the subjects in each group follow the expected trend; that is, the majority of the midrange subjects gave positive ESP results, while the majority of the extreme group gave negative results.

*Fourteen-Item Scale.* Another method of scoring the Stuart Interest Inventory has also been devised from a comparison of the way a group of high-scoring and low-scoring ESP subjects answered the items in the inventory. In the list of sixty items, fourteen were answered differently by high-scoring and low-scoring subjects in three series of ESP tests. A system of scoring these fourteen items was set up, and subjects who made a certain number of points by this system were labelled 'high', and those having inventory scores below this point were called 'low'. 'High' subjects were expected to give positive ESP scores, while 'low' subjects were expected to have ESP scores below chance expectation. The effectiveness of the new scale was tried first on thirteen of the series previously mentioned in the expansion-compression section. The subjects rated 'high' on the scale did obtain positive ESP scores, while the 'low' subjects made ESP scores below 'chance', as predicted. Again, it was found that the majority of the subjects followed the trend expected of them.

Since the first trial of the scale, it has now been applied to the

data of sixteen additional series with rather disappointing results. The trend shown in the early series has not held up consistently. In fairness to the measure, it perhaps should be mentioned that the later series include widely differing psychological conditions; and it is almost certain that the effectiveness of any measure of personality is a function of the total experimental atmosphere, including experimenter's personality, instructions to subjects, type of ESP test, physical environment, etc. Nevertheless, if we ignore these psychological differences and pool the results of all twenty-nine series to which the measure has been applied, we find that the difference between the ESP scores of the 'high' and 'low' groups is such as would be expected by chance approximately three times in a hundred.

#### COMBINATION OF THE TWO INVENTORY MEASURES AND EXPANSION-COMPRESSION

Increased efficiency in separating high- and low-scoring ESP subjects results when we combine the personality measures. For example, in combining the two inventory measures, we compare the average ESP score of those subjects who received both mid-range and 'high' ratings with the average ESP score of subjects who were rated both extreme and 'low.' The ESP results for these two groups are graphed in Figure 2 for the total of the twenty-nine available clairvoyance series (the same series as were discussed in the preceding sections). There we see that the combination of the two measures gives a greater degree of separation than was obtained by using each measure alone. The CR of the difference between the average ESP scores of the midrange-'high' subjects and the extreme-'low' subjects is statistically significant ( $P = .0005$ ).

Now if we select from the midrange-'high' group those subjects who were also rated expansive in their drawings, we find an even higher rate of ESP scoring. And, if we take from the extreme-'low' group those subjects who also received compressive ratings, we find they give a lower rate of ESP scoring than did the whole extreme-'low' group. In Figure 2 these results are also graphed for the available sixteen series.<sup>1</sup> The difference between the two groups is significant with a probability of .0016. Although the difference between these two subject groups is larger than the differences obtained from the two inventory measures combined, it is not as significant because in using three measures together we do not have as many subjects.

<sup>1</sup> Expansion-compression ratings were available for only sixteen of the twenty-nine series involved in the interest inventory studies. Subjects in the other series did not make drawings.

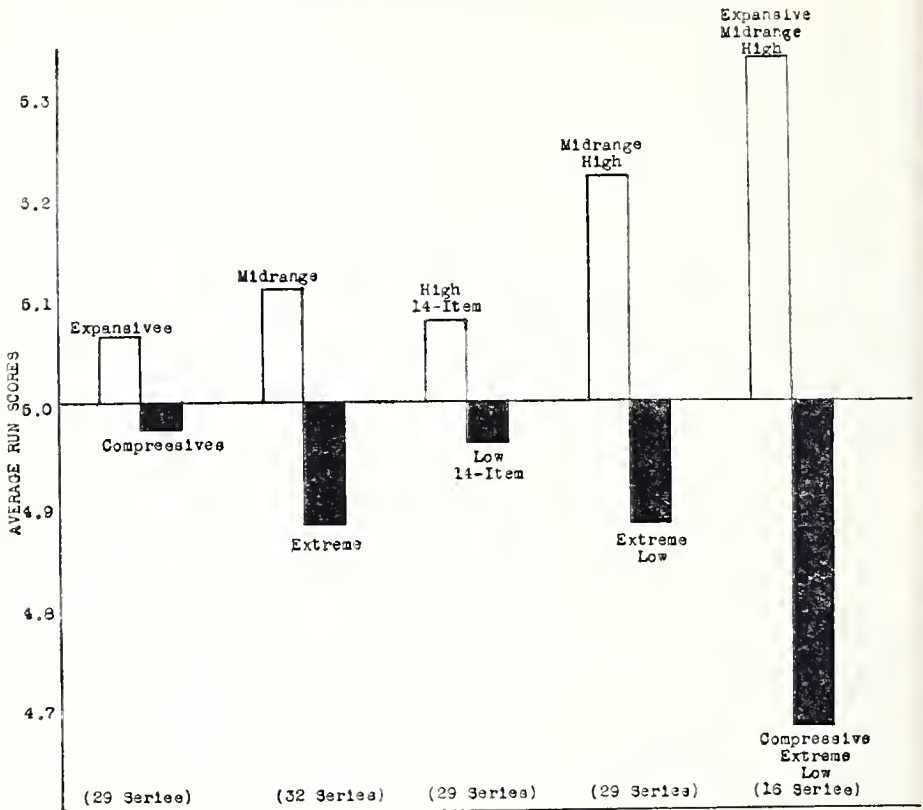


FIGURE 2

Average run scores on clairvoyance card tests of subjects rated by several measures of personality. The results for the expansion-compression criterion are shown first, followed by the results of each of the two interest inventory measures. Then the separation of the data by the combination of inventory measures is shown, and finally, the inventory measures and the expansion-compression criterion are combined.

In the expansive-midrange-‘high’ group there were 44 subjects in all. Of these 26, or 59 per cent, gave positive ESP results. In the compressive-extreme-‘low’ group of 114 subjects, on the other hand, 68 per cent gave negative ESP results.<sup>1</sup> Thus, when the three measures are used in combination, there is a higher rate of success in placing high- and low-scoring subjects in their proper group. In these series, a total of 104 subjects out of 158, or 66 per cent of the subjects, were correctly placed in high- or low-scoring groups. This increased efficiency resulting from combinations of personality measures suggests the value of giving subjects batteries of personality tests. Perhaps by tapping other areas of personality we may gain an even higher degree of precision in predicting subjects’ ESP scores.

<sup>1</sup> The chi-square from a contingency table based on these figures is significant, with a probability of .003.



OTHER MEASURES OF PERSONALITY  
IN RELATION TO ESP SCORES

In recent research two questionnaire-type personality tests have been administered to subjects in ESP series. Since the results of these experiments have not yet reached the publication stage, I shall give only a brief preview of the findings. In one clairvoyance card-test series, subjects were given the Bell Adjustment Inventory. By means of this questionnaire subjects are given ratings in several different areas of adjustment (social, emotional, health, etc.). In this first use of the questionnaire there appears to be a relationship between emotional adjustment and ESP scoring level. Subjects with 'average' or 'good' ratings on emotional adjustment gave positive ESP scores, while those with 'unsatisfactory' ratings made negative ESP scores. This is the first trial of the Bell Inventory in ESP research, but the results warrant its inclusion in other experiments.

The second questionnaire referred to is the Bernreuter Personality Inventory. To date the Bernreuter has been given to subjects in three experimental series. In all three, subjects rated 'extravert' gave positive ESP scores, while 'introverts' scored at the chance level or below. More than 70 per cent of the extraverts obtained above 'chance' ESP scores, and more than 70 per cent of the introverts gave 'chance' or negative ESP scores. Thus, this introversion-extraversion scale of the Bernreuter promises to be one of our most valuable methods of classifying ESP subjects.

## DISCUSSION

The findings of these personality studies indicate that we are making some progress in determining the distinguishing characteristics of high- and low-scoring ESP subjects. The work done so far is only a beginning, but the findings are encouraging enough to warrant a full-scale attack. It is probable that the emergence of ESP depends upon a variety of factors. The systematic study of ESP scores in relation to ratings on single personality characteristics is a worthwhile venture, but if ESP is dependent upon a number of personality factors, we should achieve a greater measure of success in predicting ESP scores if we take into account combinations of personality traits. We have seen from the graphs of Figure 2 that the most successful separation of subjects occurred when the three ratings of personality were used in combination. As we learn of other personality variables related to ESP scoring and are able to use them jointly, we may be able eventually to predict with a much higher degree of precision the

ESP score that will be given by a particular subject under certain conditions.

Once we have used a variety of personality ratings to predict a given subject's score, we must not, however, expect the subject to achieve that score no matter who tests him, no matter what type of ESP test is given, and no matter what the psychological conditions of the tests are. Until we discover otherwise, it is safest to assume that the personality factors distinguishing high- and low-scoring subjects are a function of the total experimental situation. For example, analyses of the series in which expansion-compression was used show that some experimenters did not get as large an expansive-compressive difference as did others. And as we saw in the ESP drawings experiments, expansives and compressives did not give the same results in clairvoyance tests as they gave in tests where an agent was present. Thus, whenever we find personality ratings that appear to correlate with ESP scores, we must keep in mind that they may be applicable only in experiments where the psychological conditions and the specific ESP test are closely similar to the situation in which the correlation was found.

The results of these particular personality-ESP studies so far indicate that the personality ratings are *not* separating subjects on the basis of the *amount* of ESP shown. Neither individual measures of personality nor the combination of them give us, on the one hand, subjects with a fair degree of ESP ability and, on the other, subjects with little or no ESP. Rather, what we have are subjects who hit the target material and subjects who avoid it. In each case, the ESP scores of contrasting groups based on personality ratings are about equally deviant from the expected chance score. For example, in the clairvoyance drawings data, the compressives' ESP scores are as different from the 'chance' score as are the ESP scores of the expansives. Both groups of subjects, then, demonstrate ESP to about the same degree. The measures of personality that we have been discussing separate subjects in terms of the *direction* from 'chance' in which their ESP ability functions. They tell us whether a subject will score positively or negatively, but not how much ESP he will show.

At first glance, it appears that Dr Schmeidler's Rorschach and sheep-goat studies do not fit in with the hypothesis that personality ratings have been separating subjects principally in terms of the sign of their deviations. Her well-adjusted 'sheep' gave positive ESP scores, well-adjusted 'goats' gave negative ESP scores, and the poorly-adjusted sheep and goats scored near the chance level. It appears that here we may have a combination of measures that would enable us to predict not only the direction of ESP deviation (above or below 'chance'), but also the amount of ESP—since

the poorly-adjusted subjects gave 'chance' scores. Although it would take a tedious statistical study to determine whether or not the poorly-adjusted subjects score consistently close to the chance level, Dr Schmeidler's own impression is that some of the poorly-adjusted subjects score high and that others score low, making it impossible to predict the score level of the whole poorly-adjusted group. If her impression is right, we have as yet found no personality characteristics that we can be sure are related to amount of ESP ability. And yet there must be some differences in make-up which distinguish the Pearces, Shackletons and Mrs Stewarts from the rest of us. It will be interesting to see if further research can discover any personality characteristics related to degree or amount of ESP ability.

To add to an already complex picture, I should perhaps mention briefly another type of problem encountered in research with the expansion-compression rating. There was no significant difference in the clairvoyance card-test scores of the expansives and compressives, but we have found that the 'position effects' or distribution of hits through the session are quite different for the two types of subjects. The expansives tend to start off well, decline in scoring somewhat, and then rise again at the end of the session. The compressives, on the other hand, make a very bad beginning (quite significantly negative), and then begin to score well toward the last half of the test session. In fact, the compressives do as well in the third quarter of their sessions as the expansives do in either their first or fourth quarters. Even in the series where there is no difference at all in total scores of expansives and compressives, these differences in hit distributions have been found.

This finding suggests that the expansion-compression rating is not dividing subjects according to whether they will score positively or negatively, as we might suppose from looking at the earlier graph. Rather it appears to be separating subjects as to type of hit distribution they will give. Whereas we found that compressive subjects as a group gave negative ESP scores, we now see that this score was due entirely to a bad beginning and that these compressives are also capable of making positive ESP scores. It may be that we can devise test situations to bring out high scoring rates in compressive subjects.

Position effect studies on the data of other personality-ESP researches have not yet been carried out, but they will be done also to see if the other measures could be separating subjects in terms of hit distributions instead of in terms of direction of deviation (positive or negative) from 'chance'.

So far, the majority of personality researches have involved

clairvoyance test scores. In a few experiments, tests of general ESP were given the subjects. The striking differences in results of these two types of test in relation to personality ratings suggest that we may have stumbled on to an important problem here. If we can discover the cause for what appears to be a major difference between the two types of psi test, we may gain valuable clues for the understanding of psi. For similar reasons it may be important to extend our study of personality to subjects in other types of psi tests, for example, in tests of precognition and psychokinesis.

The personality-ESP researches can hardly be said to have solved many problems yet. They have given us some fair methods of predicting subjects' ESP scores; and by separating high and low scorers, they have shown that ESP was operating in series whose total deviation was insignificant. But, on the whole, these researches have raised many more questions than they have answered. And in bringing these problems to light, they have helped to indicate a number of important areas for research.

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## TWO 'ARRIVAL' CASES

THERE is a well-known class of psychic experiences known as 'arrival' cases, the characteristic of which is that the percipient has an impression, in a dream or otherwise, of meeting a person or receiving a letter, and subsequent events verify the impression. But it is a common feature of these cases that at the time of the impression action has already been begun which will in the ordinary course of things result in its fulfilment. A question therefore arises—provided, of course, that the facts are such as to make improbable an explanation by chance coincidence—whether the impression is strictly precognitive and relates to an entirely future event, or whether it represents the state of things as it exists at the time it is received. Two examples of this type of case are printed below.

It will be noted that in each of these cases veridical statements, apparently paranormal, are mixed up in the dream with other elements that did not correspond to the facts of the case. Considered as a whole, therefore, the dream content presents a distorted picture of the actual situation. Distortion is very frequent in dreams, and it is necessary to disentangle from it any veridical elements a dream may contain and to consider these separately on their own merits. As an interesting example of distortion may be mentioned the case in Mrs Sidgwick's paper in *S.P.R. Proceedings*, Vol. XXXIII, p. 257, in which a dreamer saw his fiancée with a swollen face looking out of a window of a house on fire. The facts were that the lady had gone out on a cold night to watch a house on fire, and had caught a cold which had brought on toothache and a swollen face. The dream therefore corresponded to the actual incident in two particulars: (1) a swollen face, and (2) a house on fire, which had in fact a connexion, but jumbled them together in a way which did not at all correspond with the actual circumstances. It is conceivable, but hardly probable, that two such apparently unrelated elements should be reproduced in a dream by chance coincidence.

W. H. S.

## CASE NO. I

Professor F. J. M. Stratton, F.R.S., has communicated to the Society an account given him during the week 14-19 August 1950 by Dr J. W. Hunkin, the late Bishop of Truro, of a dream which took place on the night of Sunday-Monday, 2-3 July 1950 and appeared to refer to a parcel he received by the morning post on

the following day. Dr Hunkin put his own account of the dream in writing on 20 September 1950, and answered in a letter of 5 October 1950 some questions put to him by Professor Stratton. Unfortunately, his sudden death after an operation removed the possibility of obtaining direct evidence from him as to some points which are not clear from these written statements. These points are, however, satisfactorily covered by Professor Stratton's recollection of his conversation with the Bishop when he first heard of the case, and by written statements obtained from Dr Hunkin's secretary, Miss Norma Wheeler.

Dr Hunkin's written account of his dream was as follows:

At breakfast I told of my dream. I had gone into a shop to buy wine. They had brought me a very small bottle, price 7s. 6d. I said, 'I should like something less expensive.' They said, 'Yes—this is a proprietary wine which makes the price higher.'

After breakfast, I went into my study and my secretary, Miss Norma Wheeler, opened a parcel containing just such a tiny bottle (it was a sample of Communion wine) and noted that the accompanying paper stated the price (of a full size bottle) as 7s. 6d.

20 September 1950.

(Sgd) J. W. HUNKIN, Bishop of Truro

Appended to Dr Hunkin's statement were the following confirmations:

The Bishop related the dream as stated above to me before he or I had seen the post that morning.

20 September 1950

(Sgd) NORMA M. H. WHEELER

My father related this incident to me later the same day. I questioned the above witness and another as to the sequence of events and was satisfied that it was as stated above.

20 September 1950

(Sgd) PATIENCE HUNKIN

Dr Hunkin's statement does not make it clear where he first saw the parcel in question. Was it, possibly, in the room where he had breakfast, and was it later moved into his study where it was opened by Miss Wheeler? In Professor Stratton's account, however, of his conversation with Dr Hunkin he says that 'On going into his study later he found a parcel containing a tiny bottle', and in a subsequent letter to the Hon. Secretary of the Society Professor Stratton adds, 'My memory is clear that the Bishop had not seen the parcel until after breakfast.' There is accordingly no reason to infer that Dr Hunkin's recollection of his dream was in any way coloured by his having seen the parcel containing the bottle among the rest of his mail before he related the dream.

Dr Hunkin told Professor Stratton that he had noted the incident in his diary on the day of its occurrence, but in a letter of 21

September he wrote that when he came to examine his diary he found he had not recorded the incident after all, the reason being that on that day he went to London and did not enter up his diary until after his return some time later. He then wrote it up very briefly and forgot to record the incident.

A question, of course, arises as to whether the receipt of a sample bottle of Communion wine was a frequent occurrence in Dr Hunkin's experience. Professor Stratton questioned him on that point, and he replied by his letter of 5 October 1950 that this was the first little sample bottle which he had received for a number of years, that is, since he had been at Truro. As regards the possibility that his mind might have been directed towards samples of Communion wine by advertisements seen in Church newspapers, Dr Hunkin's reply was that he took the *Guardian* and occasionally the *Church of England Newspaper*, but was not in the habit of paying the slightest attention to advertisements. Professor Stratton examined the issues of both these papers from 3 July 1950 back to the beginning of June, and found only one advertisement of Communion wine, and that was of a brand of wine different from the sample received by Dr Hunkin, and at a different price.

After Dr Hunkin's death his secretary, Miss Wheeler, confirmed the fact that the parcel was not on the breakfast table, and added that she could not say for certain whether it was marked or labelled but did not think it was: it might have had an S.P.C.K. label, but she could not say for sure. She gave a small drawing of the parcel, which would not appear in itself to suggest the nature of the contents. There is, however, one point in her letter which may possibly suggest a reason why the purchase of Communion wine should have been in Dr Hunkin's mind at the time of his dream. It is that over the weekend there had been an Ordination Retreat, and Miss Wheeler and Dr Hunkin had prepared the Chapel late on the Friday evening for a celebration on the Saturday morning and had had a short hunt for the bottle of wine.

Dr Hunkin says nothing on the important question whether he frequently had dreams that impressed him so much as to cause him to repeat them to members of his household. On this point a question was addressed to Miss Wheeler who replied on 5 December as follows:

I am sorry for the delay in answering your letter of 26th November. I wanted to consult Mrs Hunkin before making any statement about the Bishop's dreams. I think I had better quote her letter to me. My impression was that the Bishop did dream a good deal, but that they were generally of a rather vague nature. Mrs Hunkin says: 'Regarding the Bishop's dreams, you are right in saying they were generally vague.'

But now and again he would relate a vivid one in detail. But I never remember any other which was fulfilled. He would tell me a dream like that sometimes when he first woke, but it was only occasional.' I should say this was an exceptional dream, both as regards the unusual subject and the detail remembered in it.

It would appear, therefore, that the dream was an exceptional incident, that nothing had happened of a kind likely to prompt it by normal association of ideas, and that the receipt of the sample bottle was also an unusual incident. Curious as the correspondence is, it should be noted that it is not precise, as in the dream the price quoted for a small bottle was 7s. 6d., whereas the note accompanying the sample bottle quoted 7s. 6d. as the price for a full-sized one. Moreover, the receipt of the parcel by post did not correspond to the dream-incident of Dr Hunkin going to purchase a bottle in a shop. When, however, due allowance is made for the usual degree of distortion met with in dreams, it must be admitted that the correspondence between an unusual dream and an unusual event was rather striking.

#### CASE NO. 2

The second case is of a dream of the arrival by post of a book, which was, unexpectedly, delivered by the postman the following morning. It was reported to the Society by the percipient, Mr J. E. Taylor, of Nottingham, in the following letter:

The other night I experienced a prophetic dream which was so vivid and so soon fulfilled that I thought it might interest your Society. I am a schoolmaster and have not, previously, taken any great interest in psychical phenomena but at College I remember reading G. N. M. Tyrrell's book, *The Personality of Man* in which he asked that all cases, however trivial, might be submitted to your Society.

I must first explain that in May 1950 I enrolled for a course by correspondence and ordered the set books from the University Book Co. Ltd. of London. One book, a short history of the Roman Empire, was out of stock and the book company promised to send it when it became available. I had forgotten all about this book as, owing to a recent illness caused through overwork, I was forced to suspend the course and so no longer required the book.

On Thursday evening, November 30th, my wife and I attended a function of my local R.A.F. Association and after several weeks enforced temperance the few drinks I had affected me more than usual. This fact is relevant, for a slight attack of illness caused me to rise at 5 a.m., so fixing the time of my dream as being between my return to bed at 5.15 and 7.0 a.m. when I woke.

I dreamed that I got up feeling ill from a 'hangover' and heard a scuffling at the front door. I went into the hall and saw a huge pile of



assorted magazines being pushed through the letter box and beneath the door. I thought the two temporary postmen had made a mistake and delivered the mail for the whole street to our house. Then came a knock at the door. I thought, 'The postman has come back to explain his mistake.' But no, the postman handed me a rectangular parcel, sewn up in a crimson cloth. I tore open the cloth and discovered the book from the Univ. Book Co. and a bill for the amount due.

An incongruous point was that although our hall opens directly on to the street, in my dream the postman came through a front gate and along a path through a garden to our front door.

When I woke up I told my wife of this vivid dream and remarked, 'I'd forgotten about that book. I ought to write and tell them to cancel it otherwise they'll be sending it one of these days.' When I went downstairs I did experience a hangover and halfway through breakfast a knock came at the door. My wife answered it and came back into the room with a brown paper parcel. I opened it and there was the book from the Univ. Book Co. and the bill.

This coincidence, if coincidence it was, was so strange as to give me quite a sense of shock. It is important, I think, to notice that I told my wife of the dream before it was, in part, fulfilled.

I hope this minor experience might prove of some value for the purpose of your records.

2 December 1950

(Sgd) J. E. TAYLOR

I declare that my part in the events stated above occurred exactly as my husband has related.

2 December 1950

(Sgd) W. J. TAYLOR

In view of the possibility that the connexion between the dream and the arrival of the book was fortuitous, the Hon. Secretary wrote as follows:

May I then enquire whether you are a frequent dreamer, and, secondly whether there was anything either in the degree of vividness of this particular dream or its nature, which made it appear to you exceptional at the time. Then as to the correspondence between the dream and the arrival of the book, can you suggest any reason why in the dream the book should have been preceded by a large number of magazines, and the postman should have approached your front door by a path through a garden which you knew was not there. Also, can you remember whether the dream book corresponded in colour, size, and general appearance with the real book, and whether the dream bill stated the price correctly? It is probable, however, that you already knew what the cost of the book would be. Although there had been a considerable lapse of time between your ordering this book and its arrival by post, it may be that you fairly often have received other books by post, so that there would be no very great significance in the dream of the arrival of an expected book occurring shortly before some book or other actually did arrive.

Mr Taylor replied as follows:

I am a fairly frequent dreamer but my dreams are usually of a different type. I usually start off by reading a book and then I become personally merged in the story and undergo fantastic adventures. I have tried writing some of these dreams down immediately on awaking but they make queer reading. The dream I have related to you was much more prosaic and intensely vivid.

With regard to the incident of the magazines and the non-existent garden, I believe these were some form of wish fulfilment. We live in a house without a garden, due to the present housing position, and our dearest wish is to buy a house with a garden. I am a great reader and a pile of crisp new magazines is a pleasant sight to me.

In my dream the book was of the same colour, grey, and size as the real book. I cannot remember seeing the price on the bill. I just knew it was the bill. I cannot account for the fact that the book was sewn up in red cloth in my dream. I have never received such a parcel. The real book was in a brown paper parcel.

I receive books by post perhaps three or four times a year but this particular book was not expected. I had forgotten all about it. It was the dream that recalled its existence to my mind and caused me to mention to my wife that I ought to write and cancel it, but of course it arrived the next morning.

(Sgd) J. E. TAYLOR

## REVIEWS

ANALYTICAL PSYCHOLOGY AND THE ENGLISH MIND. By H. G. Baynes. Foreword by C. G. Jung. London, Methuen, 1950. ix, 242 pp. 18s.

COLLECTED PAPERS, VOLUME V. By Sigmund Freud. Edited by James Strachey. London, Hogarth Press and the Institute of Psycho-Analysis, 1950. 396 pp. 25s.

No one in any way concerned with psychical research could afford to ignore, even should he wish to do so, the pointed relevance of psycho-analytic theories to his own field of special study, and to those who so approach them both of these volumes will be found of interest. Both are collections of essays and papers among which some are concerned with topics of psychical research.

Dr H. G. Baynes, who died in 1943, was the chief exponent of Jung in this country and the present volume contains most of his shorter papers together with the first three chapters of an unfinished book. The two papers directly concerned with psychical research will already be known to members of the Society for Psychological Research. 'The Ghost as a Psychic Phenomenon' was read at a private meeting of the society in 1936. 'Jung's Con-

ception of the Structure of Personality in relation to Psychological Research' was published in the Society's *Proceedings* in 1941. The author's interest in psychical research is also reflected, in some measure, in the essay from which the volume takes its name. 'Analytical Psychology and the English Mind' is an entertaining study of our national character as this character appears when cast into a Jungian mould. The English people are 'peculiar', he says, in possessing an extraverted psychology complicated by the influence of an introverted minority voice. Their national myth has found expression in the 'idea of a gentleman'. True, the Latin word *gentilis* had acquired even in ancient Rome something of the same connotation of 'derivation from good stock'. True, too, that the idea has affinities with others all over the world, even with the ancestor-worship of the Chinese; but the special character with which the English temperament has invested the idea of a gentleman comes 'without doubt' (*sic*) from the legends of King Arthur and his knights. The Arthurian 'principle of fair play', it is said, pervades every aspect of our life. The gentlemanly code of the public schools and the Universities is based 'solely' (*sic*) on this principle.

Characteristics of English life that cannot be so explained spring from the 'introverted interior'. Hence, side by side with conventionality is the readiness to welcome new ideas, e.g. the psychology of Jung. To this element in our nature—or is it perhaps the ancestor-worship element?—must be attributed our partiality to ghosts, and our benign attitude to the S.P.R. 'In no other civilized country are there so many haunted houses to be found.' In no other literature is there so large a place found for the 'dimension of the unconscious'. And so on.

'Well, well!' the hard-headed, extraverted Briton may say. 'All this may indeed be true, but the author hardly sets out the evidence that such is in fact the case.' To which, of course, the introverted minority voice may well reply: 'Why, indeed, should he? Someone must excogitate hypotheses for other people to test.' These two comments would be equally appropriate for all the other essays in this book. All are equally provocative to those who ask for evidence; all are equally exciting to those who get on well enough without it.

Nor, in Freud's papers, is it always easy to distinguish speculation from supporting fact. But it is not possible to generalize about the essays in this book. This fifth volume of the Collected Papers consists of odds and ends—mainly the shorter writings published since 1925 and miscellaneous papers excluded 'for various reasons' from the earlier volumes.

The essay of chief interest for psychical research is that on 'The

Occult Significance of Dreams', in which Freud extracts from evidence *against* precognition evidence *for* telepathy. Briefly the argument is that the fortune-teller's prediction expresses what the sitter desires, unwittingly obtaining the knowledge of these desires by telepathic means. Freud believes that thought transference occurs most easily when the idea is in transition from the unconscious to the conscious mind. He suggests further that telepathic messages unconsciously received during the day may come into consciousness in dreams that occur during the following night. Through psycho-analysis it may accordingly be possible to obtain further and better authenticated knowledge of this telepathic process. From these two books alone it is not possible fairly to assess the relative contributions of Jungian and Freudian psychology to the progress of psychical research. On a first impression it might be tempting to suppose that the Jungian position is more sympathetic than the Freudian to the aims and tentative conclusions of research in this field. When, however, it comes to a clear-cut issue, as for example the question of survival after death, the disciple of Jung is no less non-committal than the disciple of Freud. No statement could be more non-committal than that of Dr Baynes. 'We cannot determine whether the spirit world is an aspect of the impersonal psyche or whether it exists independently. It is practically impossible to decide whether the spirits of the dead exist independently of the psyche or whether they live because we give them life.'

Of greater interest than the question of commitments is the difference between the two schools in their attitudes to scientific method. The Jungian is much less deferential than the Freudian to the authority of science, and proportionately more sympathetic to the claims of 'intuition'. In this respect Freud himself was almost oldfashioned. These differences, however, do not carry the implication that psychical research has more to hope from Jung than it has from Freud. The implications, such as they are, work the other way. Nothing is more striking in the recent development of psychical research than the increasing rigour of its scientific methods. Those who pursue this research challenge the natural scientist on his own ground. They claim no privilege and they ask for no quarter. If a choice had to be made between mysticism and logic, Freud and the modern psychical researcher would, without question, find themselves down on the same side of the fence. If there is any difference between them it might even be that modern psychical research is in fact the more exacting in its demands for scientific proof.

I APPEAL UNTO CAESAR (The Scripts of Cleophas). By Geraldine Cummins. London, Psychic Press, 1950. 189 pp. 10s. 6d.

This further instalment of the Scripts of Cleophas needs only a brief notice. In character it closely resembles its predecessors, and as a reconstruction of certain incidents in St Paul's life and of the last days of Mary, the Mother of Jesus, is not without a certain vividness of presentation. This might be expected, since, whatever be the source of the material, it has come through the mental processes of Miss Cummins, who is an accomplished novelist. The difficulty in making any scientific use of such automatic scripts as these is obvious. Even if they do contain traces of genuine historical evidence it is quite impossible to identify or verify them. In some respects, as in the use of such names as Adaiam and Tyrronnis, the records do not sound very probable. And where they do agree with modern discoveries of one sort or another it is impossible to say whether Miss Cummins may have had access at some time to those discoveries, either directly or through the mind of Miss Gibbes or of some other person. To say this is not to deny her complete sincerity as an automatist. But whether as history or as material for the study of automatic writing the scripts have no value. Their interest, which is real enough, is simply that of a historical novel, and as such (and, it is to be hoped, only as such) they will be enjoyed by many.

L. W. GRENSTED

JOURNAL OF THE AMERICAN SOCIETY FOR PSYCHICAL RESEARCH.  
Volume 45, No. 2, April 1951. New York, A.S.P.R. \$1.50.

Dr C. B. Nash, a biologist, who was responsible for some of the better-designed early PK experiments, contributes a paper in which he discusses the possibility of Precognition in explaining dice-throwing results. 'It is possible that the significant results in PK tests so far reported have been due to precognition instead of PK. In any case a test of pure PK has yet to be performed.' This, of course, was written before the publication of Thouless's investigations which were designed to eliminate the possibility of precognition (*Proceedings*, XLIX, 1951, pp. 107-30).

Hereward Carrington discusses the criteria by which the evidence for psychic phenomena should be judged. For physical phenomena, he thinks the ideal psychical researcher should have these qualities: a thorough knowledge of trickery and the psychology of deception, accurate observation, infinite patience, calmness, extreme caution, a grounding in biology, psychiatry, etc. and some knowledge of laboratory technique.

For all branches of the subject the researcher should have made

a thorough study of the literature, and a knowledge of psychology and an open mind are all-important.

Mrs Salter reviews the scripts of the S.P.R. group of automatists who were responsible for the well-known 'cross-correspondences' during the years 1901 to 1930.

In 'Psi patterns amongst the Australian Aborigines', Lyndon Rose summarises ten interesting cases. 'There was no attempt on our part to document these stories completely. Our interest in them lay in their patterns, and the leads they could give to our research. The probability or otherwise of these stories having foundation in fact lay in the standardised tests applied to the aborigines and the analysis of them.' The last remark refers to unpublished PK and ESP tests, but one cannot help regretting the missed opportunity to present these spontaneous cases in fully documented form.

Mrs L. A. Dale reviews *Sixty Years of Psychical Research*, by Joseph L. Rinn, published last year in New York. The text is shown to be a tissue of irresponsible misrepresentations and inaccuracies. Mr Rinn, who set out in this book to discredit all psychic phenomena and research workers, is now thoroughly discredited. (See also W. H. Salter's review in the S.P.R. Journal for May-June 1951.)

Of particular interest is an item in the Report of the A.S.P.R. Research Committee. A research group consisting of Dr M. Ullman, Dr J. L. Woodruff, Mr W. Triebel, and Mrs Dale, are investigating the question of whether or not the physiological concomitants of psi effects can be detected instrumentally.

D. P.

## THE FRAUDULENT MEDIUMS BILL

THIS Bill, which received its second reading in the House of Commons on 1 December 1950 (see S.P.R. *Journal* for January-February 1951), was read a third time on 20 April 1951. It was given its first and second readings in the House of Lords on 24 April and 3 May 1951 respectively.

# Society for Psychical Research

31 Tavistock Square · London · WC1

SUPPLEMENT

TO

JOURNAL

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FOR MEMBERS AND ASSOCIATES

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## ANNUAL GENERAL MEETING

THE Annual General Meeting of the Society was held at 31 Tavistock Square, London, W.C. 1, on Thursday, 26 April 1951, at 3 p.m., under the chairmanship of Lord Charles Hope.

The Secretary having read the notice convening the Meeting, the Report of the Council and the Accounts were presented. The Hon. Secretary, W. H. Salter, in proposing the adoption of the Annual Report and Accounts, gave a review of the Society's work during the year. His remarks are printed below. The adoption of the Annual Report and Accounts was seconded by P. S. Seward, and carried unanimously.

The Chairman announced that there were no candidates for election to membership of the Council other than the six members who retired by rotation, and who offered themselves for re-election, and on the proposal of W. H. Salter, seconded by Denis Chesters, the following six members were accordingly unanimously elected: Mrs Oliver Gatty, Dr S. G. Soal, Professor F. J. M. Stratton, the Rev. C. Drayton Thomas, Dr R. H. Thouless, and G. N. M. Tyrrell. On the proposal of Mrs W. H. Salter, seconded by Professor F. J. M. Stratton, Messrs Miall, Harper & Co. were re-elected Auditors for the forthcoming year.

## HON. SECRETARY'S REVIEW OF THE YEAR

By the time of the next Annual Meeting, in 1952, this Society will have completed three score years and ten, having had a much longer period of unbroken activity than any other Society formed for similar purposes. In view of this fact, and of the recent changes in the Society's organization, it might be well that we should devote a little time this afternoon to a general stocktaking. If you will allow me, I will in proposing the adoption of the Annual Report of the Council advert briefly to some points which I think it is desirable for our members to bear in mind, taking as my text the name of the Society, the Society for Psychical Research.

I will deal first with the last word, 'Research'. This Society conducts many activities, holding meetings, issuing publications of many kinds, and maintaining a Library. All these things are useful in themselves, but their value should be judged according to the contribution they make to our research. We are, I think, all agreed that our founders were right in laying down the principle that we should approach our problems objectively and 'without prejudice or prepossession of any kind'. There have indeed from time to time been movements in the Society, the effect, if not the object, of which would have been, had they succeeded, to make the Society no longer a Society for research but one holding definite opinions and doctrines regarding the subject matter of our investigation. I think that it is now generally recognized among our members that a move in that direction would be a mistake.

I now turn to what is perhaps a more difficult and debateable question, the meaning of *Psychical* research. Our founders very wisely did not attempt any exact definition, and in fact pursued a very large number of lines of inquiry which it might have been difficult to reduce to any logical formula. The danger seventy years ago may have been one of too great dispersal of effort. But too great concentration on any one branch of our subject may have dangers of its own. We may congratulate ourselves that we have been able to avoid this. In psychical research—as, I think, in philosophy and psychology—particular problems have a way of forcing themselves on the attention of inquirers for a time, and then falling into the background, so that the vital issues of one generation seem of very remote interest to the next. The same thing can be traced in the history of psychical research, as may be seen if the amount of space devoted in our *Proceedings* in any decade to one side of our subject be compared with the amount given in the next. There was, for example, a period during which the energies of the best minds within the Society were devoted to the elucidation of automatic writing, and a corresponding amount of space in the *Proceedings* was devoted to their labours. Between the wars a very great deal of skilled energy, printing space, and money was expended in the investigation of physical phenomena. In the last year or two before the War our main attention became focussed on quantitative experiments in ESP, and is still so centred. I think this periodical change of interest is unavoidable, because in each case the phenomena raised problems which required thorough investigation and discussion. Although, however, the main interest was directed now to this and now to that branch of our studies, inquiries on other lines were going on concurrently. Speaking for myself, I feel sure that it would be a most mistaken policy if we were to abandon or to relegate to a subordinate position those traditional lines of investigation which do not happen at the moment to be in the fashion, because I believe that there is a very difficult problem of human personality involved, and that if there is any practicable solution of it, it will only be achieved by attacking it from a great number of different points at once. We all admire the notable successes which have been achieved in parapsychology by Professor Rhine and the other members of his staff whom we had the pleasure of meeting in England last summer, but parapsychology, as hitherto understood in America, seems



to be a very much narrower affair than psychical research, and it is to psychical research that we as a Society are committed.

And we are a Society, not a University department. In a University it is possible for the Head of a Faculty to direct his staff to conduct investigations along particular lines, and to exercise a close supervision over the work they are doing. This has never been attempted in our Society, and in view of the very wide scope of our investigations it would be hopeless to attempt it. Our members have always been encouraged to pursue whatever lines of research make a special appeal to them, and they are at liberty, if they so desire, to get advice and assistance from fellow members, including of course the Officers of the Society.

As a result of our having no organic connexion with any University or other official body, and being as we are dependent largely on the subscriptions of members, we find it both desirable and necessary to deal by correspondence and interview with a large number of inquiries and suggestions from within and outside the Society that would not probably come our way if we were a University department. Those who have worked for any length of time in the Society's offices would, I think, agree that they have learnt a great deal from these letters and interviews, but at the same time they absorb a great deal of the Officers' time, frequently with little to show for it. We must, however, look at it from the point of view of the persons who write to the Society or seek an interview with one of the Officers. They are often persons in a state of bewilderment, and sometimes distress, as the result of 'psychic' experiences that have befallen them. It is no unworthy task, I think, to give these people such help as we can, help that they will certainly not get in as enlightened and disinterested a way from any other quarter. Little by little each letter, each interview, helps to spread among a public lamentably ill-informed on these matters a more sensible and more scientific view of psychical research. Ultimately, therefore, and in a most roundabout way all this *does* promote research: it brings us in new members, it brings us in new contacts with psychic phenomena of every kind.

The problem which has always faced the Society is how best to combine two objects. First, the general day-to-day business of the Society, including all this business of contacts with members and the public which I have already mentioned, and (2) the more technical side of our research. A governing factor is, of course, the financial position of the Society. For the last twenty years and more the Society, as is natural in a body dependent almost entirely on the support of the professional classes, has been in difficulties. In almost every Annual Report that I have drafted during those years I have used the phrase 'financial stringency', or I have with difficulty restrained myself from doing so. Certainly a year ago things were pretty bad, and we had an excess of expenditure over income amounting to £576. During the present year things have taken a much more hopeful turn, and although the last thing the Council desire would be to encourage undue optimism, they can point with pride to the fact that they have this year achieved a surplus of some £648 of receipts over expenditure. The principal causes of this are set out in paragraph six of the Annual Report. I may

remind you that the Society has not during the year had a salaried Research Officer. Dr West has obtained an appointment at the Maudsley Hospital, and we all wish him the greatest possible success in his profession. He will still reside on the Society's premises and be able to give time to psychical research. Then we have been enjoying for a second year the fruits of the Appeal which drew such a generous response from our membership: we earnestly hope that this help will be continued. Lastly I would mention the lapse of the pension of £175 paid to Miss Newton during her life.

Some re-organization both of the general work and of the research has become necessary, and members will like to know what arrangements have been made. As mentioned in the Annual Report for 1949, the Council were able to persuade Mrs Goldney to undertake the post of Organizing Secretary, and the successful conduct of the Society's business during the recent year has been in a large measure due to the energy and efficiency which she has brought to her task and to the very friendly collaboration between herself and our Secretary, Miss Horsell. She has been able to deal successfully with a very large number of matters cropping up from time to time to which it would have been extremely difficult for any of the other Officers of the Society to give adequate attention. Grateful mention should also be made of the very great amount of work that has been put in by Mrs Gay, who has been in constant attendance at the Rooms. She has been particularly helpful in bringing order into the past research records of the Society, which owing to war conditions had fallen into great confusion. Under the joint efforts of Miss Horsell, Mrs Goldney, and Mrs Gay I have no hesitation in saying that the business of the office is being more efficiently run than it has been for many years past. But it should be borne in mind that our office staff now consists of one full-time and one part-time Secretary. If we could afford a staff of three full-time workers, we could probably find plenty of work for them to do.

Another Officer to whom the Society owes an incalculable debt is Edward Osborn, who in addition to completely revolutionizing the *Journal*, which has now become a most valuable instrument of publicity to the Society, was the mainspring in organizing the recent Appeal, which saved the situation financially. Few of our members, unless they are themselves working in the office, know the enormous amount of time that Mr Osborn has unstintingly given to the Society.

But it still remained for the Council to see that research of the kind requiring specialized knowledge was being vigorously pursued. In the Council's view the best way of securing this was to make awards for useful pieces of research, and to invite applications for grants to help in carrying out research projects that seemed to be sufficiently promising. It is hoped that many well-thought-out projects will be submitted to the Council in response to the invitation already made.

By re-organization within the office and the offer of grants and awards the Council consider that an active research policy should be practicable. But they wish to urge on the younger members of the Society the desirability of making themselves experts in one or more of the various techniques that are becoming more and more necessary in psychical

research. Although our membership is increasing, we still have far too few workers with specialized knowledge. In this connexion I must refer to the loss the Society is about to sustain by the departure for America of one of our most active workers, Mr Fraser Nicol. He has accepted a post in the Parapsychology Laboratory at Duke University, so that the loss to the Society will mean no loss, but rather a gain, to psychical research. We wish him every success and happiness in his new home and new career.

To return to finance, the improvement in the position will certainly not encourage extravagance on the part of the Council. It is desirable, and should be possible, to increase the amount allocated for printing. Our publications are the only immediate benefit that a large proportion of our membership derive from their subscriptions, but they should bear in mind that every page of *Proceedings* or *Journal* now costs over £2 3s. 6d. a page to print (exclusive of any diagrams or illustrations).

The Society may in a few years' time be faced with heavy expense, as in 1956 our existing lease runs out. The freehold has been acquired by London University, and there is a considerable chance that the building may be needed for University purposes. In that case the Society would have to find premises elsewhere and meet the costs of removal.

During the past year the Society has received some generous legacies, and it is to be hoped that members will not lose sight of the Society's need for help in this form. Legacies are now being kept separate in the accounts from current receipts and current expenditure, a procedure which enables members to appreciate more easily what the Society's financial position is. The Council intend to use the funds standing in the separate account as a nest egg on which they can draw for special expenditure which could not easily be met from current income.

To end with a few cheerful remarks, Mr Parsons has arranged for us a fuller programme of interesting meetings than we have ever had before. There is a very marked increase in the public interest in psychical research and in the Society, as shown by references in books, newspapers, etc., and, most significant, the Royal Institution last December invited Dr Thouless to deliver a discourse on 'Thought-transference and Related Phenomena'. This is the first paper on any subject connected with psychical research ever read to the Institution since its foundation in 1799. Finally, for the first time for many years, we have a membership of over 1,000, which is still steadily increasing.

## THE SOCIETY'S ROOMS

This year the rooms, including the Library, will not be closed during the summer.

## MEETINGS OF THE COUNCIL

Meetings of the Council were held as follows:

464th 16 March 1951 Chairman : The President, Dr S. G. Soal.  
465th 26 April 1951 Chairman : Lord Charles Hope.

466th 26 April 1951 Chairman : W. H. Salter.

At the meeting of the Council held on 16 March 1951, Professor A. C. Hardy, F.R.S., was co-opted as a member of Council.

At the meeting of the Council held before the Annual General Meeting on 26 April 1951, the following co-optations to the Council were renewed for the current year : The Hon. Mrs Gay, Professor A. C. Hardy, J. Fraser Nicol, Edward Osborn, Dr D. J. West, and Dr R. Wilson. G. W. Fisk was co-opted as a member of the Council for the current year.

At the meeting of the Council held immediately after the Annual General Meeting on 26 April 1951 the following were elected :

Dr S. G. Soal was elected President for the year ; Admiral the Hon. A. C. Strutt, Hon. Treasurer ; W. H. Salter and Denys Parsons, Hon. Secretaries ; Mrs. W. H. Salter, Hon. Editor of *Proceedings* ; Edward Osborn, Hon. Editor of the *Journal*.

*Committee of Reference and Publication* : Professor C. D. Broad, Mrs Frank Heywood, Denys Parsons, Professor H. H. Price, W. H. Salter, Mrs W. H. Salter, Dr S. G. Soal, Dr R. H. Thouless, G. N. M. Tyrrell, and Dr D. J. West.

*Finance Committee* : Mrs K. M. Goldney, Lord Charles Hope, G. W. Lambert, and Admiral the Hon. A. C. Strutt.

*House Committee* : Mrs Goldney, Miss I. Jephson, Edward Osborn, W. H. Salter, and Admiral the Hon. A. C. Strutt.

*Library Committee* : Dr E. J. Dingwall, the Hon. Mrs Gay, J. Fraser Nicol, Edward Osborn, and Admiral the Hon. A. C. Strutt.

## MEETINGS OF THE SOCIETY

- 235th Thursday, 8 February 1951, at 6.30 p.m. THE REV. C. C. MARTINDALE, S.J. : 'The Fatima Case'.
- 236th Thursday, 1 March 1951, at 6.30 p.m. THE REV. C. DRAYTON THOMAS : 'Some Noteworthy Incidents in my Sittings with Mrs Leonard'.
- 237th Thursday, 15 March 1951, at 6.30 p.m. MARY SCRUTTON : 'Is Psychological Research a Rational Subject?'
- 238th Thursday, 5 April 1951, at 3 p.m. H. J. D. MURTON : 'Psychical Research and the Press'.
- 239th Thursday, 19 April 1951, at 6.30 p.m. JAMES HAYES : 'Some New Techniques in ESP Research'.
- 240th Thursday, 3 May 1951, at 6.30 p.m. DR D. J. WEST : 'A New Theory of Some ESP Effects'.

## NEW MEMBERS

### MEMBERS

(Elected 16 March 1951)

CASPARY, DR A., 303 West 80th Street, New York 24, U.S.A.  
CHAFFEE, MRS K. P., c/o Mrs E. G. Hutson, 3 Pavilion Court, Hastings,  
St Michael's, Barbados, B.W.I.

- CHAMBERS, H. F., M.V.O., 7 Montpelier Place, London, S.W. 7.  
GOLDBERG, O., M.D., 53 West 76th Street, New York 23, U.S.A.  
GREENWELL, T., 74 Central Hill, Upper Norwood, London, S.E. 19.  
GROVE-WHITE, LT.-COL. T., Parklands, Glanmire, Co. Cork, Eire.  
KRASNER, H., The Haven, Selsdon Vale, South Croydon, Surrey.  
LANDAU, L., 233 West Heath Road, London, N.W. 3.  
LIBRARIAN, LOUISIANA STATE UNIVERSITY, Baton Rouge 3, Louisiana,  
U.S.A.  
MORGAN, W. H. D., PH.D., 1 Osborne Road, Redhill, Surrey.  
OLDHAM, A. V., 178 Knowsley Road, St Helens, Lancs.  
SHELTON, P. H., 11 St George's Place, Westmount, Montreal 6,  
Canada.  
THOMAS, C. W. F., L.D.S., 362 Mare Street, London, E. 8.  
WEST, R. P. H., 72 Somerset Road, Kensington, Johannesburg, S.  
Africa.  
WINN, D. S. MERRICK, 5 Queensboro' Studios, Hyde Park, London,  
W. 2.  
WOODWORTH, L. A., 21 Hugo Street, Krugersdorp, Transvaal, S.  
Africa.

*Student Associates*

- Cole, Martin J., Connaught Hall, Wessex Lane, Swaythling, Southamp-  
ton.  
Tustin, John R., 121 Poole Road, Bournemouth, Hants.

MEMBERS

*(Elected 26 April 1951)*

- ANDREWS-LEVINGE, MRS K. T., The Manor, Stoughton, nr Chichester,  
Sussex.  
KENYON, F. E., Holly Bank, Currier Lane, Ashton-under-Lyne, Lancs.  
KIRKHAM, J. L., 184 Dover Road, Folkestone, Kent.  
LIBRARIAN, SELSKABET FOR PSYKISK FORSKNING, St Kannikestraede 10,  
Copenhagen K., Denmark.  
LINTON, S. F., M.D., M.Sc., The Old Mill, Cloughton, Scarborough.  
RIGGALL, R. M., L.R.C.P., Northumberland House, Green Lanes,  
Finsbury Park, London, N. 4.  
SIBLEY, PROFESSOR M. Q., 395 Ford Hall, University of Minnesota,  
Minneapolis 14, Minnesota, U.S.A.

*Student Associate*

- Roll, William G., 10 Northmoor Road, Oxford.



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## THE EXTENSION OF MIND

A NEW THEORETICAL BASIS FOR PSI PHENOMENA

BY J. R. SMYTHIES, M.B., B.CHIR.

### SUMMARY

The results of the recent experiments in parapsychology cannot be fitted into any current scientific theory. This suggests that the fundamental assumptions, on which natural science is based, should be reconsidered and possibly revised. The aim of this paper is to examine some of these fundamental assumptions, and to suggest a series of alternative assumptions which give a more coherent account of the universe and the place of the conscious mind in it. We can then account satisfactorily for the facts of parapsychology.

### I. INTRODUCTION

I should like in this paper<sup>1</sup> to examine the implications of the present experimental position in parapsychology for the main body of natural science. The following important fact has been established by experiment:

The mind is able to abstract information from its present and future environment without the use of any of the recognized channels of sense.

We must correlate this fact with the theory at present held in natural science of the nature of the mind and its relation to the brain. If this cannot be done we must introduce a new theory which can account for this fact. The former course would necessitate the construction of some form of 'radiation' theory. No one has yet succeeded in this, nor does it seem possible for the following reasons:

<sup>1</sup> Dr Smythies has recently completed a study in which he suggests that a number of fundamental changes should be made in the basic pre-suppositions of natural science in order to account for the genesis of perceptual space and the facts of parapsychology. These changes are expressed in a new theory, an outline of which he has kindly written for this Journal.—Ed.

(a) It seems unlikely that any simple electro-magnetic radiation could convey complex information through dura mater, bone, and scalp and up to two hundred miles of space without there being any apparent organization concerned with this function. Any penetrating radiation of this type would have to be of the X-ray or wireless wave frequency, whose energy requirements would seem to be beyond the capacity of the brain. Nothing resembling transmitting apparatus has been found in the brain.

The implication of the current theory, as presented by Gilbert Ryle,<sup>1</sup> J. Z. Young,<sup>2</sup> and others, is that the mind bears the same relation to the brain as the digestion does to the viscera—no more and no less. The brain is supposed to function according to the same principles as analogue and digital computing machines. According to this theory a thought is an extremely complex pattern of electrons in the brain, and human thinking and perceiving are done entirely by the almost fabulously complex system of interconnected nerve cells and fibres which sustain and conduct these patterns. There seems to be no room in the brain for a mechanism which would have to do the following things :

It would have to scan the incoming electronic patterns carrying the sense data from the sensory nerves, and transform these into a signal code in the form of penetrating radiation. There would also have to be a separate mechanism for receiving such information and for translating it back into the form of circulating impulse patterns. For the operation of clairvoyance, this mechanism must further be capable of abstracting information from outside objects, which would require quite different apparatus. There would have to be a device for feeding this extrasensory information into the main current of information, and, it seems, a mechanism for active censorship so that only a certain number of extrasensory hits are obtained. It would seem in a successful ESP experiment, where a significant number of these complex patterns are altered in a highly specific way, that there should be a great deal of such organization to do these highly complex things, such as abstracting information from a pack of ESP cards or from someone else's brain. There does not seem to be any trace of these mechanisms in the brain. It does not seem possible to attribute these phenomena to any primitive property of matter, or protoplasm, in view of the extremely complex nature of the process involved in changing, for instance, a series of cortical patterns ABCDEF to ABDDDF. Although ESP has only been demonstrated as a statistical effect,

<sup>1</sup> *The Concept of Mind* (London, Hutchinson's University Library, 1949).

<sup>2</sup> *Doubt and Certainty in Science* (London, Oxford University Press, 1951).



nevertheless the change from C to D and E to D represents a change from one *unique* cortical pattern to another *unique* pattern, and not to any of the other enormous number of possible patterns. Thus it would seem that we are looking for the organization necessary to execute these functions.

(b) No known form of radiation extends into the future and there is no place in natural science for the demonstrated determination of a present event by a future event. This point alone is crucial.

Therefore we must seriously consider our second course; a critical examination of the whole system of pre-suppositions and postulates from which science builds.<sup>1</sup> Since natural science is able to give such a coherent and inclusive account of phenomena inside the physical space-time continuum, any radical changes here do not seem feasible, and we must look outside these bounds for any possible advances in knowledge.

## 2. THE CRISIS IN KNOWLEDGE

The crisis in human knowledge caused by the discoveries of parapsychology affects not only psychology, neurophysiology, philosophy, and metaphysics, but also, and most profoundly, physics itself. This is because the same fundamental assumptions lie at the basis of each of these systems of inquiry. The present position of scientific knowledge rests on a tangled and interconnected series of such assumptions and theories, which lead from one field of study into another. Confusion is caused by the use of the words and concepts of 'space', 'mind', and 'psyche'. These words have several different meanings, and the confusion arises when deductions are drawn from their use in one context, when one of the other meanings of the words is really intended.

We will also discover, in all these subjects, the ramifications of an entirely erroneous theory, derived from our common-sense outlook upon the world. I will call this the 'transparent nervous system' theory (or TNS theory for short). It is that the Self, the observer, is looking out directly at the physical world extended in space and time before it, through the sense organs, as though these

<sup>1</sup> As Margaret Knight suggests may be necessary in her article 'Theoretical Implications of Telepathy' in *Science News*, No. 18. Professor Broad has called for 'very radical changes in a number of our basic limiting principles' ('The Relevance of Psychical Research to Philosophy', *Philosophy*, vol. 24, 1949, p. 306). Dr Thouless has suggested a radical re-orientation of thought ('Experimental Precognition and its Implications', *J. Soc. Psychic. Res.*, vol. 35, 1950, p. 209).

were transparent. The familiar world of perception is thus supposed to be a *direct* view of the physical world. The error is aided by our erroneous identification of the familiar body-image<sup>1</sup> in consciousness, with the physical body itself.

The following passages from E. L. Hutton<sup>2</sup> are very relevant :

‘The part played by the body in our perception of the external world is of the greatest importance, and is much neglected by realist philosophers.’<sup>3</sup> The author feels that not only has it been neglected by the realist school of philosophy but by all philosophers, and that this neglect is responsible for many of the difficulties and inconsistencies of both realists and idealists.

First and foremost it is important to draw attention to the fact that a percipient’s awareness of his own body is, like his awareness of other objects, based upon the occurrence of temporo-spatial patterns in his own cerebral cortex. . . . (pp. 154-5).

The difficulty of both realists and idealists seems to arise from a confusion of the perceived body with the percipient’s actual body. Because of the experienced dependency of all perceived objects upon the perceived state of the percipient’s body the ability to perceive them is located within this perceived body, and the mind is thought of as being located within this perceived skull ; but what is often forgotten is that this body and this skull are themselves objects of perception.

Now not only am I aware of my own body but also of other bodies similar to mine, and *spatially related* to my body, to each other, and to all other objects within my perceptual field. . . . (p. 156. My italics).

Now a scientist is a sentient individual, and as such may observe or be observed by other sentient beings, and what applies to them applies equally to him. His researches are directed towards a limited number of objects and events selected out of the total field of which he is perceptually aware, and this limitation of his interest is designated by his scientific title, geologist, physicist, biologist, anthropologist, etc. The objects of his study are brought by means of attention into the foreground of his consciousness, and as far as possible isolated and studied in detachment from the objects constituting the background of the field, including his own body. For reasons already stated many scientists are misled into believing that the objects and events *they are studying* have a real existence independently of them, whereas in fact, as we are learning from neurophysiology, these objects and events are only

<sup>1</sup> The meaning I convey by the phrase ‘body-image’ is conveyed by some people by the phrase ‘immanent perceived body’. I am postulating three spatially distinct entities : (a) the *physical body* (b) the *body-image in the brain*, and (c) the *body-image in consciousness* or *immanent perceived body*. When I use the phrase ‘body-image’ by itself I refer to (c).

<sup>2</sup> ‘The Relationship of Mind and Matter to Personality’ in D. Richter (ed.), *Perspectives in Neuropsychiatry* (London, H. K. Lewis, 1950) pp. 141-72.

<sup>3</sup> From W. Russell Brain in *Philosophy*, vol. 21 (1946) no. 133.

determined by things existing independently, their actual nature depending entirely<sup>1</sup> on the temporo-spatial patterns occurring in the scientists' own cerebral cortices. They are actually but unwittingly studying the effects of the dynamic activity which is occurring each in his own central nervous system, and the *observed* 'laws of nature' are the *observed* sequences in their own perceptual field. If similar cortical patterns are aroused in several individuals, either by a common stimulus or by separate but similar stimuli, they will all observe similar sequences in their perceptual field, and they will all deduce the same 'laws of nature'. They can be observed, both by themselves and by other sentient individuals, performing similar experiments, using similar measuring devices, and describing their work in similar phraseology. None the less, and it cannot be too strongly stressed, each man is studying what he himself perceives, and what he perceives depends directly upon his own cortical activity; the objects of his study have their existence only in him (pp. 159-60).

Not only are many scientists thus misled, but the whole structure of physical and biological science rests on assumptions arising out of this error. Not only do the objects and events the scientist is observing depend upon the temporo-spatial patterns occurring in his own cerebral cortex, but the perceptual space-system, in which they are extended, and in which they are spatially related to his perceived body, is *the space-time system of his mind*.

A useful working concept to use during this argument is that we are each connected to the physical world by an extensive signalling mechanism. At one end this is connected to various sense-organs on the surface of and inside the body. The other end we experience as our familiar visual picture of the world, its sounds, and its smells, and the signals from our body which we experience as our body-image in consciousness with its pressures, aches, and pains. Changes in these sense organs, produced by various physical stimuli, are swiftly signalled via the sensory nerves and central nervous system into our perceptual fields. We must note that we are given (as data) the Self, examining its own private world, whose events are merely determined by events in the outside physical world (including the physical body), by the signalling mechanism in between. Science has traced this signalling mechanism as far as the brain, but, as Margaret Knight says in her recent article :

Even the strictest adherent of epiphenomenalism admits that there is a

<sup>1</sup> There is no justification for the use of this word *entirely*. The author's identification of perceptual space with the spatial organization of the brain is merely an assumption. All that neurophysiology shows us is that we cannot identify *objects as observed* with the physical objects themselves. It does *not* show us that we must identify them with these patterns in the brain.—J.R.S.

complete gap in our knowledge at one point. We now know a great deal about the mechanics of sense-perception. We know, for example, that when we hear a noise, a sound-wave has affected our ear-drum, and set up a chain of activities in the middle and inner ear, in the auditory nerve, and finally in the cerebral cortex. But how electro-chemical activity in the cortex can give rise to the conscious experience of hearing a bell, is still completely obscure. As James wrote in 1892, 'something definite happens when to a certain brain-state a certain "sciousness" corresponds.'<sup>1</sup>

So we must further note that, while physical space-time is on the distal side of this gap in our understanding of the signalling mechanism, perceptual space-time is indubitably on the proximal side, and is in part a creation of the 'mind', as we may call it at present. This is not clearly recognized at present by theoretical physics.

I will return to the whole question of the extension of the mind later when considering the current concepts of philosophy, but it will suffice to mention here that both the theory that the mind does not occupy space, and the theory that it is meaningless to discuss whether the mind does or does not occupy space, are consequences of the TNS fallacy of common sense. The mind must be regarded as both clearly extended in part (the perceptual world) and apparently not so extended (Self).

There is an important passage in Sherrington's *Man on His Nature*. (My italics show where I disagree).

At the core of this difficulty is the attribute 'unextended' as applied to the finite mind. It is difficult to reconcile this attribution with certain facts. The mind of the individual, finite mind, as judged by an impressive concensus of opinion, has 'place'. It has 'whereness'; *nor does it matter for our purpose of the moment what 'where' it has*. It has a 'where'. Speaking for myself, although I can allow dialectically a Euclidean point and admit its artificiality to be a useful convention, it is beyond me to conceive or figure or imagine even approximately a concrete anything as having 'whereness' without magnitude. A thing without extension as descriptive of the mind, even though negatively descriptive, fails for me to be more than a conventional symbol. . . .

Accepting finite mind as having a 'where' *and that 'where' within the brain*, we find that the energy-system with which we correlate the mind has of course extension and parts and exhibits, moreover, marked spatial organization of those of its parts correlating in space and time with finite mind. . . . We have, I think, to accept that finite mind is in extended space.<sup>2</sup>

The acceptance of finite mind having a 'where' does not,

<sup>1</sup> 'Theoretical Implications of Telepathy' in *Science News*, No. 18.

<sup>2</sup> Sir Charles Sherrington, *Man on His Nature* (Cambridge University Press, 1940) pp. 314-15.

however, compel us to put that 'where' in the brain, as both Hutton and Sherrington suppose. We are only compelled to do so as long as we lack any 'where' else to put it.

### 3. THE CRISIS IN PHYSICS

If we examine the concept of space we find five categories recognized:

- (i) Perceptual space.
- (ii) Physical space.
- (iii) Absolute space (of Newton).
- (iv) Abstract space.
- (ivA) Higher-dimensional space.

These are taken from Jeans.<sup>1</sup> I have only altered his classification by extracting (ivA) out of (iv). If we further examine how these are supposed to differ from each other, and how they are derived, we come across the first series of assumptions to be considered, and, furthermore, we find that the first of them is based on the TNS theory. They are:

- (1) That perceptual space is coincident with all physical space.
- (2) That four axes are sufficient to describe the physical universe, and all objects and events in it; that the same four axes may be used to describe the perceptual world, and all objects and events in it; and that there is nothing outside these four axes, i.e. in higher-dimensional space.

In order to illustrate how these assumptions lie at the basis of theoretical physics, we may consider the suppositions made by Einstein in the Special Theory of Relativity. The following passages are taken from *Relativity: the Special and the General Theory*.<sup>2</sup> The italics are mine throughout.

*The practice, for example, of seeing in a 'distance' two marked positions on a practically rigid body is something which is lodged deeply in our habit of thought. We are accustomed further to regard three points as being situated on a straight line, if their apparent positions can be made to coincide for observation with one eye, under suitable choice of our place of observation. . . . (p. 3.)*

It is not necessary here to investigate further the significance of the expression 'coincidence in space'. *This conception is sufficiently obvious to ensure that differences of opinion are scarcely likely to arise as to its applicability in practice. . . . (footnote b to p. 6.)*

The purpose of mechanics is to describe how bodies change their

<sup>1</sup> Sir James Jeans, *Physics and Philosophy* (Cambridge University Press, 1943).

<sup>2</sup> Albert Einstein, *Relativity: the Special and the General Theory* (London, Methuen, 1920).

position in space with time. I should load my conscience with grave sins against the sacred spirit of lucidity were I to formulate the aims of mechanics in this way, without serious reflection and detailed explanations. Let us proceed to disclose these sins.

It is not clear what is to be understood here by 'position' and 'space'. I stand at the window of a railway carriage which is travelling uniformly, and drop a stone on the embankment, without throwing it. Then, disregarding the influence of the air resistance, *I see the stone descend in a straight line.* A pedestrian *who observes the misdeed* from the footpath notices that the stone falls to earth in a parabolic curve. (p. 9.)

It is clearly being assumed here that the objects and events of the physical world are being observed directly. The Special and General theories of Relativity are, in part, attempts to correlate physical and perceptual space.

Einstein supposed there to be a four-dimensional space-time continuum containing observers travelling through it. Each observer is supposed to 'see' successive cross-sections of the continuum as his private three dimensions of perceptual space in time. Observers in relative motion to each other 'see' cross-sections cut in different directions. Thus all places<sup>1</sup> in perceptual space are supposed to be coincident with places in physical space-time. Perceptual space, in which it is necessary to emphasize all observations of science are made, is treated as though it were a moving cross-section of physical space-time, much as a traveller in a train sees a thin cross-section of the countryside through which he is travelling. As Jeans<sup>2</sup> says, '... all observers stand on the same footing, each dividing the space-time unity into his own perceptual space and his own perceptual time.' These suppositions overlook the fact that there is a great deal of signalling mechanism interposed between objects in physical space-time and objects as observed in perceptual space, which mechanism is quite opaque. To imagine that perceptual space and time can be derived by cutting a simple cross-section of physical space-time is to base our whole system on the TNS fallacy. The objects which the scientist actually observes, and the space in which they are observed, are a product of his own mind. It follows that these observed objects and events *may* be coincident in space with cerebral events, but certainly cannot be with the objects and events of the external physical

<sup>1</sup> In order to avoid confusion I must define 'place' as that space occupied by any object of any kind that can actually be observed, or that space occupied by any object whose activity can potentially determine the behaviour of the former object. This avoids confusion with abstract space or with the concept of space.

<sup>2</sup> Sir James Jeans, *Physics and Philosophy* (Cambridge University Press, 1943) p. 69.

world, which only determine what is observed by means of the signalling mechanism in between.

Thus we see that Einstein's statement that the nature of coincidence in space is self-evident (see p. 483) is only apparently self-evident, if we are basing our habits of thought on the TNS fallacy. Coincidence in perceptual space is easy to understand though subject to illusions, and we all know what is meant by coincidence in physical space, though we have never actually observed this. The problems are raised by considering what relation perceptual space bears to physical space.

The phrase 'coincident in space' has many pitfalls of which scientists have in general been unaware. If the objects of the scientist's study have their existence only in him, then so do those axes, of which he can be aware and by which he describes their behaviour, and so does the space system in which the objects of perception are extended, and in which these observations are executed. The realization of this fact requires a study of the relationship between these observed events and the events in the physical world. For this we need, if we are to account for the facts of parapsychology, an extended theory of relativity, a theory of the relativity of extension; in short, a theory of extension.

If we now return to the concepts of space in use in contemporary physics, we find that the observer, by his motion through the physical space-time continuum, is supposed to see this directly as its three-dimensional perceptual cross-section. The abstract and absolute spaces are supposed to be derived by reason's experience of the perceptual cross-section of physical space-time.

These concepts do not give an adequate account of what actually happens. Perceptual space cannot be regarded as a simple cross-section of physical space-time, as this depends on the TNS fallacy. Perceptual space is the only space of which we can have direct experience. We live and move and have our being in it. We note that objects as observed have three space dimensions and endure in time. If we confuse this perceived world with the physical world, we jump to thinking we are looking at the physical world directly, which then is supposed to have the same three dimensions of space and one of time. *But we are no longer allowed to do this by neurophysiology.* Physical space and physical objects are in fact hypotheses, as we can never observe them directly. We have hypothecated them out of our experience of perceived objects extended in perceptual space presented to us by this part of the mind. If we wish to correlate perceptual space with anything, we can only attempt, as Sherrington<sup>1</sup> does (*if we are to keep within the*

<sup>1</sup> See the passage quoted from Sherrington on p. 482.

*present boundaries of natural science*), to correlate it with the spatial organization of the brain, a physical object in physical space.

It should be noted that there are two categories of events in perceptual space: the events of the perceptual world over which the Self has no control; and the activities of what we call 'visual or mental imagery', which is under the control of the Self. This space is also three-dimensional in time, as we are capable of 'sketching in our imaginations' or 'visualizing' lines, planes, or cubes existing in time, but fail when we try to visualize a four-dimensional solid, the tesseract, in time. The fact that we can cope with cubes but not with tesseracts is not an indication that the physical universe has three space dimensions, as is generally supposed, but that our minds have. This internal space, which geometers are always using in their practical problems, seems to be coincident with the space in which the perceptual world is presented. For we can sketch any manner of lines round the objects of our perception, as the artist does when he sketches the outlines of his picture on the canvas in his mind before he ever puts brush to canvas. This category of space has been rather neglected. If the mind is supposed to be something unextended, how one is supposed to be able to visualize a triangle, which cannot but be extended in space, I do not know.

We may recast the space systems given above thus:

(i) Perceptual space, then, is primary, and is that in which objects of our perception, including our own body-image and our own visual imagery, are extended.

(ii) *Physical space* is that in which physical objects, including our own physical bodies, are extended. These latter are for ever unobservable, and strictly are hypotheses in Dingle's sense.<sup>1</sup> The *space of physics* is the mathematical system of extensional relationships of physical objects. It is a postulate in Dingle's sense. These relationships have been discovered by observations made by the Self on the extensional relations of perceptual objects extended in perceptual space. For instance, all measurements of length in physics depend ultimately and solely on the Self's direct power of telling which of these two lines

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is the longer. These lines, it cannot be emphasized too often, are, *as observed*, extended in the perceptual space system of the mind. Physics has been able to overlook these points and tacitly to assume the TNS fallacy, because the observed relations reproduce their determining relations in the physical space-time continuum with

<sup>1</sup> See Herbert Dingle, *Through Science to Philosophy* (London, Oxford University Press, 1937).



sufficient accuracy, by reason of the mechanical excellence of the intermediate signalling mechanism.

(iii) Abstract space comprises also a series of postulates derived by reason from our experience, not of (ii) but of (i). Although it belongs to the class of concept and so is not extended in the same way as (i), when the geometer does exercises in abstract geometry in his 'mind', he is using this internal space system described above (part of (i)).

The location of perceptual space within the brain is also only an assumption. The strength of this assumption arises from the observed facts that when we do certain things to other peoples' brains, and when certain things happen to our own brains, the state of consciousness is altered, or even 'lost'; also from a legacy of the common-sense confusion placing our Selves as observers in the physical head, and looking out at the physical world. If this latter cannot be so, then at least all this activity, Self and perceptual world, it is felt, must be located in the brain. This latter is a dangerous prejudice of thought.

It is possible to account for these facts just as well by postulating interference with transmission, instead of with actual function, or interference with the activity of certain *subsidiary* electronic mechanisms in the brain. There is no *a priori* reason why perceptual space should be coincident with physical space. We have *no* warrant for accepting this blindly. The problem must be settled by experiment, once the alternative hypothesis has been worked out, and critical experiments devised from it.

The alternative hypothesis is this :

The perceptual space-time system of the mind may be an *independent* space system, i.e. it may be higher-dimensional relative to physical space. We fill this space system with matter and attribute this with organization, and then we can postulate that the gap between the electro-chemical events in the brain and the corresponding events in perceptual space-time, which we observe, may be crossed by signalling mechanism. It is already clear that in our own individual world of perception we are observing the events presented by one end of a signalling mechanism (the stroboscope shows us this : see p. 497), so the postulation of some additional mechanism should not prove too difficult for the understanding to accept. Events in our perceptual world follow faithfully the events in the physical world, except when there is a breakdown in the mechanism. The only question remaining is whether this presentation is done by brain-mechanisms or by this, as we may term it, *psychical mechanism*. When we look at someone else's living brain, we do not see this mechanism because it is on the other side of a dimensional interface. We are thus in the

same position as Planemen<sup>1</sup> would be when confronted with a solid object passing through their world. We must also postulate that minute physical forces cross the interface in each direction, so that events in one half of the system can affect events in the other half.

This theory can be derived from the Thouless and Wiesner 'Shin' theory by postulating that Shin may be divided into Self and perceptual world, the latter the product of a series of signalling mechanisms extended in higher-dimensional space.  $\Psi_\gamma$  and  $\Psi_\kappa$  then become minute trans-dimensional physical forces. The difficulties previously falsely transferred, under the TNS theory, to the 'mind-brain' relationship, may now be placed where they belong, in the relation of the Self to the perceptual world. The relation of the perceptual world to the physical world is effected by mechanism. Just as events in the brain are merely determined (on the afferent side) by events in the outside world by the signalling mechanism in between, so may this independent and organized system of mind-stuff (or psychical mechanism) merely mirror the events in the brain, and so the events of the outside world.

Thus I suggest a revision of our fundamental assumptions to :

- (i) Perceptual space is not coincident with physical space.
- (ii) We must use a system of seven axes to describe the total physical universe.

The whole system may be described thus :

The physical universe, for one observer, consists of a seven-dimensional space-time continuum, divided artificially by reason of the nature of his observing mechanism into two subsections, each a four-dimensional space-time continuum, the two manifold systems sharing one common time dimension. (Perceptual space and time may also be considered to form a continuum, since we now have other means of distinguishing them from physical space-time than by supposing that the passage of the observer through this latter splits it up. The only means of distinguishing between them before was that one system was supposed to form a continuum, and the other was not). We may term one the 'physical' space-time continuum, and the other the 'psychical' space-time continuum, though the difference between them is not one of kind but of location.

These two space-time systems are filled with extended material systems which interact across the interface between them (by means of  $\Psi_\gamma$  and  $\Psi_\kappa$ ). Events in physical space-time merely determine, by means of the signalling mechanism in between, the

<sup>1</sup> See E. A. Abbott, *Flatland*, 3rd edition (Oxford, Blackwell, 1926).

events in perceptual space-time, which latter are the objects of the scientist's direct study. The Self can also determine events in physical space-time by means of other signalling mechanism.

Thus the world we observe as scientists, and live in as humans (existing only in ourselves), may be made not of brain stuff, as Dr Hutton suggests, but of organized mind-stuff (or psychical mechanism) *extended* in higher-dimensional space relative to the physical world. Its events are only *determined* by events in the physical world because of the extensive system for *communication* existing between the two, comprised of, in one direction, sense organ, sensory nerve, central nervous system, psi-gamma, and psychical mechanism: in the other the psychical mechanism, psi-kappa, central nervous system, motor nerve, and muscle, which conduct the will of the Self to the physical world.

Hinton,<sup>1</sup> in evolving his theory of the four-dimensional stationary universe (further developed by Warner Allen<sup>2</sup>) traversed by an observer to whom all four-dimensional objects appear directly as their three-dimensional cross-section, and changes of four-dimensional shapes as movements, was misled by the TNS theory. In such an extended universe the body of the observer cannot be the moving agent as it is extended as is the rest of the material universe of which it is a mere 'eddy'. His theory requires a psychic factor as observer, an extracorporeal agent, to produce not only the moving agent but also the 'now' of time which cannot be produced in such a universe in any other way. Everything *in* the universe merely extends from the beginning to the end of time as frozen and complex four-dimensional shapes including the body of the observer and his brain, through which this extracorporeal observer must be supposed to be travelling. This has seemed so unlike the world we know that his theory has remained very much in the background. Dunne<sup>3</sup> resurrected it, and correlated it with the higher-space dimensions. He confused these, however, both with an unnecessary serial regress of time, and the serial regress involved in thinking about the Self thinking about the Self, etc, which is merely a manoeuvre of reason. The present errors of assumption at the basis of knowledge may not be over time as Dunne imagined, but over space.

A tentative return to it has been made in the current attempt to account for the observed fact of precognition, which phenomenon is much easier to account for by using such a model of the universe,

<sup>1</sup> C. H. Hinton, *A New Era of Thought* (London, 1888).

<sup>2</sup> Warner Allen, *The Uncurtained Throne* (London, Faber, 1951).

<sup>3</sup> J. W. Dunne, *The Serial Universe* (London, Faber, 1934).

as Dr Thouless<sup>1</sup> points out. This is the theory of the 'specious present',<sup>2</sup> which, however, falls between two stools as it seems to imply a partial extension of the physical universe in this manner, but just how far is not stated. Such an extension must be all or nothing, and if it is to be all, an extraneous factor must be provided to account for a single unique and travelling position, the 'now' of time, dividing the 'Eternal Now' into a past and a future.

We may, however, suppose that the Self is not travelling through the brain, as the previous theories suggested, but that the psychical mechanism, extended in one sub-section of the continuum, merely exchanges information via the specific forces  $\Psi_\gamma$  and  $\Psi_\kappa$  with the brain. In this manner events in one subsection affect events in the other. We have then accounted, not only for the 'now' of time in Hinton's universe, the erstwhile inexplicable change from electronic patterns in the brain to the phenomena of experience, and all paranormal phenomena, but we have also been able to attribute more dignity to the human Self than by supposing it just to be an expression of the internal co-ordination of a machine.

It should be noted, however, that the question whether the material system in the 'physical' subsection of the seven-dimensional total universe is three-dimensional (orthodox theory), or four-dimensional (Hinton's theory) is only a detail as far as this theory is concerned. The psychical system could be imagined to react equally well with either, although the latter is suggested by the fact of precognition. I should therefore like to suggest that the relevant mathematical study for psychical research might be the study of the properties of hyperspace systems in relative movement to each other.

(The universe, for one observer, may even be an eight-dimensional continuum, since, if we postulate that the two sub-sections have no common dimension, then we merely have to attribute additional powers to psi.)

$\Psi_\gamma$  and  $\Psi_\kappa$  may then be regarded as minute physical forces, as far as we can detect them directly, which penetrate the physical world from outside it. They are normally focused upon the brain, but have a 'penumbra', which would be responsible for ESP and PK. The interior of the psychical mechanism, which presents that which we actually observe, reproduces faithfully the events in the physical body and its environment. This reproduction is so good that the TNS fallacy of common sense was indeed

<sup>1</sup> R. H. Thouless, 'Experimental Precognition and its Implications,' *J. Soc. Psychic. Res.*, vol. 35 (1950) pp. 201-210.

<sup>2</sup> H. F. Saltmarsh, 'Report on Cases of Apparent Precognition,' *Proc. Soc. Psychic. Res.*, vol. 42 (1934) pp. 49-103.

unavoidable. The geographical arrangement and probably relative size of objects or object-images (including the body and the body-image as observed) in each world are similar to each other, but each very different from that of the object-images in the brain, which merely forms an integrative and informative link between the two.

So if we consider the observations of two observers in relative motion to each other, only the physical bodies of each observer have this relative motion, and so cut different cross-sections of the 'physical' continuum, as described in the special and general theories. These cross-sections do not form perceptual space and time directly, but the process causes different information to be signalled back to the observer, and so causes different patterns to be thrown on to his visual screen, and other integrated changes to be made in his perceptual world. Each observer, thus misled by the nature of his observing mechanism, interprets this to mean that he himself is in relative motion to the other observer, whereas in fact it is not necessary to postulate that he moves at all. All that may be moving is the focus of the psi-fields *past* Hinton's universe, or *with* a three-dimensional physical universe travelling in time, as well as his physical body. The only movements we have ever observed are those occurring in our own perceptual fields, and these may as well be determined by mechanism abstracting information from changing four-dimensional shapes (the equivalent of movement in Hinton's universe) as from particles moving in time.

It should be borne in mind that any such psychological mechanism must be of a fabulous complexity, and in parts employing principles of which we know nothing, as we do not meet them in the 'physical' world: although we can gain a fair idea, by observing the stroboscopic patterns (see p. 497) and by analogy, of the workings of the proximal end of the visual mechanism by comparing it with television. We may represent the brain by the actors in the studio, and the physical external world by the script which is determining their behaviour. The psychological mechanism would then include the television cameras, transmitters, and receivers, all for ever hidden behind the screen of the set, upon which is continually and tirelessly presented the visual phenomena for our daily attention.

The relation between the Self, the psyche, and the body would seem to be analogous to that between the controller in the parent aerodrome and a radio-controlled pilotless aircraft. Imagine this latter to be fitted with television cameras and other instruments, which constantly signal information about its own state and the state of the environment to the control tower, inside which is

placed the controller. He observes all that is taking place on the television screen, and attends to the other information signalled back. Imagine further that he has been there for as long as he can remember, and that he cannot see, and has never seen, anything except the screen before him. If no one had ever told him about his circumstances, would he not consider that he was actually in the nose of the robot craft, and seeing personally the country over which it was flying?

We come, then, to the following inescapable conclusions about the mind.

(i) It has 'whereness'.

(ii) It has three dimensions of space and one of time.

(iii) Our concepts of physical space are derived from our experience of perceptual space and not vice versa.

(iv) There are *only three* possible topological relationships between perceptual space and physical space. The former may be coincident (*a*) with the *whole* of the latter; (*b*) with *part* of the latter; or (*c*) with *none* of the latter. The first, the TNS fallacy, is disallowed by neurophysiology.

(v) Perceptual space may be coincident with the space system of the brain (*b*), or with higher-dimensional space (*c*). If the latter, then the psychic factor of mind may be partly an extensive and organized material entity located in this space. The former hypothesis (*b*) is simpler, but perhaps it is too simple, as it does not account for the 'something definite' which happens when to a 'certain brain-state a certain "sciousness" corresponds', and it is quite unable to account for the facts of parapsychology. The latter hypothesis (*c*), although more complex, can account for both. The facts of parapsychology become not incredible but only to be expected.

#### 4. CERTAIN MISCONCEPTIONS IN PHILOSOPHY

Since the perceptual world is so very clearly extended in space and time, and since it is also clearly a product of the mind, how, we may ask, do many philosophers believe either that the mind is something unextended or that it is meaningless to discuss whether the mind is or is not extended?

We owe the first concept to Descartes, whom we find not only to be well in the grip of the TNS theory but to have made an additional mistake on top of this. Cartesian dualism presents the familiar picture of the TNS theory of 'unextended thinking spirits', located in the physical head of the observer and regarding the physical world 'through' their sense organs. There is a

further confusion between those qualities of objects which can actually be measured, the primary qualities, with the physical world; and the secondary qualities, those that cannot be measured, are confused with the observing Self. These qualities, however, are derived by the action of reason on the perceptual world, and have nothing to do with the way this world is presented to us.

The fundamental fallacy of Cartesian dualism is thus derived from the TNS theory. The 'mind' is identified with the Self and the perceptual world with the physical world. The unextended mind regards and wonders at the external physical world arrayed in space and time before it. This leads to the attempt to describe 'mind' by attributes more suitable to the Self, *immaterial* and *unextended*. It leads also to the direct transference to the physical world of the attributes *extended* and *material*, which properly belong to the perceptual world, a part of mind. (Not that they do not apply also to the physical world; they do, but only at second-hand.) The gulf between the material and the immaterial seems, however, unbridgeable.

If we try to repair this by postulating that the mind has a 'psychic' factor, we do not progress any further, if we seek to differentiate between this psychic factor and the brain on the grounds that the former is again immaterial and unextended, while the latter is material and extended. The difficulty of all immaterial psychic factor theories is that as soon as we postulate that an immaterial factor can move an electron in the brain, as we must to make any sense of our theory, it becomes material. The ghost in the machine becomes, as Ryle points out, just a ghostly machine itself. Besides, it is difficult to see what, in experience, this immaterial factor is supposed to represent. If the Self, how does this insinuate itself into the machine? How does the machine present it with the extended perceptual picture of the physical world, and how does it surround this factor with the body-image? Further, how does the immaterial factor, tucked away somewhere in the brain, move matter, as it must do in voluntary movement and psycho-kinesis? If on the other hand it is supposed to correspond with total mind, Self and perceptual world together, how does this immaterial and so unorganized entity manufacture the manifest organization of the perceptual world?

If we try to repair this, as Gilbert Ryle does, by so defining mind to mean a verbal cloak to cover such processes as thinking, perceiving, feeling, etc. we have merely avoided the problems of perceptual space. It is, of course, meaningless to discuss whether the 'mind' is extended or not if by mind we mean what Ryle means by the word. There is no argument, however, about the extension of the physical world, nor could there be about the

extension of the perceptual world. Any description of mind which does not account adequately for this latter extension, other than confusing it with the former, is not acceptable, once the fallacy of the transparent nervous system has been exposed.

In *The Concept of Mind* Ryle points out correctly that Descartes' separation of mind and brain on the attribute 'material' leads only to absurdity. He goes on to deal with the way that we form certain concepts from our experience by the exercise of reason, but says nothing about how this world before us is presented to us. He has not noticed Descartes' basic error, of which those errors he has noticed are but consequences. It is clear that his use of such words as 'seeing' and 'observing' rests upon the common-sense fallacy that when I say, 'I see a man', it is as simple as that. Here am I, and there in 'space' and time is 'the man'. He ignores the fact that what he calls the 'common world' is presented to each of us individually by our minds. Ryle is indeed misled in the way that Hutton describes (see p. 480). His arguments in *The Concept of Mind* are presented as though they were only common sense.<sup>1</sup> They are, in so far as they are based on the common-sense fallacy of the TNS. 'Common sense' has proved in the past a most misleading guide. It was the common-sense idea for a very long time that the earth was flat. The history of science should make it sufficiently clear that these questions cannot be settled by the unaided activity of the human reason, but only by undertaking the relevant experiments, in this case those of parapsychology. There is no point in repairing a set of logical absurdities with a set of practical impossibilities.

Ryle's use of the word 'mind' enables us to avoid the insistent question of perceptual space. If it is meaningless to discuss whether the mind is extended or not, then we can happily deny the most cardinal fact of observation, that the mind has 'whereness'. Alternatively, we can account for the extension of the perceptual world by confusing it with the physical world or we may do the only proper thing, and that is to work out the consequences of locating the perceptual world in the brain, as long as we remember that we are no longer allowed to slip back unnoticed into the TNS fallacy as soon as any difficulties present themselves.

To illustrate the strains involved by postulating a coincidence of perceptual space, and the objects contained in it, with the physical space system of the brain and its electron patterns, it is only necessary to point out certain consequences of this, singularly neglected by protagonists of this theory.

<sup>1</sup> For example, see *The Concept of Mind*, p. 249, where the main fault in Ryle's argument is clearly demonstrated by his inference that objects, *as observed*, (in this case his pen nib) are extended in physical space.



If I choose to call my body-image in consciousness, as observed by the Self 'six feet high', as common sense does, then, as my physical body is twelve times larger than the body-image in the brain, I must realize that it is, if measured by my familiar ideas of feet, some seventy-two feet high: that, if I regard my toe, as felt when wriggled, as possessing six feet of 'thereness' from my Self, then my physical toe possesses seventy-two feet of this 'thereness'; and that, if I extend my arm sideways, and in the space system in which I feel it to be 'that far away' I extend a line for about an equal distance by mental imagery, I shall then reach *the bone of my physical skull*. It will be clear that these considerations will do violence to our common-sense views of the world, still based obstinately on the TNS theory. To think of the familiar world of experience, as observed, as locked up inside one's skull makes the physical world into a monstrously large place, relative to our common-sense ideas of it. This is clearly put by E. A. Burt in *The Metaphysical Foundations of Modern Physical Science*.<sup>1</sup>

As Hutton continues in his article:

One of the most important features of these transient cortical patterns is that many of them show extraordinary similarities in their *temporo-spatial relationships* to the patterns from which and to which their movement flows, and, in fact, they appear to reproduce these *on a smaller scale*, the ratios remaining the same, though the quantities vary. (p. 162. My italics.)

Since we fall so easily into the TNS fallacy, it might have seemed suspicious that the body-image in consciousness is so like the physical body, and so unlike what seems to be the body-image in the brain. It should be noted in the above passage that the temporo-spatial relationships that we observe are those that the Self can determine in the perceptual world, and it is only an assumption that Hutton has made that these may be identified with the patterns in the cerebral cortex. He does not go on to draw the necessary conclusions from the words 'on a smaller scale'.

And having done all this, we find ourselves no nearer to accounting for the simple facts of parapsychology than we were to start with. Ryle rejects the erroneous two-world theory of Descartes, but the facts require neither this nor his untenable substitute, but an adequate two-world theory.

To supply this I suggest that the differentiation of psyche from brain should be made on the basis of *different geographical location*. Descartes' description of the psyche seems to have sprung from a hasty choice of qualities, opposite to those most clearly exhibited

<sup>1</sup> London, Routledge & Kegan Paul, 2nd ed. 1932, pp. 314-16.

by the brain, i.e. *immaterial, unextended, and unorganized*. But if we locate the psyche in higher-dimensional space, all the difficulties of traditional dualism, skilfully exposed by Ryle, evaporate. Both these difficulties, and Ryle's attempts at repair, may be seen to be based on a confusion in our ideas of space, and over our use of the words 'mind' and 'Self'. We also avoid the difficulties of monism, which Ryle so skilfully passes over that we are almost persuaded that they do not exist.

Psyches may be *real* machines. They have only appeared spectral owing to their confusing geography. The experimenter cannot enter mind's private world, not because it does not exist, but because of the barrier presented by a dimensional interface. He may nevertheless confirm its existence by the indirect but relevant experiments of parapsychology. We thus avoid the insoluble conflict between the facts of parapsychology and Ryle's unanswerable criticisms of Cartesian dualism. We also avoid Ryle's impossible position when he seeks to deny any sort of private world to mind. This theory also relieves much of the burden on neurophysiologists, since the brain would seem to be a much simpler organ than it has hitherto been supposed. Many functions attributed to it may now be attributed to the geographically independent psyche and Self.

##### 5. THE FUNCTIONS OF THE BRAIN

We may suppose that the brain has the following functions :

(i) To *collect, integrate, and present* information about the outside world, and the state of the physical body, for  $\Psi\gamma$  to pick up and transfer to the psychical mechanisms, which in turn present this to the Self in its familiar field of awareness.

(ii) To *translate* the Self's instructions (conveyed by  $\Psi\kappa$  which interferes with the statistical behaviour of electrons in the cortex, just as it can interfere with the statistical behaviour of dice) into the electronic messages for the muscles.

(iii) To *supply* a battery of servo-mechanisms designed to aid voluntary movement thus conveyed.

(iv) To *learn* unconscious skills and to *store* them as one form of memory.

(v) To *supply* various protective reflex mechanisms, for instance the eye-blink.

(vi) To *act* as a thought screen and perhaps a screen for visual imagery, and to *supply* a series of thinking mechanisms. It may also store other types of memory. Functions (iii) to (vi) correspond to Aldous Huxley's 'physiological intelligence'.

It is of the first importance for all the enthusiastic exponents of the fashionable analogy between brains and computing machines to realize that the brain only functions in part along these lines. It has *two* input systems and *two* output systems. It is but a station on the way to the soul.

We do not need any longer to ask how the machine can appreciate beauty, write a symphony, and undergo a mystical experience. The soul does these things; the brain is merely part of the control panel for the observing and executive instrument, which is the body. The universe may be both larger and more wonderful than we have supposed.

## 6. THE GENESIS OF PERCEPTUAL SPACE

Let us see how Sherrington,<sup>1</sup> having accepted that the mind is in extended space, goes on to deal with the problem. 'The vexed question,' he says, 'of the mode of origin or genesis of sensual space *we have not to enter on*. We accept the brute fact that our minds' perception *through the senses* immerses us in space.' (pp. 325-6. My italics.) Thus we see that this fundamental question on which much else depends, the genesis of sensual (or perceptual)<sup>2</sup> space, is mentioned only to be shelved, and, of course, the phrase 'through the senses' enables us to slip back almost unnoticed into the TNS fallacy of common sense. Indeed the manoeuvre is unavoidable, since it is no use trying to persuade ourselves or other people that the world, as we individually live in it, is not extended in space and time.

We now know a little more about the mode of origin of perceptual space. The following observations prove our working hypothesis (see p. 481) that the visual part at least of the perceptual world in consciousness is built up by a signalling mechanism. The problem of visual space is complicated by the fact that it is of dual origin. Observations made using the stroboscope have shown that the visual picture in consciousness is built up along lines very similar to three-dimensional television in colour. The stroboscope is an electronic lamp which flashes at regular intervals. The duration of the flash is extremely short, some one millionth of a second. Now if we look at such a lamp flashing at a frequency of between 4 to 20 times a second, we observe that a large number of complex zig-zag lines and mosaics appear in the perceptual field before us vibrating with incessant movement, as it were upon a

<sup>1</sup> *Man on His Nature*.

<sup>2</sup> Perceptual space includes sensual space and the space of mental imagery.

screen situated, it seems, a few inches in front of our observing selves. These lines are of various colours and execute their movements against a pale, almost colourless, background. As Grey Walter<sup>1</sup> points out, this is very similar to the effect produced on the screen of a television set if the studio is illuminated by a stroboscopic lamp, due to the basic nature of the television process.

Now in the following discussion I shall use the word 'screen' to indicate the pale background on which these patterns are executed. I do not feel that it brings to mind any illegitimate associations that would not be aroused were you actually to observe the phenomenon, as anyone has witnessed this will agree. So we come to realize that our familiar visual picture of the world is continually being built up before us on this 'screen' by thousands of little scanning beams travelling to and fro across it very rapidly just as the three-dimensional television picture is built up.

If you examine this black cross on a white field



you will realize that it is continually being built up in this manner. A little beam starts here



and throws a patch of white on to the 'screen' and scans rapidly across until it gets here



when it changes momentarily to black and produces one segment of the cross and then turning white again speeds off producing the whiteness of the paper. While it is producing its black speck another scanning beam is producing another black speck directly under it, thus



and so on. Thousands of these little coloured beams, whose 'grain' we never notice since their resolving power seems to be greater than the limiting resolving power of the retina, continually build up our everyday picture of the world in this manner. We can only demonstrate the grain by cutting down the time it has to

<sup>1</sup> 'Features in the Electro-physiology of Mental Mechanisms' in D. Richter (ed.), *Perspectives in Neuropsychiatry* (London, H. K. Lewis, 1950).

work in to almost nothing. Thus our three-dimensional stereoscopically produced visual space is mostly as illusory as the apparent space in which the television actors on the two-dimensional screen move about: we do not look for these *inside* the television set. So in normal stereoscopic vision two complex and ever-changing patterns are thrown on to this 'screen', and the illusion is produced, which depends *entirely on the properties inherent in the screen<sup>1</sup> itself and on the capabilities of the observing self*, since all other mechanisms in between the screen and the outside world merely determine the detailed nature of the patterns to be projected on to the screen, and execute this projection. (The apparent space produced by the stereoscope has a similar origin, as the stereoscope merely converts out of two flat pictures conditions from which this process can start.) The name 'optical illusion' is a misnomer again derived from the TNS theory. It is the Self that is deluded, as the visual mechanisms present the information correctly. Nevertheless, this illusory space is built up on a basis of real space. The screen itself is two-dimensional; we can describe the position of any point on it in terms of two co-ordinates, but our observing Selves are clearly set back from it: it is 'there' while Self, as always, is 'here'. We can sketch a pyramid in our mental imagery using any four points on the patterns not in a straight line as base and the Self as apex. To describe the whole process three co-ordinates are needed. This space has evidently a dissimilar origin from the other.

The screen forms the fundamental background of visual perception. In the current theory held by some workers in neurophysiology, this presentation is done by a series of electrical switch-scanning mechanisms present in the brain, but under this theory no clear exposition is given as to what is looking at these patterns, or how these space-time relationships of the patterns, amongst themselves and to the observer, may be fitted into the space-time system of the brain. However, all that we can be certain of, from direct observation, is that an exceedingly complex mechanism must be directly responsible for this picture. I have suggested that there is no necessity to regard this as being in the brain, but that this presentation may be done by the independent psychical mechanism whose *behaviour* only is determined by events in the brain. In this way we can account for the observed spatial relationships in a less strained way (see p. 495), and leave room for the observer. Furthermore, the facts of parapsychology can be explained.

<sup>1</sup> I postulate that this screen is a material screen composed of mind-stuff.

## 7. THE NATURE OF THE PSYCHE

When we come to consider the possible nature of the psyche, we find only two theories in common use.

(i) That the psyche is merely a noun descriptive of certain complex and determining electronic processes in the brain. It cannot then be capable of functioning or existing in any way as an independent agent, since it is held to be merely an expression of the internal co-ordination of a machine. We can say all that we wish to say without using the word at all.

(ii) That the psyche is a potentially independent agent and is in control of the brain. To *distinguish* it from the brain it is supposed to be *unextended* and *immaterial*. No account has ever been given of how this control is exercised, nor how this concept may be fitted into the world as I know it or into the manner of my knowing it. As this theory has been in the field for as long as history has been recorded, there does not seem to be much hope that this integration will ever be made. There does not seem to be any credible bridge between the material and the immaterial as presented by this theory. It is impossible to find a functional relationship between them, between the 'energy-concept' and 'naked mind' of Sherrington, since, as products of reason, there is no relationship between them except what reason itself has given them in sundering them apart: a relationship of antithesis outside which reason cannot get.

The alternative theory I am presenting here suggests that there has indeed been a grave category-mistake in this attempt to distinguish between the physical and psychological worlds on account of the attributes 'material' and 'immaterial'. This division precludes the erection of any bridge between the two worlds because by definition and on logical grounds the immaterial can play no part in the material world. If, however, the division is made on account of geographical location, we may suppose that the psyche is both material and extended (in higher-dimensional space). We can then build our bridge and account for the nature of the perceptual world in a manner which is credible, which explains the erstwhile discordant facts of parapsychology, and which suggests a method by which the 'mind' may convert electronic patterns in the brain into the familiar phenomena of mental experience. Thus we may suppose that we cannot see someone else's psyche, not because it is ghostly and immaterially invisible, but because it is on the other side of the most impenetrable of all barriers, a dimensional interface.

The nature of the Self remains inexplicable. We may, if we like, reserve the attributes 'apparently unextended' and 'ap-

parently immaterial' for the Self, though of course it may consist of a complex and organized system too.

#### CONCLUSION

I have expressed these ideas in the form of four postulates which I have called the Theory of Extension.

(i) To give a consistent and unitary account of phenomena it is necessary to use a framework of 'n' space dimensions ( $n > 3$ ).

(ii) The three dimensions comprising the manifold of mental position (perceptual space) are at right-angles to the four dimensions of physical space-time. The two manifold systems may share one common time dimension. The universe is thus a seven-dimensional continuum, which we divide artificially into two subsections by reason of the nature of our observing mechanisms, and from which we separate time from space by our motion through it.

(iii) The psyche may consist of an organized material entity located in higher-dimensional space. This may abstract information from the brain, and transform it into the familiar phenomena of experience which it presents to the Self. On the motor aspect the Self, through another part of this mechanism and the brain, may order its thoughts and actions.

(iv) The axes of the psychical world (a hitherto unrecognized part of the total physical universe) may be called  $OX'$ ,  $OY'$ ,  $OZ'$ , and  $OT$ . The axes of the physical world are  $OX$ ,  $OY$ ,  $OZ$ , and  $OT$ . (The misconception, if such, would not have been apparent before since the *number* of axes, the mathematically important factor, for the 'physical' world is the same.)

This theory, from the point of view of experimental parapsychology, suggests that ESP and PK are incidental side-effects of the normal psyche-brain relationship, as Thouless and Wiesner put forward in their Shin Theory. It would further explain:

(a) The apparent independence of ESP of spatial, and to some extent temporal, separation of percipient and target or agent, since we may imagine that  $\Psi\gamma$ , coming from higher-dimensional space, may traverse our entire universe as quickly as we can push a pencil through a piece of paper. Its penumbra, extending so widely through space, could also be imagined to extend a little way, or even long distances, into the future.

(b) The deleterious effect of trying too hard on scoring rates in ESP tests, since this might be expected to increase the focus of  $\Psi\gamma$  on to the brain and so lessen the penumbra effect subserving ESP.

(c) It also provides the organization seemingly required by the activity of the 'censor' as described by Soal.<sup>1</sup>

If mind is regarded as something unextended and immaterial, it is very difficult to see how it could do the work necessary to turn a die, which was going to fall with one face up, so that another does, as PK shows it must do. Likewise, in clairvoyance experiments it is difficult to see how it could extract information from the physical cards. One of the most striking things about the phenomena of parapsychology has been the lack of any apparent *organization* to undertake these formidable tasks. Such apparent 'action at a distance' as parapsychology discloses is anathema to science. The gap must be closed. This theory suggests where this organization may be found, once we realize that things may be invisible, not because they are immaterial, but because they may be on the other side of a dimensional interface. The theory also supplies the necessary forces to close the gap, and instruments of sufficient sensitivity should be capable of detecting them. It can also account for certain discrepancies in neurophysiology and has links with the experiments of Gestalt psychology. It suggests a series of experimental investigations, not only for parapsychology but also for physics.

## ESP EXPERIMENTS WITH AN INFANT AS SUBJECT

REPORTED BY G. W. FISK

THE following are translated extracts from a letter from Docteur Paul Vasse of the Société Metaphysique Picarde, 136 Boulevard Châteaudun, Amiens, France, dated 25 February 1951 addressed to Dr D. J. West.

I should like to make an ESP experiment with you across the Channel. Mme Vasse has experimented with our very young daughter (Poyette—14 months) with very curious and successful results. This is what has been done. The DT technique has been used. The pack of twenty-five ESP cards is shuffled without Mme Vasse seeing the cards. The baby then reads the cards, but as she cannot speak she makes her 'divination' by gesture. The 5 symbols are put into 5 envelopes (without their being seen). The envelopes are marked with the 5 letters, A, E, I, O, U. The little child then picks up one envelope and Mme Vasse records the letter it bears. When the run is completed the

<sup>1</sup> 'Some Aspects of Extrasensory Perception,' *Proc. Soc. Psychic. Res.*, vol. 49 (1951) pp. 142-7.



symbol corresponding to the letter is noted and the score calculated. After each run the five symbols are withdrawn from the envelopes, shuffled, and again placed in the envelopes in any chance order—always without Mme Vasse seeing them. This amuses the child immensely, but she is soon bored and a complete run cannot be made at one sitting. It is continued later when convenient.

Dr Vasse continues :

Evidently here is a beautiful example of 'polypsychisme' between mother and infant. For although the mother has had successful results in PK experiments with plants and dice her successes with cards have been almost nil.

Dr Vasse gave the following totals of hits for each of ten runs made :

7, 11, 8, 10, 15, 8, 12, 11, 13, 15  
 Total no. of hits in 250 trials = 110  
 Chance expectation = 50      Deviation = +60

The C.R. is thus approximately 9.5 !

With so fantastic a result it seemed well worth while to accept Dr Vasse's offer and to arrange a cross-channel test. Accordingly dates were fixed and I exposed on my desk in Ditton Hill a pack of cards each week. As many 'K-objects' or *points d'appui* were arranged as possible, such as exchange of photographs, etc. Unfortunately there were only chance results, as the following summary of scores shows. As an alternative test I also sent sealed packs of cards to Mme Vasse to see if any better scores would result, but again there was no success. It will be noted, however, that while Poyette only scored negatively in the cross-channel tests she scored positively when the packs were actually in the room with her ; but the deviations from chance are too small in both cases to be more than very slightly suggestive.

## SUMMARY OF SCORES

### SERIES A

A pack of ESP cards exposed weekly in G.W.F.'s study, Ditton Hill, Surrey.  
 Packs of 25 cards in random order—copies of random order posted weekly to Dr West.

March 1951	Run 1	Hits 5	
	2	5	
	3	4	
	4	6	20
		—	
April-May	5	0	
	6	4	
	7	3	
	8	4	11
		—	

31

Deviation for 200 trials = -9

## SERIES B

Sealed packs of 25 cards posted to Amiens. Gussed DT and returned unopened to G.W.F.

April 1951	Run 1	Hits 9	
	2	7	
	3	4	
	4	6	26
		—	
May	5	8	
	6	5	
	7	5	
	8	4	22
		—	
		48	

Deviation for 200 trials = +8

## REVIEWS

PROCEEDINGS OF THE SCIENTIFIC AND TECHNICAL CONGRESS OF RADIONICS AND RADIESTHESIA. Published by the Committee of the Congress, 8 Ashburn Gardens, London, S.W. 7 [1951]. 202 pp.

These Proceedings contain the papers presented at the first British Congress on Radionics and Radiesthesia, held in 1950, and organized by a Committee representing the British Society of Dowzers, the Radionic Association, and the Psychosomatic Research Association. Some discussion of the papers is also printed.

A. T. Westlake in a paper on *Vis Medicatrix Naturae* identifies this concept with the odic force of Reichenbach and the orgone energy of Reich, and with various other supposed forces and fluids of debatable reality.

Several papers deal from an experimental viewpoint with postulated radiations and force fields of different kinds. E. A. Maury and M. Maury mention some experiments on the human electromagnetic field. They dispense with a detailed description of their apparatus in order not to complicate the subject. M. Ash describes the detection of radiation from the body by feeling it with his hand. He finds a stimulus to occur at critical distances from the body, and also describes some instrumental methods of detection. Some general remarks on human radiations are contributed by W. E. Arnould-Taylor. Conducted radionic emission from drugs and bloods is considered by L. E. Eeman.

The question of harmful rays from the earth is discussed in a paper by H. Larvaron. J. C. Maby, however, adds that this paper proves nothing. L. P. Corté examines the effect of modula-

tion on postulated radiations from body cells and other sources. G. de la Warr considers the manifestation of what he calls 'Fundamental Energy' and proceeds to enunciate some new laws of physics relating to this energy. These laws, however, are not clearly formulated and it would seem better to rank them as speculations.

A theoretical paper by A. J. K. Glazewski attempts to relate some of the concepts of radiesthesia to modern theoretical physics. His ideas are presented in a somewhat obscure way and will be largely incomprehensible to the general reader. They appear too unsound to attract the specialist reader. Some further remarks on theoretical physics are contributed by R. C. Thornton.

A good number of claims of interest in psychical research are advanced by J. C. Maby. He asserts that all matter radiates weakly with frequencies specific to different materials, and notably determined by atomic and molecular weight. Moving water is supposed to produce an additional and stronger flow field. The human nerve-muscle system reacts to both these stimuli. Maby states that these facts can be witnessed by any patient and unprejudiced observer. However, a great many physical factors are said to interfere with observations on these radiations. Conditions favourable for dowsing experiments are stated to favour telepathy and clairvoyance experiments to such an extent that an almost entirely correct score may be obtained with good subjects over short time periods. Maby also claims that an imaginative person can project a psycho-radiant energy and influence suitable physical instruments. Indeed, these radiations from bystanders interfere with dowers.

The Proceedings are terminated by a very clear summary of the present position by J. C. Maby. Radiesthesia and Radionics now occupy an intermediate position between science and psychical research. It is apparent from these Proceedings that at present their experimental and theoretical basis is slender. However, as Maby observes, orthodox science can be influenced by an internal ferment arising from the minds of scientists who have been presented with clear and repeatable evidence.

A. J. B. ROBERTSON

**SOLID PROOFS OF SURVIVAL.** By Einer Nielsen. London, Spiritualist Press, 1950. 193 pp. 5 plates. 10s. 6d.

It is always interesting for the psychical researcher to read books by mediums since it affords a glimpse into the minds of the writers. Such mediums as G. A. Redman, D. D. Home, and Mrs Leonard have all contributed something in this direction, and in the present

volume the Danish medium, Einer Nielsen, has added an item to the list.

Mr Nielsen is predominantly a medium for materialization, his productions sometimes resembling those of Eva C. and at others those of Linda Gazerra or Mrs Henderson. In his book he tells the story of his life and work, and it is full of the handsome tributes paid to him by his admirers. Moreover, he prints his own version of the disastrous series of sittings in Oslo in 1922, the awful days of which he thinks he was only able to endure through the kindness of Miss Ramsden.

Although the Oslo sittings are discussed by Mr Nielsen with his own explanations of the suspicious circumstances reported, he does not seem to have dealt even in outline with the Carstensen story and how the medium rebutted it.<sup>1</sup> This is unfortunate, since it would have been interesting to see Mr Nielsen's attitude and an account of the legal case connected with it. Apart from these and a few other omissions, the book appears to provide a concise story of the growth and development of the author's mediumship. Unfortunately, the evidence he adduces is hardly sufficient to persuade even the moderately critical reader that he possesses any supernormal powers, an opinion to which I myself inclined after seeing something of his work in 1924. The conditions under which the sittings are conducted appears to have been lax, the control inadequate, and the lighting poor. Thus, although this book purports to be solid proofs of survival, it can scarcely be said to be proof of anything, unless some unkind reader insists that it is yet another proof of human credulity in the seance room.

E. J. DINGWALL

JOURNAL OF PARAPSYCHOLOGY, Vol. 15, No. 1, March 1951.  
Durham, N.C., Duke University Press. \$1.50.

The main article in the present Journal is Professor Rhine's Myers Memorial Lecture on *Telepathy and Human Personality*. This will be known already to members of the Society for Psychological Research.

The remainder of the contributions are three minor articles on a new method of PK experimenting in which the aim is to get the dice or other objects to fall in a given place and not with a given face uppermost. W. E. Cox was the originator of this method of experimenting, and he reports significant success. His method was, however, to throw the dice by hand from a cup on to a

<sup>1</sup> See J. Carstensen, *Spiritismens Princip og Fejlgreb* (Copenhagen, 1931).

surface marked out in squares, and I do not think it is possible to agree with the experimenter's opinion that results could not have been due to skilled throwing. The other two sets of experiments (those by H. Forwald reported by Rhine and by G. Cormack reported by Pratt) are not open to this criticism since in both cases the dice were released mechanically. In both cases the results are highly significant. It is clear, therefore, that this is a practicable alternative to the usual method of PK experimenting. Professor Rhine considers that it has the advantage of making it possible to decide between alternative theories of how PK works.

R. H. THOULESS

### PSYCHICAL RESEARCH AND PHILOSOPHY

THE following passage is taken from 'The Philosophy of 1951', an article in a special section, 'The Mind of 1951', of *The Times Literary Supplement* of 24 August. It is here reproduced by kind permission of the Editor.

'While British philosophers have shown themselves comparatively uninterested in the discussion of Existentialism, which occupy the thought of so many of their Continental contemporaries, an important development of recent years has been an increasing concern with the problems raised by psychical research. Under one or other of its many modern names, *psi* phenomena, paranormal phenomena, extra-sensory perception, what used to be called psychical research has increasingly occupied the attention of philosophers, and Professors Broad and Price in particular have urged that, if these phenomena are to be taken seriously—and in view of the weight of contemporary evidence, not so to take them argues a degree of credulity which an increasing number of scientists and philosophers seem unable to achieve—their implications demand a re-orientation of much of our thinking. Mind and its powers, the nature of time and the status of the future, the biased and, it may be, limiting characteristics of the human intellect, the relation of the unconscious to consciousness—these are instances of the many and various topics on which totally fresh thinking would seem to be required in the light of the disturbing implications of some of the evidence.'



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A NOTE ON PRECOGNITION

BY C. T. K. CHARI

In the *Proceedings* of the S.P.R. for July 1950<sup>1</sup> and in the issue of *Mind* for the same month,<sup>2</sup> Mr C. W. K. Mundle raised the question whether ostensible precognition can be accounted for by some conceivable combination of PK and non-precognitive ESP. Psychical researchers must be aware that Dr Tanagra once put forward the suggestion that a person who makes a 'supernormal prediction' actually brings about its fulfilment by exercising in some cases his telekinetic powers.<sup>3</sup> 'L'absence de l'agent', he declared, 'ne paraît pas toujours influencer les manifestations . . . J'ai été ainsi persuadé que le dynamisme producteur du thorybisme et des phénomènes télékinétiques peut aussi à distance, comme d'ailleurs des personnes absentes'.<sup>4</sup> Tanagra's hypothesis, like the PK hypothesis, requires some very *ad hoc* extensions before it can cover all the recorded cases of spontaneous precognition. While I agree with Mr Mundle that every attempt should be made to ascertain whether we have reached technically rigorous limits in the experimental isolation of precognition, I am afraid that most definitions of precognition are *petitiones principii*. There are two parts of Mr Mundle's definition likely to invite misconceptions.

Mr Mundle does not pretend to offer us any theory of the *modus operandi* of precognition; yet his phraseology suggests that 'later events' can exert a '*direct causal influence*' (italics mine) on 'earlier human events'. Now it seems pretty certain to me that any attempt to 'understand' or 'explain' precognition will involve the most extreme metaphysical reconstruction of our notions of 'causality' and the characteristics of 'before-after' and 'past-present-future'

<sup>1</sup> Vol. XLIX, Part 178.

<sup>2</sup> Vol. LIX, No. 235.

<sup>3</sup> See H. Driesch, *Psychical Research* (Bell, 1933), p. 85.

<sup>4</sup> *Transactions of the Fourth International Congress for Psychical Research* (S.P.R., 1930), pp. 242-3. Richet in his *L'Avenir et la Pré-*

which we associate with time. It is important that no relevant metaphysical possibility should be excluded by the psychical researcher. Whether or no Mr Mundle intends it, his terminology may persuade us that the most hopeful approach to the problem of precognition lies in sticking to a time the instants of which are all ordered by 'linear betweenness', and that the only departure from common-sense notions we have to make lies in recognizing an unfamiliar 'later-to-earlier' direction in which 'causality' can operate. I venture to suggest that in working out a theory of precognition, the conception of a time the instants of which are all linearly ordered may have to be abandoned.<sup>1</sup> The door should be kept open to all possibilities, no matter how weird or fantastic.

About the only prudent course for the experimental psychical researcher is to adopt definitions of the kind proposed recently by Mr George E. Hughes in a critical notice<sup>2</sup> of Rosalind Heywood's *Telepathy and Allied Phenomena*. The statistical type of experiment in psychical research is designed to exclude the null hypothesis that no significant differences can be detected between our two samples, i.e. that the samples could have been randomly drawn from the same population and differ only by accidents of sampling. Precognition in the experimental situation, therefore, should be defined in terms of the 'greater-than-chance coincidences' between the pronouncements or actions of a percipient and certain facts 'normally inaccessible' to him at the moment the pronouncements are made or the actions executed. We can adopt operational definitions of 'normal access' to distinguish 'autosopic' precognition (i.e. foreknowledge of events in one's

*monition* remarks that the possible telekinetic stopping of a roulette ball at a number which has been previously announced should be taken into consideration in discussing certain cases of precognition. He refers to an interesting experiment described by Dr Ochorowicz in the *Annales des Sciences Psychiques*, 1909-10. In the cases reported by Professor G. Hulin of Ghent (*Proc. S.P.R.*, Vol. XI, Part 29, pp. 545-7), the percipients drew numbers from an urn. It may be argued that, assuming we have something more than 'chance coincidences', non-precognitive ESP without PK would suffice as an explanation of the choice of a previously announced number. But how could the percipient be sure that the number would not be drawn by somebody else after he had made the ostensible prediction? The interval between the prediction and its fulfilment was nearly two months—the report says 'près de deux mois à l'avance'—in the case of Charles-Louis Casset. In principle at least, a PK influence on others exercised at a distance would seem to be necessary. It may be said that the cases are too few and too inadequately reported to afford a basis for theoretical discussions. But the possible extensions of the PK hypothesis must be visualized by psychical researchers.

<sup>1</sup> See my note in *Mind*, Vol. LVIII, No. 230 (April 1949), pp. 218-21.

<sup>2</sup> *Mind*, Vol. LVIII, No. 229 (January 1949), pp. 109-10.



own life, including here all those things about which one receives verbal reports of any sort) from 'non-autoscopic' precognition, and telepathic from clairvoyant precognition. The considerations will apply *mutatis mutandis* to simultaneous and post-cognitive telepathy and clairvoyance.

Mr Mundle complains that definitions in terms of 'extra-chance hits' alone will not enable us to distinguish precognition from PK in experimental situations. Surely his own admirable treatment of the data suggests a way out of the difficulty. There may be qualitative and quantitative peculiarities associated with precognitive 'hits' and 'misses' that are absent in the class of cases in which PK presumably operates. A significant feature of Carington's experiments, as Mr Mundle remarks, was the temporal clustering of the 'extra-chance hits' more or less symmetrically around the occasions on which certain drawings were displayed. While Professor Bartlett has raised some technical questions about the evaluation of the 'dispersed hits' in Dr Soal's experiments, there is a suggestion that, in experimental situations designed to test ESP, 'fore-hits' and 'back-hits' occur together in ways that do not seem to be characteristic of the 'PK effect'.

Mr Mundle's definition of precognition leaves out the element of 'knowing' or 'believing'. Such a feature must, of course, be excluded when we are trying to define precognition as behaviouristically as possible in experimental situations. But is it altogether unrepresentative of cases of spontaneous precognition, as Mr Mundle seems to imply? The problem was discussed by Professors Broad and Price at the Joint Session of the Mind Association and Aristotelian Society in 1937.<sup>1</sup> Professor Broad declared that, in most spontaneous cases, precognition was known to be such only in the light of the later verification. Although it may be bumptious to differ from an authority, I think that Professor Broad's remark would not apply to many cases published in the *Proceedings* of the S.P.R. A precognition may be a vague premonition or presentiment or a quite specific prediction.<sup>2</sup> The cases of ostensible premonition in which the percipient took steps to ward off an impending danger most definitely imply the cognitive attitudes of 'expectation' and 'belief'.<sup>3</sup> We have also those rare instances in which a year, a month, and a day were indelibly impressed on a dreamer who woke up with the inexplicable con-

<sup>1</sup> Aristotelian Society, Supplementary Volume XVI, pp. 216-17, 232-3.

<sup>2</sup>Mrs Sidgwick, *Proc. S.P.R.*, Vol. V, Part 13 (December 1888), pp. 291-2, 320-1, 326-9, 330-2; F. W. H. Myers, *Proc. S.P.R.*, Vol. XI, Part 29 (December 1895), the case of Lady Q., pp. 577-80.

<sup>3</sup>Dame Edith Lyttelton, *Our Superconscious Mind* (Philip Allan, 1931), pp. 113-14, 167-70; *Some Cases of Prediction* (Bell, 1937), pp. 66-8.

viction that a great calamity would engulf him on that date.<sup>1</sup> In the extensive, if not always strictly evidential, literature on the subject, we find it stated that 'mind-pictures' have sometimes empirical features corresponding to the temporal marks 'past' and 'future'. V. N. Turvey noticed that the 'mind-pictures' which came to him were on a continuously moving 'film' or 'ribbon' of pale heliotrope colour; some of the 'pictures', which appeared to be engraved on the 'film' itself, were found to refer to the 'past'; others, which were like 'pale blue photographs' stuck on the 'film', were found to refer to the 'future'.<sup>2</sup> One of Dr Osty's 'metagnomes' located the 'pictures' in a hallucinatory space the median line of which divided the 'past' from the 'future'.<sup>3</sup> Even when the 'pre-presentative image' is not dated, it *may* have a subtle flavour or feeling-tone which leads the subject to say, 'Some day I *know* that I shall see that scene in real life'.<sup>4</sup> An inferential cognition supplementing and interpreting a *de facto* precognition may not be the whole story.

Dr D. J. West not long ago<sup>5</sup> drew our attention to the numerous pitfalls and possibilities of false scent in the investigation of spontaneous cases. But the *a priori* dismissal of them is dangerously misleading in a philosophical speculation on precognition. Mr Tyrrell's reminder<sup>6</sup> that, for theory-building in psychical research, *all* the available material must be utilized is salutary. Dr Soal himself has recently cautioned us that 'card-guessing is very like studying the habits of the badger by taking him from his native woodland and shutting him up in some wretched cage. In such cramped circumstances the animal would be unlikely to exhibit many of his true characteristics'.<sup>7</sup>

It is true that, in a large proportion of cases of spontaneous

<sup>1</sup> e.g., the case of Edisbury, Mrs Sidgwick, *loc. cit.*, p. 318. The case lacks the usual corroborative testimony and there are uncertainties on some points. But does it seem likely that the whole incident was fabricated by a pseudo-memory?

<sup>2</sup> *The Beginnings of Seership* (Stead's Publishing House, 1909), Ch. VI, pp. 167-8. Turvey was known to Professor J. H. Hyslop. A perusal of the book will convince the psychical researcher that the evidential standards maintained in it are much higher than those he associates with 'spiritualistic' literature generally.

<sup>3</sup> *Supernormal Faculties in Man* (Eng. tr., Methuen, 1923), Part III, Ch. 3.

<sup>4</sup> For a case of this type, see Dame Edith Lyttelton, *Some Cases of Prediction*, pp. 120-5.

<sup>5</sup> *Proc. S.P.R.*, Vol. XLVIII, Part 175 (July 1948), pp. 264-300.

<sup>6</sup> *Proc. S.P.R.*, Vol. XLVIII, Part 173 (May 1947), pp. 65-120.

<sup>7</sup> *Hibbert Journal*, Vol. XLVIII, No. 3 (April 1950), p. 234. Cf. Mr George Hughes in *Mind*, *loc. cit.*, p. 110, and Dr J. B. Rhine in *Telepathy and Human Personality* (Myers Memorial Lecture, 1950), p. 33 *et seq.*

precognition relating to trivial events, the 'foreknowledge' was not identified as such at the time when it emerged spontaneously. But not enough attention has been paid, I think, to a curious circumstance linked to some cases of 'autosopic' precognition in this group. At the moment the precognition was fulfilled or was about to be fulfilled, there occurred a *sensation du déjà vu*; the whole scene seemed oddly familiar to the percipient.<sup>1</sup> I quite realize that, in view of all that orthodox psychologists have said about 'false recognition' or 'paramnesia', the *déjà vu* cannot serve as *evidence* for supernormal non-inferential precognition. But where an 'extra-chance' element is regarded as most likely on other grounds, the *déjà vu* is not without its possible theoretical significance. We have, then, two seemingly discrepant facts. On the one hand, an image arising spontaneously in the mind is regarded as *pre-cognitive* rather than as *retro-cognitive*. On the other hand, the situation in which the ostensible precognition finds its fulfilment seems oddly familiar to the subject, as if he has already been in it.

A hypothesis of great value in trying to assimilate precognition was hinted at by F. W. H. Myers<sup>2</sup> and elaborated by H. F. Saltmarsh.<sup>3</sup> Its importance for psychical researchers has been stressed by Soal and Goldney.<sup>4</sup> Saltmarsh groped for a solution of the problem in terms of a 'saddleback' theory of the 'specious present'. Let us suppose that the 'maximal degree of psychological presentedness' in the 'specious present' tails off to zero in two intrinsically opposite directions corresponding to our empirical 'past' and empirical 'future'. To account for the ostensive cognition of a distant 'future event', we have to extend the 'specious present' beyond its hitherto acknowledged narrow limits. Saltmarsh argued that there may be a 'subliminal' awareness of a 'specious present' much longer than the one with which we are familiar in ordinary experience. On this supposition, we can account for the cognitive attitudes of 'expectation' and 'belief' arising in connexion with the emergence of spontaneous precognition and also the *déjà vu* which occurs when the precognition is fulfilled. *Ex hypothesi*, an experience that has been lived through in the 'subliminal' mind comes to the surface, but it lies beyond the 'supraliminal present' in the direction corresponding to the empirical 'future'. The fact that ostensible precognition

<sup>1</sup> For illustrations, see Mrs Sidgwick, *loc. cit.*, pp. 314-17, 328, 346-7, and Myers, *loc. cit.*, pp. 491-2, 495, 504-5, 533.

<sup>2</sup> *loc. cit.*, pp. 342-4.

<sup>3</sup> *Proc. S.P.R.*, Vol. XLII, Part 134 (February 1934), pp. 74-93; *Foreknowledge* (Bell, 1938), p. 94 *et seq.*

<sup>4</sup> *Proc. S.P.R.*, Vol. XLVII, Part 167 (December 1943), pp. 28-9.

shows omissions, distortions, and dislocations can be understood if we suppose that 'subliminal' knowledge is 'mediated' to ordinary consciousness by various psychological devices such as 'symbolization', 'automatic writing and speech', etc. I dissent from Professor's Broad's view that there is an intrinsic implausibility about any prehensive analysis of ostensible precognition. Saltmarsh's statistical treatment of cases of spontaneous precognition<sup>1</sup> and recent psycho-analytical studies<sup>2</sup> suggest that precognition, even if it is ostensive, has to negotiate a 'barrier' of some kind before manifesting itself at the empirical level of observation. The significant precognitive 'misses', for which statistical evidence is accumulating, may have their origin in an unusual amount of psychological 'resistance' encountered at the 'barrier' which necessitates a 'subliminal' substitution of items paranormally known to be 'wrong' for items paranormally known to be 'right'. I may refer in this connexion to some valuable observations made by Dr Soal in his recent experiments with Mrs Stewart.<sup>3</sup> The tendency for telepathic precognition and retrocognition to occur, sometimes together, in the experimental situation, receives a suggestive interpretation on Saltmarsh's hypothesis if we suppose that telepathic interaction takes place between 'subliminal selves' with 'specious presents' extending beyond their ordinarily recognized limits in the two directions corresponding to empirical 'past' and 'future'. The pattern of the paranormal response emerging at the empirical level of observation (e.g. the inverted U-shaped curve in Carington's experiments) may be determined not only by 'subliminal specious presents', but by the time-units (seconds, minutes, or hours) employed by the experimenter and the psychological rhythms of 'supraliminal' minds. The entire terrestrial life of a man, for aught we know, may constitute the 'specious present' of his 'subliminal self'. Of very great significance for this speculation are those cases of spontaneous 'autosopic' precognition in which the percipient enacts a scene in the remote future, experiencing hallucinations that are almost like percepts. In a case published by Dame Edith Lyttelton<sup>4</sup>—I have referred to it in an earlier footnote—the percipient had a 'vision' of a fireplace in front of which stood two men, both dark. She noticed that one of them had beautiful teeth. They were talking. A fair man, whose face she could not see, walked across. She was so anxious to see him

<sup>1</sup> *Proc. S.P.R.*, Vol. XLII, Part 134, pp. 99-103.

<sup>2</sup> J. Ehrenwald, *Telepathy and Medical Psychology* (Allen & Unwin, 1947).

<sup>3</sup> *The Experimental Situation in Psychological Research*, Section 28, pp. 38-40.

<sup>4</sup> *Some Cases of Prediction*, pp. 120-5.

that she jumped out of her bed and knocked her head on the chest of drawers. The whole scene came to life many years later in a Club in Central India. She started forward to see the fair man and knocked into a stranger. At the time of the 'vision', the Club house had not been built and the two men had been boys at Eton. The percipient's testimony was corroborated by her sister and a friend. There is a correspondence between these cases of 'pre-vision' and the cases of 'hypermnnesia'<sup>1</sup> in which the subject 're-experiences' the 'past' with a wealth of detail arguing for something more than mere 'memory'.

Many cases of spontaneous precognition lend support to the hypothesis that telepathic interaction occurs between 'subliminal selves' with 'extended specious presents'. Discussing the case of Mrs Schweizer,<sup>2</sup> Saltmarsh remarks<sup>3</sup> that the details are 'utterly inexplicable unless we adopt the rather unlikely hypothesis that the precognition occurred in the first place to Mr Deverell and never reached his normal consciousness, but was transmitted telepathically to Mrs Schweizer, and that the detail of the name, "Henry Irvin", was added as a sort of extra identification'. I do not quite understand why Saltmarsh regarded this as a 'rather unlikely hypothesis'. Telepathic interaction at a 'subliminal level' may be the rule rather than the exception. As Deverell was an eye-witness of the accident which killed Frederick Schweizer, his 'subliminal specious present' would have included the details about the fall, etc.

Deverell's personal encounter with Mrs Schweizer might have been anticipated by a telepathic interaction of their selves at a 'subliminal level'. Apropos of a well-known case<sup>4</sup> in which the Duchess of Hamilton was the percipient, Saltmarsh<sup>5</sup> says that "There seems to be no reason whatever why this scene should have been foreseen by the Duchess. It looks like a perfectly meaningless and sporadic happening." Not altogether, if we suppose that Alfred Cooper's 'subliminal specious present' extended into his empirical 'future' to include the details about the bath with the red lamp over it and the man with the red beard, and that the infor-

<sup>1</sup> See Boris Sidis and S. P. Goodhart, *Multiple Personality* (New York, Appleton, 1905), Part II, Ch. 10, pp. 156-9; Part III, Ch. 14; *Modern Trends in Psychological Medicine*, edited by Noel G. Harris (Butterworth, 1948), Chs. 1 and 12.

<sup>2</sup> Mrs Sidgwick, *loc. cit.*, pp. 322-4. I am afraid I cannot share all Dr D. J. West's doubts about the fallibility of the early S.P.R. investigators who furnished the details about the case. Cf. W. H. Salter, *Proc. S.P.R.*, Vol. XLVIII, Part 175, pp. 301-5.

<sup>3</sup> *Foreknowledge*, p. 51. Saltmarsh sets down the name as 'Schweitzer'. I have adopted the spelling given in Mrs Sidgwick's original report.

<sup>4</sup> Myers, *op. cit.*, pp. 505-6.

<sup>5</sup> *Op. cit.*, p. 60.

mation was transmitted to the Duchess by telepathic interaction at a 'subliminal level' and emerged as a 'vision' in her 'borderland' state. The 'personality linkages' seem to have been all in favour of such a hypothesis. Cooper was on friendly terms with the Duke and the Duchess, and moreover was attending the Duke in a professional capacity. I shall not list here all the cases which can be dealt with along these lines. I shall merely indicate the sort of speculative analysis that can be undertaken. The telepathic situation described by Hubert Wales in the *Proceedings*<sup>1</sup> of the S.P.R. involved ostensible retrocognition<sup>2</sup> as well as ostensible precognition.<sup>3</sup> In the Gordon Davis case<sup>4</sup> arguing for telepathic interaction, there were curious features suggesting that Davis's 'supraliminal' awareness focussed on the empirical 'present' (a visit to the neighbourhood of the Eastern Esplanade, an interview with Mr Short at the Marine Parade, etc.) was embedded in a 'subliminal' awareness of the 'co-conscious' variety extending into Davis's empirical 'future' as well as his empirical 'past'.<sup>5</sup>

It is also worth noticing that, in some alleged instances of telepathy from 'discarnate minds', queer 'temporal displacements' occurred. Mrs Piper's control 'G.P.' sometimes referred to empirically 'past' happenings as if they were contemporaneous events. A very odd circumstance is set down in Nea Walker's *The Bridge*.<sup>6</sup> During a sitting with Mrs Warren Elliott, a 'communicator' referred to a trivial event that happened three or four days *later* in language that implied it was already 'past'. One of the Rev. C. Drayton Thomas's 'communicators' volunteered the

<sup>1</sup> Vol. XXXI, Part 80 (November 1920). Cf. The 'Telepathic diary' of Dr and Mrs S. published by Myers, *op. cit.*, p. 455-7.

<sup>2</sup> *Ibid.*, cases I, XIII, XXX, XXXII, XXXVI, XLII, XLIII, and XLV. Mr Wales remarks that, assuming a telepathic connexion, the transmission or the emergence of thought was sometimes 'apparently considerably deferred' (p. 145). In case XXXVI, the only unity of the telepathic impressions was provided by Mr Wales's drawing-room seen in a 'retrospective vision', as it were.

<sup>3</sup> *Ibid.*, pp. 200-208.

<sup>4</sup> *Proc. S.P.R.*, Vol. XXXV (1926), Part 96.

<sup>5</sup> *Ibid.*, Note, p. 572. Dr Soal draws our attention to the fact that, during the sitting with Mrs Blanche Cooper on *January 4*, Playle was mentioned as an old school friend of Davis. Davis's diary showed that on *January 6* he had an interview with Playle. It is necessary to realize that, in laboratory investigations of precognition, the subject's interest is focussed on small units of behaviour. The experimenter can seldom use a time-unit longer than a few hours without relaxing his 'control'. Outside the laboratory, we usually think in terms of larger chronological units (days, weeks, months) which derive their significance from the concrete events of our lives. Cases of spontaneous precognition seem to refer to these larger units.

<sup>6</sup> Cassell, 1927, pp. 285-6.

puzzling explanation: 'I myself have often thought that your next week is sometimes our today'.<sup>1</sup> All this may be dismissed as 'non-evidential' stuff, but I submit that it is quite in keeping with the hypothesis of an 'extended subliminal present'. So is an incident discussed by Mr J. G. Piddington in his elaborate attempt<sup>2</sup> to refute the hypothesis of telepathy *inter vivos* as an explanation of the 'one-horse dawn' scripts. He argued that the appropriate contexts to the allusions in the script could be found only in the *Oedipus Coloneus* of Sophocles and in Sir Richard Jebb's notes thereon and that this pointed to 'discarnate intervention', since the intentional organization of the material could not, in the specific circumstances, be plausibly attributed either to the experimenter (Dr A. W. Verrall) or the automatist (Mrs A. W. Verrall). In a further note,<sup>3</sup> Mr Piddington maintained that at least one day, or perhaps several days, *before* Dr Verrall began the 'one-horse dawn' experiment, a cryptic reference was made, in Mrs Verrall's script, to the quotation of a Juvenal passage in Jebb's notes. He conjectured that Dr Verrall might have carried out an experiment that a 'discarnate intelligence' had devised and imposed on him. But, on the 'communicator' hypothesis, may we not suppose that the 'praecox olea' ('precocious olive') of Mrs Verrall's script was a *precognitive insight* into Dr Verrall's mind as well as design? So experienced an investigator as Mr Kenneth Richmond indulged in a not dissimilar speculative flight after analysing the evidence of intention in the book-test material supporting the 'La Vita Nuova' case.<sup>4</sup> It may be my fancy. But I do think that the allusions to the 'herb moly' in the 'one-horse dawn' scripts go deeper than the hypothesis of 'buried memories' or 'cryptomnesia'. The associations of the *Comus* passage<sup>5</sup> linking it with Dr Verrall, Mrs Verrall, and Dr Benson of Wellington College hint, though obscurely, at a 'past' which is not 'dead' and is far different from any stored-up 'mnemic traces'. A 'subliminal specious present' may serve at least as a guess at the truth about complex telepathic situations which *prima facie* involve 'discarnate' as well as 'incarnate' minds.

From the standpoint of psychical research, there is a great deal

<sup>1</sup> *Proc. S.P.R.*, Vol. XLVIII, Part 175 (July 1948), p. 326; *Precognition and Human Survival* (Psychic Press, 1949), p. 103. Cf. The remark by a 'communicator' in *The Bridge*, p. 304: 'I think that your "to-morrow" is our "to-day", and your "yesterday" is our "to-day" very often. We all think that.'

<sup>2</sup> *Proc. S.P.R.*, Vol. XXX, Part 76 (November 1918), pp. 175-229; Part 77 (July 1919), pp. 296-305.

<sup>3</sup> *Proc. S.P.R.* Vol. XXXIV, Part 91 (July 1924), pp. 159-65.

<sup>4</sup> *Proc. S.P.R.* Vol. XLIV, Part 145, p. 52, last paragraph.

<sup>5</sup> W. H. Salter, *Proc. S.P.R.* Vol. XXXIV, Part 91, p. 113.

to be said for Saltmarsh's speculative venture. Metaphysically viewed, however, it is exposed to grave difficulties. It should be obvious, even to moderately critical readers, that the statements about telepathic interaction 'occurring' at a 'subliminal level' that I have made in this note would be philosophically unintelligible without some very drastic revision of our notions of temporal becoming. I shall not attempt to explore here the lines along which the metaphysical reconstruction can be carried out. In view of the distinctive merits of Saltmarsh's hypothesis, any reformulation of it seems worth while.

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## HOME-TESTING ESP EXPERIMENTS

SECOND REPORT BY G. W. FISK

SINCE the preliminary report (*Journal*, January-February 1951, pp.369-70) which gave results up to 31 October 1950, the tests have continued and the following are the details from the commencement in January 1950 up to 31 July 1951.

No. of percipients tested	. . . . .	233
Total runs (25 trials)	. . . . .	3694
Average no. of runs for each percipient	. . . . .	16
Total trials : On target	. . . . .	92,350
„ „ On -1 and +1 displacements	. . . . .	88,656

	<i>Backward Displacement (-1)</i>	<i>Target (0)</i>	<i>Forward Displacement (+1)</i>
Total Hits	17278	18518	17320
Expectation	17731	18470	17731
Deviation	-453	+48	-411
Critical Ratio	3.81	0.40	3.46
P	.00015		.00056
Odds against Chance	6900 : 1		1780 : 1

With one exception (whose figures have been excluded from the results tabulated above) no individual subject was found whose



scores were consistently significant either on the target or in displacement directions. The significant negative scoring on both  $-1$  and  $+1$  displacements has continued. It will be seen (if a comparison be made with the first report) that the  $P$  figure for these  $-1$  and  $+1$  negative deviations has diminished from  $\cdot004$  to  $\cdot00015$  and from  $\cdot001$  to  $\cdot00056$  respectively. The very close conformity of the direct hits with chance expectation has also been maintained.

The persistent displacement negative scoring is very puzzling. It is a very small effect—the average deviation from chance expectation per run is only  $-0\cdot12$  for  $-1$  and  $-0\cdot11$  for  $+1$  displacements—and it is only its continuance over a large number of runs that raises it to significance. It is important to ascertain whether the effect is a genuine phenomenon and not merely a statistical artifact; the analysis is being pursued in order to determine this. In order to obtain further data, I shall be glad if any who can take part in these Home ESP Experiments will get into touch with me (at 6 Ditton Grange Close, Ditton Hill, Surrey) and also if any who have already received packs of cards and score-sheets will continue the tests and let me have the results, no matter how insignificant they may appear to be.

As mentioned above, one percipient has produced strikingly abnormal scores. In his first 400 trials he scored 190 hits against a chance expectation of 80—which gives a fantastic critical ratio of  $13\cdot75$  ( $P$  is electronically small!). In his 9–12th runs he made successively 16, 18, 22, and 20 hits. I doubt if anyone in this country has ever before, in experiments with ESP cards, scored 22 hits in 25 trials. I had an opportunity of meeting this percipient, and the agent and experimenter. They are all trained physicists and I, personally, was satisfied that the conditions under which the experiments were being conducted were reasonably good. Random numbers were used to determine the order of the cards guessed in a variant of Dr Soal's technique. Since my visit the percipient has suddenly begun scoring negatively—but still significantly. His latest 200 trials give a C.R. of  $-2\cdot65$  ( $P = \cdot008$ ). The experiments are continuing.

A disconcerting discovery is that one percipient is sometimes scoring significantly on the 'wrong' pack. In one instance her score was normal on the pack actually being turned up by the agent but significant (C.R. =  $3\cdot4$ ,  $P = \cdot00068$ ) on the *next* pack already prepared for use. That appears to be a forward displacement effect of a whole pack (25 cards) at a time. One is dismayed at the prospect of the re-examination of the 3,694 runs so far recorded in order to determine whether the occurrence is an isolated freak!

Finally, I think it is advisable to repeat a sentence from the preliminary report: 'As the experiments were not supervised, it is, of course, legitimate only to regard the results as suggestive, however significant they may appear statistically.'

## AN ESP EXPERIMENT WITH A DOUBLE TARGET

BY G. W. FISK AND D. J. WEST

THIS experiment failed to yield evidence of any ESP effects, but a brief summary is reported here to illustrate a particular line of investigation.

Ten of the subjects who had taken part in G.W.F.'s Home-Testing experiments (reported in the *Journal* for January-February 1951, No. 662) acted as percipients in these tests. Some were chosen because their results seemed promising; others because they were available.

The subjects were each sent a different set of sixteen sealed packs of ESP cards and were allotted a different time for recording their guesses. They were told that at the specified time G.W.F. would 'concentrate' on the target cards in his home while they 'concentrated' on the sealed packs. The subjects presumed that the sealed packs contained cards in the same order as the targets in G.W.F.'s home, but in fact this was not so. The sealed packs were randomised by D.J.W. and their order was unknown to G.W.F. The purpose was to find out which would prove the more effective target, the cards in the subject's possession or the cards being looked at by G.W.F.

The question, unfortunately, was not answered, for the results showed no significant correspondence between the subjects' guesses, taken as a whole or individually, and either of the two possible targets.

### SUMMARY OF SCORES

(4000 trials)

	<i>On Sealed Targets</i>	<i>On Fisk Targets</i>
Total hits scored by subjects - - - - -	790	786
Chance expectation - - - - -	800	800
Deviation - - - - -	-10	-14

## HOW DO BIRDS NAVIGATE?

## A REVIEW OF FURTHER EXPERIMENTS

BY A. J. C. WILSON

Two years ago I read a paper to the Society (printed in the *Journal*, Vol. 35, pp. 30-6) on theories of bird navigation, with special reference to that of Professor H. L. Yeagley.<sup>1</sup> He suggested that homing pigeons recognize (i) their latitude, and (ii) the vertical component of the earth's magnetic field, and fly towards familiar values of these variables. They could thus navigate towards home over completely unfamiliar territory, and would need to make use of landmarks for the last 20 or 30 miles only. The suggested method (Coriolis acceleration) by which the birds could recognize latitude came in for severe criticism, and biologists in general were unwilling to accept the evidence of the experiments he devised and carried out to test his theory. These, however, seemed more successful than would be expected if his theory were entirely wrong, and one therefore turns with interest to his second paper.<sup>2</sup> I should say immediately that in reading this one experiences the kind of disappointment so familiar in psychical research. A line of investigation yields promising but not conclusive results when first taken up, and when continued the further results can only be described as still promising and still inconclusive.

Professor Yeagley recognizes the force of some of the objections urged against the Coriolis-acceleration suggestion, and acknowledges the help that shore lines may give to birds in suitable localities (what he calls 'pilotage', as distinct from 'navigation'), but describes a further six experiments in support of the general correctness of his theory. These fall into three groups: (i) a repetition of the experiment of attaching magnets to the pigeons' wings, (ii) further experiments on the conjugate-point idea, and (iii) following groups of three to ten birds by aeroplane.

The repetition of the magnetic-wing experiment can only be called a failure. Most of the birds went well east of their objective, whether they were provided with magnets, or with copper plates, or were without attachment. Professor Yeagley is inclined to connect this with magnetic disturbances on the day of the experiment, but it is astonishing that it was not tried again when conditions had become normal. An independent magnetic-wing

<sup>1</sup> Henry L. Yeagley, 'A Preliminary Study of a Physical Basis of Bird Navigation. Part I.' *J. Applied Physics*, 18, 1947, 1035-63.

<sup>2</sup> *Ibid.*, Part II. *J. Applied Physics*, 22, 746-60.

experiment by Professor Gordon<sup>1</sup> was also inconclusive ; all birds reached home within a few hours of release. Perhaps the distances involved (36-58 miles, compared with Yeagley's 92) were too small to give any discrimination.

The conjugate-point experiments, taken singly, were about as convincing as that described in my previous review. Lumped together, the evidence looks more impressive. A total of 671 birds was released in seven experiments (three described in his first paper, four in his second), and *on the average* the birds flew about 36 miles in a direction only  $3.2^\circ$  from that to be expected on Professor Yeagley's theory. The probability of the direction being so nearly right by chance is, I suppose,  $3.2^\circ/180^\circ$ , or less than 0.02.

The experiment of following birds by aeroplane has been tried previously by Griffin and Hock (see *Journal*, Vol. 35, p. 31) with gannets released inland ; these sea birds seemed to fly at random till they reached a coast line. In Professor Yeagley's experiments involving 31 groups of birds, 8 groups flew more or less directly in the expected direction, 2 groups flew in the directly opposite direction, 2 made more or less continuous circling movements without getting far, and the rest flew in more or less irregular polygonal curves. The pilot of the aeroplane observed that the pigeons seemed to follow mountain ridges for some time before summoning sufficient resolution to fly up and over them, and to go out of their way to circle over towns before continuing on their route.

As mentioned earlier, these results cannot be described as more than promising, and worth following up. Possibly an attempt to study separately the two components of the proposed navigational grid would be fruitful. This might be done by choosing release points that required the birds to home in latitude only, or in magnetic vertical component only, preferably with aeroplane following.

<sup>1</sup> Donald A. Gordon, 'Sensitivity of the Homing Pigeon to the Magnetic Field of the Earth'. *Science*, 108, 710-11. (This forms part of a symposium occupying pages 705-11, but is concerned mainly with *migration* rather than navigation.)

## REVIEWS

THE CLAIRVOYANT THEORY OF PERCEPTION: a New Theory of Vision. By M. M. Moncrieff. Foreword by H. H. Price. London, Faber, 1951. 315 pp. 21s.

This is an interesting book, as the author has made a detailed examination of one particular way of developing Bergson's suggestion that the function of the central nervous system is in the main eliminative and not productive. Captain Moncrieff first analyses the current theories of perception, paying particular attention to vision. He demonstrates their various weaknesses from the point of view of plain observation, logic, and neurophysiology. He then presents his new theory. This suggests that the nervous impulses reaching the cortex from the sense organs there 'evoke or release in the mind clairvoyant vision from points of view at each retina of those external material things from whose surfaces are reflected the physical radiations that stimulate the retina' (p. 106). Hence the observer is supposed to look out at the outside world from a position just in front of the retina. This allows diseases of the cornea and lens to affect clairvoyant vision directly. It appears that the distorted form of the nervous stimuli arriving at the cortex consequent on the distortion of the cornea and lens bears no relation to the subsequent disordered perception of the external world. This latter is due to the direct distortion of clairvoyant vision trying to peer out from its position just in front of the retina through the foggy lens of cataract or the scarring left by corneal ulcers. It seems to be a necessary consequence of this theory that diseases of the nervous system proximal to the retina should not affect vision. For if we consider the case of a progressive cataract, as long as a glimmer of light can get through to the retina impulses will reach the cortex and there exercise their powers of evocation on the mind. This latter must belong to the 'all-or-none' class of reaction, as the resultant defective vision is held to be due directly to the fact that clairvoyant vision cannot see through the cloudy lens.

Now, if we consider cases of bilateral vascular lesions of the parietal lobe, the nervous impulses reach the cortex correctly and any clairvoyant vision released would have a perfect set of lenses at its disposal. Yet marked disturbances of space perception result. Thus we are forced to accept that a disordering of the electronic patterns in the brain causes the disorders of space perception directly, which is the very position this theory was designed to avoid.

The author continues to elaborate his theory with much ingen-

uity, paying particular attention to optical matters. He gives an account of an interesting series of experiments conducted using a Pigeon-Cantonnet stereoscope. It is difficult to accept the theory, however, on account of the large number of phenomena which are not discussed in the book and which would appear to offer insuperable difficulties. The following are some of these :

(1) After-images.

(2) The neurological condition mentioned above which follows bilateral lesions of the parietal cortex. In this condition the patient cannot estimate distance, and out of a multiplicity of objects can never see more than one or two at a time. Right and left orientation is entirely lost, and he cannot tell whether an object is receding or approaching. Vision in depth is, however, maintained and the patient remains keenly aware of his disabilities.

(3) The formed visual hallucinations (of people talking together, etc.) produced by stimulating the temporal cortex in certain conditions where this part of the brain is unstable.

(4) The marked changes in the perceived world produced by mescaline.

(5) The demonstration by the stroboscope that the visual field in consciousness is built up by a scanning mechanism.

With regard to (4) and (5) the following extract from Professor Meyer-Gross's recent article ('Artificial psychoses and other mental abnormalities produced by drugs'. *British Medical Journal*, 11 August 1951) is of interest :

Within the field of vision the drug [mescaline] interferes with the stillness and movements of objects. The *remarkable mechanism* by which we normally see things fixed and of persistent shape seems to break down in the intoxication. The process of perceiving and of picturing our surroundings at an instantaneous glance is disturbed, probably slowed down, and *we observe the scanning of the picture in progress*. (My italics).

The chief value of this interesting book lies in the author's keen criticisms of the current theories of perception.

J. R. SMYTHIES

SECOND SIGHT IN DAILY LIFE. By W. H. W. Sabine. London, Allen & Unwin, 1951. 208 pp. 12s. 6d.

The author, an Englishman now resident in the United States, has for over twenty years followed the commendable practice of recording in his diary all instances in which there seemed to him to be a significant connexion between his dreams or waking

experiences and events of which he had no normal knowledge and which he could not rationally infer at the time of his experience. As he is well read in the literature of psychical research, and alive to the bearing on his experiences of other schools of psychology, great interest attaches to his comments and to the theory which he propounds.

He argues that there is a much larger precognitive element in dreams and in waking experiences than is generally recognized, and discusses various causes for the prevalent under-estimate. Thus, the connexion between experience and event may fail to be recognized owing to the symbolic presentation of the event to the percipient's conscious mind. Or it may be overlooked because the percipient, if he is on the watch for possible fulfilment, is expecting it to take the form of a more important event than actually occurs ; or, again, because he does not realize that the connexion, if any, may be not between his earlier experience and some subsequent external event, but between two experiences of his own of which the second consists of, say, hearing or reading a perhaps inaccurate account of an event in the external world. Or it may be that a significant connexion is observed, but owing to a bias in favour of telepathy, factors in the case pointing to precognition may be overlooked.

Even where the manifest content of a dream corresponds closely with some contemporary event not normally known to the dreamer, it is difficult to be sure that there has been any form of paranormal activity, owing to the frequency of dreams and the multiplicity of incidents, some one of which might fortuitously correspond. It is in fact only when the manifest content of the dream and of the event are very unusual, and the correspondence between them exceedingly close, that a paranormal explanation seems to have any validity except for the dreamer.

But if things happen in this order : (1) a dream or other experience, (2) an external event not on the face of it closely corresponding to the experience, (3) the percipient's acquaintance with the event by some normal means (e.g. a newspaper account of it), (4) a retrospective interpretation of the experience, possibly a symbolic interpretation made by the percipient in the light of (3), the scope both of chance coincidence and of subjectivity is greatly extended, and the case for paranormality proportionately weakened. The interpretations of his experiences given by Mr Sabine seem to me in general tenable, but not so certainly and uniquely the right ones as to make one say at once, '*Of course*, that experience foreshadowed that event!' But the cases he cites are interesting and his comments are instructive.

The latter part of the book is devoted to a criticism of current

theories regarding precognition, and a statement of the author's own hypothesis. He is dissatisfied, for reasons which he gives, with all explanations based on a two-dimensional view of time. He suggests that both the act of precognition and the physical perception of the fulfilment are acts of memory of a 'Basic Experience' antecedent to both.

The process . . . may be set out in its entirety as follows :

- A. Basic experience.
- B. Basic memory.
- C. Basic memory intermittently transferred to Conscious Memory (Precognition).
- D. Physical experience.
- E. Conscious Memory of 'D', which may be observed to correspond to 'C'. (p. 180.)

The author regards the Basic experience as 'an experience solely in the sphere of thought', and not attempting to define it, adds :

The most that can be said, and speaking in relation to the problem under discussion, is that a Basic experience is an experience of the mind, one which, when transferred to the conscious memory, constitutes Precognition of later experiences through the physical sense organs. (p. 181.)

It will be seen that Mr Sabine has difficulty not only in defining the Basic experience, which is the central point of his theory, but in offering the reader more than the vaguest notion of it. This is not due to any failure on his part to analyse the available evidence, but to the fact that at present the available evidence is too scanty. Apart from his own careful records, there are a number of published spontaneous cases, not all particularly cogent ; if dreams are omitted from the calculation, rather a small number, as Saltmarsh's *Foreknowledge* shows. If we turn to the results of experiment, we find a large number of successes, obtained under good conditions indeed, but with few subjects. Many more good cases, and successes with many more experimental subjects, seem to me desirable before we plunge further into a morass of speculative theory, where nothing seems certain except that all our everyday matter-of-fact notions are wrong.

W. H. S.

MORS ET VITA. Introduction par Gabriel Marcel ; textes et document réunis par Robert Aron et Jean-Claude Renard. Paris, Plon, 1951. 290 pp. 360 fr.

This book is a collection from a number of authors of evidence on survival provided by psi phenomena. The cases quoted have



all been published before and many are taken from S.P.R. sources. Various theories on the subject, including that of Whately Carington, are discussed.

Its main interest for psychical researchers lies in the introduction by Gabriel Marcel, the eminent French philosopher who has recently given the Gifford Lectures in Aberdeen. He regrets the 'incredible bad faith' of philosophers on the Continent, and particularly in France, in refusing even to consider the possibility of psi, but he acclaims what he calls the infinitely more open-minded and honest attitude of the Anglo-Saxons. This ostrich-like attitude of philosophers he puts down partly to the pretention that humanity is already sufficiently adult to comprehend its surroundings, and partly to the fear of falling to a sub-human level or of being forced to doubt certain postulates which seem necessary to an intelligible order of things.

He finds the Anglo-Saxons too inclined to confine themselves to the collection of facts and, while expressing himself with great caution about the origins of psi phenomena, he considers it high time that more study was given to their implications.

R. I. H.

WISDOM, MADNESS AND FOLLY : the Philosophy of a Lunatic. By John Custance. Foreword by Canon L. W. Grensted. London, Gollancz, 1951. 254 pp. 16s.

In this book the philosophy forms the major contribution in what is surely the most comprehensive subjective study of cyclophrenia yet produced. It is scarcely possible to do the book justice in a review—another book assessing the various philosophical and psycho-pathological concepts might almost be said to be necessary. The play on the negative-positive, the theory of actuality against a Jungian background, is comprehensively and admirably arranged, although it is sometimes difficult to maintain the distinction between negative and positive, and, upon occasion, to be certain which one is dealing with.

Canon Grensted has commented on the apparently unrecognized trend towards existentialism ; nevertheless, the whole work is well arranged and remarkably objective considering the subjective nature of its inspiration. Obviously, Mr Custance is a man of great intellect and culture, and, although a tremendous bibliography is involved, he has been at pains to maintain readability without loss of purpose or direction.

From the point of view of the psychiatrist this book reveals far more than similar works—for instance, those of Clifford Beers and *The Maniac*—for even during periods of mild psychopathy he is

still able to describe objectively his mental process during periods of elation and depression with their accompanying fragmented delusional systems and clang associations. It is here, of course, that the Jungian reveals himself (and it is not surprising that an Adlerian approach failed to serve a useful purpose during one of his episodes).

Obviously one reason why Mr Custance is able to write this book at all is because he has been able to develop philosophical and psychological concepts secondary to his command of language and imagination, without which intelligence alone would not have been enough, and without which the manifestations of the 'collective unconscious' would have remained isolated instead of being woven into the pattern of 'as if' experience. It is interesting that Mr Custance, having been through the 'hell' of depression, now feels able to face the future, in that he is not now terrified of further depressive illness as formerly.

One notices that both Mr Custance and Canon Grensted use the word 'lunatic', presumably in their desire not to be mealy-mouthed. From the onlooker's point of view it must be recognized that the words 'lunatic', 'psychopath', and 'neurotic' now involve unpleasantly tinged concepts of personality whereas, from the therapeutic point of view, it is obviously socially desirable that mental illness should not be so contaminated and it would probably be worth while to eradicate these terms from descriptive literature.

As it is, even wit is not denied us; halfway through the book a litter of pigs is given beautiful names which had to be changed to 'more prosaic ones suited to the herd book' and later, in a comment about Ashtaroth, there is an aside about ecclesiastics 'without mentioning names' who 'may even be worshippers of her cruel spouse Moloch'.

The appendixes call for brief comment in so far as hospital management and treatment are concerned, and nobody would disagree with most of the views expressed. It is notoriously difficult to establish culpability when it is suspected and when the greatest efforts are made to prevent collaboration between members of the staff concerned, as I once found as a member of an official tribunal of inquiry.

With regard to the relative values of different forms of treatment, it is not possible here to do more than suggest that results differ in the hands of different therapists at different times. Weir Mitchell's own experience of failure only when he began to examine the rationale and effects of treatment is one example, and it would hardly be fair to assess the value of two electro-convulsive treatments as against half-a-dozen given during a different episode

in a different hospital by a different therapist, with probably a different type of machine.

It is altogether a most stimulating and exciting book, which will become a classic.

LOUIS ROSE

THE UNCURTAINED THRONE. By Warner Allen. London, Faber, 1951. 217 pp. 12s. 6d.

A number of years ago the author of *The Uncurtained Throne* had a mystical experience which transformed his previous 'patternless existence' into the state of serene certitude about the existence of God common to mystics of all ages and creeds. After fifteen years spent 'in contemplation of this mystery' he wrote two books, *The Timeless Moment* and *The Happy Issue*<sup>1</sup> whose argument, he says, 'started from the central fact of the spiritual experience as an assumed reality transcending logic, and subordinated to it the experience of the objective self or soul and the facts of the external world.'

In *The Uncurtained Throne* he reverses this argument, 'moving from scientific experiment to spiritual experience and seeking to prove the truth that science denies from the facts that science affirms'. Among those facts he cites the findings of psychical research.

His success in this aim is still likely to depend on the previous attitude of the reader. A young mathematician recently had an experience similar to Mr Allen's, and—much to his astonishment, for he was quite ignorant of mystical literature—he also found his attitude changed in a split second from agnosticism to blissful certainty. His subsequent comment about the thesis of this book was a placid, 'Of course.' It is safe to say that with his type of mind his comments before the experience—which he, like others, insisted was impossible to describe—would have been unprintable.

The mystic shares this language difficulty with humbler psychics, though he more often escapes another: that of confounding the image through which an experience may be recorded with the experience itself. To do this may lead both percipient and researcher astray if only because one image may symbolise a number of things and a number of images may be used to convey one experience.

Books like *The Uncurtained Throne* are only likely to be of

<sup>1</sup> Both published by Faber.

interest to the psychical researcher who constantly bears such difficulties of language and imagery in mind.

R. H.

JOURNAL OF THE AMERICAN SOCIETY FOR PSYCHICAL RESEARCH.

Vol. 45, No. 3, July 1951. New York, A.S.P.R. \$1.50.

I did not do justice to a paper by C. M. Cooper, M.D. in the October 1950 issue of the Journal, and a new report from his pen in the July 1951 issue gives me an excuse to refer back to it.

Cooper has used a similar approach to both his problems—that of self-observation. His handling of this simplest of tools is very effective and we can all profit from it.

His first paper, rather cumbrously entitled 'An Inherited Baffling Perception and its Uncovering', deals with three spontaneous experiences which Cooper had as a young medical graduate, and which he formerly attributed to a psychic gift inherited from his mother.

*Incident No. 3* may be summarised here. Cooper had been sitting for half an hour in the staff quarters of the Royal Infirmary of Edinburgh when a house physician entered. Glancing casually at him Cooper exclaimed, 'Forbes, whatever made you quarrel with your head nurse?' With some resentment Forbes admitted the quarrel.

Cooper 'uncovers' his perception in this way: 'Casually looking up, I saw that Forbes, . . . had entered . . . He skirted the end of the big centre table, picked up a pitcher of water, and began to fill a glass. In doing it he turned, so that I got a clear view of his face. It was much redder than usual . . . He is both perturbed and angry, I subconsciously divined . . . he must have quarrelled with someone. With his chief? No, he would be more perturbed but not so angry. With a student? No, he might be as angry but he would not be so perturbed. It must have been with an equal. Who is there in his ward, from which he has evidently just come, who to all intents and purposes in his equal? The head nurse.'

The other two incidents he presents and explains in like manner. But there is a weak point. Cooper developed the *dénouements* some years after the occurrences.

Sceptics are never tired of rubbing in the warning that because memory of a given event is vivid and distinct it does not follow that it is accurate. If we apply this criterion to allegedly psychic cases, we must apply it with equal rigour to alleged exposures. We may accept the plausibility of Cooper's explanations but we must reserve judgement on the accuracy of such

detailed analysis long after the event. Further, the present account (some years later still) seems to be the first written one.

What *is* important in Cooper's work is the principle he thus established (and now practises)—that a 'hunch', intuition, or premonition, should always be followed by a spell of acute introspective concentration before any claims are made for it. 'In playing games', Dr Cooper writes, 'I would come to know, without knowing how I knew, where a key card or domino lay, . . . what faulty bid or play [an opponent] would make if given the opportunity. Always in such cases the uncovered key to the discernment was the subconscious registering and correct interpretation of an *involuntary* behaviour reaction on the part of the individual concerned.'

'Self-Experimentation in Water Divining' is the title of Dr Cooper's second paper. Cooper and his brother both exhibited the dowsing reaction. Cooper began 'to toy with the twig, and to try to make it behave as it seemingly behaved of itself . . . keeping the wrists and elbows slack, I separated the ends of the prongs sufficiently to produce a decided tension where they join. I then with the fingers and forearms rotated the prongs downward and inward. At once the stem began to dip. As I continued to rotate the prongs, the twig dipped more and more, and then quite suddenly the rod itself took command and jerked my hands downward, stretching the wrists'.

Cooper now propounds the interesting theory that the dowsing reaction is a function of the anatomy and gait of the dowser. As far as I know this is new. 'I am sway-backed, and as I walk I project my body forward and brace back my shoulders. This carries my elbows backward, upward, and outward. As this occurs my forearms pronate. The result of these movements is that I produce a decided tension where the prongs join, and also rotate them. This causes the stem of the rod to begin to dip. I grip tighter to stop the movement, but in so doing I automatically further pronate and hence without realising it, increase the force that is causing the movement.'

After describing the results of holding the twig in a different fashion and of using other dowsing implements, Cooper refers to experiments in approaching a pail of water from various distances. He observed that he held the prongs very slackly until he approached to a certain distance from the pail, then he subconsciously tightened his grip, thus alerting his muscles, and giving the dowsing reaction.

Cooper concludes that water divining is a spurious<sup>1</sup> pheno-

<sup>1</sup> It has been brought to my notice that certain dowsers have been offended by my use of the word 'spurious' in an earlier review. It should

menon—that the employment of the dowsing reaction in the location of water in his own case depends on his ability to read terrain, ‘and that those professional dowsers who possess a marked flair in this direction would achieve a correspondingly high percentage of dowsing successes’. Cooper’s own experiments, however, can hardly rank higher than a pointer to such a generalisation.

The other papers in the July issue are ‘A Series of Spontaneous Cases in the Tradition of *Phantasms of the Living*’ by L. A. Dale; ‘The Prophet as a Subject for Psychological Research’ by William E. Hulme, and ‘The Medical Section: the first Three Years’, a note by Geraldine Pederson-Krag.

DENYS PARSONS

## CORRESPONDENCE

### PREJUDICE

SIR,—There are a large number of people who do not accept any of the findings of psychological research. This may be because the evidence is not as yet good enough. The evidence for any phenomena we are trying to prove must be considerably greater if they are in disagreement with previously accepted ideas than if they are in agreement with these ideas. These ideas are not usually accepted blindly, but because all the available experimental evidence points in a particular direction. The philosophers refer to the antecedent improbability of a phenomenon in order to describe this fact. Many of our critics find refuge in this concept of antecedent improbability, and to a certain extent legitimately. But many of us suspect that the real stumbling block is blind unreasoning prejudice. It is my purpose to suggest a logical method of distinguishing between the two.

If there is an antecedent improbability of a phenomenon like telepathy, it may be stated in exact terms *before* any experiments are considered. One may say, for example, that the antecedent improbability of the phenomenon is  $10^{-35}$  to 1 that the phenomenon will occur. Then, if we have a statistical experiment, for example, we may consider our case as proved as soon as we have antichance odds  $10^{35}$  to 1. It is no use now for the critic to change

hardly be necessary to explain that in common scientific usage this epithet carries no derogatory attributes such as ‘counterfeit’ or ‘fraudulent’. A scientist might easily use the word in reference to a facet of his own work, meaning that certain evidence had led him to a wrong conclusion.

his mind : if he changes his mind, we know that his criticism and his assessment of antecedent improbability was unreasoned, and we can therefore ignore it.

Of course, a critic may not have the mathematical knowledge to state the antecedent improbability in the terms we require. He may say, for example, that the effect is about as likely to occur as obtaining a bridge hand where every player has a complete suit. The mathematician can soon transpose this into the terms required for accurate comparison with ESP data. In the example quoted the probability is  $4! (13!)^4 \div 52!$  that such a bridge hand will occur, which is approximately  $10^{-27}$ . We take this now as the antecedent improbability of the phenomenon.

Soal and Goldney's experiments (*Proc. S.P.R.*, vol. 47, pp. 21-150) gave a probability of  $10^{-35}$  that their results were due to chance. If we take the antecedent improbability from the example quoted, we find the *a posteriori* probability from the relation

$$\begin{aligned} a \text{ posteriori probability} &= \frac{\text{experimental probability}}{\text{antecedent improbability}} \\ &= \frac{10^{-35}}{10^{-27}} \\ &= 10^{-8} \end{aligned}$$

as the *a posteriori* probability that Soal and Goldney's results were due to chance.

It might be interesting here to note the probability, as calculated from the physical principles of quantum mechanics, of what is apparently an impossible event. A very slowly moving car, of weight 1 ton, and very small velocity, encounters a bump in the road 1 foot high and 100 feet long. According to classical physics, the car cannot get past the bump ; according to quantum mechanics there is a small but finite chance of the car being observed at the other side of the bump. Calculation gives a probability of  $10^{-39}$  for this event ; not much more improbable than the idea that Soal and Goldney's results are due to chance. (This example was taken from notes of lectures given by Professor Enrico Fermi at the University of Chicago. It should be noted that the probability is zero that the car will be observed on top of the bump; it is only finite on the other side.)

It must be realized that there may be other objections to the experiments besides the one that the results are due to chance. For example, Soal, Goldney and the whole horde of distinguished persons who were brought in as witnesses may have been conspiring to put on a gigantic hoax. Is this likely? Of course the answer to the question, Is it likely or not? is largely a matter of

opinion. Here again the same criterion applies as has been enunciated above. The critic must first name a figure, no matter how large, for the probability that this could be so; after this, he cannot legitimately change his mind. The only legitimate time for him to argue about antecedent improbabilities is before the experiment has been carried out, or the results have been made available to him. If now the critic allows that the chances, for example, of 7 people being deceived in 7 different experiments are independent, we can multiply the number of experiments and witnesses until we have a figure sufficiently big to overcome the antecedent improbability which he has named.

Unless the critic complies with the conditions stated above, it can be mathematically proved *that he can never be answered*. For, as soon as a new experiment is performed with sufficient anti-chance odds to overcome the antecedent improbability, the critic will change his mind and increase the antecedent improbability. This can proceed ad infinitum. It will be clear to the mathematician that the arguments given above are the same as those required for the convergence of an infinite series.

The question of how to combat prejudice when it has been recognized is difficult, and is probably our main task for the future.

RICHARD WILSON

#### IS PK GUIDED BY KNOWLEDGE?

SIR,—Herewith a thought on the problem whether PK must be guided by knowledge of the system to be affected, acquired either normally or paranormally.

One way of attacking this problem, suggested by Mr C. W. K. Mundle, is to use as a PK tester two random selectors combined with delay switches, as described in *Proc. S.P.R.*, Part 178, pp. 67-8, and testing two groups of people with this apparatus. To one group is explained the *modus operandi* of the selectors so that they can intelligently direct their act of willing; the others are given a simple false explanation so that any effort at directing their willing will be ineffective. Should both groups score equally well, it is suggested that the hypothesis that PK is guided by knowledge would suffer a severe blow.

This is probably the best suggestion so far made on the subject; but unfortunately neither this nor any other such experiment can be conclusive. Dr Thouless's experiments on  $\Psi_{\gamma\kappa}$  (*Proc. S.P.R.*, Part 179, p. 116), show that if PK is guided by knowledge, then this knowledge need not be sensorily acquired. Presumably the first group in this experiment could, from the information given them, reason out that a certain part of the mechanism would have



to be affected in a certain way in order to effect a desired result. The others could discover this either by clairvoyance on the machine (which is most unlikely) or telepathy from the first group ; and so we are back where we started. Both groups are equally well equipped with the necessary information ; in the first case, this is acquired sensorily, and in the second, extra-sensorily.

For such a reason, I believe that it can never be experimentally proved either that PK is guided by knowledge or not. It seems to me, however, that a decision can be reached by theoretical argument.

If PK is an act of conation (disregarding the question raised on p. 382 of this volume of the *Journal*), then I think it consists of at least two processes, that of willing an event to happen and that of causing it to happen, with possibly an intermediate linking process. Since the second process or 'force' *must* be directional, then either the primary act of willing must also be directional (whether it is or not being the point in question), or else the intermediate psychophysical process must transform a vague willing into a directional causing ; and since the intermediate process is at least something to do with a department of the agent's mind, this second suggestion is merely a variant of the first. In the series of processes we call PK, there must at one stage be a process of a directional nature and this must be a process to do with the agent and requiring the possession of the relevant knowledge. Otherwise the 'force' or whatever it is that makes the die come to rest with a certain face uppermost must be made directional independently of the agent ; and if so, why have an agent at all? PK would then be little less than an 'act of God'. All this need not mean that the agent must have a detailed knowledge of the processes causing the occurrence of the event willed. A person moving his arm need have no knowledge of the physiological changes involved in brain, nerves, and muscles ; all he needs to know is that the arm must be moved in a certain direction. In the PK case, all I mean to imply is that the agent must know what is to be affected and in what way. The rest he leaves to the PK equivalents of nerves and muscles.

To sum up ; my conclusion is that this problem is incapable of experimental solution, but that it is clear from theoretical argument that PK must be guided by knowledge, acquired, probably, paranormally.

I wish to thank Mr C. W. K. Mundle for clearing up some misconceptions concerning his suggested experiment mentioned above, and for initiating trains of thought leading me to the idea expressed in this letter.

MICHAEL C. PERRY

## THE FRAUDULENT MEDIUMS ACT, 1951

THIS Act (14 & 15 Geo. 6., Ch. 33) was given the Royal Assent on 22 June 1951. It is described in its preamble as 'An Act to repeal the Witchcraft Act, 1735, and to make, in substitution for certain provisions of section four of the Vagrancy Act, 1824, express provision for the punishment of persons who fraudulently purport to act as spiritualistic mediums or to exercise powers of telepathy, clairvoyance or other similar powers.'

The only difference between the text of the Act and that of the Bill (printed in the *Journal* for January–February 1951, pp. 370–1) is in the wording of subsection (2) of clause 1, which now reads as follows :

(2) A person shall not be convicted of an offence under the foregoing subsection unless it is proved that he acted for reward ; and for the purposes of this section a person shall be deemed to act for reward if any money is paid, or other valuable thing given, in respect of what he does, whether to him or to any other person.

## THE PERROTT STUDENTSHIP

The Perrott Studentship in Psychological Research at Trinity College, Cambridge, has been awarded to Mr G. Spencer Brown. He will work under the direction of Dr R. H. Thouless during his tenure of the Studentship.

# Society for Psychical Research

31 Tavistock Square · London · WC1

SUPPLEMENT

TO

JOURNAL

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FOR MEMBERS AND ASSOCIATES

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## EXPERIMENTS IN THE ELECTRO- ENCEPHALOGRAPHY OF MEDIUMISTIC TRANCE

ON 20 and 21 September two experiments were carried out in the Society's seance room in which E. E. G. records were made of Mrs Eileen Garrett's trance state. A report will be published in due course, but the Council would like to take this opportunity of recording their indebtedness to Mrs Garrett, without whose co-operation, unhesitatingly given, the experiments would not have been possible. They also wish to express their gratitude to Dr C. C. Evans, head of the Department of Electro-Physiology at Belmont Hospital, Sutton, and to Dr E. B. Strauss, Physician for Psychological Medicine at St Bartholomew's Hospital, London, for their part in the experiments, and to Mr F. Wilson, electro-physiologist of the Edison Swan Electrical Company for making available an 8-channel E.E.G. and analyser and for the great amount of time which he devoted to the installation and running of the equipment.

## DR ALICE BUCK'S VISIT TO DUKE UNIVERSITY

DR Alice E. Buck, a member of the Society, has been awarded a Full-bright Travel Grant to enable her to visit Duke University for the purpose of studying the relationship of parapsychology and psychiatry with Dr J. B. Rhine.

## OBITUARY

MISS HERMIONE RAMSDEN AND MR J. ARTHUR HILL

WE regret to record the deaths of two very old members of the Society. Miss Hermione Ramsden joined the Society in 1897. Between 1905

and 1907 she took part as percipient in several long-distance telepathy experiments in which Miss Clarissa Miles was the agent, and which are reported in Vols. XXI and XXVII of *Proceedings*. During the experiments Miss Ramsden was either at her home at Gerrard's Cross or in the Scottish highlands or in Yorkshire, while Miss Miles was in London or Belgium or the north of France. For several years she made her home in Norway and was an active member of the Norwegian S.P.R.

Mr J. Arthur Hill joined the Society in 1905 and took part in various mediumistic and experimental investigations. He was the author of several books, including *New Evidences in Psychical Research* (1911), *Spiritualism: its History, Phenomena and Doctrine* (1918), and *Experiences with Mediums* (1934). In a foreword to his *Letters from Sir Oliver Lodge* (1932) Lodge wrote that Hill had offered to help him with his psychic correspondence a quarter of a century back, when it was beginning to be heavy, and since then he had acted 'as a sort of amateur private secretary'. He was a member of the Council from 1927 to 1936, when he resigned owing to ill-health and failing eyesight. At the time of his death he had been for more than thirty years an Hon. Associate.

## OBITUARY

### B. ABDY COLLINS

B. Abdy Collins, C.I.E., a well-known member of the S.P.R., died on 22 October after undergoing the ordeal of a leg amputation two months earlier.

Mr Collins had a distinguished career in the Indian Civil Service, holding the post of Education Secretary, Bihar and Orissa (1922-6) and Director-General of Commerce and Industry, Hyderabad State (1927-33).

He joined the Society in 1939, and became well known through his activities in the spiritualist field and as Managing Director of the London Spiritualist weekly *Psychic News*. He contributed a paper to *Proceedings*, entitled 'Is proof of survival possible?' (Vol. 46, 1940), and was author of many books, pamphlets, and articles in the spiritualist press.

Psychical research implies a critical approach to the phenomena of spiritualism, but in spite of the judicial character of much of his career in India, and a careful study of S.P.R. literature, his own approach to the phenomena was one of childlike faith and, as it seemed to many investigators, one of extreme *naïveté*. Indeed, he accepted the accounts of seances, direct voice, materializations and the like which are weekly features of *Psychic News* not as phenomena of probable abnormal psychology or subliminal motivation (to leave out the rarer cases of fraud), but at their face value, with a readiness surprising to those with practical experience of investigations in this field. Yet his attitude had its roots, I think, in a conviction that this subject was of such crucial importance to human life and behaviour that it behoved one to make up one's mind on the major issue, to answer 'Yes' or 'No' to the best evidence for survival and a world beyond that recognized by today's science, and then to stride boldly forward, refusing to be hampered by close examination of matters unlikely to be satisfactorily resolved in this generation.

Whatever may be thought of such a rough and ready approach, no one

who knew Abdy Collins well had other than esteem and affection for him as a man of outstanding sincerity, of entire integrity, and of unfailing friendliness and courtesy towards those who did not share his views.

K. M. G.

### MEETINGS OF THE COUNCIL

- 467th 25 May 1951 Chairman : G. N. M. Tyrrell.  
468th 5 Sept. 1951 Chairman : The President, Dr S. G. Soal.

### MEETINGS OF THE SOCIETY

- 241st Thursday, 24 May 1951, at 6.30 p.m. DR ALICE E. BUCK ;  
'Spontaneous Phenomena in Modern Psycho-analytic Tech-  
nique.'  
242nd Thursday, 14 June 1951, at 6.30 p.m. JOHN ADDEY :  
'Astrology.'  
243rd Thursday, 5 July 1951, at 6.30 p.m. PHOEBE PAYNE :  
'Seeing Apparitions.'  
244th Thursday, 27 September 1951, at 6.30 p.m. DR S. G. SOAL on  
his recent visit to Duke University.  
245th Thursday, 11 October 1951, at 6.30 p.m. MATTHEW MACKAY :  
'Psychical Experiences in Central Africa.'

### NEW MEMBERS

*(Elected 25 May 1951)*

FOX, MISS J., 11 Bisham Gardens, Highgate Village, London, N. 6.  
MITCHELL, A. M. J., 13 Weymans Avenue, Kinson, Bournemouth.

*(Elected 5 September 1951)*

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PARROTT, PROFESSOR IAN, M.A., D.Mus.Oxon., Edgecombe, Penglais  
Road, Aberystwyth, Cards.

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STEPHENSON, S. R., 3 Market Place, Morpeth, Northumberland.  
WEBB, THE REV. G. C. N., The Parsonage, Southwick, Hants.  
WOOLCOCK, C.E., M.Sc., c/o P.O. Box 150, Portland, Victoria,  
Australia.

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'THE EXTENSION OF MIND': COMMENTS  
ON DR J. R. SMYTHIES'S PAPER

I

BY H. H. PRICE

Philosophical readers of Dr J. R. Smythies's very interesting and provocative paper on 'The Extension of Mind' in the September 1951 issue of this *Journal* can hardly fail to feel uneasy about some of his contentions ; and in this note I propose to mention some of the difficulties which are likely to occur to them. But I should like to say at once that these difficulties must not be taken too seriously. If Dr Smythies's theory turns out to be an effective tool of research, by all means let us use it as such, whatever epistemological difficulties there may be in it. He claims himself, in the last sentence of his paper, that the theory 'suggests a series of experimental investigations not only for parapsychology but also for physics' (p. 502). If he is right, let us get on with these experimental investigations. The epistemological mess can be cleared up afterwards.

In any case, I suspect that the most important and valuable part of his theory is logically independent of the epistemological doctrines which I shall criticise presently. If they are false, it might still be true that there is a psyche or psychological mechanism distinct both from the self and from the brain ; that this psychological mechanism is spatially extended ; and that the space in which it is extended is distinct from physical space, but included along with physical space in a single seven-dimensional manifold. It is true that this idea of a spatially extended psychological mechanism is not wholly original. The Sankhya philosophers of ancient India suggested something very like it some 2,500 years ago. They held that the mind, as opposed to the Self or 'Witness', is an extended and material entity, different from the physical organism. But, of course, they did not have the technical resources of modern geo-

metry at their disposal, as Dr Smythies has. It may well be that this has enabled him to reformulate an ancient idea in a novel and experimentally-testable form.

After these prefatory remarks, designed to warn the reader against taking my criticisms too seriously, I proceed to mention one or two difficulties which I find in Dr Smythies's theory as it stands.

The first concerns the status which he assigns to the human nervous system ('signalling mechanism') or rather to our knowledge about it. The science of neurophysiology appears to have a privileged and almost sacrosanct position in his theory. The trouble, of course, is that human sense-organs and nervous systems are themselves parts of the physical world. But, according to the theory, no scientist, no matter who he is, has any direct access to the physical world; and in this respect physiologists are no better off than other people. All that any scientist can directly observe is his own *perceptual* world. 'Physical space and physical objects are in fact hypotheses, as we can never observe them directly' (p. 485 *ad fin.*). But then the human nervous system and its signalling mechanism is itself only a hypothesis. Or can it be that physiologists, at any rate in their working hours, are entitled to accept the Naive Realist TNS theory, though nobody else is entitled to accept it? Do they have a direct access to the physical world itself, which nobody else has? I am sure that Dr Smythies would not seriously say so. In other places he carefully distinguishes between the perceived body and the physical body. Yet when he is writing about neurophysiology he seems to forget this distinction. He writes as if physiologists were directly aware of the *physical* bodies of other human beings. And if after all they are not, if they can only study their own percepts, and all the detailed information they give us about the structure and working of *physical* nervous systems is only a set of hypotheses, how are these hypotheses arrived at, and how are they justified?

I have said that human sense-organs and nervous systems are themselves part of the physical world, and most people would agree that this proposition is not only true, but platitudinously true. Yet there are passages when Dr Smythies seems to deny this platitude, by implication at least. He says more than once that the 'signalling mechanism' of the nervous system is *between* the physical world and the perceptual world (cf. p. 489). The word 'between' implies that the signalling mechanism itself belongs to neither of these worlds. This might be said of the psychological part of the signalling mechanism, though doubtfully, since we are told elsewhere (p. 492) that it is located in the space



of the perceptual world. But it plainly cannot be said of the physiological part, which is certainly in the physical world if anything is. If the word 'between' is to be used, the physiological part of the mechanism is between one part of the physical world and another, e.g. between environmental objects outside the percipient's skin, and his cerebral cortex which is inside it. And despite this betweenness, or rather because of it, it is itself as physical as the wires 'between' an electric light bulb and an electric battery.

I am not of course suggesting for a moment that we ought to go back to the TNS theory of 'common sense'; and I agree with Dr Smythies that Professor Ryle and other philosophers of his school are much too common-sensical in this respect. I am only suggesting that the difficulties which arise when the TNS theory is rejected should be fairly and squarely faced, and complaining (I hope not unjustly) that Dr Smythies has not faced them. To put it crudely, it seems that the physical world (including those parts of it which neurophysiologists investigate) is something less than an object of direct awareness, but at the same time something more than a mere hypothesis.

I now turn to another difficulty, or perhaps another aspect of the same one. There is a curious passage on page 496 about the barrier which prevents the experimenter from entering mind's private world, and I think that Dr Smythies himself attaches considerable importance to it. The experimenter, he says, cannot enter mind's private world 'not because it does not exist, but because of the barrier presented by a dimensional interface' (cf. also p. 500 *ad fin.* and p. 502). Here it looks at first sight as if the experimenter must be observing the *physical* world: for the dimensional interface, it would seem, is between the physical world and the perceptual world, both of them being included in one seven-dimensional continuum. If that is the right interpretation, the difficulty is the one already mentioned, that scientists who study the signalling mechanism somehow have a direct access to the physical world which other people lack.

But another interpretation is possible. Perhaps this dimensional interface, 'the most impenetrable of all barriers' (p. 500), is not between the physical world and the perceptual world, but between two perceptual worlds—the perceptual world of the experimenter and the perceptual world of the other percipient whom he is investigating. This would be consistent with what Dr Smythies insists on elsewhere, that his own percepts are the only entities which an experimenter (or anyone else) can directly observe. But in that case Dr Smythies's total universe, including *both* percepts

and physical objects, must be far more complicated than he wants it to be. Seven dimensions will be far too few. We shall need dimensional interfaces not merely between the physical world and 'the' perceptual world, but between the physical world and *each* perceptual world; and there will be as many perceptual worlds—and perceptual spaces—as there are percipients. Not only so. We shall also need dimensional interfaces between each perceptual world and every other perceptual world; for instance, between your perceptual world and mine, if you are the experimenter and I am the person experimented upon.

To put it another way, it would seem that on Dr Smythies's assumptions there is no such thing as 'the' perceptual world at all, though there is such a thing as 'the' physical world, or at any rate we may entertain the hypothesis that there is. It would seem that there must be many perceptual worlds, and many perceptual spaces, each of them private to the percipient who perceives it. And as for the physical world, it is not exactly public either, though it could be called neutral as between all the different percipients. To call it public would suggest that it is *observed* by all of them, as the ordinary man, of course, believes that it is; but on Dr Smythies's assumptions it cannot be, because it is not observed at all.

This privacy of perceptual worlds makes it very difficult for Dr Smythies, as for Berkeley, to explain how different percipients can communicate with one another, or indeed how one percipient can learn, or even suspect, that other percipients exist. Like Berkeley, he seems to be in grave danger of Solipsism. I think it is incumbent on him to explain how a public perceptual world ('the' perceptual world) extended in a public perceptual space, is constructed, or postulated, when each of us has direct access only to his own private one. Or does each of us first arrive at the hypothesis of a *physical* world (however he justifies it) and then proceed from this to the further hypothesis that there are other *perceptual* worlds correlated somehow with his own? Whatever the right answer is, this problem of the privacy of perceptual worlds and perceptual spaces should surely be looked into. Incidentally, there is some difficulty in understanding how even one person's private perceptual space is constructed. If we stick to the bare data, it would appear that visual space and tactual space are different. Before I can be aware of perceptual space, even a private one, I must somehow correlate the two.

Moreover, Dr Smythies's treatment of the space of mental images (p. 486) seems to be much too simple. It is true that there are 'projected' mental images, and there are eidetic images. No doubt we can find room for these in perceptual space, or at least in

visual space. (Dr Smythies's imaged lines sketched 'round the objects of our perceptions' seem to be a special sort of projected visual images.) But there are many visual images which are neither projected nor eidetic. They certainly have spatial properties, but they seem to be 'in a space of their own', which is neither perceptual space nor physical space. The same is true of at any rate some visual hallucinations (when the percipient's entire visual field is hallucinatory, and not merely a part of it). And what about dream images? Dr Smythies has not mentioned them at all. Yet it is quite plain that no student of telepathy and precognition can afford to neglect them, even if others can. Dream images, too, seem to be located 'in a space of their own'; moreover, even if we confine ourselves to the experiences of a single dreamer, there are no discernible spatial relations between the dream image I have tonight and those I had yesterday night.

It would seem, then, that Dr Smythies's theory of space, ingenious as it is, is much too neat and tidy. But here I would venture to remind the reader of what was said in the second paragraph of these notes. I would suggest that the most fruitful and original part of his theory is the part concerning the psychical mechanism, and I do not believe it was necessary for him to go into the problems of perceptual space at all. By all means let us carefully consider his hypothesis that there is a spatially extended psychical mechanism. And in so doing, let us carefully consider his very helpful suggestion that the space in which it is located is not physical space but another one, separated from physical space by a 'dimensional interface', and included along with physical space in a total seven-dimensional continuum. But granting that the psychical mechanism is not in physical space, does it follow that it *is* in perceptual space, as Dr Smythies seems to say on p. 492, sub-paragraph (v)? Not in the least, and the suggestion is most unpalatable. For if it were true, one would think that every percipient ought to be able to perceive his own physical mechanism, since after all it would be somewhere or other in his perceptual world; and notoriously no percipient is able to do this. Of course, Dr Smythies can maintain that the psychical mechanism is the proximate cause of our percepts and therefore of their spatial properties, since it is the last link in the causal chain by which they are generated. But it will not follow from this that it is *spatially* related to the percepts which it causes.

There are many other points in Dr Smythies's paper which call for comment, for instance his 'like to like' theory of causation (pp. 493, 500) and his very surprising estimate of the philosophy of Descartes (pp. 492-3). I will mention only one, because it seems to be crucial for the understanding of his theory. At the top of

page 488 he says we must postulate that 'minute *physical* forces cross the interface in either direction' (my italics) and identifies these forces with  $\Psi_{\kappa}$  and  $\Psi_{\gamma}$ . If I understand his theory rightly the word 'physical' here is just the wrong one. For surely these forces are not confined to the physical part of the seven-dimensional continuum; they belong in a way to both parts at once, and indeed that is the whole point of postulating them. If they were just physical, they could not cross the interface. A few lines below he calls them 'trans-dimensional physical forces'. It seems to me that 'trans-dimensional' is the right adjective, but 'physical' is the wrong one.

## II

BY ROBERT H. THOULESS

ONE of the most obvious needs of parapsychology is the formulation of hypothetical systems which will lead to expectations that can be tested by experiment. In judging any suggestion that may be made, the first question to be asked is not whether it is true or false but whether it is likely to be experimentally fruitful. Dr Smythies's ingenious speculation seems to offer good hope of proving fruitful in suggesting directions of experimental inquiry. It seems to me, therefore, to be deserving of a welcome. The first welcome we can give it is by discussing it, but the best tribute we can pay it in the end is to test its expectations experimentally.

I agree with Dr Smythies that the first thing to get out of our minds is what he calls the 'transparent nervous system' theory. The name is new, but the inadequacies of the theory have been obvious to experimental psychologists for many years now. I have criticised it in my own lectures under the name of the 'transmission theory', and it is very much what Köhler means by the 'constancy hypothesis'. For this reason, experimental psychologists have given up the use of the term 'sensation' as the name for an element of experience, and they talk rather of 'sensory cues' implying that what happens to a sense-organ is not transmitted as such to consciousness but acts as a signal or cue to set off an appropriate perceptual response.

I am not sure that Dr Smythies is always just in regarding the passages he has quoted from Einstein and others as necessarily implying the TNS theory. I think they could be otherwise interpreted. Whether it is so or not, however, the TNS theory is taken for granted by most physicists and old-fashioned physiologists and is probably part of the unexamined structure of what is taken for granted in the mind of the man in the street. And it

must be got rid of if we are to begin to talk sense about the perception of the world around us.

The main novel point of Dr Smythies's theory is, as I understand it, that one can represent the relationship between physical space, time, and psychical space, as a seven-dimensional system with one dimension for time and three each for the two space systems. I am not sure whether he would accept my phrase 'one can represent'; it is intended to imply that this is a way of looking at these things, not a fact that can be extracted from them. Furthermore, he contends that if we use this conceptual system many of the difficulties both of paranormal psychology and of the 'mind-body problem' of normal psychology disappear. This is an idea which seems to promise well as a guide to experimental inquiry. Its test as a fruitful hypothesis must be whether it proves possible by its means to pose questions sufficiently precisely to make them verifiable by the method of experimental test.

### III

BY C. W. K. MUNDLE

I SHALL discuss two features of Dr Smythies's stimulating paper 'The Extension of Mind', the first being the theory of perception which he adopts as his starting point. This is what philosophers call 'the representative theory of perception', and it is nowadays widely regarded by them as untenable. Dr Smythies dismisses as a fallacy the common-sense view, which is usually labelled by philosophers 'Naïve Realism' or 'Direct Realism', and is rechristened by Dr Smythies 'the TNS (transparent nervous system) fallacy'. Regarding the common-sense view—'thinking we are looking at the physical world directly'—Dr Smythies says 'we are no longer allowed to do this by neurophysiology' (p. 485). I suggest, however, that he is mistaken in thinking that the discoveries of physiologists have demonstrated the falsity of the common-sense view. He seems to take for granted a certain theory about the function of such trains of events as light-waves impinging on a retina, followed by processes in the optical nerve and the brain. He assumes that their function must be to create private mind-dependent entities—'objects in perceptual space' (or 'sense-fields', to use a philosophers' term). But their function may, for all the physiologists can show, be quite different; it may be selective rather than creative, i.e. their function may be to direct one's attention to this rather than that part or feature of the public physical world, to put us, so to speak, in 'direct contact' with objects which are not mind-dependent. The selective

interpretation is certainly not without its difficulties (see, for example, Professor Broad's discussion in *Scientific Thought*, p. 523 *et seq*). But Dr Smythies does not seem to be fully aware of the formidable difficulties of the representative theory. The basic difficulty is that if we maintain, like Dr Smythies, that we can only observe 'objects in perceptual space' (or private sense-fields) how do we, how could we, verify the existence of physical objects in physical space. Sometimes Dr Smythies seems to be acknowledging this difficulty, as when he says 'Physical space and physical objects are in fact hypotheses, as we can never observe them directly' (p. 485). But frequently he speaks, as he is not entitled to, as if we *know* facts about physical space. He assumes, for example, that we know that the observed relations within sense-fields are faithful reproductions of their physical causes (see pp. 486-8).

I have been making what is a stock criticism of the representative theory of perception. Locke's version of this theory was criticised on such lines by Berkeley, who realized that if Locke were right in holding that one is only acquainted with mind-dependent entities, then it is an arbitrary and unnecessary hypothesis to assume physical objects as the causes of our sensations. It is simpler and more intelligible, Berkeley concluded, to assume that all our sensations are produced directly by God.

I am certainly not suggesting, however, that Dr Smythies's theory should be rejected out of hand because it is based on the representation theory of perception. We should be obliged to reconsider the case for the representative theory *if* Dr Smythies's theory of mind proved more successful than any of its rivals in reconciling psi phenomena with what is known about normal perception. And certainly the rival theories which Dr Smythies considers (Descartes' and Ryle's) seem to contribute little to this end. Professor Ryle's approach, in making Common Sense the final court of appeal and in seeking to dispense with private mental occurrences, seems likely to resolve our problem only by inducing us to forget that psi phenomena demand some explanation.

What I want to discuss next is the way in which Dr Smythies's theory is supposed to help us to explain psi phenomena. I do not find Dr Smythies's paper very explicit on this point. I shall take it that the main puzzle about psi phenomena is that they apparently involve 'action at a distance' (i.e. causal dependence between events separated by a spatio-temporal gap, not attributable to a chain of intervening events). How does Dr Smythies's theory help us here? According to his theory both the physical world and a mind are four-dimensional, having only one dimension (time) in common. A mind is supposed to *comprise* its sense-fields (or perceptual objects), and as these are extended in three spatial

dimensions, a mind is said to be *material* (though not *physical*). The feature of Dr Smythies's theory which I find most difficult to understand concerns the relationship which is supposed to exist between

- (a) the trio <sup>1</sup> of dimensions belonging only to sense-fields, and
- (b) the trio of dimensions belonging only to the physical world.

Some of Dr Smythies's statements suggest that there are spatial relations between (a) and (b), as when he says 'the differentiation of psyche from brain should be made on the basis of *different geographical location* (p. 495; his italics); and again when he speaks of  $\Psi_\gamma$  and  $\Psi_\kappa$  as 'minute trans-dimensional *physical forces*' (p. 488; my italics). But judging by what he says on page 492, Dr Smythies wishes to deny that there are any spatial ('topological') relations between (a) and (b). (And this seems an intelligible view, for we would, I think, deny that there are any spatial relations between the space belonging to the contents of a dream and perceptual space.) On this interpretation, Dr Smythies could, I think, claim that our problem concerning action at a distance evaporates. For his theory would imply that it is meaningless to speak of a distance (a spatial or spatio-temporal gap) between any mental event and any physical event; and all cases of psi phenomena could, I think, be interpreted as involving one or more transactions between mental and physical events.

There is, however, a serious snag. For this theory would leave us with an apparently insoluble puzzle concerning *normal* phenomena. If there are no spatial relations between a person's sense-fields and his physical body, why, under normal circumstances, should a mind directly affect (and be directly affected by) one and only one segment of the physical world—the brain of the body which it 'animates'? It seems to me that, on this interpretation, Dr Smythies's theory would remove our main puzzle about the paranormal only at the cost of making the normal inexplicable. I do not know whether Dr Smythies has considered his theory from this angle. If he has, I think he was dismissing it too lightly when he said ' $\Psi_\gamma$  and  $\Psi_\kappa$  . . . are normally focussed upon the brain, but have a "penumbra" which would be responsible for ESP and PK' (p. 490).

Dr Smythies may feel that I have been wantonly ignoring what he says in his final summing-up—"The three dimensions comprising the manifold of mental position (perceptual space) are at right angles to the four dimensions of physical space-time" (p. 501). I find it very difficult, however, to see what could be meant by speaking of spatial manifolds being 'at right angles to' each other.

<sup>1</sup> Or should I say 'trios'? Does each mind have a *different* trio of space-dimensions?

This is presumably due solely to my mathematical incompetence ; but I still want to ask those who find this theory clear whether it can explain the intimate relations which exist between our minds and our bodies.

## IV

BY C. C. STEVENS

IN his opening paragraph Dr Smythies writes :

The following important fact has been established by experiment. The mind is able to abstract information from its present and future environment without the use of any of the recognized channels of sense. Unfortunately for the argument advanced, no jot of evidence requires observed phenomena to be described as entailing the existence of 'mind'. If Dr Smythies is satisfied with the implications of his statement on p. 478 that 'Nothing resembling transmitting apparatus has been found in the brain', then he should also recognize that nothing resembling 'mind' has ever been found anywhere.

One can agree with much that he says, particularly (p. 500) :

(i) That the psyche is merely a noun descriptive of certain complex . . . processes in the brain. . . . We can say all that we wish to say without using the word at all.

It is remarkable that Dr Smythies makes no further reference to this theory. He quotes a second theory and expands on its deficiencies but is silent about this one. Such behaviour bears all the marks of an unconscious assumption. Like so many other psychical researchers and innumerable ancestors and contemporaries, Dr Smythies has assumed, unconsciously and therefore involuntarily, there must be a 'mind' around the place somewhere. The presence of the term in the language he learned in his cradle is sufficient to inculcate the assumption surreptitiously, and its adoption is confirmed and made a fixed habit by subsequent conversation with others also in its grip. Once the existence of 'mind' is postulated, then the difficulties and confusions listed so exhaustively by Dr Smythies necessarily follow. On that postulate, of course 'mind' must have some extension or 'whereness' ; of course, it may be distinguished from brain 'on the basis of different geographical location' ; of course, it is reasonable to regard the brain then as 'but a station on the way to the soul'. If you assume, without knowing you are doing so, that every game includes scoring runs, your interpretation of a tennis tournament will be confused, to put it mildly. As long as you continue to be unaware of your assumption and of its effects on your interpreta-



tion of every aspect you observe in the match, so long will you automatically think tennis-players should see the folly of their ways and that it was time the rules were altered. Dr Smythies's remarks about the Crisis in Knowledge and the Crisis in Physics follow from his assumption, but not from anything else.

Though Dr Smythies is in excellent company, in my view the situation is the more deplorable. He writes (p. 500):

As this theory [No. (ii)] has been in the field for as long as history has been recorded, there does not seem to be much hope. . . .

I regard this remark as apposite to all theories erected on a postulated, and hitherto undetected, 'mind'. It is not generally recognized that the theory (i) quoted above represents, not merely a relation between 'mind' (=psyche) and brain, but a new epistemology, a new recognition of the relations between three distinguishable states, viz. (a) the world around us, (b) our experience of it, and (c) what we say about our experience. In the traditional epistemology these three have been either (1) identified, e.g. 'I'm telling you exactly what happened', no distinction being made, or (2) regarded as in one-to-one correspondence with each other, e.g. Out there is a pebble<sub>a</sub>, inside my skin is my cognition<sub>b</sub> of the pebble<sub>a</sub> ( $a : b = 1 : 1$ ) and I can say [That is a pebble]<sub>c</sub>, my remark<sub>c</sub> being apparently related one-to-one to my experience<sub>b</sub> ( $a : b : c = 1 : 1$ ). The slightest acquaintance with nervous system function renders this view untenable and in its place is required a new epistemology postulating, not one-to-one, but many-to-one relations between *a*, *b* and *c*. When this was adopted, whether consciously or unconsciously, new theories became possible, in physics, biology, communication, neuropsychiatry, etc., but such new theories admittedly cannot be made sense of, or understood, by anyone still employing—as a matter of unconscious habit—the ancient epistemology we have inherited from Neanderthal Man.

There is no reason why the new epistemology should not also be used to formulate new and more effective theories of psi phenomena, provided that psychical researchers will train themselves in the novel and largely unfamiliar technique.

## V

BY ANTONY G. N. FLEW

## A

'THE mind is able to abstract information from its present and future environment without the use of any of the recognized channels of sense' (p. 477). Rightly Stevens<sup>1</sup> picks out this sen-

<sup>1</sup> Mr Stevens sent a copy of his comments to Mr Flew.—ED.

tence as crucial. It is supposed to express a 'fact . . . established by experiment' (p. 477). And indeed so it does : but only provided that it is interpreted as equivalent to the statement that some people have ESP capacity. But if it is to be interpreted (and this must surely be how Smythies means it : or else why does he think that psychical research is relevant to his theory?) in a different way, as stating that 'the psyche . . . a potentially independent agent . . . in control of the brain' (p. 500) picks up this information it most certainly does not express a fact, experimentally established. Indeed it is hard to see how this fact—if fact it be—ever could be established, if psyches—those putative so long elusive objects—are really to be found only (as Smythies suggests) 'on the other side of the most impenetrable of all barriers, a dimensional interface' (p. 500).

Clearly something is going wrong : and it is—as Stevens claims—something quite fundamental to the entire theory. I suggest that the trouble arises mainly from two mistakes : *first*, Smythies misconstrues the word 'mind' and its associates ; *second*, he confuses enquiries about the meanings of words with investigations of the facts about things.

There are in the English language (and indeed in innumerable other languages) many picturesque and suggestive 'mind'-idioms. We speak of Miss Helen Keller's achievement as a triumph of mind over matter, of Sir Stafford Cripps as having an absolutely first-class mind, and of the perfect co-ordination of body and mind displayed by a ballerina. Such idioms suggest that the word 'mind' refers to an object, as does the word 'brain'. Though of course to an unfindable object; whose elusiveness has to be excused by explaining that it is invisible and immaterial, or concealed behind a 'dimensional interface'. But this is a mistake. 'Mind' is not that sort of word : it is much more like the word 'temper' which—*pace* the Red Queen—does not denote an object at all. The meaning of 'mind' expressions can be rendered adequately into talk of the things which people can do and feel and understand, talk which suggests no elusive objects. To say that a man has a third-class mind is not to say that he possesses some clandestine organ, unhappily rather defective ; but to say that his academic ability is very slight. Disputes about such assertions are settled—if they are settled at all—by examining his capacities ; and not by investigating his problematical shadow anatomy. I will not labour the point here ; for I have recently developed it elsewhere<sup>1</sup> against Rhine ; and in any case Smythies also knows his Ryle (cf. pp. 494 ff.).

<sup>1</sup> 'Minds and Mystifications' printed in the *Listener*, 27 September 1951.

Nevertheless it does seem that he fails fully to appreciate the force and relevance of Rylean theses. For instance, he suggests that Ryle's proceedings are arbitrary: writing of his 'defining mind to mean a verbal cloak to cover such processes as thinking, perceiving, feeling, etc.'; and conceding 'It is, of course, meaningless to discuss whether the "mind" is extended or not if by mind we mean what Ryle means by the word' (p. 493: his inverted commas; and lack of them). He also assumes (and for this Ryle's mode of expression<sup>1</sup> is probably to blame) that Ryle is just appealing to commonsense. But this is wrong. Ryle is not just being arbitrary here: for he is using the word 'mind' in the accepted way. Nor is he here appealing primarily to commonsense (which indeed is notoriously 'a most misleading guide' (p. 494) about the facts of nature). He is appealing—and this is a very different matter—to common linguistic usage, as the standard of correctness in speech. Neither is this trivial or irrelevant to the present theory. For it is surely only by unwittingly slipping to and fro between the ordinary and correct interpretation of 'mind'-expressions (which he thinks of as an arbitrary invention of Ryle's) and his peculiar and mistaken one (which he presumably takes to be correct) that Smythies is able to persuade himself that the ESP phenomena prove, or even tend to prove, that psyches, conceived as potentially independent agents in control of the brain, pick up information, or even exist at all. To put it crudely: Smythies interprets the sentence quoted in our first paragraph in one way—the correct way—when he is establishing that it expresses an experimental fact; but in a wholly different—and mistaken—way when he goes on, assured that it expresses a fact, to erect his theoretical structure upon the alleged fact which it is now supposed, wrongly, to state.<sup>2</sup> It is unkind, but irresistibly apposite, to quote his own warning that 'Confusion is caused by the use of the words . . . "mind" and "psyche". These words have several different meanings, and the confusion arises when deductions are drawn . . . when one of the other meanings of the words is really intended' (p. 479).

We turn now to the second sort of mistake, the confusion of problems of meaning with problems of fact. Smythies notices

<sup>1</sup> In *The Concept of Mind* he often makes his points as if his business were with the non-linguistic world rather than, as it is, with words or concepts. His paper 'Systematically Misleading Expressions', reprinted in my anthology *Logic and Language* (Oxford, Blackwell, 1951) gives a better idea of his method, and would be suggestive here.

<sup>2</sup> This may also suggest why Rhine now seems unable to make up his mind—in spite of his earlier certainty—whether parapsychology has as yet experimentally established the existence of any sort of soul (cf. *Telepathy and Human Personality*, *passim*).

two theories of 'the possible nature of the psyche' (p. 500). But the first is about the meaning of the word 'psyche': while the second is a speculation about the nature of psyches, the things which the word 'psyche' is thought by some to denote. He thus bundles together two radically different sorts of question. This is all too easily done: for unfortunately both can be asked in the same form of words 'What is (the nature of) (an or the) X?'; which may express a question about the meaning of the word 'X'; or about the nature of the things, X's, which the word denotes. But it is essential to distinguish these two sorts of question: which is perhaps best done by putting inverted commas round the word when it is a word that is being mentioned (hence my petulant parenthesis about Smythies's faulty inverted comma-ing). When this distinction is made, it becomes clear first that questions about the nature of the objects, psyches, can only arise if there are such objects and, second, that the existence of these putative objects is neither guaranteed by our ordinary significant use of 'mind'-idioms nor yet established by the extraordinary discoveries of parapsychology. Once it has been shown that neither of these gives grounds for believing any such entities to exist, it is difficult to justify further speculation about their possible nature and location (cf., e.g., p. 500 at foot).

Another but much more subtle and complicated example of the same sort of confusion can be found in the attack on 'the common-sense fallacy of the TNS' (p. 494): which consists in supposing that 'the familiar world of perception is . . . a *direct* view of the physical world' (p. 480); and ignoring that we are all wholly dependent for our knowledge of things on 'an extensive signalling mechanism' (p. 481), the nervous system. This cannot be relied on to be perfect, perfectly transparent as it were. It may be mechanically defective: and its operations can at best permit us to put forward the very existence of the world only as an hypothesis. Smythies, following a great tradition, but adding all modern technological sophistications, pictures our human cognitive predicament on the analogy of 'the controller in the parent aerodrome and a radio-controlled pilotless aircraft . . . fitted with television cameras and other instruments. . . He observes all that is taking place on the television screen, and attends to the other information signalled back. . . he cannot see, and has never seen, anything except the screen before him' (pp. 491-2). Many if not most philosophers nowadays—Ryle certainly—reject totally all such epistemological models. This rejection really is total: they do not accept such models and then argue or assume that, fortunately, the nervous system is transparent. Certainly the mechanism is essential: perception could never occur without it.

Certainly it may go wrong: and even when it is working with its usual near-perfection it is still possible for us to make perceptual mistakes (being, for example, deceived by optical illusions or eidetic imagery). But neither of these facts has the slightest tendency to show that we never really see anything (*directly*). We do not look out on the world *through* this mechanism: the mechanism is *part* of us. To explain why so many philosophers now reject as fantasies all such ghost-in-the-machine models would take us far too long.<sup>1</sup> But it must at least be pointed out that this rejection is not based either on invincible ignorance of discoveries in neurophysiology or on a mere appeal to the notoriously fallible pronouncements of commonsense. It rests rather on a constant more or less explicit appeal to common and correct linguistic usage. It is, for example, simply not correct to describe what we should normally call a case of being able to see a tank (when, of course, the suggestion that it is part of an insubstantial pageant or is only an hallucination has been eliminated) as if it was not really a case of being able to see a tank (*directly*): no one in such a situation, if he modestly claims to be able to see a tank, is making any reckless inferences to the forever unobservable; for this is precisely the sort of thing which is *meant* by the expression, 'I can see a tank'. Neurophysiologists have much to teach us about the mechanism without which perception could not happen: but they do not and cannot prove that (*direct*) perception does not occur at all. (In the only sense, that is, which has so far been given to '(*direct*) perception'—i.e. the everyday sense. If—as Smythies maintains—(*direct*) perception does not occur, what is it that he *means* by 'perceiving *directly*'?)

This treatment has necessarily been crudely skeletal and inadequate, presenting just two major objections in harsh and garish outline: but to deal fairly and fully with all the philosophical issues raised would need several entire numbers of the *Journal*.

## B

Yet even if we have been right in arguing that this paper is radically misguided, much might be excused if it nevertheless held out prospects of heuristic fertility. But I do not think that it is even intended to offer that sort of theory: the Summary suggests that it is not, that its 'aim . . . is to examine some . . . funda-

<sup>1</sup> But it is perhaps worth mentioning some important papers in contemporary Anglo-Saxon epistemology: in my *Logic and Language*, chapters V (G. A. Paul) and VI (M. Macdonald), and in P. A. Schilpp's *The Philosophy of G. E. Moore* the papers by J. Wisdom and N. Malcolm are both reasonably accessible and extremely relevant. Papers by N. Malcolm in the *American Philosophical Review* are particularly apt to the present physiological variant of this fantasy.

mental assumptions, and to suggest a series of alternative assumptions which give a more coherent account of the universe and the place of conscious mind in it' (p. 477). This enterprise has, I think, been unsuccessful. And partly, surely, because it is so premature. It seems to me—as I have argued elsewhere—that what we need at present in our subject is primarily and above all more experiments and ideas for experiments. We must build up a solid body of experimental fact: both by confirming through repetition the work already done; and by establishing new facts and correlations. Then we shall surely find, as Dr Thouless recently suggested, that 'Either the essential step in thought will have become easy or at any rate the road will have become easy for a future Einstein of parapsychology. Let us, then, do more experiments' (this *Journal*, Vol. 35, No. 657, p. 210).

### C

In two recent broadcasts<sup>1</sup> I poured scorn on philosophical sensationalism in parapsychology and argued that it was a mistake to think, with Rhine, that it had metaphysical implications. I should like to take this chance of conceding that, while the errors I tried to expose are indeed errors, the results of parapsychology do have some implications which might be called metaphysical, and that those who consider them philosophically sensational are to that extent right. *First*, ESP while it may not formally contradict any established laws or theories in natural science, does certainly conflict with something more fundamental even than the most basic of these. For it is incompatible with some of the fundamental beliefs—'absolute presuppositions'<sup>2</sup> if you like—still almost universally shared even by contemporary scientists, about the sort of thing which can happen and the sort of law which can apply. These scientific beliefs can be modified by reason and experience, and it is absolutely necessary to have some such beliefs to guide research: so 'prejudices' is not the word for them. Being concerned with the 'ultimate nature of reality' they might well be dubbed 'metaphysical assumptions'. The great Helmholtz was expressing one such when he declared, 'Neither the testimony of all the Fellows of the Royal Society, nor even the evidence of my own senses could lead me to believe in the transmission of thoughts from one person to another, independently of the recognized channels of sensation.'<sup>3</sup> This basic belief now has

<sup>1</sup> Printed in the *Listener* for 27 September 1951 and 4 October 1951.

<sup>2</sup> Cf. R. G. Collingwood, *An Essay on Metaphysics*.

<sup>3</sup> Quoted by Professor Michael Polanyi in his 'Scientific Beliefs' in *Ethics* for October 1950. Though he would probably still not approve this paragraph, I owe anything of value in it to his writings, his lectures, and to discussions with him.

to be scrapped; because ESP does occur. *Second*, as indeed I suggested in my second talk, the scientific handling of ESP phenomena may well call for some revision of, or innovation among, the basic explanatory concepts. We may, for instance, here have to use a new concept of 'cause', perhaps one revised along the lines suggested by Mundle (*Proceedings of the Aristotelian Society*, Supp. Vol. XXIV, pp. 222 ff.). These two facts might well be said to constitute metaphysical implications of ESP, and even to be philosophically sensational.

## VI

BY C. T. K. CHARI

THE paper by Dr Smythies in the *Journal* for September-October 1951 merits the most careful attention not only of physicists, biologists, and psychologists, but also of philosophers. I am afraid that philosophical opinion concerning psychical research still lags behind the most informed British opinion. Dr T. M. P. Mahadevan, Reader in Indian Philosophy at the University of Madras, told some of us at a meeting that, during a recent tour of the United States, he happened to mention Dr Rhine's work and the S.P.R. in private conversation with some prominent philosophers and evoked only 'peals of laughter'. I imagine that most of these philosophers lack the candour of Dr Archie Bahm of the University of New Mexico who, in a letter to me dated January 27, 1951, said: '... I am still distrustful of "parapsychology"—partly because I am not sufficiently familiar with its findings. . . .' After sketching a tentative theory of time, he added: 'It also leaves open the possibilities involved in parapsychology'. I have been busying myself for some time with a topological generalization of the theories propounded by Dunne and Saltmarsh. A crude outline of my hypothesis may appear in *Mind*. I should like to say something here about the difficulties I have encountered in 'spatial' models of the universe designed to accommodate psi phenomena.

(1) Dr Smythies urges that his hypothesis offers a 'credible' explanation of 'normal' and 'paranormal' facts (p. 500). Ought we to accept 'credibility' or 'probability' as a criterion for evaluating the *theories* of psychical research? *If* we assume that the uniformities established by orthodox physics, biology, and neurophysiology have a 'high probability'—just what the statement means on the theories proposed by Kolmogorov, Doob, Feller, Neyman, Cramér, von Mises, Reichenbach, Carnap, and Jeffreys is another question—*no* theory of psi phenomena sets out with

any 'high probability'. It has been conceded by the best authorities on the subject that it demands a very drastic reconstruction of our system of scientific expectations. But to be conceivable at all, the new system of postulates that we erect must bear some analogy to the existing framework. And if it does, will it go far enough to be really serviceable in psychical research? How formidable is the task of reconstruction may be judged with reference to the 'basic limiting principles' that Professor Broad has formulated.<sup>1</sup> One of them is that 'it is self-evidently impossible that an event should begin to have any effects before it has happened'.<sup>2</sup> I maintain that any attempt to modify or reconstruct (and precognition leaves us no option but to modify or reconstruct) a principle like this necessitates a collateral epistemological inquiry into the 'limits of conceivability'. So far as I am aware, only Mr Tyrrell has paid serious attention to the problem. The perspectives that he has unfolded in his *Homo Faber*<sup>3</sup> must be kept well in view in all our attempts at theory-building. I shall return to this point at the end of my letter.

(2) Dr Smythies makes a great deal of what he calls the 'TNS' fallacy. I quite realize that to anybody who has much to do with neuro-physiology Naïve Realism must appear very naïve indeed. But is it necessarily fallacious? It seems to me that the hypothesis proposed by Dr Smythies involves us in some at least of the difficulties of the older epistemological dualism. How can we know that 'events in our perceptual world follow faithfully the events in the physical world' when the latter, *ex hypothesi*, are never directly accessible? The analogy of the controller in the aerodrome and the radio-controlled aircraft (p. 491) gives us no help here. Perhaps Dr Smythies would like to appeal to the 'isomorphism' of Gestalt psychology. But does it eliminate the epistemological difficulty? I am not at all sure that we can afford to ignore the ingenious reconstruction of Direct Realism attempted by Captain M. M. Moncrieff in his book *The Clairvoyant Theory of Perception*.<sup>4</sup> Professor Price in commending the theory says: 'Perhaps we shall no longer be obliged to suppose, as so many philosophers have, that sense-data or sensa or sensation contents are the only entities we are directly aware of in perception. . . . It is curious that an important and influential school of contemporary philosophers has arrived at a very similar result in an entirely different way, by using the methods of linguistic analysis developed by Dr Wittgenstein'.<sup>5</sup> I agree with Professor Price that Captain Moncrieff's rehabilitation of Direct Realism is worth while. The late

<sup>1</sup> *Philosophy*, 1949, xxiv, 291-309.

<sup>3</sup> London, Methuen, 1951.

<sup>5</sup> *Ibid.*, Foreword, p. 8.

<sup>2</sup> *Ibid.*, p. 293.

<sup>4</sup> London, Faber, 1951.



Whately Carington, as is well known, argued that all material bodies could be resolved without any remainder into fundamental particulars which he christened 'cognita'. I believe that a theory could be worked out along these lines without accepting the kind of neutral monism or logical positivism to which Carington so wholeheartedly committed himself in his later years. We have a wide range of possibilities to explore, even the seeming metaphysical lunacies. It is noteworthy that J. C. F. Zöllner,<sup>1</sup> who was probably the first distinguished psychical researcher to adopt the working hypothesis of a multi-dimensional space, held that it was quite compatible with an extreme Mentalism or Berkeleism. He conjectured that the 'representation of our whole *real corporeal world*' (italics not mine) may have a status not dissimilar to that of the 'hallucinations' evoked in the hypnotic and post-hypnotic states. The 'parable' that Mr Tyrrell developed in his *Grades of Significance*<sup>2</sup> may be far more significant than many of us imagine.

(3) Dr Smythies says (p. 502) that his hypothesis 'provides the organization seemingly required by the activity of the "censor" as described by Soal'. Possibly. But does it account for the peculiar symbolism often associated with spontaneous psi phenomena? My own extended investigation of a case involving ostensible paranormal cognition and ostensible para-physical phenomena (telekinesis and apports) suggested symbolism of the weirdest kind. I found strong indications that the 'spirit communicators' who claimed to be responsible for the phenomena were compound entities formed on the same lines as the 'dream figures' of Freudian and Jungian psychology. My observations lend support to the hypothesis advanced by Dr John Layard.<sup>3</sup> The fact that the cases cited by him came out poorly when judged by stringent evidential standards does not rob them of their very great theoretical value.<sup>4</sup> My evidence (of better quality and obtained in an intimate circle) argues for those queer creatures having 'one foot in the world of reality and the other in a dream . . . worthy of the maddest idealism' to which F. C. S. Schiller referred once.<sup>5</sup> While I quite approve of 'psi' as a non-committal omnibus term for 'paranormal' phenomena, I am by no means convinced that it has a great theoretical advantage over Myers's 'Subliminal' hypothesis. I think it is as likely as not that a reformulation of his hypothesis, perhaps in the language of a non-metricized dynamics of the kind sketched by Kurt Lewin and his followers, will suggest fresh lines

<sup>1</sup> *Transcendental Physics* (Eng. tr. with preface and appendices by C. C. Massey, 3rd ed., Boston, Colby & Rich, 1884, pp. 150-3).

<sup>2</sup> 2nd ed., London, Rider.

<sup>3</sup> *Proc. S.P.R.*, 1944, xlvii, 237-47.

<sup>4</sup> See Dr Wiesner's remarks, *Proc. S.P.R.*, 1945, xlvii, 270-1.

<sup>5</sup> *Proc. S.P.R.*, 1922, xxxii, 145-6.

of experimental inquiry. We may say that a 'boundary' is something in crossing which the phenomena change their character. Psychical research and modern 'depth psychology' suggest, at least as a first generalization, that the empirical ego is a 'boundary' in a complex 'psychological field'. We might adopt a term like 'praeter-conscious' as a generic term for all deep-lying or not ordinarily introspectable mental processes. As we move away from the empirical ego into the praeter-conscious, the laws of orthodox scientific psychology become less and less relevant. By a cautious interpolation and extrapolation we can posit praeter-conscious processes starting with the behaviour data of experimental psychical research. And 'spatialization' of a certain kind is not precluded.

(4) As anybody who cares to read my article 'Time as Minkowski's Fourth Dimension' appearing in the 1952 Annual (January) Number of the Indian *Astrological Magazine*<sup>1</sup> will see, I am not a little suspicious of the attempts made by Dunne, Ouspensky, G. C. Barnard, C. A. Richardson, and others to extract from Relativity suggestions for a theory of precognition. I demur to the sort of view Dr Smythies seems to imply when he says (p. 484) that 'Einstein supposed there to be a four-dimensional space-time continuum containing observers travelling through it'. I do not see that this view is entailed by the mathematics of Relativity, especially if we accept the interpretations put forward by A. A. Robb and E. A. Milne.<sup>2</sup> Even in psychical research, may not *time-relations* be more fundamental than *space-relations* and the latter a manifestation of the former? I am trying to speculate on the possibility.

(5) I turn to a fundamental issue. How far will the 'geometrization' of time take us? Following Professor Broad,<sup>3</sup> I shall speak of the extensive, the relational, and the transitory characteristics of time. All attempts to represent time as geometrical extension and 'passage' as a 'travelling' through it entail a *second* series exhibiting the irreversible relation of *temporal* before-after and the transitory characteristics 'past', 'present', and 'future'. A three-dimensional psyche 'travelling' along the fourth dimension of a

<sup>1</sup> Edited and published by Dr B. V. Raman, Bangalore 3, South India; available at John M. Watkins, 21 Cecil Court, Charing Cross Road, London, and the International News Co., 131 Varick Street, New York City. Notwithstanding his 'occult' interests, Dr Raman has been recently devoting some space to the scientific and philosophical issues of psychical research. See my article 'Fathoming the Deeps of Mind' in the 1951 Annual Number of the Magazine.

<sup>2</sup> My article gives references and quotes Relativists *in extenso*.

<sup>3</sup> *Examination of McTaggart's Philosophy* (Cambridge, 1938), Vol. II, Part I, Ch. xxxv.

physical space-time will generate relational and transitory characteristics which cannot be assimilated to any or all of the seven *spatial* dimensions postulated by Dr Smythies. This does not mean, of course, that the 'spatialization' of time is worthless in speculation on psychical research. But it does mean that the topology of manifolds alone will not carry us far without *qualitatively new ideas about 'transitoriness' or 'passage'*. I believe that Professor Broad, at the Joint Session of the Aristotelian Society and the Mind Association in 1937,<sup>1</sup> hinted at the need for such notions when he made a distinction between the extra dimensions of *space* postulated by Hinton and Dunne and extra dimensions of *time*. There may be an unfamiliar time in which the distinction between a 'past' and a 'present' is irrelevant but not the distinction between a 'present' and a 'future'.

There is unquestionably an urgent need for theorizing of the rigorous sort in psychical research. But 'rigour', in this domain, may consist largely in discovering the 'limits of assertability'. The muddles about 'survival', for instance, may be just muddles about its 'assertability' in any ordinary language. My approach invites comparison with Mr Tyrrell's. Dr Smythies does not deny, I think, that there may be intrinsic limitations in all speculation on psychical research. He speaks of a psychic mechanism of 'a fabulous complexity, and in part employing principles of which we know nothing' (p. 491). And he admits (p. 500) that 'The nature of the Self remains inexplicable'.

#### REPLY BY DR SMYTHIES

I WILL try and answer Professor Price's comments first. His first point (p. 538) centres round, I think, my use of the word *hypothesis* which I used throughout my paper in a modified sense of the rather special way in which Dingle employs it. Dingle uses it to mean a pseudo-atom or pseudo-molecule, and gives as a familiar example the planet Neptune between the time that its presence was hypothecated to account for the irregularities in the motion of Uranus and the time it was first seen. He regards hypotheses as being 'potential experiences though they are not actual memories or (in general) representatives. These are known as *hypotheses*. . . . These pseudo-atoms may have all the definiteness and locability of actual experiences, but they are neither memories or representatives'.<sup>2</sup> Thus it would be *logically* possible to observe events in the physical world directly *if* our witnessing Selves could

<sup>1</sup> *Proc. Arist. Soc.*, Supp. Vol. xvi (London, Harrison, 1937), p. 203.

<sup>2</sup> Herbert Dingle, *Through Science to Philosophy* (London, Oxford University Press, 1937), p. 165.

leave their place inside the head of the perceived body, cross the dimensional interface, and then observe in physical space, *if* Selves could then react directly to light. Until someone does this they remain strictly hypotheses (but not 'mere' hypotheses) from which the witnesses are individually cut off by material and spatial barriers, but which we observe indirectly by means of signalling mechanisms.

We might extend Dingle's definition of *hypotheses* thus :

(i) To include the class of all physical objects which are only indirectly observable *via* signalling mechanisms. A sub-set of this class comprises the set of hypotheses in Dingle's present sense. These have not yet been indirectly observed, but their presence is inferred to account for the indirectly observed behaviour of other objects, and it is supposed that it will be possible to observe them indirectly at some future date and under certain special conditions.

(ii) To include the class of all objects which (it seems probable) will never be even indirectly observable in the nature of things, but whose presence we infer to account for the correlation between directly observed events (i.e. sense-data) and brain events. This class includes the intermediate parts of those psychological mechanisms connecting the field of direct observation<sup>1</sup> with the brain, the events at the proximal surface of which mechanisms we inspect or observe directly as sense-data.

We might term these hypotheses of the first, second, and third degree (or term) respectively. Third degree hypotheses border on postulates in Dingle's sense.

Taking Professor Price's next point (p. 538) I should have said in this passage that the signalling mechanisms of the nervous system and psychological system are between the observer and the *outside* physical world. The afferent function of the nervous system may be to fashion a mnemically organized model of the internal and external environment to form the target for  $\Psi\gamma$ .

I must apologize for my error, which Professor Price notes next, in locating the psychological mechanisms in *perceptual* space. Only the proximal surface of any such mechanisms, which actually present sense-data, can be located in perceptual space. The rest of the individual observer's own mechanism would then be hidden from his own direct observation, just as the inside of a television set is obscured by the screen. The whole of other people's psychological mechanisms would then be outside his range of

<sup>1</sup> I take perceptual space to be the spatial sense-field open to inspection or direct observation. It is the space system in which sense-data are extended. Would a better term for this system be *inspectual* space, since it is the space system in which inspection (in Broad's sense) takes place? If so, what I have called the perceived body would be known as the *inspected* body.

indirect observation hidden behind a dimensional interface, as his own mechanisms would be hidden from their sight. Perceptual space is strictly limited to the confines of the perceived body and the visual screen demonstrated by after-images and the stroboscopic patterns, and to the limits of mental imagery and the 'thereness' of sounds. The rest of visual space may be illusory. The unobservable volume of space in which perceptual space is embedded might be called *psychical* space. These changes are summarized in the revised postulates of the theory given on pp. 568-9 and answer to some extent, I hope, some of the difficulties that Professor Price has raised. The theory might also be regarded as an extension of Henry More's doctrine of 'essential spissitude'.

With regard to the point that Professor Price makes on page 540 (line 21), I did not mean to imply that the common physical world is not observed at all, but that it is observed indirectly by means of a signalling mechanism. Visual space, or rather that part which is not illusory, and tactual space, which is the space occupied by the perceived body, are different locations in perceptual space. When I suggested that  $\Psi_\gamma$  and  $\Psi_\kappa$  are physical forces, I meant that they may prove detectable by instruments of suitable design. 'Trans-dimensional' forces would, I agree, be a better term.

One reason for my 'surprising' version of Descartes' philosophy, which Professor Price mentions, and for the possible distortions of the implications of relativity theory, to which Dr Thouless and Mr Chari draw attention, is that I have learned about these systems of knowledge through their interpreters. This has been regrettably necessary owing to the size of the field I have tried to cover and to the mathematical difficulty of relativity theory. For Descartes I have relied largely on Burtt, and for relativity on Eddington and Jeans. If perceptual space is regarded as being a direct cross-section of physical space-time, as Jeans seems to suggest is implied in relativity,<sup>1</sup> is that not the TNS theory?

I have been in touch with Mr Warner Allen since the publication of my paper and I now see that I was mistaken in attributing to him the TNS fallacy. James's 'Chamber of Consciousness' should, however, I think, be attributed with spatial extension. It seems to have more than the merely temporal extension with which Mr Warner Allen attributes it in his works. I must apologize for my misinterpretation of his views.

To pass on the main epistemological difficulties that have been raised. These may be summarized as follows:

<sup>1</sup> Sir James Jeans, *Physics and Philosophy* (Cambridge University Press, 1942), pp. 63-9.

(i) How is it possible to gain accurate information about one series of events (i.e. physical events) by observing a second series of events (i.e. experiential events or sense-data) occurring at one end of a signalling mechanism the other end of which is connected to the second series of events, the whole arranged in such a way that the first set may be said to represent the second, when it is impossible ever to observe the second set directly?

(ii) How can it be possible to know that the first set is giving an accurate representation of the second set and indeed that the second set exists at all?

Such a fundamental question as this can hardly be dealt with adequately in the space available, so I can only attempt a brief defence of the representative theory. I would answer the first objection by saying that perceptual situations of this nature, though at a lower level, play an increasing part in modern life. When I perceive a fluctuating series of grey and white dots on my television screen I do not limit myself to an account of the electronic behaviour of my set. I assume that I am perceiving, for instance, a game of cricket at the Oval, and thus am gaining a lot of information and knowledge that I would not otherwise have obtained. It is then only necessary to postulate that my sense-data, as I inspect them (in Broad's sense), representing the screen of my set and the events occurring there, bear the same type of relation to the actual screen and its actual events as these latter in turn do to the actual cricket match at the Oval. In the latter case the signalling mechanism is formed by light, television camera, transmitter, and receiver: in the former case it is formed by light, eye, optic nerve, brain, and I would add  $\Psi\gamma$  and psychical mechanism. This analogy with television is in fact more than an analogy, as recent work in neurophysiology supports the view that the actual mechanisms concerned in visual perception function along lines very similar to television. As Grey Walter<sup>1</sup> has said, 'the televisual system behaves very like the neuro-visual one'.

It may be objected that this postulated relation between sense-data and physical events cannot be checked directly as can the relation between the television set and the cricket match. This is, in fact, the second objection given above. There may be various ways of replying to it. In the first place, we do not always know if anything does correspond in the physical world to certain experiences (for instance, some kinds of visual hallucination induced in normal subjects under hypnosis) if we are restrained from

<sup>1</sup> 'Features in the Electro-physiology of Mental Mechanisms' in D. Richter, ed., *Perspectives in Neuropsychiatry* (London, Lewis, 1950), pp. 67-78; see also C. T. Morgan and E. Stellar, *Physiological Psychology* (New York, McGraw-Hill, 1950), pp. 593-4.

making more complex observations (looking for shadows, for example—that is, making fresh perceptions for the purpose of making observations from which inferences may be drawn) and if we are not allowed to touch the object (i.e. checking by using another sensory path). As E. A. Burtt<sup>1</sup> points out, we do not check doubtful perceptions by making fresh observations in a new manner on the physical objects themselves, but by making fresh perceptions or observations in the old manner. At any rate, we know soon enough when events in the perceptual world cease to follow the events in the physical world to any severe degree, and hallucinations and illusions develop, unless we lose insight. In the latter case our behaviour will very largely be determined by the events in perceptual space, as a study of schizophrenia shows. The basis of the madness of a schizophrenic is not that he has 'lost his reason' but that he ceases to realize that the extraordinary events he is experiencing are occurring in his own perceptual world only and consequently behaves as though they were occurring in the public physical world also. There are, of course, distortions of reasoning in schizophrenia—the thought disorder and the peculiar symbolism—but the schizophrenic remains a moderately reliable witness for the phenomena he is experiencing.<sup>2</sup> This may be better understood if one experiences oneself a short artificial schizophrenic-like psychosis such as is produced by the alkaloid mescaline. In any signalling mechanism 'false' information may be inserted at any level, but it remains information for all that. Its falseness is merely a relative quality. It can tell us nothing about the objective of the signalling mechanism, the system at which the mechanism is directed, but such interference (e.g. hallucinations) can tell us a great deal about the signalling mechanism itself and the environment of the mechanism, if this should be more extensive in any way than the system comprising the objective of the mechanism and the distal end of the mechanism itself. *All* experiences, including schizophrenic ones, must be correlated.

The mode of presentation of our picture of the external world, and certain aspects of our knowledge of it, have been built up by many years of subconscious learning along the lines described by J. Z. Young,<sup>3</sup> in which the mode of action and electronic behaviour of the mechanisms of the brain have been radically changed. Our knowledge of the external world has also been built up, in the child

<sup>1</sup> *The Metaphysical Foundations of Modern Physical Science*, 2nd ed. (London, Routledge & Kegan Paul, 1932).

<sup>2</sup> See, for example, Thomas Hennell, *The Witnesses* (London, Peter Davies, 1938).

<sup>3</sup> *Doubt and Certainty in Science* (London, Oxford University Press, 1951).

as in the race, by conscious learning and by 'logical processes of construction and hypothesis, and by inferences therefrom'.<sup>1</sup> By the time we reach adult life these two processes have established the condition where, by the former process, the sense-fields are presented in an orderly manner, and we can gain meaning from what we inspect. When these mechanisms go wrong, or where they have received no instruction, the sense-fields are presented in a disorderly and confusing manner. In cases reported by J. Z. Young<sup>2</sup> and Paul Schilder<sup>3</sup> of patients who are restored to sight following congenital blindness, the visual field is just a confused mass of colour in which no objects are perceived for several months while the electronic mechanisms transmitting the patterns of vision are *learning*. By the logical process of construction, etc., we gain the idea of a physical world which may be supposed continually to feed information into the manifold inputs of the machine, of which one set of manifold outputs *form* the perceptual world. Dobbs has dealt with this question with clarity in the article already referred to. He states that all knowledge of physical events, and thus of processes in physical objects, is inferential and never direct knowledge. The only change I would suggest in his account is that the one-to-one correspondence between experiential events and neural events is not gained though coincidence in space but by connexion by signalling mechanism. Experiential events and their antecedent neural events are geometrically entirely incongruous.

May I now answer some of Mr Mundle's points.

(i) It is not I who am responsible for the notion that Direct Realism has been rendered untenable by neurophysiology.<sup>4</sup> I have quoted the eminent authorities in this field upon whose deductions I have based my arguments—E. L. Hutton and Sir Charles Sherrington. To state that 'the objects of his study have their existence only in him', and 'accepting finite mind as having a "where" and that "where" within the brain' is surely to refute Direct Realism.<sup>5</sup>

I am not suggesting that the function of the nervous system is

<sup>1</sup> H. A. C. Dobbs, 'The Relation between the Time of Psychology and the Time of Physics', *British Journal for the Philosophy of Science*, 1951 ii, 122-41.

<sup>2</sup> *Op. cit.*, pp. 61-6.

<sup>3</sup> *Brain and Personality* (New York, International Universities Press, 1951).

<sup>4</sup> The sense in which I use this includes the findings of neurology, neuropsychiatry, and experimental psychology (parts of).

<sup>5</sup> See also Russell Brain's criticisms of Direct Realism in 'The Neurological Approach to Perception', *Philosophy*, 1946, xxi, 133-46.



either 'selective' or 'creative' but mainly transmissive (with subsidiary subconscious functions of control and communication). Grey Walter <sup>1</sup> has said, 'The basic concept which runs through the new approach to brain and mind is that the brain is an organ for handling signals.' It is perhaps an advantage for this theory that it entails the minimum change in the current theoretical position in neurophysiology, and, I think, does not clash with any facts of neurophysiology. The changes I am suggesting are mainly additive.

I do not assume that the function of the optic nerve and brain 'must be to create private mind-dependent entities'. I suggest that these latter are created by the mechanical part of the spatially independent psyche. The total psyche-brain mechanism would then be both transmissive, creative, *and* contain internal focussing devices to aid the direction of attention. The brain may be one half of Sherrington's telephone exchange. The subscribers may inhabit the other half. I can see little justification for supposing that we observe events in the physical world directly, in order to persuade ourselves that there is such a world, when all the evidence from neurology, neurophysiology, neuropsychiatry, and experimental psychology indicates, to an overwhelming degree, that we do not observe physical events in this fashion.

(ii) I have already to some extent dealt with Mr Mundle's second point. The relation between *A* and *B* is given by postulating that they form together an *n*-dimensional continuum. On page 492 of my paper I said that they may be non-coincident. That is not to say that they do not have any spatial relation to each other. They have the spatial relation of non-coincidence. The intimate relations that exist between our minds and our bodies may be accounted for by supposing that the body and the psyche (including the Self) form together a single and unified organism.

Mr Flew states firmly that it is a mistake to use the word 'mind' as though it referred to an object, as does the word 'brain', and that his opinions are based on 'common and correct linguistic usage'. I feel, however, that common linguistic usage can hardly be held to be sufficient authority for deciding fundamental issues in cosmology. No one can possibly claim to know whether events do or do not occur in higher-dimensional space and whether or not these events have anything to do with human beings. These questions can only be answered by observation and experiment. I have no objection if the phrase he finds most noxious in my article be changed to '*People* are able to abstract information, etc.' In that case, it is necessary to postulate that a person comprises a

<sup>1</sup> *Perspectives in Neuropsychiatry*, p. 69.

body and an X. The particular label you attach to X does not seem to me to be as important as Mr Flew claims; though, of course, it avoids misunderstandings if the same term is used in the same sense by everyone who uses it. X may be in part, or perhaps wholly, an organized and material entity extended in a space of a different dimensionality to that in which the body is extended. I suggest that *psyche* is a good name for it. Part of the psyche seems to divide naturally into Self and not-Self, the observer and what is observed, the thinker and what is thought.

Mr Flew admits that he possesses a mechanism by the aid of which he perceives the external world. It is, he says, part of him. Where then, in the world that he perceives, is this mechanism located? To sustain his case he must suppose that it is located inside his perceived head. If so, then it must be transparent since his observing Self is presumably inside the semi-liquid mass of his brain encased in an opaque and solid skull. If, however, he supposes that his perceived skull and body are inside the mechanism, as Hutton suggests, then he is propounding the representative theory of perception at present widely held in neurology, where perceived events *are* electron patterns in the brain. My theory is only a variant of this latter theory where a part of the mechanism is cut off from the brain by 'a great gulf fixed'. It is interesting to note that Moncrieff, in his ingenious attempt to resurrect Naïve Realism, was forced to place the point of observation in the cavity of the eye-ball behind the transparent cornea, aqueous humour, crystalline lens and vitreous humour, and in front of the opaque retina, sclera, optic nerve, etc.<sup>1</sup> It seems to me that until someone can construct a detailed theory of brain function which can account for all the determined facts of neurology and allied disciplines and can yet support Naïve Realism, the only three defensible theories of perception are those given by Russell Brain in his article in *Philosophy* to which I have already referred.

Mr Flew constructs his theories by a 'constant more or less explicit appeal to common and correct linguistic usage'. How does Mr Flew know that common linguistic usage is in fact correct? Language is merely the system of inter-observer communication and clearly cannot be used as the sole basis for an infallible theory of perception, mind, or anything else, since it will inevitably embody and reproduce any mistaken ideas that these observers may entertain, and any mistaken opinions that they, as fallible human beings, may form of their own nature. The mere argument that we all mean the same thing when we say 'I see a tank' is really not a sufficient indication that Direct Realism is a

<sup>1</sup> M. M. Moncrieff, *The Clairvoyant Theory of Perception* (London, Faber, 1951).

true theory. We would be equally in agreement if a representative theory were true, as Hutton points out in the passage I quoted from her work in my article.

Mr Flew seems to me to give words an undue importance relative to the entities that words are the labels we use to describe. His philosophical criteria could not, for example, today be used to support the Ptolemaic concept of the universe. This is because science has progressed and common linguistic usage has changed drastically in its train. Five hundred years ago, if Mr Flew's system had been applied to astronomical matters, we would have discovered the truth that the earth lay at the centre of the universe. It seems doubtful whether this system can be decisive in answering the more difficult questions of the human mind and soul.

It is generally agreed that, while the facts of parapsychology do not need any further experiments to establish them, they do need a vast number of experiments to discover more about them. But, as Plesch,<sup>1</sup> among others, has pointed out, there is need for a theory to account for them, a theory which may be tested by further experiments. Mr Flew asks in his first paragraph how objects as elusive as psyches may be detected. This may be done by detecting the postulated and necessary influences they may exert on material systems in physical space. Electrons are unobservable for another reason. They are too small. But their behaviour can be followed by using such devices as Wilson's cloud chamber. We require analogous devices for investigating psi-fields.

Mr Stevens wonders why I did not make any further reference to the theory of the psyche that he favours ( (i) on p. 500). I had already pointed out its shortcomings in the first section of my paper. I do not unconsciously assume that 'there must be a mind around the place somewhere'. I have suggested an exact formulation for this 'whereness' of the entity that might well be called 'mind', though I would agree that it is better to call this additional organ the psyche. In an article in the *Journal of Parapsychology*<sup>2</sup> Mr Stevens says, 'Those who study behaviour in relation to brain structure . . . have endeavoured to observe the activity of the whole organism in its environment . . .' (p. 130) and 'we would deal with the function of the organism as a whole' (p. 132). It is a very bold assumption to hold that the body and brain of a man is the whole organism and that the physical world is his total environment.

I should like next to deal in more detail with a most important

<sup>1</sup> P. H. Plesch, 'Psychical Research as seen by a Physical Chemist', *Jnl. S.P.R.*, 1950, xxxv, 272-83.

<sup>2</sup> 1950, xiv.

point : the relation of the perceived body to the physical body. I am suggesting that they ought to be differentiated under the determinable of location in space. I do not think that anyone would deny that the perceived body is extended in space. Russell Brain<sup>1</sup> has stated, 'In our awareness of our own bodies we are directly aware of a three-dimensional object.' The question then immediately presents itself, 'What is the necessary spatial relation between the perceived body and the actual physical body?' There are three possible replies :

(i) The perceived body is coincident in space with the physical body. (This is the almost universal view of common sense.)

(ii) The perceived body is coincident in space with the body-image in the brain of the physical body. (This is the current position in neurology and allied disciplines, though it is not usually stated in this form. It is usually left implied, so deeply rooted are our common-sense notions.)

(iii) The perceived body is made of mind-stuff and is spatially independent of any part of the physical body.

The neurological and neuropsychiatric evidence must make us very doubtful of (i). The perceived body can change its shape and size quite independently of the state of the physical body and does this in cases where the mind is quite clear, and the judgement unclouded. The best examples may be found in the phenomena of 'phantom limb' and in people under the influence of mescaline. One of our subjects, who was at the time in a state of observational integrity, experienced the sensation that his perceived body executed a series of complete turns from the waist down around its longitudinal axis. He described it as being a most alarming sensation. As he said later : 'It felt just as though I was being wrung out like a sponge. I could never have imagined the experience without having taken the drug. It was quite foreign to me.' Surely it is not possible for a body with its lower half reversed to be coincident with the physical body which of course cannot execute such movements? Mescaline interferes with the biochemical processes in the brain that support the electron patterns that delineate the body-image in the brain. It may even cause the perceived body to disappear altogether, and to be replaced by a humming feeling. All that has happened is that the electronic arrangements conducting the patterns of bodily sensation have been 'scrambled'.

In (ii) the difficulty is that the perceived body is not the same shape as what appears to be the body-image in the brain. This latter is upside down, split in half, and grossly distorted, and the parts

<sup>1</sup> *Philosophy*, 1946, xxi, 142.

rearranged.<sup>1</sup> These difficulties may be overcome by advances in physiology, but I do not think that anyone will ever be able to fit the perceived body into the brain with any degree of plausibility.

I am suggesting that the third possibility may be the true one.

As a tentative lead in the experimental investigation of this theory, I should like to suggest that the extraordinary behaviour of Ehrenhaft's dust particles<sup>2</sup> may possibly be due to the fact that they are reacting to the extracerebral 'penumbra' of psi-fields (or Eccles's *mind influences*<sup>3</sup>). Ehrenhaft's system of minute dust particles suspended in a low vacuum in a beam of concentrated sunlight would seem to possess an important property similar to that postulated by Eccles for the 'neurone net' in his development of this concept: e.g. a high degree of 'poise'. They also possess abundant free energy, as does the brain. It is interesting that Ehrenhaft's discoveries were made when he was using an instrument devised to detect minute magnetic forces. It is evident that any physicist investigating the behaviour of such a delicately balanced system, with which psi was in fact interfering, would be led to construct curious theories to account for these phenomena, if he left psi out of his range of possibilities. Ehrenhaft has in fact attempted to account for his phenomena by means of theories with which other physicists disagree violently. I visited Professor Ehrenhaft in March 1951, and am able to confirm Rabel's report from personal witness, and further to suggest that Rabel's one proffered explanation for this phenomenon (that it is due to irregularities in the shape of the particles) seems to be untenable since dusts of such widely differing shapes as graphite (flat plates) and sponge sperm dust (globules) may be observed to spin along exactly similar complex paths. Incidentally, the dust particles seem to be tracing out Lissajou figures and a mathematical analysis of these might yield interesting results.

It is also just possible that  $\Psi\gamma$  may be responsible for the movement of electrons in the brain that causes the alpha-rhythm (and perhaps other rhythms) of the Electroencephalograph, and also for Rohracher's phenomenon<sup>4</sup>—the minute, constant, and regular mechanical vibration of the human body and the earth at a frequency of about 10 cycles per second.

<sup>1</sup> See M. A. B. Brazier, *The Electrical Activity of the Nervous System* (London, Pitman, 1951), figs. 3 and 4 on pp. 5, 6.

<sup>2</sup> See article by Gabrielle Rabel, 'Matter Moved by Light', *Discovery*, 1951, xii, 151-3.

<sup>3</sup> See the most important article by Professor J. C. Eccles, F.R.S., 'Hypotheses relating to the Brain-Mind Problem', *Nature*, 1951, clxviii, 53-7, which I had not read when I wrote my paper.

<sup>4</sup> Hubert Rohracher, *Mechanische Mikroschwingungen des menschlichen Körpers* (Vienna, Urban & Schwarzenberg, 1949).

I also suggest that, if we take into account the facts of parapsychology, the most probable of the three relationships between sense-data and the nervous system that Russell Brain<sup>1</sup> considers is the second: 'A sense-datum is a psychical event unlike the neural stimulus which initiates it and thus doubly removed from the original physical stimulus.' When we change from inspection to perception (both in Broad's sense),<sup>2</sup> we should change the frame of reference we are using. 'The two frames may be so related that there is at least one axis in each set which is in that set only.'

The relation R in Broad's formula o—R—s may be one of connexion by signalling mechanism.

The two fundamental processes in the central nervous system of excitation and inhibition may also extend to the relation between the brain and the psyche. The brain may also have the additional function of exerting a continual *inhibition* over mind-stuff, which may not be a mere passive mediator of sense patterns and will but may possess the potentiality of a fierce life of its own. The natural activity of mind-stuff may be the production of images and pictures of startling beauty, intense poetic integrity, and charged with an elemental significance and meaning. It is possible that this inhibitory function of the brain may itself be inhibited by mescaline. I feel that the phenomena produced by mescaline are of absolutely fundamental philosophical importance.<sup>3</sup> It seems unlikely that visions of such overpowering beauty as mescaline produces, and the extraordinary way in which the drug causes the perceived world to become very much more beautiful, are products of a sick brain. Some mescal visions are the most beautiful things that man on this planet has ever witnessed, so many intelligent people who have experienced these things have alleged. They can only be given an adequate explanation by supposing that they represent the beauty of the soul (in Plato's phrase) gaining partial release from its domination by the brain.

## A REVISED SET OF POSTULATES (WITH NOTES)

### *The basic postulate*

I. Perceptual space is not coincident with physical space.

<sup>1</sup> *Philosophy*, 1946, xxi, 145.

<sup>2</sup> C. D. Broad, *The Mind and its Place in Nature* (London, Routledge & Kegan Paul, 1923).

<sup>3</sup> See, for instance, A. Rouhier, *Le Peyotl* (Paris, Gaston Doin, 1927) and A. J. C. Wilson, 'Ayahuasca, Peyotl, Yagé', *Proc. S.P.R.*, 1949, xlviii, 353-63.

*The subsidiary postulates*

II. Perceptual space is part of a larger space system, psychical space.

NOTE. This latter is usually unobservable for the reasons given, but events in it can determine events in both observable space systems.

III. Psychical space is filled with mind-stuff, part of which forms a signalling mechanism between the brain and the observer. The proximal *surface* of this mechanism forms the limits of direct observation.

NOTE. These limits comprise the limits of the perceived body and the visual screen as demonstrated by after-images and the stroboscopic patterns. The limits of auditory space and the space of mental imagery are included but are less definite. The perceived body may for this purpose be regarded as a complex sort of surface.

IV. Psychical space and physical space may be thought of as forming together an  $n$ -dimensional continuum, where  $n$  may be 4, 5, or 6. These endure in time. If psychical space and psychical time form a continuum, then psychical space-time and physical space-time may form an  $n$ -dimensional continuum, where  $n$  may be 5, 6, 7, or 8. Thus they may partially intersect or be non-intersecting. The movement of the total human organism through the  $n$ -dimensional space-time continuum may divide it into space dimensions and one or two time dimensions.

NOTE.

Two non-coincident and infinite straight lines form a 2-dimensional system.

Two non-coincident and infinite planes form a 3- or a 4-dimensional system.

Two non-coincident and infinite cubes form a 4-, 5-, or 6-dimensional system.

Two non-coincident and infinite hypercubes form a 5-, 6-, 7-, or 8-dimensional system.

V. We must distinguish between

- (1) *Direct observation* : the Self's direct view of experiential events or the behaviour of sense-data *made* of mind-stuff, and
- (2) *Indirect observation* : the Self's view of the physical world thus obtained.

NOTE. (1) corresponds to our view of the events on a television screen, and (2) to our view of events in the television studio thus obtained.

*Notes on general topology*

An issue fundamental to the problems of perception seems to be what events are to be regarded as taking place *inside* the human

organism and what events *outside* it. Perceptual space may be embedded in psychical space in that the two are of the same dimensionality. The boundary surface between them is formed as described in the note to postulate III. The boundary surface between psychical space and physical space is a dimensional interface. In this case the topological boundary of the total human organism is formed by the skin and the surfaces of the psychical mechanisms associated with the individual observer. Thus *inside* comprises

- (a) a portion of physical space inside the skin ;
- (b) a portion of psychical space containing the psychical mechanisms ;
- (c) the whole of perceptual space (which is limited in extent).

*Outside* comprises

- (a) the rest of physical space ;
- (b) the rest of psychical space.

Physical space is public : perceptual space is wholly private; psychical space may be public if we all share one or private if we each have one to ourselves. There may be no way of telling.

POSTSCRIPT. Since writing this reply, I have read Professor Broad's *Scientific Thought*. His conclusions on pp. 392-3 and 543-4 are relevant to this theory.

## REVIEWS

RELIGION AND THE NEW PSYCHOLOGY. By Alson J. Smith. With Introductions by Dr J. B. Rhine and the Right Rev. Austin J. Pardue. New York, Doubleday, 1951. 192 pp. \$2.50.

The New Psychology, for Dr Alson J. Smith, is parapsychology, and he has discovered parapsychology as bringing new hope, 'the hope of the world in this time of crisis', to 'the millions of nominal Christians . . . whose acceptance of the scientific method has shaken their faith'. For 'parapsychology . . . can make religion intellectually respectable and science emotionally satisfying' (p. 6). This enthusiasm, appropriate enough in a good Methodist, has not prevented him from writing a book based upon a very wide knowledge of the history and present position of the subject. And the two Introductions, by the Protestant Episcopal Bishop of Pittsburgh and by Dr Rhine, both much more than formal commendations, should secure his book a reading in both camps. That by Dr Rhine has indeed a note of personal autobiography which gives it considerable importance, and the whole book has the authentic hall-mark of Duke University upon it. The writer has a first-hand and up-to-date knowledge of the experimental work which is going on there, and has had access to the records of



its early successes as well as to the later more carefully controlled experiments in which those successes are now being tested. He has also read widely in the history of psychical research, on both sides of the Atlantic, and with all his enthusiasm gives a by no means uncritical survey of the history from Patience Worth and the Fox sisters to the present day.

His purpose lies in the presentation of the growing rigour of the application of scientific method to fields of enquiry which involve a re-casting of some of the most cherished presuppositions of modern science. Parapsychology is now a scientific subject, but it obviously opens the door to hypotheses for which science has, until lately, found no use. Some of man's oldest intuitions are becoming respectable again, after long years of discredit. Personally I find Dr Alson Smith's theological interpretations a little too enthusiastic. I respect and agree with his intentions, but there are a good many gaps between the findings of parapsychology and his vision for the Church of tomorrow. We are a long way yet from seeing in parapsychology the hope for the re-union of Christendom 'by emphasizing that supernormal element that all denominations have in common', and by the revival of 'religion's ancient faith in man' (p. 174). And a church whose password is not 'Credo' but 'Amo' (p. 175) will in fact have a creed, drawn directly from the New Testament (I John iv. 21, *et passim*), and one which parapsychology has not yet gone very far to affirm.

Real students of parapsychology will, naturally enough, find little that is new in this vigorous survey, and some inaccuracies were inevitable, as in the references to Miss Beauchamp, where 'Sally' is given a second identity, and Doris Fisher, where the more critical survey by T. W. Mitchell would have helped the writer. He also does not know how open to criticism is the evidence for *An Adventure*, though he shows a proper hesitation about it. And some of his accounts of the phenomenal scoring by subjects at Duke University, though true as they stand, need to be read with a caution which those who know something about the later work there will be able to provide, but which may be lacking in the general reader.

But it is a stimulating and a lively book, and I am glad to have read it.

L. W. GRENSTED

SECOND SIGHT: its History and Origins. By Lewis Spence.  
London, Rider, 1951. 190 pp. 18s.

The focus of Second Sight manifestations seems by definition to be the Scottish Highlands. Mr Spence has collected together a

great number of stories from old sources and claims that the collection is exhaustive up to the end of the eighteenth century. He shows that originally Second Sight was associated with the worship of ancestral spirits.

The most interesting feature of the book is the tracking down of the story material to roots in folklore. Although Lewis Spence believes that Second Sight can be supernormal, 'When ignorant and unlettered folk seek to enhance the circumstances of a tale in order to make themselves momentarily important, they usually fall back upon the stock material of primitive fiction'. This may well 'ring a bell' with the student of poltergeists, particularly the classic cases.

It is unusual in a book of this type to find not a single mention of the Society for Psychical Research or of its publications.

Mr Spence appeals to psychologists to digest and elaborate on the material he has collected, so it is a pity he has not provided an index.

D. P.

MIRACLES. By Olivier Leroy. Bruges, Desclée de Brouwer, 1951. 152 pp.

M. Leroy defines a miracle as 'an extraordinary external event not explicable by any natural cause known or conceivable, and suggesting by its antecedents that its cause is invisible, personal and intelligent'. He gives a number of interesting and well-documented cases which he regards as falling within this definition, especially some cases of faith-healing. In some other cases, readers may feel that the incidents he relates are more easily explicable by normal means than M. Leroy seems to admit. He is, however, commendably cautious in the conclusions he draws, and students of psychical research will agree with him that an incident should not be dismissed as impossible on *a priori* grounds. We should consider all available evidence and determine, if possible, what actually occurred.

H. DE G. S.

JOURNAL OF PARAPSYCHOLOGY, Vol. 15, No. 2, June 1951. Durham, N.C., Duke University Press. \$1.50.

Professor Rhine's editorial on Parapsychology and Physics discusses the relation of psi phenomena to physical principles and the part which their study may play in advancing understanding of those problems in biology which defy physical methods of explanation.

The first main article is a shortened reprint of the report of Thouless's experiments on PK which has already been printed in our *Proceedings*.

Two articles deal with Soal's work on reinforcement effects in card-guessing experiments where two successive target cards are the same. Soal's original way of estimating the reinforcement effect was criticised by Professor M. S. Bartlett, and Greville contributes 'A method of evaluating the reinforcement effect'. In a longer article by Pratt, some of the Soal-Goldney reinforcement data is re-evaluated by this and another method. Significant evidence for reinforcement is found.

The most novel and interesting report in the Journal is one by Lyndon and Ronald Rose on 'Psi experiments with Australian aborigines'. Highly significant results were obtained in card-guessing experiments, but no significant results in PK experiments. Both high ESP capacities and PK capacities are supposed by the aborigines to be found amongst a small number of 'clever men'. None of these was tested, but the authors intend to try to get some such subjects in a later investigation.

Mrs Rhine reports a long series of PK tests of the 'placement' type. The total results were not significant, but there are some suggestive indications of relationships within the total series.

Casper reports an experiment on the effect of attitude of subject on ESP scoring. He adopts the erroneous method of comparing the total scores of different kinds of subjects instead of treating it as a problem of contingency in which the numbers of subjects scoring at various rates in different groups are compared. The method adopted systematically over-estimates the significance of any observed difference, and such over-estimation of significance is liable to lead to contradictions between the findings of different experimenters.

ROBERT H. THOULESS

## CORRESPONDENCE

### ESP AND INFORMATION THEORY

SIR,—I listened recently to a wireless discussion of the results of experiments in psychical research and after I had thought over what I had heard, an idea came to me for an interesting test to be applied to existing data. My suggestion concerns the application of the concepts of the mathematical theory of information.

The results of all the experiments which I have heard described have been analysed without any explicit account being taken of the time element involved. It is true that the effects of varying the rate of presentation in the five-card experiments have been noted, but time does appear as a variable in the statistical analysis. But in the communication of information from one place to another—and the telepathy experiments would appear to be tests of some communication system—time is an essential parameter. In a fundamental paper, C. E. Shannon has shown that the physical properties of a channel place an upper limit on the rate at which information can be transmitted through it, and that this limit is independent of the form the signal takes. It is thus of interest to determine whether the same sort of limitations apply to the psi channels.

The starting point of information theory is the setting up of a numerical measure of amount of information, and the unit now generally adopted is the binary digit—the information contained in the correct choice from a pair of equally probable alternatives. The next step is the determination of the *a priori* probabilities  $p_n$  of the possible messages. The *a priori* 'entropy' of an ensemble of possible messages is defined as the sum of  $-p_n \log_2 p_n$  over the whole ensemble. When a message is received unequivocally, the *a posteriori* probability of that particular message becomes unity and that of all the others zero. When it cannot be assumed that the received message is correct, i.e. that it is the one transmitted, our *a posteriori* knowledge can be expressed only as a new set of probabilities  $p_n'$  calculated from experience of receiving a number of messages. The average information contained in a message is the difference between the *a priori* and *a posteriori* entropies. In the first case quoted above—where the message is received unequivocally—the *a posteriori* entropy is zero, so the average information in a message is directly measured by the entropy of the ensemble from which the message is selected.

In the five-card experiment the *a priori* probability of each elementary message is  $1/5$ . Let us denote the probability of the received message being correct by  $(1/5 + x)$ . If  $5x$  is small compared with unity, then it is easily shown that the average information in each message is  $2x \log_2 e$  binary digits. The rate at which information is being transmitted is  $2xN \log_2 e$  binary digits per minute, where  $N$  is the number of cards exposed per minute.

Now, if the psi channel is saturated, we should expect this transmission rate to be constant independently of how rapidly cards are presented, so that *the excess of the receiver's score above chance should be inversely proportional to the rate at which cards are presented*. To within the limits of the approximation used in this

calculation the excess of score over chance should be independent of the number of alternative cards. It would seem an easy matter, by analysis of existing data, to test whether or not these relationships hold.

The significance of the theory of information would be underestimated if the test were confined to the five-card experiment in its simplest form. If an information rate limit exists, it is independent of the way the messages are coded. For example, if serial correlation between symbols was introduced, the information content of each symbol would be reduced and the number of correct guesses should rise.

E. R. R. HOLMBERG



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TWO SERIES OF PK TESTS ON  
PARAMECIA

BY NIGEL RICHMOND

INTRODUCTION

THE following experiments were designed to test whether an ability of PK exists which would influence the behaviour of protozoa. Paramecia were chosen as suitable subjects for these tests because they are very common, are easily recognized, and usually swim about in random fashion in search of food.<sup>1</sup> The purpose of the experiments was to influence, by thought alone, the direction in which a chosen animalcule would swim during a selected period.

The method of assessing the success of each attempt, which was made under a low-power microscope (magnification  $\times 75$ ), was to divide the field of view of the microscope into four by cross wires in the eyepiece of the instrument (Figure I). The attempt was timed by a stop-watch for a given period, and the quadrant containing the paramecium at the end of this time was noted. The quadrant into which the paramecium was to be willed was selected by turning up cards from an ordinary pack of playing cards, each suit indicating one of the four divisions.

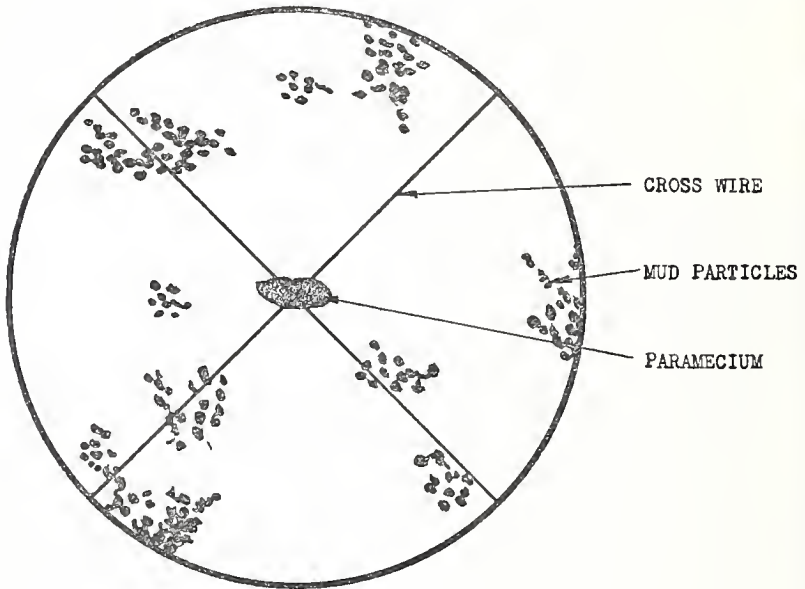
PROCEDURE IN THE FIRST SERIES

A sample of water containing paramecia and mud particles was placed on a flat microscope slide and a cover slip placed over it to prevent movement due to air currents on the surface of the water. A typical field of view at the start of an attempt is shown in Figure I.

<sup>1</sup> The paramecium is a single-celled organism, about 0.01 ins. long. It is common in all natural pond water and propels itself in the water by moving the hair-like processes (cilia) on its body.

A paramecium was then chosen which was making more or less random movements about the field of view. No attempts were made on paramecia swimming in a straight line, for these were moving too fast, and it was thought that strong stimuli were already acting upon them. The microscope stage was then moved to bring the paramecium under the cross wires, as in Figure I. A stop-watch timing of 15 seconds was started, and at the same moment a card indicating one of the quadrants was turned up.

The timing, card-turning, and scoring may be done by a second person if these activities disturb the concentration of the experimenter. In this first series some tests were made with this



*Figure I*  
TYPICAL FIELD OF VIEW IN FIRST SERIES

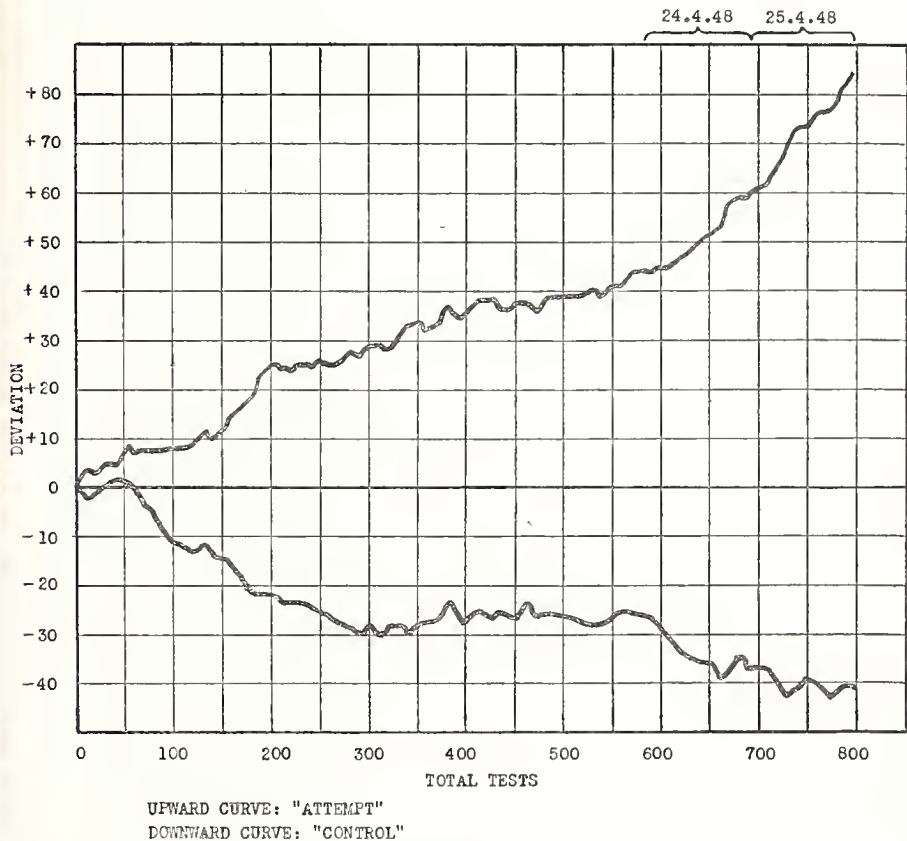
assistance and some without, with no noticeable change in the results. There was, nevertheless, a possibility of some error in the timing in this series, and the technique was improved for the second series of tests.

The experimenter then attempted, by thought alone, to drive the paramecium into the quadrant selected by the card. Precisely at the end of the 15 seconds the position of the paramecium was noted, together with the selected quadrant. The microscope stage was again moved to centre the paramecium under the cross wires and another attempt was then made. When the paramecium swam out of the field of view during the test period the exit quadrant was scored. Tests were considered void in which the paramecium appeared under a cross wire at the end of the period.



A separate series of runs was also made, interspersed with the tests already described, in which the procedure was exactly the same as above except that no conscious effort was made to influence the paramecia and the card was not turned up until the end of the 15-second period.

I have called these 'control' runs to distinguish them from the 'attempts', although perhaps this term is not entirely appropriate



*Figure II*

DEVIATIONS FROM CHANCE EXPECTATION. SERIES I

since there was a target (post-determined) for each and, as Dr Rhine has commented, the possibility was not excluded of the experimenter becoming aware of the card extrasensorily and then applying the influence unconsciously either in a positive or negative direction.

RESULTS OF THE FIRST SERIES

Figure II shows the deviations from chance obtained in the 794 'attempts' and 799 'controls' which make up this series. The

final results are tabulated in Table I. Although the field of view of the microscope is divided into four, the chance expectation is taken to be one half. This is because I suspected that influence applied in one direction would sometimes have its effect in the diametrically opposite direction, according to the kind of mental work employed. The experimenter may either influence the paramecium according to his conscious will, or alternatively he may set up an unconscious resistance which will strengthen its direct opposite, in this case the equivalent of scoring in the diametrically opposite quadrant.

As it was not possible to ensure that the thought process was controlled correctly, both the chosen quadrant and that diametrically opposite were, for assessment purposes, counted as targets, although during the experiment only the selected quadrant was treated as such. It should be remembered, however, that the experimenter was aware of a success in the opposite quadrant even though he would will towards the selected one. The results for the selected quadrant and the one diametrically opposite may be found in Table I.

TABLE I

		<i>Series I</i>	<i>Series II</i>	<i>Series I &amp; II</i>
'Attempt' Runs	Number of Runs	794	701	1495
	Deviation in chosen direction	+31.5	+77.75	+109.25
	Deviation in opposite direction	+54.5	+15.75	+70.25
	Total Deviation	+86	+93.5	+179.5
	Critical Ratio	+6.1	+7.06	+9.28
	Percentage success	+21.7	+26.7	+24
'Control' Runs	Number of Runs	799	701	1500
	Deviation	-41.5	-15.5	-57
	Critical Ratio	-2.94	-1.17	-2.94
	Percentage success	-10.4	-4.4	-7.7

Assessing the 'attempt' runs first, shown in Figure II by the curve above the chance (o) line, the total of 86 extra-chance successes in 794 attempts gives a critical ratio of +6.1. Turning then to the 'control' runs (shown in Figure II by the curve below the

chance line), there appears the peculiar circumstance of a significant negative deviation of 41.5 (C.R. - 2.94).

Another useful way of evaluating these results has been suggested by Dr Thouless which expresses the success as a percentage of the attempts made. Thus, in the 'attempts' of the first series, chance would predict 397 successes. There are, however, 86 more than this, which means that 86 predicted failures have been turned into successes. As there are also 397 failures predicted, the proportion of successful runs is 86/397—i.e. 21.7 per cent.

In the 'control' runs the percentage of 'success' is -10.4, or about 1 in 10 less correspondence with the chosen quadrants than predicted by chance.

#### CONSIDERATION OF THE RESULTS OF THE FIRST SERIES

An interesting and quite unexpected factor in the results is the negative deviation in the 'control' runs. Figure II indicates that not only is a negative deviation obtained, but the curve of this deviation bears a crude likeness to the positive curve in the 'attempt' runs.

This significant negative deviation can only reasonably be thought of as the result of ESP, unless so large a deviation can be considered attributable to chance. It is only in the mind of the experimenter that the relation between target and card suit exists; it is thus reasonable to suppose that his mind is the only link existing between the pack of cards and the direction the paramecium will take, and that it is responsible for the negative deviation found experimentally. In practice this means that the experimenter must have extrasensory knowledge of the card suit before he can influence the paramecium effectively in 'control' runs.

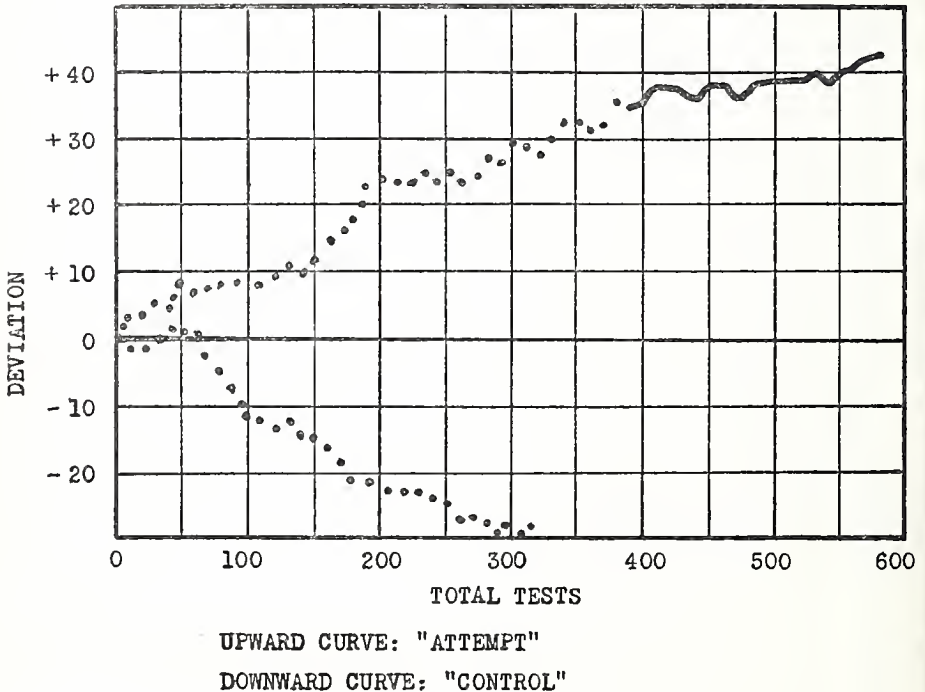
If the resemblance between the two curves of Figure II is due to 'negative' PK applied through extrasensory perception of the card, there would probably be a relation between the rate of success of 'attempts' and 'controls' made at approximately the same date. The average deviation per 100 tests, and date, is tabulated below. This shows an approximate resemblance between 'attempts' and 'controls' made on the same day, or within a day of one another (except for those made on 24 and 25 April 1948, which are discussed later).

This would seem to indicate that when one is successful in a normal PK run, one is at the same time successful in keeping the 'controls' well below chance expectation by ESP and PK, and if one faculty is disturbed the other is also.

TABLE II

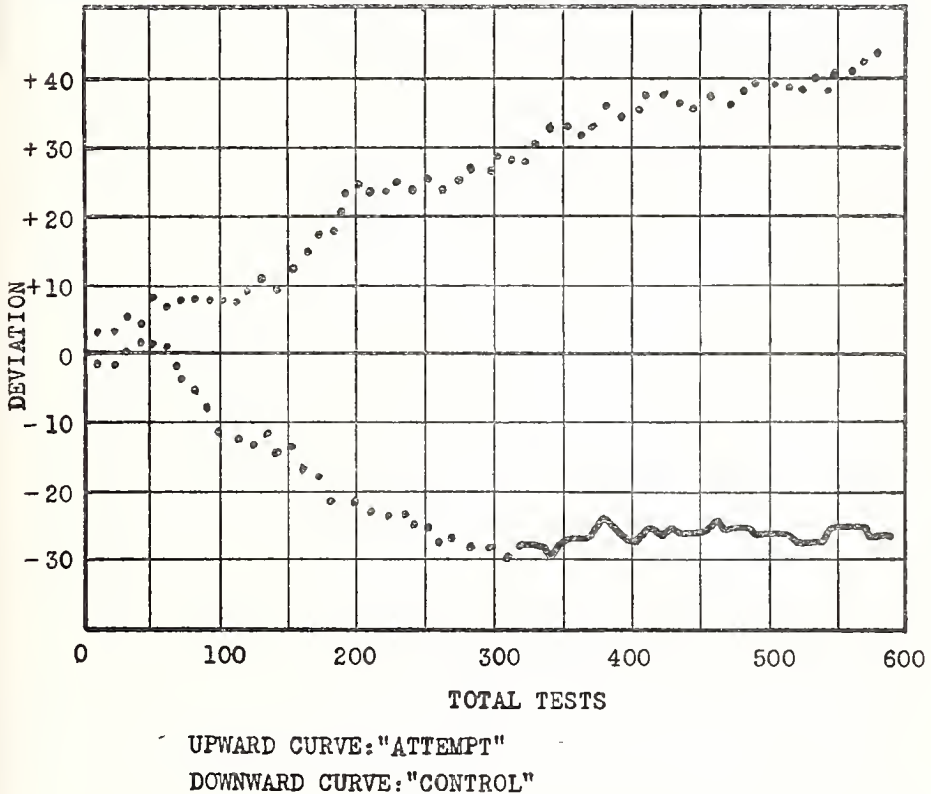
Date (April 1948)	Average Deviation per 100	
	Attempts	Controls
13th	9.35	
15th		8.5
17th	11.5	
18th		11.9
19th	8.0	7.8
21st	7.5	
22nd	3.5	2.6
23rd		0.5
24th	17.0	8.0
25th	22.0	6.2

As this series of tests progressed, the number of 'control' runs carried out were gradually allowed to lag behind the total number of 'attempts'. At the same time the successes obtained in the 'attempt' runs began to fall off until on 22 April a deviation of only 3.5 per 100 was obtained. (See Table II and Figure III—solid line.) Upon noticing this at the time, it seemed that there might be a connexion between the negative deviation in the 'control' runs and the successes obtained in the 'attempts'; so on

*Figure III*

SOLID LINE SHOWS 'ATTEMPT' RUNS MADE ON 22 APRIL 1948 WITH 'CONTROL' RUNS OUT OF STEP. SERIES I

23 April 'controls' were made to bring the total number up to the same as for 'attempts' (Figure IV), and thereafter I intended to keep them in step. To this end, on 24 April, 40 'controls' were made after every 40 'attempts' (Figure II), which gave the highest success yet obtained. Carrying this idea further on 25 April 'control' runs were carried out after every 5 'attempts'. This gave the highest positive deviation of all (Figure II).



*Figure IV*

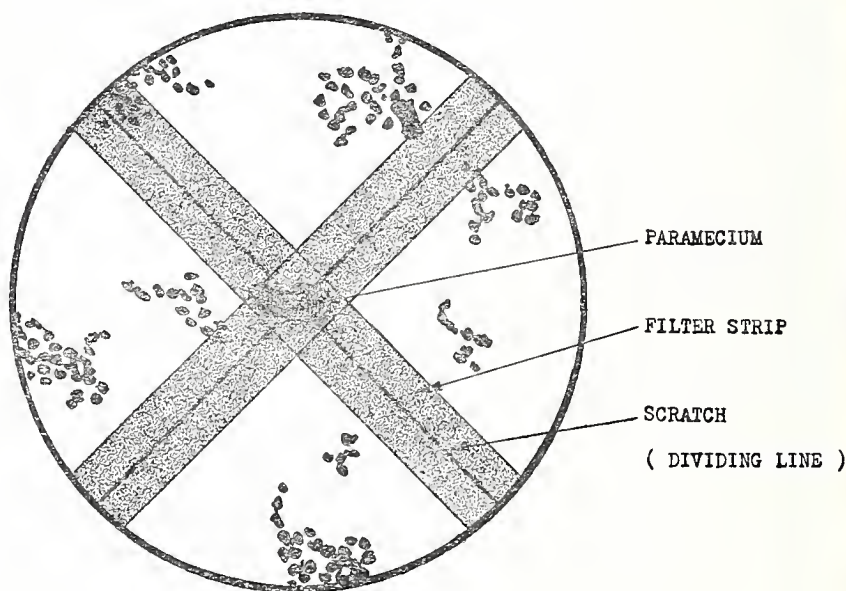
SOLID LINE SHOWS 'CONTROL' RUNS MADE ON 23 APRIL 1948 TO BRING THEM INTO STEP WITH 'ATTEMPT' RUNS. SERIES I

These last two results are in sharp contrast to the previous two, and the negative deviation does not correspond with the positive one as did the earlier tests (see Table II). Some fundamental change was caused by keeping the 'control' runs in step with the 'attempts'.

One of the possible interpretations of this result is to consider the 'control' series as releasing forces formed in opposition to the intention of the experimenter, whether these are thought of as existing solely in the mind of the experimenter, or in some psychic

structure including the paramecia. In either case these forces would be released through the mechanism of extrasensory perception of the suit of the selecting card by the experimenter in the 'control' series.

The small negative deviation obtained on 24 and 25 April, where high positive scores were made, is also in keeping with this compensative explanation. If a compensating force is being built up, the more often it can be released the less effect it will produce in negative deviation in the 'control' runs and depressive effect in the 'attempt' runs.



*Figure V*  
TYPICAL FIELD OF VIEW IN SECOND SERIES

#### CHANGES OF PROCEDURE FOR THE SECOND SERIES

Other than the decision to make this series with 'controls' closely interspersed with 'attempt' runs, it was suggested by a number of people that borderline cases, where the paramecium is near the cross wire at the end of the 15-second period, might be misjudged in favour of a high score. I agreed that this was a possible source of error, and devised a guarding procedure to overcome it.

The cross wires in the eyepiece of the microscope were replaced by crossed strips of light blue gelatin filter, with a scratch longitudinally down the centre of each, giving a field of view as shown in Figure V. As can be seen from this illustration, the scratches

take the place of the cross wires and the width of filter on each side of the scratch is about the width of a paramecium when viewed under  $\times 75$  magnification.

All scores were to be noted down as usual, and in addition all those in which the paramecium was within, or partly within, the area covered by the filter strips at the end of the 15-second period. If misjudgements were made to favour a high score within the filter area, the average scoring rate for results within this area would be higher than for results in which the paramecium was elsewhere in the field, so that comparison of average scores within and

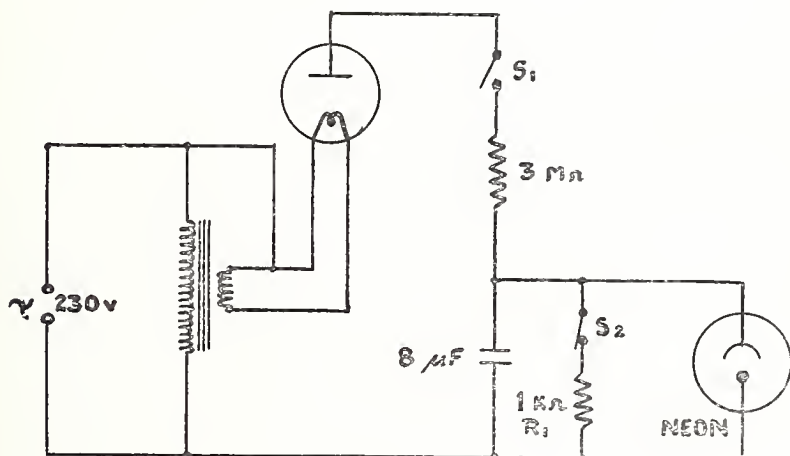


Figure VI

#### FIFTEEN-SECOND DELAY CIRCUIT

Before the beginning of a trial the linked switches  $S_1$  and  $S_2$  are as shown in the figure, and the condenser is discharged through the resistance  $R_1$ . When a trial is due to begin,  $S_1$  is pressed on (automatically releasing  $S_2$ ), and the condenser takes 15 seconds to charge up to the striking voltage of the neon (110 volts). The neon then flashes, flooding the field of view with red light and a spot judgment can easily be made.

outside the filter area would detect any misjudgement near the dividing line.

In addition to errors on the dividing line, misjudgements could also be made in timing the 15-second period, and this also could be made unconsciously to favour the score. This would apply almost entirely where no assistance was used in timing the period.

To get over this difficulty while working alone, a neon indicator tube was strapped immediately under the microscope slide, to which the 15-second delay circuit shown in Figure VI was connected to enable a spot judgement to be made.

## RESULTS OF THE SECOND SERIES

This series consists of 701 'attempts' and 701 'controls', the 'controls' being made after every 5 or every 1 'attempt'. The difference between these was only one of personal convenience; the scoring shows no advantage to either. Making 5 'attempts' and then 5 'controls' made the better working rhythm. The deviations obtained are plotted in Figure VII and the final results are shown in Table I.

Of the 444 successes in the 'attempt' runs, 253 were in the desired direction and 191 in the diametrically opposite one (Deviation +93.5; C.R. +7.06; percentage success 26.7). In the 'control' runs there is a negative deviation of 15.5 (C.R. -1.17).

Of 501 'attempts' which were made with the guarding procedure, 96 ended within the guarded area, giving an average deviation of +10.4 per 100 trials. The remaining 405 trials gave an average deviation of +13.7 per 100, showing that the scoring is, if anything, slightly disfavoured by judgements near the dividing line.

106 trials were made using a magnification of  $\times 25$  instead of the usual  $\times 75$  (Figure VII). The results of these were markedly poor, only 58 out of 106 being successes (Dev. +5). This low scoring agrees with an impression obtained during the tests that the first impulse of influence appears to be the strongest, and that its maintenance during the whole 15-second period is difficult. When a  $\times 75$  magnification is used, the paramecium often leaves the field of view during the first half of the attempt period (when the departure quadrant is scored), and in consequence the first part of any influence the experimenter has will have more effect. During the trials using  $\times 25$  magnification, the paramecium very rarely left the field of view during the whole 15 seconds, and the effort to influence it had to be maintained for the whole period.

## CONSIDERATION OF THE RESULTS OF THE SECOND SERIES

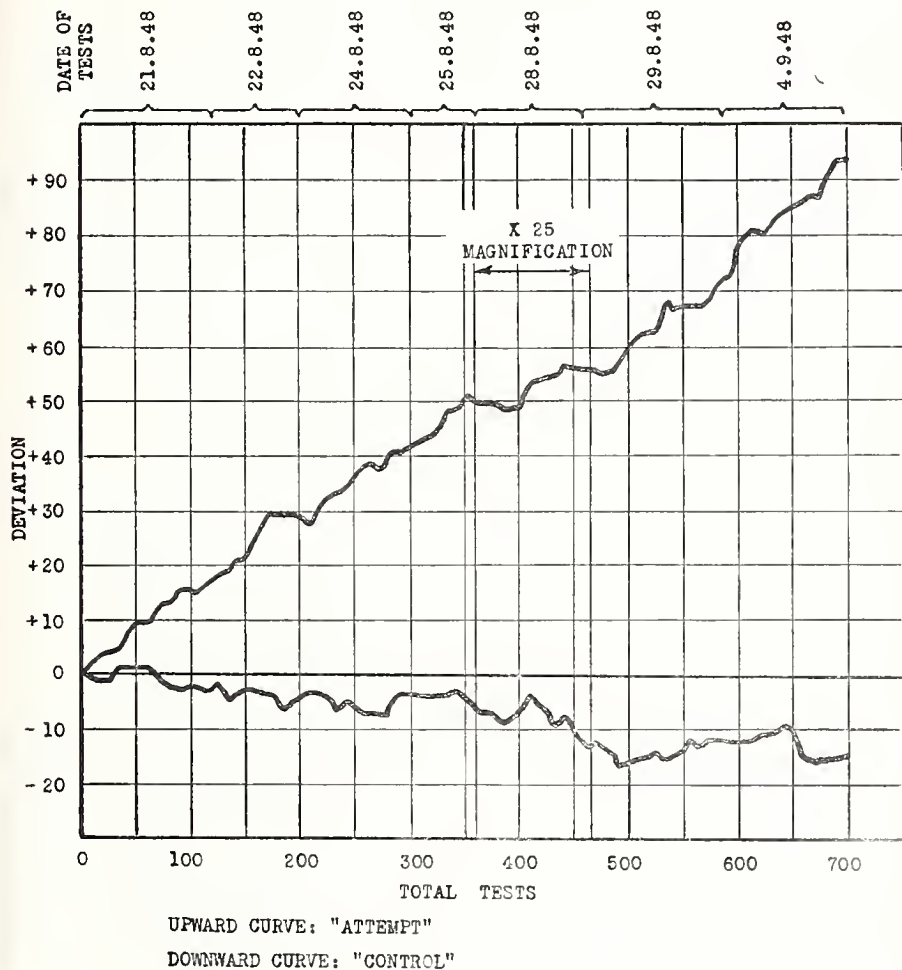
The high rate of success in the 'attempt' runs, the general straightness of the plot (Figure VII) (discounting the period of  $\times 25$  magnification), and the small negative deviation in the 'control' runs, is in keeping with the explanation put forward for some of the secondary effects in the first series (page 583).

The results of the tests using  $\times 25$  magnification indicate that this is a fairly critical factor with which the length of test period may be interdependent.

The first part of the 'control' curve of the first and second series (Figures II and VII) bears a similarity which is worth noting, although no conclusions may be drawn from only two such results.



When more tests of this nature have been made it will be possible to see whether this form occurs to a significant extent.



*Figure VII*  
DEVIATIONS FROM CHANCE IN SERIES II

#### CONCLUSION

The overall results of these two series of tests are tabulated in Table I. These results show that throughout the whole experiment an average of approximately every fourth failure predicted by probability calculation has become a success.

The absence of an overall decline in successes may be related to the procedure of making a second and contemporary series of tests in which no conscious effort is made to influence the

score. This, together with the practice of allowing diametrically opposite results as successes without prejudicing the statistical procedure, may have the effect of reducing or eliminating the causes of decline, which are probably psychological.

If the faculties of ESP and PK are accepted, however, there is the possibility of results being biased by these abilities in favour of a theory held by the experimenter; successes being unconsciously reduced under circumstances in which the theory may predict chance results. The lack of reproduceability in this type of work makes it very difficult to find out whether this occurs or not, but unless such secondary effects are found to be generally reproduceable they cannot be taken as more than reflections of the psychological situation in the experiment in which they occur.

## AN EXPERIMENT IN THE ELECTRO- ENCEPHALOGRAPHY OF MEDIUMISTIC TRANCE

BY C. C. EVANS AND EDWARD OSBORN

### INTRODUCTION

IF it be accepted (*a*) that in mediumistic trance information has been given which could not have been obtained by normal means, and (*b*) that the trance state is an essential factor in the process, it is important that inquiry should be made into the nature of the trance itself. Even if the genuineness of the phenomena be regarded as open to question, it is still of interest to try to discover whether the mediumistic trance possesses distinctive features of its own.

There are those who regard it as a state of hysterical dissociation, in which 'a greater or smaller part of the personality takes command and dictates the general behaviour, the rest of the normal personality becoming apparently incapable of consciousness for the time being and having no influence on conduct'.<sup>1</sup> It has also been described as a state of 'self-induced hypnosis'. Such opinions have been based on personal judgements, because no objective evidence has been available. It was in an attempt to provide data whose interpretation would not depend on individual assessment that an exploratory experiment with the electro-

<sup>1</sup> Henderson and Gillespie, *A Text-book of Psychiatry*, 7th ed. (London, Oxford University Press, 1950), p. 179.

encephalograph<sup>1</sup> was carried out in the Society's rooms on 20 and 21 September 1951. Although the electro-encephalograph has been used for clinical purposes for some years, the principal difficulty has been to find as subjects for experiments mediums whose trance is regarded as genuine and who are able to enter the trance state in the conditions required for the recording of the E.E.G. The experimenters were fortunate in securing the full co-operation of Mrs Eileen Garrett as subject, and welcome this opportunity of thanking her publicly for making possible what is believed to be the first experiment of its kind to be carried out in this country.

A few general remarks about the E.E.G. may be helpful. The electrical activity of the cortex consists of more or less continuous rhythmic fluctuations of potential. In normal adults, awake, relaxed, and with eyes closed, the brain rhythm, in the absence of specific stimulation, ranges in frequency from 8 to 13 cycles per second, with an average of about 10 c/s., while the amplitude may fluctuate spontaneously. Both frequency and amplitude vary from individual to individual, and to some extent within the E.E.G. of a single individual. This rhythm, which is known as the alpha rhythm, is suppressed by attention or perception. During the process of going to sleep, marked changes occur in the E.E.G. The various stages may be described in general terms as follows: (a) relaxed and resting, but awake—normal alpha rhythm, followed by diminution of amplitude; (b) drowsy, entering the stage of light sleep—the alpha rhythm fades away and short spindles of faster activity often appear, followed by slow waves of 4 to 5 c/s.; (c) real sleep—spindle-shaped bursts of waves, about 14 c/s., appear, and slow waves increase in magnitude and decrease in frequency; (d) deep sleep—spindles disappear, and slow waves become very large and very slow, less than 1 c/s.

In cases of hysterical dissociation there is no change in the alpha rhythm. It is also regarded as established that in hypnosis the E.E.G. shows none of the characteristics present in sleep records. If, therefore, the E.E.G. in mediumistic trance were not in accordance with these findings—and there are reports<sup>2</sup> which give reason for thinking that this may be so—this would indicate

<sup>1</sup> The electro-encephalograph is an instrument for recording the electrical activity of the cerebral cortex. Electrical discharges, picked up by electrodes normally placed on the scalp, are amplified about a million-fold and activate inkwriting oscillographs which trace records on a moving strip of paper. The resulting record is known as an electro-encephalogram—E.E.G. for short.

<sup>2</sup> For example, Thomson, Forbes, and Bolles, 'Brain potential rhythms in a case showing self-induced apparent trance states', *Amer. J. Psychiat.*, 1937, 93, 1313-14.

that it was not identical with hysterical dissociation or the hypnotic state.

Those who took part in the experiment were Dr C. C. Evans, head of the Department of Electro-physiology at Belmont Hospital, Sutton, Surrey ; Dr E. B. Strauss, Physician for Psychological Medicine at St Bartholomew's Hospital, London ; F. Wilson, of the Development Laboratory of the Edison Swan Electric Company ; and Mrs K. M. Goldney and Edward Osborn, both members of the Council of the Society. The Council are greatly indebted to the Edison Swan Electric Company for lending an 8-channel electro-encephalograph and frequency analyser for the experiment.

E. O.

#### EXPERIMENTAL SET-UP

An 8-channel ink-writing recorder was used in conjunction with a frequency analyser.<sup>1</sup> Six channels were used to record the E.E.G., while another recorded the E.C.G.<sup>2</sup> and another the P.G.R.<sup>3</sup>

The 6 E.E.G. channels could be arranged to record either in one continuous chain extending from low down on the right temporal area, up over the vertex and down to the left temporal area ; or, alternatively, in two parallel rows each about 2 inches from, and on opposite sides of, the mid-line, and running in the A.P. direction, each row utilising 3 channels.

The subject was seated in a chair for all experiments, and a microphone close by recorded all conversations and utterances on a magnetic tape recorder. At 30-second intervals a time signal was recorded simultaneously on the sound track (through a second microphone) and on the E.E.G. record for the purpose of synchronising the two records.

<sup>1</sup> An instrument which resolves a compound wave form into its simple sinusoidal components. The E.E.G. often contains several oscillations of different frequency compounded together, and sometimes it is required to know the actual frequencies of the components present, in addition to seeing the composite wave form.

<sup>2</sup> The electrocardiogram is a record of the electrical changes accompanying the muscular contractions of the heart, and in this case was recorded to observe the changes in the heart rate during the various phases of the experiment.

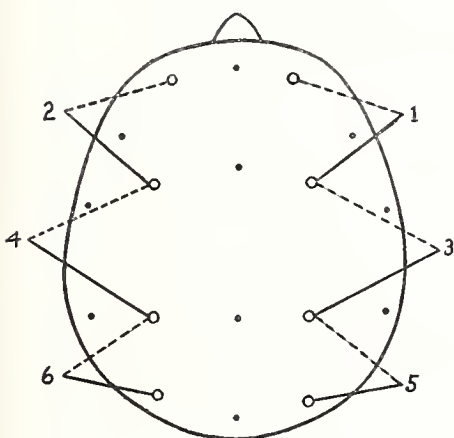
<sup>3</sup> The psychogalvanic-reflex depends on the changes of electrical resistance of the skin which accompany changes in the emotional state of the subject under test. Any stimulus word given, or other sound or situation which causes emotional change in the subject, results in a deflection of the recording instrument which is beyond voluntary control of the subject.

Recordings were made on the two successive evenings of 20 and 21 September 1951, and the sequence of events and results of both occasions will be dealt with in turn.

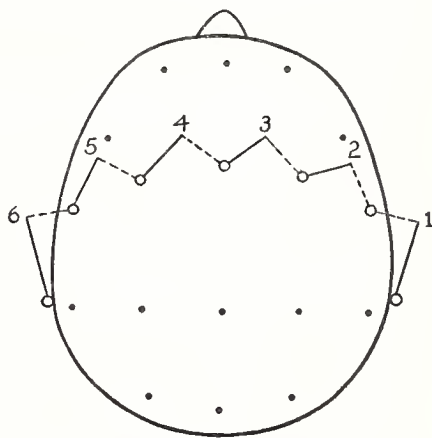
### 20 September

Preliminary samples of the resting E.E.G. were taken, with the subject seated in a chair but not attempting to go into the trance state. Recordings were made for 7 minutes with the electrodes arranged in the A.P. lines, and then for 6 minutes in the transverse plane.

The resting record was of low voltage, and showed an alpha rhythm of 11 c/s. dominant, but this was poorly maintained, and at times was absent altogether. Traces of faster activity (beta



*A.P. arrangement*



*Transverse arrangement*

rhythm) and of slower activity (theta rhythm) were also present at times, but without any definite localization. The record is rather lacking in any decisive rhythm or in localization of specific features, and in that respect does not lend itself readily to accurate or detailed description. Such inhibition of alpha rhythm is often caused by mental apprehension or by the subject taking a vital interest in the proceedings, and being unable to relax sufficiently. This factor was probably partly responsible for the low voltage of the activity, as at the end of the experiment, just before switching off, the amplitude of alpha activity was greater than at the start.

It was decided that the A.P. arrangement for recording was the most informative, and recording was continued thus while the subject was asked to go into trance. In this phase a coarse tremor of the eyes and eyelids became apparent, and was recorded as an artefact in the frontal leads of the E.E.G. ; this eye tremor re-

mained a constant feature of the trance state. There was also increase in the muscular tone of the arm, which probably reflected a generalised increase of muscle tone. This was recorded as an artefact in the channel recording the P.G.R. from the hand and wrist, and, combined with clenching movements, completely masked any P.G.R. response.

The heart rate rose from about 93 during the resting record to about 105 during the commencement of the trance state, and there was much yawning.

After about 2 minutes, the alleged Control 'Uvani' began to make some utterances, and it was assumed that trance had commenced.

For the sake of clarity, the sequence of events is given below with the times of occurrence :

*Time*

- Zero Subject asked to go into trance—pronounced eye tremor, yawning, increased muscle tone and movements, increase of heart rate. No appreciable change in E.E.G.
- + 2 mins. First utterances of Uvani heard, and Mrs Goldney establishing a conversation.
- + 4 mins. Uvani talking freely about Harry Price, and continued to do so for some while.  
No appreciable change in the eye tremor, nor muscle tone nor E.E.G. Subject now less restless.
- + 7' 20" Uvani asked by Mrs Goldney to try and relax the muscles and eyes of the medium but without any change resulting.
- + 8' 20" Mrs Goldney inquired whether Uvani considered the state of trance light or deep, and the reply was that he 'thought the trance was light'.
- + 8' 40" Subject becoming more restless—much movement artefact on P.G.R. channel. Pulse 108. E.E.G. no appreciable change—low voltage as seen in the initial recording.
- + 9' 20" Uvani now asked by Mrs Goldney to remain present but to be silent for a while, and to try to keep the medium still. This was followed by a marked reduction in the eye tremor.
- + 9' 50" The first significant change in the E.E.G. was observed. The amplitude increased by about 100% and 4-5 c/s. activity appeared in conjunction with rather unstable alpha rhythm in all channels. The increase in cortical activity persisted for about 30 seconds until the silence

was broken by Mrs Goldney speaking again. The pulse rate fell to about 93.

+ 10' 20" Mrs Goldney speaks, and the E.E.G. resumes its former character.

+ 10' 40" Mrs Goldney asks Uvani to open the medium's eyes, and a few seconds later Mrs Goldney opens them with her own fingers. There is no significant change in the E.E.G., but the eye tremor temporarily increases in frequency, and muscle artefacts become superimposed on the frontal channels.

+ 11' 30" Eyes closed again, and Uvani asked to remain silent as before. In spite of a longer period of silence lasting 1' 40", no increase of amplitude was observed as before.

+ 13' 10" Mrs Goldney again speaks to Uvani. Eye tremor now increases in amplitude. E.E.G. remains essentially the same.

+ 14' 30" Uvani asked by Mrs Goldney to leave the subject temporarily, her other Control, 'Abdul Latif', requested to take control.

+ 15' 30" Uvani still present—asked again to call Abdul Latif.

+ 16' 20" Silence—awaiting Abdul Latif.

+ 17' 0" Medium sighing—head bent forward. Eye tremor moderate. Pulse rate 105. E.E.G. low voltage and as in preliminary record.

+ 18' 10" Medium raises head and becomes more restless. Muscle tone increases.

+ 18' 40" Laughing. Abdul Latif takes control, and remains talking for nearly 2 minutes, during which time the increased muscle tone remains present.

+ 20' 20" Abdul Latif requested to remain present, but be silent for a while—no appreciable change in record, except for reduction of movement artefact.

+ 21' 10" Abdul Latif invited to talk again.

+ 23' 10" Eye tremor and frontal muscle artefacts pronounced. Dr Strauss requests Abdul Latif to relax medium's tremor, but no appreciable change is recorded.

+ 23' 45" Abdul Latif asks Dr Strauss to place his fingers on the medium's eyelids, but again no appreciable relaxation is obtained.

+ 25' 0" Mrs Goldney requests Abdul Latif to leave, and allow medium to come out of trance.

A noticeable decrease of the eye tremor and muscle artefact followed this request.

+ 26' 26" Sudden increase in movements and muscle tension. Medium sighing, and coming out of trance.

- + 26' 55" Out of trance. Eye tremor and muscle tremor artefacts disappear temporarily, and spontaneous brain rhythms show some increase.
- + 27' 10" Burst of normal alpha rhythm of about 2 seconds duration, of the greatest amplitude yet recorded in this experiment. Traces of low voltage theta rhythms are still present.
- + 28' 10" Recording ceased.

### 21 September

This session again began with recording in the 'normal' state with the A.P. grouping of electrodes as used previously. After some minutes of relatively low voltage and ill-defined activity, as recorded at the commencement of the previous session, the alpha rhythm became more prominent, and approximately 30 seconds later some 5 c/s. theta rhythm appeared, mainly in the parieto-occipital leads, either replacing normal alpha rhythm or else superimposed on it. This phase lasted some 15 seconds, and gradually disappeared, leaving once again the lower voltage and irregular rhythms as seen at the start. Pulse rate was approximately 70. This change, involving increase of amplitude and the presence of slower frequencies, and also considerable reduction of eye tremor artefact, was similar to that described in the first experiment, when the Control was asked to remain silent on the first occasion. This time, however, the medium was not in trance.

After several more minutes recording, no further changes occurred, and Dr Strauss began to induce the hypnotic state.<sup>1</sup> The immediate response to this was a violent temporary increase in the eye movement artefacts, but no significant change occurred in the E.E.G. throughout the period of hypnosis.

### *Time*

- Zero Hypnosis commences—no significant E.E.G. change.
- + 1' 20" Considered to be in the hypnotic state.
- + 4' 0" Dr Strauss suggests that Mrs Garrett shall pass from hypnotic 'sleep' into real sleep.
- + 4' 40" Silence—no E.E.G. change.
- + 7' 40" Silence still maintained—no E.E.G. change. Dr Strauss now suggests that Mrs Garrett shall pass back from 'real' sleep to hypnotic 'sleep'. No significant E.E.G. change.

<sup>1</sup> Mrs. Garrett had, according to her own statement, been hypnotised on several previous occasions. In a preliminary test before this session began, she appeared to respond to hypnotic suggestions given by Dr Strauss. The other experimenters were not present at the time.



- +9' 30" Dr Strauss now suggests Mrs Garrett shall pass into the trance state.
- +9' 45" Movements and yawning. At times some well-defined 11 c/s. alpha rhythm in the E.E.G.
- +11' 10" Mrs Goldney asks, 'Is that you, Uvani?' Eye movement artefacts more prominent.
- +11' 40" Uvani replies, speaking slowly.
- +12' 20" Uvani talking. Muscle tension increased, and showing as artefact on P.G.R. channel. Eye movement artefacts increased.
- +13' 50" A cushion placed under Mrs Garrett's arms to give more comfortable support, in an attempt to reduce the muscle artefact in the P.G.R. channel.
- +14' 26" Dr Strauss gives 'suggestion' that the arms become more relaxed—no appreciable result.
- +15' 10" Mrs Goldney talking to Uvani. Muscle and eye movement artefacts still present, and no appreciable change in the general E.E.G. pattern.
- +17' 40" Uvani still talking.
- +20' 0" Mrs Goldney talking with Uvani.
- +22' 0" Uvani asked to remain silent for a while.
- +22' 22" Increase in amplitude of rhythms by about 100% and also increase in the slower components at about 5-6 c/s.
- +23' 0" Gradual and spontaneous return to original amplitude and pattern of rhythms.
- +23' 5" Dr Strauss talking to Uvani; suggests that he should leave, and that the medium should transfer again to the hypnotic state. No appreciable E.E.G. change.
- +24' 20" Medium now out of trance, but in hypnotic state and told to wake in one minute's time.
- +26' 30" Opening eyes and general movements—awake. No significant change in E.E.G.
- +28' 0" Recording ceased.

## SUMMARY

The E.E.G., as already stated, is of low voltage and at times shows some 11 c/s. alpha rhythm but at other times is unstable and no clearly defined rhythms can be described.

A slight but significant change occurred on three occasions in the two sittings: (1) while taking a sample of 'normal' record before the trance state and in silence; (2) and (3) while in trance but with the Control silent.

The change was characterised by a fairly sudden increase of amplitude, of about 100%, although the voltage was low through-

out all records, and by the appearance of 5-6 c/s. activity, which was most prominent in the parietal areas. On one occasion this new pattern disappeared when the Control was addressed, while on the two other occasions it disappeared spontaneously. As this changed pattern occurred out of trance as well as in trance, it can not be claimed that it is related to the trance state. A condition common to the three occurrences was silence. Spontaneous changes in the E.E.G. are often caused by the subject becoming temporarily drowsy, and this may be the explanation of the changes observed in these experiments.

There was no significant E.E.G. change when the subject went into or came out of either the trance state or the hypnotic state.

Eye tremor and muscular tension appeared to be maximal in the trance states.

The P.G.R. readings were masked by movement artefacts to such an extent as to be unreliable, and are therefore ignored as such.

In conclusion, I would stress that this is only a factual account of preliminary tests carried out on one individual, and that no claims are made to prove nor disprove the genuineness of mediumistic trance by this means.

C. C. E.

## REVIEWS

WEST AFRICAN PSYCHOLOGY: a comparative study of psychological and religious thought. By G. Parrinder. London, Lutterworth Press, 1951. (Lutterworth Library, Vol. XXXVII) ix, 220 pp. 25s.

In this volume Dr Parrinder, who was for some years doing missionary work in French West Africa and is now at University College at Ibadan, discusses and summarises the various psychological and religious beliefs of some West African peoples. Although the book is really a summary of such beliefs, Dr Parrinder shows himself well aware of the linguistic pitfalls into which so many anthropologists have fallen. He points out that there is something to be said for the African view that white people cannot understand things that Africans know, and that when their beliefs are described in a European language, the idea in the translator's mind may be very different from what the African actually thinks.

From the point of view of the psychical researcher the book

contains much of interest, although it is clear that Dr Parrinder has not attempted any really serious investigation. He notes the presence of phenomena which may be classed under such headings as possession, hypnotism, telepathy, and clairvoyance, and has a good deal to say on African dreams and their interpretation according to modern Western standards. He points out that the mechanism of the African mind is not different from that of the West, but that what is different is environment and tradition, a fact which ought never to be lost sight of when dealing with mediumship amongst African or Oriental peoples. But even when we take these things into account, it would seem that investigation, even though it be slight, indicates that the search for the supernormal in Africa might be as unrewarding as it is in Europe. Thus, in quoting the account of Dr Field's work amongst the Gā people, the author shows that, in cases of glossolalia, knowledge of foreign tongues seems to proceed from the medium's subconscious memories or from words and sentences which have been overheard.

As a succinct account of the main features of West African psychology and religious ideas this book is useful, and it is to be hoped that later Dr Parrinder will be able to describe in greater detail some of those features which it is the duty of the psychical researcher to examine in whatever part of the world they may be found.

E. J. DINGWALL

MENTAL PRODIGIES. By Fred Barlow. London, Hutchinson, 1951. 256 pp. 12s. 6d.

This is a fascinating and long overdue account of the extraordinary calculating feats performed by certain gifted individuals who have attracted public attention from time to time during the last 200 years. Many of them, such as Zerah Colburn, were at the height of their powers as young children and lost the ability when they reached maturity. Occasionally, as with George Parker Bidder, the power was retained in adult life. Most of us have heard of these two, but Barlow brings upwards of twenty others to our notice in chronological order. With Miss Shakuntala Devi (born 1920), who appeared recently on television, he brings us up to date.

Many instances of the remarkable calculating achievements are given—for example, Colburn named the cube root of 413,993,348,677 in five seconds (answer, 7453). Johann Dase of Hamburg multiplied 79,532,853 by 93,758,479 in 54 seconds. Maurice Dagbert of Calais raised 89 to the sixth power (496,981,290,961) in 10

seconds and gave the number of seconds in 58 years, allowing for leap-years, in 23 seconds (answer, 1,830,297,600).

Barlow devotes a chapter to considering the *modus operandi* of the various prodigies. (There is almost a virgin field here for experiment.) Few of them were able to analyse the workings of their own brains. Bidder, however, says: 'If I am asked the product of, say, 89 by 73, the answer (6497) comes immediately into my mind. I multiply 80 by 70, 80 by 3, 9 by 70 and 9 by 3.' Dagbert and others made much use of mnemonics and in a later chapter the author reveals to us some of the secrets of stage 'mentalists'. For example, any reader can learn in a few minutes how to astonish his friends by giving with lightning speed the day of the week for any date from 1753 to 2,000 A.D. The Giant Memory Act, practised by many magicians, is also revealed and Mr Barlow's own private code is a very ingenious one.

There are chapters on arithmetical recreations and magic squares which lie rather outside the author's chosen theme, but they make very interesting reading, and indeed entertaining and startling facts and stories worthy of quotation can be found on every other page.

D. P.

UNSEEN ADVENTURES. By Geraldine Cummins. London, Rider, 1951. 183 pp. 15s.

Members of the S.P.R. will welcome this book, written by a lady who has the rare distinction of being a contemporary automatist of repute. Like Mrs Osborne Leonard and Mrs Piper, Geraldine Cummins has invited scientific examination and criticism. Both she and her collaborator and recorder, the late Miss Gibbes, deserve the commendation and appreciation of all students of psychical research for their work in its cause.

But on the evaluation of her published work many of us will part company with Miss Cummins.

On page 46 of her book, she says: 'My extra sense has not merely related these correct facts, which were unknown to me, it has also insisted that these messages come from people who are dead. Why does my extra or sixth sense invent this falsehood? For it is a falsehood if the dead do not survive.' Then on page 133 we read: 'I am suspicious of the inventive powers of my subconscious mind.' Alas, the history of psychical research shows how well founded are her suspicions!

For people who are inclined to oversimplify the problem of mediumship, a perusal of the Smead Case in the *Proceedings* of the American S.P.R. and in our own *Proceedings* 'The Psychology of Mrs Piper's Trance', by Mrs Sidgwick, will give them cause to

reflect. Mrs Smead, a non-professional medium, was the wife of a clergyman who was himself a keen student of psychical research. Mrs Smead went into trance and the information given in this state invariably claimed to come from the dead. Professor Hyslop was satisfied that the trance condition was genuine, but the facts clearly pointed to subliminal impersonation, and in this lengthy verbatim record there quite clearly emerges evidence that however honest and worthy a medium may be consciously, the danger of subconscious fabrication is always a factor with which we must reckon. The object of this paper was to illustrate the adaptability of the subconscious to the requirements of so-called communication. Mrs Sidgwick's detailed analysis of the Piper mediumship made it obvious that there were excellent reasons for refusing to accept the 'communications' at their face value. Contrary to a common view, and one apparently shared by Miss Cummins, mediumship has by no means been neglected by officers of the S.P.R. The fact is, it often raises far more problems than it solves.

Since those papers were written, further progress has been made. On page 29 Miss Cummins states: 'My communicators on the whole fail, while I am fairly successful in prophecy. . . . Their gross inaccuracy was to me evidential of a separate entity from myself using my hand. . . .' With this statement we need not concern ourselves. Nevertheless, it is evident that Miss Cummins regards precognition as strengthening the evidence for communication, but thanks to the work of Rhine, Soal, Carington and others, it has come to be regarded as experimentally established—in other words it is a form of ESP. Most of the sensitives employed by Osty never claimed to be in touch with the dead, yet not only were they able to acquire knowledge paranormally, but they were also successful in predicting events in the lives of people with whom they were able to make contact.

It must be undue modesty which causes Miss Cummins to describe herself as 'slow of speech and an uninformed conversationalist'. A moderately successful authoress and playwright must be thought to possess an imagination capable of being drawn on. It is not difficult to conceive that the possession of ESP in addition could produce almost anything, and we need not necessarily postulate an extraneous personality to account for the material, however striking.

On the other hand, the case of 'Marguerite Foote' mentioned in the Appendix to the book, which was originally printed in the *Journal*, is impressive. If an investigator is satisfied that the evidence from psychical research taken as a whole reveals an apparently non-physical level of the personality, capable of sur-

viving the dissolution of the body, then he might regard this as one of those rare cases pointing significantly to communication from the dead. He would, however, want much more information than is contained in the printed report.

Miss Cummins devotes a chapter to psychometric and healing experiments. The few examples of psychometry given do not justify specific conclusions, but in respect to healing Miss Cummins says: 'The proof of the pudding is in the eating.' Unfortunately we not only have an intangible pudding in this instance, but a purely hypothetical one. Patients were said to have been cured. It may very well have been the effect on the minds of the uninitiated of a few paranormally acquired facts which made them more susceptible to constructive suggestion. In the light of our present knowledge, however, we can only maintain the proverbially 'open mind'.

All students of psychical research should make a point of reading this book, not only because it is interesting but because we can trust Miss Cummins to give us the facts accurately. We are all at liberty to form our own conclusions.

LEAH LONGMAN

MORE THAN MEETS THE EYE. By James Langham. With an Introduction by Lord Dunsany. London, Evans, 1951. xii, 214 pp. Illus. 12s. 6d.

This book is written in a popular style and is not intended for the expert. Within the limits which he has set himself, Mr Langham gives a very fair picture of the various types of phenomenon which have intrigued and puzzled men through the ages. He includes a description of modern methods of conducting ESP experiments and a short summary of research carried out in this field. There is even a chapter dealing with hoaxes which have been perpetrated on the Society for Psychical Research.

Under the heading 'Some Stage Mysteries' Mr Langham discusses various stage mystery acts and comments that, while the majority of such entertainers do not claim psychic faculties, the Piddingtons are an exception to the extent of their statement, 'So far as we are concerned, telepathy works'. This would rather seem to depend on just what meaning the Piddingtons attach to the word 'telepathy'. Mr Langham could have pointed out that this statement is a good example of the technique used by stage 'mind-readers' to encourage belief in their possession of special powers. Avoiding any direct claim, they make ambiguous pronouncements, and in next to no time the uncritical credit them with psychic faculties. Members of the Magic Circle have

assured me that a skilful illusionist can duplicate and even improve on all the turns in the Piddingtons' repertoire.

From 'magicians' he turns to the study of the minds of animals and states that while various animals are capable of realizing instinctively the power of man and the demands made upon them, it is the dog and horse that usually 'react' with greater speed and versatility, presumably because of their long familiarity with men's ways. I am not sure from this remark whether we are meant to presume the inheritance of this acquired characteristic. However this may be, he goes on to review a number of classic cases of calculating horses and talking dogs and other animals as well as some more modern ones.

Up to this stage of the book, Mr Langham has generally just stated the known or reported facts, has pointed out any gaps there may be in our knowledge of what occurred, and left the reader to draw his own conclusions without any comment from himself. From this stage on, however, one finds such comments as, 'I believe her story to be true although it is not corroborated by documentary evidence' (p. 108) or 'Both . . . are "second-hand", but I have no reason to doubt their authenticity' (p. 137). This is after earlier chapters where he has pointed out the essential requirements on which to base evidence. Mr Langham is, of course, as entitled to his own ideas as any of us, but it seems rather a pity that he should abandon the purely objective statement.

Some of his ideas come out incidentally and it is rather startling to find (p. 139) the phrase 'as the dreamer enters the body . . .'. There has been no mention of astral travelling, but it makes one wonder.

Ghosts and poltergeists are dealt with in the penultimate chapter, while the last is simply entitled 'The Future Can Be Seen' and is mainly composed of reported cases of 'Second Sight' from the past.

There is a Bibliography and complete acknowledgement of sources and of help given by the Society.

Blemishes in the book are few and unimportant, and it is greatly to be desired that all popular expositions of the subject should give as objective a picture to the general public of the state of present knowledge concerning matters pertinent to psychical research.

H. S.

JOURNAL OF THE AMERICAN SOCIETY FOR PSYCHICAL RESEARCH.

Vol. 45, No. 4, October 1951. New York, A.S.P.R. \$1.50.

In honour of an English Quaker who settled in the United States and who was interested in psychical research, the John

William Graham Lectures have been founded. The first of the series was delivered in April 1951 by Professor C. J. Ducasse, one of the A.S.P.R. Board of Trustees, who makes a scholarly and penetrating survey of the whole field of paranormal phenomena. He has some interesting things to say on the intellectual dilemma which confronts many of us in the study of the work of Sir William Crookes (and elsewhere). 'In the face of this [the conditions being beyond reproach], I submit that if, as indeed is the case, I still find psychological difficulty in believing that the levitations reported occurred, then there is for me only to confess that my psychological reluctance to follow where the evidence leads means that I am not as rational as I should be.'

Hereward Carrington in a short paper on Hypnosis gives his opinion that from the point of view of educating paranormal powers in hypnotic subjects, hypnotists have until now been barking up the wrong tree. Instead of saying, 'You are sinking down deeper and deeper into relaxed sleep, etc.' they should say, 'You are rising *higher* and *higher* into a superconscious realm, etc.'

'An Unusual and Recurrent Experience' by Esther De Leau describes a typical haunting of the 'evil presence' type. An editorial comment by L.W.A. accords with my own assessment of the report and expresses it a lot better than I could. It is not a type of case that can be summarised briefly in a review.

D. P.

## CORRESPONDENCE

### 'THE CLAIRVOYANT THEORY OF PERCEPTION'

SIR,—In reference to the review of my book, *The Clairvoyant Theory of Perception*, in the *Journal* (November-December 1951) the reviewer has, probably from lack of clearness of exposition on my part, drawn certain inferences that are contrary to the tenets of my thesis. Hence the following.

First of all, no theory can be expected to provide a full and adequate explanation both in respect of normal and abnormal conditions of vision. On the other hand, if the Clairvoyant Theory is able to give a better explanation of vision in the case of the majority of normal conditions but is held to fail in a few abnormal conditions—and the conventional theories of vision are, as we believe, incapable of adequately explaining even the majority of *normal* conditions of vision—then, as a working hypothesis, it is,



we would contend, preferable to the latter. My thesis is, as your reviewer states, mainly a criticism of current theories of perception. If they have any basis in fact and are valid, the above contention of mine holds. I may cite here one of them, namely, the inadequacy of the conventional theories to explain the compound vision of insects.

As an example of abnormal conditions, the reviewer of my book states that distortion of clairvoyant vision due to cataract will not fit into my theory. He says: 'It seems to be a necessary consequence of this theory that diseases of the nervous system proximal to the retina should not affect vision. For if we consider the case of progressive cataract, as long as a glimmer of light can get through to the retina impulses will reach the cortex and there exercise their powers of evocation on the mind.'

With reference to the above the following two examples may be of interest. A resident of Nelson, New Zealand, had his lenses removed for cataract and had in consequence to wear three kinds of glasses, for long distance, middle distance, and short distance (for reading). Some years after the operation he had occasion to go to an optician to have the short glasses repaired. While the optician was fitting the repaired glasses he had to leave the patient to attend to a customer in an adjoining compartment. When he returned he was amazed to see the patient reading (without glasses) a pamphlet that happened to be lying on a table close beside him. The optician asked him to read out several passages in the pamphlet that he selected, and the patient accomplished this task correctly. On being asked how he could read without the glasses he replied that he had in the course of time and with practice come to see things both near and distant, and even to read a book, but only for a short time as it brought on a headache. These details I unfortunately only obtained after the patient had died at the age of eighty years. I have every confidence in the truthfulness and integrity of the optician as I had known him for several years.

A possible explanation of the above in terms of my theory would be as follows. The patient had learnt mentally by training to overcome the lack of well-defined and therefore weak stimulations of the retinae from the removal of the lenses, but there remained sufficient stimuli to initiate impulses up the optic nerves to evoke clairvoyant sight from points of view at the retinae.

I read a book some time ago entitled *A Hypnotist's Case Book*—the name of the author I have forgotten, and as I am writing this away from my home I cannot give it now—in which the author claimed to have effected in one case a permanent cure of blindness and in the second only an impermanent one. If the facts related

are genuine, they would seem to support the suggestion given above.

(a) The case of a girl 16 years of age who was born only with a small part of the outer periphery of the retina intact, and consequently was totally blind. After a series of hypnotic treatments lasting several months the patient began first to distinguish fairly large block forms with no detail, then these forms gradually assumed more definite details or characteristics, and at the end of the treatment she perceived them in their natural colours, eventually acquiring approximate normal vision. She was very keen to become a ballet dancer, and had so far recovered her sight after this hypnotic treatment that she was accepted as a pupil in a dancing academy, and eventually became a professional dancer in the ballet. On the other hand, she was never able to read a printed page.

(b) The case of a woman who had normal sight up to middle age and then became totally blind from 'white atrophy of both retinae'. She attended the London Ophthalmic Hospital, but was informed that nothing could be done to restore her sight. After a number of treatments in the form of hypnotic suggestion that she could eventually see, in spite of the fact that not a vestige of a healthy retina remained, she acquired the power of sight quite as good, if not better, than before she became blind. But unfortunately this power of sight diminished every hour after a treatment, and in the course of a few days she became totally blind again. After each treatment the same thing happened. The cure was only temporary since there were no retinal stimulations and consequently no evocation of clairvoyant sight.

If these two cures were actually effected by a succession of mental hypnotic suggestions, they would seem to point to the ability of the mind in respect of sight to overcome the diseased conditions in the visual sensory organs, very much in the same way as the rectification of damaged and diseased parts, and the taking over vicariously of the functions of these parts by other healthy parts in the process of growth of living organisms.

From the conventional standpoint, it is generally admitted that visual perception is of a mental nature; that is to say, visual sensations of external material things are mental and not physiological. The production of visual sensations by stimuli at the end terminals of the optic nerves in the cortex is still a mystery for science, and there are many other unexplained things in connection with vision that have no satisfactory explanation in terms of the conventional theories.

As evidence against my theory of vision, Dr Smythies mentions certain abnormal cases such as bilateral vascular lesions of the parietal lobe which produce inability to perceive simultaneously

a number of external objects and in which right and left orientation is entirely lost, which abnormal physiological conditions in the brain cause 'the disorders of space perception directly, which is the very position this theory was designed to avoid.'

I would reply that my theory does not reject the view that abnormal physiological conditions in the brain may produce abnormal effects in clairvoyant vision. For, if clairvoyance is a mental faculty, such abnormal cerebral conditions as above would naturally be expected to produce them. For instance, I cited in my book how a drug such as santonin causes us to perceive external objects as yellow instead of their proper colour. Thus, in the case of 'vascular lesions of the parietal lobe' we would hold that their effects on vision may possibly be due to the lack of integration or correlation in respect to the cerebral processes. Again, the inability to estimate distances and to recognise whether an object is receding or approaching may be due to the patient not being able to perceive more than one of two objects at a time.

Explanations on somewhat similar lines could be put forward for Dr Smythies's other examples, namely from (1) to (5), which for lack of space we cannot give here. Accordingly, my theory does not entail 'that diseases of the nervous system proximal to the retina should not affect vision', which I exemplified in my explanation of colour blindness and the effects of santonin.

With reference to the cases (a) and (b), I would classify (a) as an abnormal example of what I call 'Common Perceptual Clairvoyance' or C.P.C.; that is to say, it comes under the category of ordinary visual perception since the fragments of the retinae enabled a very small amount of neural impulses to reach the brain. Case (b) comes under an entirely different category as there is *no* stimulation at the retinae (the latter being non-existent) and consequently there is no stimulation at the cortex. It might be classified as a form of 'Travelling Clairvoyance', which requires no retinal stimulation. Thus, instead of a 'point of view' outside the body, the mind of the patient as a result of strong hypnotic suggestion clairvoyantly sees from 'points of view' at the retinae. Moreover, since no neural impulses travel up the optic nerves to the brain, the power of exercising 'Travelling Clairvoyance' cannot be sustained without repeated hypnotic suggestions.

I hope this letter will go some way in removing certain misconceptions of my theory, namely, that 'the distorted form of the nervous stimuli arriving at the cortex consequent on the distortion of the cornea and lens bears no relation to the subsequent disordered perception of the external world.'

M. M. MONCRIEFF,  
Nelson, New Zealand.

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# Society for Psychical Research

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SUPPLEMENT

TO

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FOR MEMBERS AND ASSOCIATES

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## ANNUAL REPORT OF THE COUNCIL FOR 1951

### 1. OUR SEVENTIETH YEAR

THE Society, which came into being in February 1882, has now completed seventy years of existence. The Council feel this to be a suitable occasion to pay a tribute to the memory of the men and women no longer with us, many of them persons of the highest distinction, who have promoted the Society's work both by taking an active part in it and also by generous donations and bequests. They consider that they may reasonably congratulate themselves and their fellow-members on the number of those who have shown a desire in recent years to engage actively in psychical research, and to assist the Society in its financial difficulties. These causes for satisfaction are all the more notable in view of conditions prevailing since the War which have increased the burden of other work on most of our members and imposed very heavy financial burdens on all. The serious economic position is reflected in the increased number of resignations during the year. Although the total of Members and Student-Associates elected in 1951 was only one less than in 1950, there has been a net decrease of 17 in the total membership (see p. xii).

The Council have for some time been considering plans for celebrating the completion of the Society's seventieth year suitably, and an announcement will shortly be made.

### 2. RESEARCH

The Home-testing ESP experiments organized by Mr G. W. Fisk have continued, and a large quantity of data has been accumulated. Contact has been made with experimenters as far afield as France, Iceland, India, Nigeria, Sweden, and the United States of America. The curious negative displacement scoring has persisted (see *Journal*

for November 1951) and all possible analyses are being made to try to determine whether these scores represent a genuine phenomenon or a mere statistical freak

A considerable number of tests have been made with a new technique of differential scoring in ESP experiments in which 'Clock' cards are used. First results are certainly promising. The formidable mathematical problems involved in the correct appraisal of results have been tackled by Mr J. Fraser Nicol (now at Duke University) and by one of our new members, Mr Alan Mitchell, to whose enthusiastic co-operation we are deeply indebted. It is hoped to publish a preliminary report shortly.

Two series of ESP tests have been carried out by Dr West using as subjects psychotic patients in mental hospitals. A report for the *Journal* is in preparation. An account of some earlier work with mental patients has just been published in the *Journal of Parapsychology*, and Dr Humphrey's report on her work in mental hospitals in London is expected to be available shortly. It will be interesting to compare the results of the various investigators.

Dr West is at present working on an experiment designed to investigate the connexion between motivation and results in ESP tests.

It is gratifying to hear that ESP research is in progress in at least three English Universities, Oxford, Durham, and Nottingham.

At Oxford, Mr William G. Roll, a member of the Society, has received a grant from the Committee for Advanced Studies and is conducting experiments in ESP under the supervision of Professor H. H. Price. Collaborating with him are Mr B. Babington-Smith, Lecturer at the Institute of Experimental Psychology, Dr F. B. Steiner, Lecturer in the Department of Social Anthropology, and Dr R. Kosterlitz, Extra-mural Lecturer in Psychology. Personality tests (the Bernreuter Personality Inventory and the Rosenzweig Picture Frustration Test) are given to each subject. At present the subjects are being tested for ESP in their normal state; it is proposed at a later stage to test them under hypnosis.

In the University of Durham, Dr G. D. Wassermann, Lecturer in Applied Mathematics at King's College, Newcastle-upon-Tyne, has received a grant from the College to carry out ESP experiments. Randomisation of targets will be effected by means of an electronic computer which he has constructed.

In the University of Nottingham another member, Dr S. C. Wallwork, a Lecturer in the Department of Chemistry, has been conducting ESP experiments in conjunction with the electro-encephalograph. In an attempt to see whether there is any correlation between successful calls or sequences of successful calls and changes in the character of the alpha rhythm, the subject makes his calls with the recording electrodes in position on his scalp. This type of experiment has its difficulties, one of them being the need to find subjects who can be relied upon to produce significant scores and who also have clearly defined alpha rhythms. The experiments, which followed a suggestion made by Dr J. R. Smythies, must be regarded as exploratory at this stage.

In September (as reported in the Supplement to the November 1951 issue of the *Journal*) an experiment in the electro-encephalography of mediumistic trance was carried out at 31 Tavistock Square. The Council are greatly indebted to Mrs Eileen Garrett for acting as subject and to the Edison Swan Electric Company for lending their latest 8-channel electro-encephalograph and a frequency analyser. This experiment, which is believed to be the first of its kind to be attempted in this country, may be of importance as a first step in a new approach to some of the problems of trance mediumship. A report appears in the *Journal* for March 1952. The magnetic tape-recorder used in this experiment was the gift of Mr T. E. Wood, to whom the Council wish to express their warmest thanks. This piece of equipment is indispensable for mediumistic sittings and has many other important uses.

In September Mr A. J. Garratt, then physicist to the Festival of Britain organization, and the staff of Ferranti Ltd kindly offered to place at the disposal of the Society for experimental purposes the 'Nimrod' electronic computer set up in the Science Exhibition in South Kensington. Mr Parsons was able to arrange two test sessions and was fortunate enough to secure the co-operation of Mrs Gloria Stewart, Dr R. H. Thouless, and Mrs Frank Heywood as subjects for ESP and PK tests. The computer was specially modified by the Ferranti engineers and provided a very convenient and attractive method of presenting a random series of symbols. Unfortunately, no significant results were obtained, and it proved impossible to arrange further sessions before the Exhibition closed.

The Officers of the Society receive from time to time requests, sometimes very urgent ones, to investigate poltergeists and other phenomena of haunting at houses in various parts of the country. So far as is practicable they meet all these requests in cases where there is any *prima facie* reason to suppose that there is anything worth investigating. It is not, however, always recognized by the persons making these requests that compliance with them may involve several visits to a distant place or of continuous residence on the spot by an experienced investigator, and that this makes a very great demand on the time of the investigator and is in present conditions very expensive. Notwithstanding these difficulties, a number of cases have been investigated, but none of sufficient interest to warrant a formal report. The Council wish to thank members of the Society who have shown themselves willing to co-operate in undertaking investigations in their own districts.

Many members will have read in the press about a poltergeist case in West Norwood during July. Mr Augustus Greenfield and his family were disturbed by poltergeist phenomena over a period of about five weeks. The case, which was in many ways typical, was ably followed up for the Society by Mr Thomas Greenwell, a member who lives in the neighbourhood. We are also indebted to Mr Philip Paul, a journalist, who provided some additional notes on the case.

In the psychic press much publicity has been accorded to the Super Ray machine of the Spirit Electronic Communication Society, which

has its headquarters in Manchester. Under the influence of the Super Ray, it is claimed, many people fall into trance and are controlled by spirit entities. The nucleus of the apparatus is a glass-enclosed coil said to have been invented by a Dutchman named Zwann. Mr Alan Mitchell, a Bournemouth member of the Society with a considerable knowledge of electronics, reports that the device would not be expected to produce any physiological effect. Another electrical engineer has confirmed this. Dr P. H. Plesch kindly reported on the device in action in Manchester. It had no effect on him personally, but he witnessed its effect on others. He was not impressed, and there was no way of distinguishing the effect of the machine from the influence of suggestion.

An Oxford County Engineer, Mr George De La Warr, received some publicity during the year in connexion with his claim to have designed a camera which photographs the past. It is said to be based on the familiar idea that every animal, vegetable, and mineral substance has a specific radiation. Mrs Oliver Gatty, a member of the Council, made contact with Mr De La Warr, but owing to the cloak of secrecy which surrounds his laboratory she was not able to make a full investigation.

Much publicity has lately been given in the press to psychic healing. At the suggestion of the late Mr Abdy Collins, efforts have been made to conduct an inquiry into the healing powers of Mr Harry Edwards, the well-known spiritualist healer. At the instance of Mrs Goldney, the Organizing Secretary, Dr Louis Rose, a London psychiatrist, has agreed to carry out an investigation. This must be considered a long-term project.

Two large meetings for platform clairvoyance from a well-known medium were held on the Society's premises during the year in test conditions. These involved a great deal of preliminary work and subsequent analysis, and it is hoped that a report will be published later.

A great part of the Society's work throughout its history has depended on the co-operation of persons appearing to have supernormal powers of some kind, and willing to have those powers investigated under such conditions as will exclude the sources of error that experience has shown to be prevalent in the absence of proper precautions. Notwithstanding widespread inquiries and strenuous efforts, the Council have not recently been fortunate in finding suitable subjects for investigation in many of the traditional branches of psychical research.

In the Spring Dr Soal, accompanied by Mrs Soal, visited the United States and spent some weeks at Duke University, where he worked on an inquiry into 'position effects' in Mrs Stewart's data (see report in the *Journal of Parapsychology*, September 1951). Dr Soal delivered lectures at Yale University (Wooderson Foundation Lecture), Pittsburg University (at a meeting sponsored by Sigma Xi); the College of the City of New York, Psychology Dept.; the American Society for Psychical Research, New York; the All Universities Psychology Club at Boston University; the Washington Society for Psychical Research; Duke University; and the University of North Carolina (Statistical Dept.).



Members of the Society will be interested to learn that since August Dr Soal has been engaged on a book entitled *Modern Experiments in Telepathy* to be published under the authorship of himself and Mr F. Bateman by Faber and Faber Ltd.

In May Mr J. Fraser Nicol, a member of the Council, joined the staff of the Parapsychology Laboratory at Duke University. The Council consider that Dr Soal's visit and Mr Fraser Nicol's work at Duke University are likely to further the very fruitful collaboration that has always prevailed between British and American psychical researchers.

Another officer of the Society who visited the Parapsychology Laboratory was Mr Edward Osborn, Editor of the *Journal*, who spent a few days at Duke University in June while on a short visit to the United States.

### 3. FINANCE

The accounts show that the total excess of income over expenditure of the three Funds (Blennerhassett, Research, and General) taken together is £869. This is in spite of the fact that salaries show a considerable increase over the year, owing to the appointment of Miss Nangie (part-time) and the honorarium now being paid to the Editor of the *Journal*. It must, however, be borne in mind that expenditure on printing has been lower than usual, owing to the issue of fewer publications during the year, and that there has been little demand on the Research Fund. Further, the Society may have to pay some £600 in death duties on the gift of £1000 made by the late Lord Rayleigh a few years ago. A further anxiety is the question of the Society's future premises when the present lease runs out in five years time.

The Society has been fortunate enough to receive several bequests and gifts during the year. Among these may be mentioned a legacy of £300 from the estate of the late Miss May Walker, and a most generous gift of £100 from a member who wishes to remain anonymous.

The Council wish to express their warm appreciation of the donations which many members regularly make in addition to the annual subscription. In view of the fact that the subscription has not once been increased during the past seventy years, such gifts are a valuable addition to the Society's revenue. The Council earnestly hope that more members will find it possible to make donations of this kind, however small.

### 4. THE PRESS AND BROADCASTING

More space than usual was devoted to articles on psychical research in the popular press. In May and June a series of articles under the heading of 'The Sixth Sense' was published by the London evening newspaper the *Star*. The articles were by a *Star* features writer, Merrick Winn, who is a member of the Society, and technical advice was given by Mr Denys Parsons. The subjects covered were telepathy,

clairvoyance, apparitions, psycho-kinesis, and precognition. Spiritualism and mediumistic phenomena were not included. Although the articles had to be popular and entertaining in character, the author must be commended for avoiding the sensational note which is so often associated with popular articles on our subject. The articles were followed by publication of spontaneous cases from readers' letters extending over six weeks, one guinea being paid to the writer of each letter published. About 2,000 letters were received by the *Star*, and about 800 were published. A few cases which appeared promising were followed up by members, but with disappointing results. Mr Winn's articles were also printed by a number of provincial newspapers.

Towards the end of the year, the *Sunday Chronicle*, the *News Chronicle*, the *Sunday Pictorial*, and *Reynolds News* each published several articles on the more sensational kinds of alleged paranormal phenomena. In the United States, the mass-circulation magazine, *Collier's*, printed an article by Robert Musel rather inappropriately entitled 'Can Man Foresee the Future?' This was based on material supplied by the Society, but contained inaccuracies and was accompanied by drawings which can scarcely be said to have contributed to an understanding of the subject.

Psychical research was the subject of several B.B.C. broadcasts during the year. On 27 July the Home Service programme included a discussion on telepathy, previously broadcast in the Midland Home Service, the speakers including Dr S. G. Soal, Dr R. H. Thouless, and Professor A. C. Hardy, all of whom are members of the Council. On 3 September Mrs K. M. Goldney gave a broadcast in the 'Books and People' programme of the 'London Calling Asia' service of the B.B.C. It was a general talk on psychical research, linked with a review of Paul Tabori's book *Harry Price : the biography of a ghost-hunter*. On the 19th and 26th of the same month Mr Antony G. N. Flew, Lecturer in Philosophy at King's College, Aberdeen, and a member of the Society, gave two talks in the Third Programme on 'The Significance of Parapsychology' (published in the *Listener* of 27 September and 4 October). During the summer Mr W. H. Salter took part in a programme, broadcast in the Midland Home Service, marking the centenary of the birth of Sir Oliver Lodge.

Psychical research also featured in the B.B.C's Television Service. On 11 July Dr J. Bronowski and Anthony Barnett devoted the fourth of a series of programmes called 'Enquiry into the Unknown' to one entitled 'Telepathy : is it fact or fake?' This included a demonstration, in which Mrs Gloria Stewart acted as percipient and Mr Denys Parsons as agent, of one of the methods used in properly conducted ESP experiments.

## 5. THE PRESIDENCY

Dr S. G. Soal was re-elected President for a second year of office.

# INCOME AND EXPENDITURE ACCOUNTS FOR THE YEAR ENDED 31 DECEMBER 1951

## FREDERIC W. H. MYERS MEMORIAL FUND

INTEREST ON INVESTMENTS (Gross) - - - - -	£31 6 11	1950 £31
SALES OF LECTURES - - - - -	27 6 8	10
<b>TOTAL INCOME</b> - - - - -	<b>£58 13 7</b>	<b>£41</b>
<b>EXPENDITURE :</b>		
Tenth Myers Memorial Lecture - - - - -	115 4 11	38
<b>EXCESS OF EXPENDITURE OVER INCOME FOR YEAR</b> - - - - -	<b>£56 11 4</b>	<b>£3</b>

## BLENNERHASSETT RESEARCH FUND

INTEREST ON INVESTMENT (Nett) - - - - -	£24 7 2	£25
INCOME TAX RECOVERABLE - - - - -	20 19 2	20
<b>TOTAL INCOME</b> - - - - -	<b>£45 6 4</b>	<b>£45</b>
<b>EXPENDITURE :</b>		
Postages - - - - -	5 6	
<b>EXCESS OF INCOME OVER EXPENDITURE FOR YEAR</b> - - - - -	<b>£45 0 10</b>	<b>£45</b>

## RESEARCH ENDOWMENT FUND

INTEREST ON INVESTMENTS (Nett) - - - - -	£410 11 4	£413
INCOME TAX RECOVERED AND RECOVERABLE - - - - -	111 14 0	110
<b>TOTAL INCOME</b> - - - - -	<b>£522 5 4</b>	<b>£523</b>
<b>EXPENDITURE :</b>		
Travelling and General Research Expenses - - - - -	48 6 1	46
Grant for Dr Betty Humphrey's Visit - - - - -		150
Award to P. H. Marsh for ESP Experimental Work - - - - -		15
Contributions towards General Fund :		
Rent - - - - -	62 10 0	125
Secretarial Assistant - - - - -	34 10 0	
Sittings with Mediums - - - - -		3
Sundry Expenses - - - - -	1 0 5	1
Grant to A. T. Oram - - - - -	10 0 0	
	<b>£156 6 6</b>	<b>340</b>
<b>EXCESS OF INCOME OVER EXPENDITURE FOR YEAR</b> - - - - -	<b>£365 18 10</b>	<b>£183</b>

## GENERAL FUND

<b>SUBSCRIPTIONS :</b>		
Members 1950 - - - - -	112 8 6	
1951 - - - - -	1,394 5 11	
	<b>£1,506 14 5</b>	
Associates 1949 - - - - -	1 15 1	
1950 - - - - -	10 16 11	
1951 - - - - -	94 4 0	
	<b>£1,613 10 5</b>	<b>£1,751</b>
PROPORTION OF LIFE MEMBERS' SUBSCRIPTIONS - - - - -	52 10 0	152
INTEREST ON INVESTMENTS - - - - -	225 0 1	226
INTEREST ON BANK DEPOSIT ACCOUNT - - - - -	3 8 5	
INCOME TAX RECOVERED AND RECOVERABLE - - - - -	55 0 3	54
CONTRIBUTION TOWARDS RENT - - - - -	62 10 0	125
CONTRIBUTION TOWARDS SECRETARIAL ASSISTANT'S SALARY - - - - -	34 10 0	
MOBULET RENT - - - - -	162 10 0	100
<b>TOTAL ORDINARY INCOME</b> - - - - -	<b>£2,208 19 2</b>	<b>£2,408</b>
<b>DONATIONS (Annual)</b>		
Life Members - - - - -	£20 17 11	
Members - - - - -	219 7 6	
Associates - - - - -	14 8 0	
	<b>254 13 5</b>	<b>317</b>
DONATIONS as per Schedule - - - - -	104 11 6	83
LEGACY - - - - -	300 0 0	225
PROCEEDS OF PIPER FUND - - - - -	—	325
DEPOSIT OF DEPOSIT FOR PURCHASE OF LIBRARY BOOKS - - - - -	—	63
SALE OF BOOKS AND WASTE PAPER - - - - -	3 2 9	4
FEES FROM PRESS, FILM COMPANY AND PUBLISHERS - - - - -	74 1 8	
<b>TOTAL INCOME</b> - - - - -	<b>£2,945 8 6</b>	<b>£3,425</b>

INCOME AND EXPENDITURE ACCOUNTS (cont'd)

TOTAL INCOME (brought forward) - - - - £2,945 8 6

1950  
£3,425

Less EXPENDITURE :

OFFICE EXPENSES :

Rent - - - - -	£240 0 0
Rates - - - - -	65 15 7
Insurance - - - - -	24 9 6
Fuel and Lighting - - - - -	110 6 5
Salaries (Secretary, Organizing Secretary, Secretarial Assistant, and Editor of <i>Journal</i> ) - - - - -	721 0 0
Clerical Assistance - - - - -	45 4 11
Cleaning and Caretaker's Wages & Uniform - - - - -	210 4 9
National Insurance - - - - -	23 7 5
Stationery and General Printing - - - - -	127 2 10
Telephone - - - - -	23 2 0
Postages - - - - -	158 14 6
Repairs - - - - -	170 7 5
Sundry Expenses - - - - -	22 7 7

£1,942 2 11

£240
67
18
109
533
74
197
20
159
25
150
143
27
£1,762

COST OF PRINTING AND BINDING PUBLICATIONS :

<i>Proceedings</i> - - - - -	128 14 1
<i>Journal</i> - - - - -	555 10 9
Binding - - - - -	26 16 1
Other Books and Pamphlets - - - - -	5 5 0

£716 5 11

165
490
1
126

306 7 5

£781  
336

£409 18 6

445

Less Proceeds of Sales - - - - -

EXPENSES OF MEETINGS - - - - -	12 0 1
PENSION—MISS I. NEWTON - - - - -	—
AUDIT FEE - - - - -	26 5 0
LIBRARY BOOKS AND BINDING - - - - -	40 16 7
LEGAL EXPENSES - - - - -	5 10 0
TRAVELLING EXPENSES - - - - -	—

41
27
21
49
15
13

TOTAL ORDINARY EXPENDITURE - - - - - £2,436 13 1

DEPRECIATION OF FURNITURE, ETC. - - - - - 50 18 0

£2,373

TOTAL EXPENDITURE - - - - -

£2,487 11 1 £2,373

EXCESS OF INCOME OVER EXPENDITURE -

£457 17 5 £1,052

BALANCE SHEETS—31 DECEMBER 1951  
FREDERIC W. H. MYERS MEMORIAL FUND

CURRENT ASSETS:

CASH AT BANK - - - - -	£44 9 10	£101
INVESTMENTS AT BOOK VALUE as per Schedule - - - - -	1,106 17 6	1,107
(Market Value at 31 December 1951 = £855 18 9)		
	£1,151 7 4	£1,208

CAPITAL AND ACCUMULATED INCOME:

Balance at 1 January 1951 - - - - -	1,207 18 8
Less Excess of Expenditure over Income for the year to date as per Income and Expenditure Account - - - - -	56 11 4
	£1,151 7 4
	£1,208

BLANNERHASSETT RESEARCH FUND

CURRENT ASSETS:

CASH AT BANK - - - - -	£118 0 8	£39
INCOME TAX RECOVERABLE - - - - -	10 15 3	10
DEBTOR - - - - -	—	35
INVESTMENT AT BOOK VALUE as per Schedule - - - - -	1,510 0 0	1,510
(Market Value at 31 December 1951 = £1,165 0 1)		
	£1,638 15 11	£1,594

CAPITAL AND ACCUMULATED INCOME:

Balance at 1 January 1951 - - - - -	£1,593 15 1
Add Excess of Income over Expenditure for year to date as per Income and Expenditure Account - - - - -	45 0 10
	£1,638 15 11
	£1,594

BALANCE SHEETS (*cont'd*)  
RESEARCH ENDOWMENT FUND

1950

**CURRENT ASSETS:**

CASH AT BANK AND IN HAND	-	-	-	-	-	-	-	£1,216 7 8	£791
INCOME TAX RECOVERABLE	-	-	-	-	-	-	-	57 7 2	54
INVESTMENTS AT BOOK VALUE as per Schedule	-	-	-	-	-	-	-	17,643 10 4	17,644
(Market Value at 31 December 1951 = £13,943 12 3)									
								£18,917 5 2	£18,489
Less Amount owed to General Fund	-	-	-	-	-	-	-	97 0 0	-
								<u>£18,820 5 2</u>	<u>£18,489</u>

**CAPITAL AND ACCUMULATED INCOME:**

Balance at 1 January 1951	-	-	-	-	-	-	-	18,454 6 4	
Add Excess of Income over Expenditure for year to date as per Income and Expenditure Account	-	-	-	-	-	-	-	365 18 10	
								<u>£18,820 5 2</u>	18,454
CREDITOR	-	-	-	-	-	-	-		35
								<u>£18,820 5 2</u>	<u>£18,489</u>

GENERAL FUND

**FIXED ASSETS:**

OFFICE FURNITURE, ETC. at cost less sales	-	-	-	-	-	-	-	£144 13 3	
(Purchase prior to 31 December 1946 remaining unvalued)									
Additions during year:									
Furnishings, etc.	-	-	-	-	-	-	-	271 12 10	
Recording Machine	-	-	-	-	-	-	-	92 13 6	
								<u>508 19 7</u>	
Less Depreciation	-	-	-	-	-	-	-	50 18 0	
								<u>£458 1 7</u>	£145
LIBRARY BOOKS AND EXPERIMENTAL APPARATUS	-	-	-	-	-	-	-	Not valued	
								<u>£458 1 7</u>	£145
								<u>£458 1 7</u>	£145

**CURRENT ASSETS:**

CASH AT BANK AND IN HAND	-	-	-	-	-	-	-	£1,619 2 0	£1,347
NOT YET RECEIVED FOR SALES OF PUBLICATIONS	-	-	-	-	-	-	-	10 15 6	25
AMOUNTS OWING BY RESEARCH ENDOWMENT FUND	-	-	-	-	-	-	-	97 0 0	
INCOME TAX RECOVERABLE	-	-	-	-	-	-	-	35 8 7	34
PAYMENTS IN ADVANCE	-	-	-	-	-	-	-	12 9 9	12
INVESTMENTS AT BOOK VALUE as per Schedule	-	-	-	-	-	-	-	9,585 12 10	9,556
(Market Value at 31 December 1951 = £8,179 1 0)									
								<u>£11,360 8 8</u>	<u>£11,004</u>

**Less CURRENT LIABILITIES:**

LIFE MEMBERSHIP ACCOUNT: Balance at 1 January 1951	-	-	-	-	-	-	-	£1,176 0 0	
Add Life Subscriptions received during year to date	-	-	-	-	-	-	-	84 0 0	
								<u>1,260 0 0</u>	
Less Transfer to Income & Expenditure Account Balance, being 50% of existing Life Members' Subscriptions	-	-	-	-	-	-	-	52 10 0	
								<u>1,207 10 0</u>	1,176
ANNUAL SUBSCRIPTIONS AND DONATIONS RECEIVED IN ADVANCE	-	-	-	-	-	-	-	118 10 6	92
SUNDRY CREDITORS	-	-	-	-	-	-	-	72 14 3	12
								<u>9,961 13 11</u>	<u>1,280</u>
								<u>£1,398 14 9</u>	
TOTAL NET ASSETS	-	-	-	-	-	-	-	<u>£10,419 15 6</u>	<u>£9,869</u>

**CAPITAL:**

GENERAL FUND: Balance at 1 January 1951	-	-	-	-	-	-	-	£9,051 8 4	
Add Excess of Income over Expenditure for year to date as per Income & Expenditure Account	-	-	-	-	-	-	-	457 17 5	
Transfer from Income Reserve Fund towards cost of furnishings	-	-	-	-	-	-	-	264 8 10	
Contribution from T. E. Wood for purchase of Recording Machine	-	-	-	-	-	-	-	92 13 6	
								<u>£9,866 8 1</u>	

BALANCE SHEETS (*cont'd*)

Brought forward - - - -	£9,866	8	1		1950
Less Transfer to Income Reserve Fund being Legacy and Anonymous Donation - - - -	400	0	0	£9,466	8 1
INCOME RESERVE FUND :					
Balance at 1 January 1951 - - - -	633	6	3		
Add Transfer from General Fund - - - -	400	0	0		£9,051
	<hr/>				
Less Transfer to General Fund - - - -	£1,033	6	3		
	264	8	10		
	<hr/>				
		£768	17	5	£633
SPECIAL DONATIONS :					
Balance Unexpended - - - - -		184	10	0	185
		<hr/>			
		£10,419	15	6	£9,869
		<hr/>			

*Note.* No Account has been taken of Post War Credits for Income Tax amounting to £2 15 0.

REPORT OF THE AUDITORS TO THE MEMBERS OF THE INCORPORATED SOCIETY FOR PSYCHICAL RESEARCH

We have obtained all the information and explanations which to the best of our knowledge and belief were necessary for the purposes of our audit. In our opinion proper books of account have been kept by the Society so far as appears from our examination of those books. We have examined the above Balance Sheet and annexed Income and Expenditure Accounts which are in agreement with the books of account. In our opinion and to the best of our information and according to the explanations given us the said Accounts give the information required by the Companies Act, 1948, in the manner so required and the Balance Sheet gives a true and fair view of the state of the Society's affairs at 31 December 1951, and the Income & Expenditure Accounts give a true and fair view of the Society's revenue transactions during the year ended on that date. We have also verified the investments of the General, Research Endowment, Myers Memorial, and Blennerhassett Funds.

MIALL, HARPER & Co.  
*Chartered Accountants.*

9 Idol Lane, Eastcheap, London, E.C. 3.  
14 February 1952.

DONATIONS

Denys Parsons - - - - -	£2	2	0
W. H. Salter - - - - -		2	9 6
Anonymous - - - - -	100	0	0
	<hr/>		
	£104	11	6
	<hr/>		

## SCHEDULE OF INVESTMENTS

	Nominal	Book Value	Value 31 December 1951
<b>MYERS MEMORIAL FUND</b>			
3½% Conversion Stock 1961 - - -	£250 0 0	£287 10 0	£204 7 6
3% Savings Bonds 1960/70 - - -	750 0 0	819 7 6	651 11 3
		<u>£1,106 17 6</u>	<u>£855 18 9</u>
<b>BLENNERHASSETT RESEARCH FUND</b>			
British Transport 3% Guaranteed Stock 1978/88 - - - - -	1,510 10 10	£1,510 0 0	£1,165 0 1
		<u>£1,510 0 0</u>	<u>£1,165 0 1</u>
<b>RESEARCH ENDOWMENT FUND</b>			
4% Consolidated Stock - - -	1,460 0 0	£1,699 1 6	£1,310 7 0
3% Funding Stock 1959/69 - - -	2,300 0 0	2,504 2 6	2,009 12 6
3½% War Stock 1952 or after - - -	800 0 0	864 0 0	645 0 0
British Transport 3% Guaranteed Stock 1978/88 - - - - -	6,626 1 5	6,472 17 11	5,110 7 0
British Transport 3% Guaranteed Stock 1967/72 - - - - -	514 0 0	539 14 0	416 6 10
3% Defence Bonds - - - - -	600 0 0	670 5 0	597 15 0
3% Savings Bonds 1960/70 - - -	3,157 9 2	3,449 9 5	2,743 0 10
Australia 2½% Stock 1967/71 - - -	995 2 9	1,040 10 0	801 1 8
2½% Consolidated Stock - - - - -	504 3 8	403 10 0	310 1 5
		<u>£17,643 10 4</u>	<u>£13,943 12 3</u>
<b>GENERAL FUND</b>			
4% Consolidated Stock - - - - -	86 11 11	£100 15 4	£77 14 5
York 3% Redeemable Stock 1955/65 - - -	800 0 0	848 0 0	716 0 0
Australia 3% Stock 1955/8 - - - - -	250 0 0	262 10 0	240 0 0
Nigeria 3% Stock 1955 - - - - -	1,200 0 0	1,272 0 0	1,197 0 0
East India Railway Deferred Annuity Class D - - - - -	23 8 0	590 17 0	511 17 6
3% Savings Bonds 1960/70 - - - - -	3,000 0 0	3,240 2 3	2,606 5 0
3% Savings Bonds 1955/65 - - - - -	1,664 12 4	1,703 10 3	1,525 4 1
Australia 3½% Registered Stock 1965/9	1,500 0 0	1,567 18 0	1,305 0 0
		<u>£9,585 12 10</u>	<u>£8,179 1 0</u>

## 6. MEMBERSHIP OF THE COUNCIL

During the year Professor A. C. Hardy, F.R.S., and Mr G. W. Fisk have been co-opted members of Council. The Council have renewed the co-optation of Mrs Goldney, having been advised that her position as Organizing Secretary did not debar her from membership of the Council.

## 7. OBITUARY

The Council regret to record the death of several Members and Associates of long standing, some of whom had taken an active part in the Society's work, in particular, Lady Clwyd, Mr Abdy Collins, Miss E. B. Gibbes, Nina Duchess of Hamilton, Mr J. Arthur Hill, and Miss Hermione Ramsden.

## 8. MEMBERSHIP OF THE SOCIETY

DURING the year 82 Members, 9 Student-Associates, 1 Corresponding Member, and 1 Hon. Associate were elected. The total loss in membership from deaths, resignations, etc., was 108, resulting in a net decrease of 17 in the total membership, which now, including Hon. and Corresponding Members (16) and Hon. Associates (12), stands at 985.

## 9. THE STAFF

At the last Annual General Meeting reference was made to the need for additional staff. In June the Council engaged Miss Ava Nangie, M.A., for part-time work. Miss Nangie, who took a first-class honours degree in modern and mediaeval languages at Cambridge, was formerly on the staff of the International Institute of African Languages and Cultures and of the Medical Research Council. Since her appointment she has been assisting both in the research and the general secretarial work of the Society.

## 10. PUBLICATIONS

Part 179 of *Proceedings*, containing Dr Thouless's paper 'A Report on an Experiment on Psycho-kinesis with Dice', was published in February.

DR Soal's Presidential Address, delivered on 3 November 1950, was published in April 1951 as Part 180 of *Proceedings*. The tenth Myers Memorial Lecture, which was given by Professor Rhine on 10 May 1950 under the title 'Telepathy and Human Personality', was also published during the year.

Six numbers of the *Journal* and three Supplements were issued during the year.



II. THE LIBRARY

DURING the year the number of books borrowed by Members and Associates was 670, and 85 books were borrowed by the National Central Library.

12. MEETINGS

- 8 Feb. 'The Fatima Case' by the Rev. C. C. Martindale, S.J.  
1 Mar. 'Some Noteworthy Incidents in my Sittings with Mrs Leonard' by the Rev. C. Drayton Thomas.  
15 Mar. 'Is Psychological Research a Rational Subject?' by Mary Scrutton, M.S.  
5 April 'Psychical Research and the Press' by H. J. D. Murton.  
19 April 'Some New Techniques in ESP Research' by James Hayes.  
3 May 'A New Theory of Some ESP Effects' by D. J. West, M.B., Ch.B.  
24 May 'Spontaneous Phenomena in Modern Psycho-analytic Technique' by Alice E. Buck, M.D.  
14 June 'Astrology' by John Addey.  
5 July 'Seeing Apparitions' by Phoebe Payne.  
27 Sept. The President, Dr S. G. Soal, on his Recent Visit to Duke University.  
11 Oct. 'Psychical Experiences in Central Africa' by Matthew McKay.  
13 Dec. 'The Investigation of Haunts and Spontaneous Cases' by Professor F. J. M. Stratton, F.R.S.

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MEETINGS OF THE COUNCIL

- 469th 27 Sept. 1951 Chairman : The President, Dr S. G. Soal.  
470th 16 Nov. 1951 Chairman : The President, Dr S. G. Soal.  
471st 13 Dec. 1951 Chairman : The President, Dr S. G. Soal.  
472nd 24 Jan. 1952 Chairman : The President, Dr S. G. Soal.

MEETINGS OF THE SOCIETY

- 246th Thursday, 13 December 1951, at 6.30 p.m. PROFESSOR F. J. M. STRATTON : 'The Investigation of Haunts and Spontaneous Cases'.  
247th Wednesday, 16 January 1952, at 6.30 p.m. DR J. A. HADFIELD : 'The Biological Function of Dreams'.  
248th Tuesday, 12 February 1952, at 6.30 p.m. ANTONY G. N. FLEW : 'Minds and Mystifications'.

## NEW MEMBERS

## MEMBERS

*(Elected 27 September 1951)*

- BOWDLER, DR W. A., Leadon Court, Fromes Hill, Ledbury, Herefordshire.
- CHARI, C. T. K., M.A., Staff Bungalow, Madras Christian College, Tambaram, S. India.
- GARBUTT, G. F., 14 Palace Gardens, Enfield, Middx.
- HADFIELD, DR J. A., 4 Upper Harley Street, London, N.W. 1.
- JORDAN, WILLIAM K., M.D., Dept of Neurology, School of Medicine, University of Arkansas, Arkansas, U.S.A.
- SCHWARTZ, DR EMANUEL K., 65 East 76th Street, New York 21, U.S.A.
- SHALLIKER, J., 272 Colne Road, Burnley, Lancs.
- SMITH, REV. A. HANDEL, 2 Maxey Road, Helpston, Peterborough, Northants.

*Student-Associate*

- Richards, Douglas S., 204 Rugby Road, Binley, nr Coventry.

## MEMBERS

*(Elected 16 November 1951)*

- BRANDSTETTER-KLAUSNER, MRS M., 144 Hayarkoustreet, Tel-Aviv, Israel.
- CAMPBELL, J. G., B.A., M.Sc., University of Melbourne, Melbourne, N. 3, Victoria, Australia.
- CANETTI, ELIAS, D.Phil., 14 Crawford Street, London, W. 1.
- CHITTY, ERNEST E., 65 Salisbury Road, Dover, Kent.
- CLOVER, ALBERT E., 36 Churchill Avenue, Bendigo, Victoria, Australia.
- CRAWFORD, MERWIN R., 36 East Fourth Street, Cincinnati, Ohio, U.S.A.
- GILMOUR, W. D., B.A. Oxon., 27 Heath Drive, Gidea Park, Romford, Essex.
- HEWAN, T. A. D., Whins, Hook Heath Avenue, Woking, Surrey.
- IVENS, M. W., 26 Nevern Place, London, S.W. 5.
- MATHESON, J. D., B.Sc., 15 High Street, Invergordon, Ross and Cromarty.
- MAYNE, ALAN J., B.A., B.Sc.
- PHILLIPS, N., 10 Linnell Drive, London, N.W. 11.
- RICHARDSON, W. R., 28 Meadway, Barnet, Herts.
- SCHUMACHER, E. F., Holcombe, Weald Way, Caterham, Surrey.
- VINOD, D. G., M.A., Ph.D., 864 Sadashiv, Poona, India.
- WILLIAMS, MISS J., 400 Ferry Road, Edinburgh 5.

*Student-Associates*

Brett, M. J., 18 Salisbury Road, Banstead, Surrey.  
Freeman, M. A. R., 27 Fountain House, Park Lane, London, W. 1.

MEMBERS

*(Elected 13 December 1951)*

BANNISTER, R. T., M.B., B.S., Trelorgan, Lidden, Penzance.  
HIMES, G. H., Post Office Box 472, Las Vegas, Nevada, U.S.A.  
KEATING, MRS G. M., 113 Byng Drive, Potters Bar, Middx.  
KERR, T. H., B.Sc., 22 Birchett Road, Cove, Farnborough, Hants.  
LIBRARIAN, NATIONAL LIBRARY SERVICE, Wellington, New Zealand.  
LITTLE, J. G., B.Sc., 33 Dorchester Way, Kenton, Harrow, Middx.  
VAN DER MAAS, C. J., Netherlands Consultat-General, van Meteren-  
kaai 4, Antwerp, Belgium.

*(Elected 24 January 1952)*

BROWN, ERNEST R., 36 Dale View Crescent, Chingford, London, E. 4.  
BROWN, SLATER, Sherman, Conn., U.S.A.  
FARRAR, W. V., B.Sc., Ph.D.(Lond.), 75 High Street, Manchester 13.  
HAMER, J. E., 67 Revelon Road, London, S.E. 4.  
LEADLEY-BROWN, Miss C., M.B.E., 18 Devonshire Road, Liverpool 8.  
LIBRARIAN, THE MEADVILLE THEOLOGICAL SCHOOL, 5701 Woodlawn  
Avenue, Chicago 37, Illinois, U.S.A.  
LIBRARIAN, STARR KING SCHOOL FOR THE MINISTRY, 2441 Le Conte  
Avenue, Berkeley 9, California, U.S.A.  
MACTAGGART, M., Farce, Tewin, Welwyn, Herts.  
MUSGRAVE, FLYING OFFICER W., R.A.F., 175 Coppins Road, Clacton-  
on-Sea, Essex.  
NELSON, REV. ROBERT A., 16 Kenilworth Park, Rathgar, Dublin, Eire.  
NORONA, DELF A., P.O. Box 344, Kingwood, West Virginia, U.S.A.  
SANDSTRÖM, INGE, M.A., 10B Banersgatan, Malmö, Sweden.  
SELBY, P., 79 Roundwood Way, Banstead, Surrey.  
TILFORD, JUDGE HENRY J., 2911 Lilac Way, Louisville, Kentucky,  
U.S.A.



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THE DIEPPE RAID CASE

A COLLECTIVE AUDITORY HALLUCINATION

INVESTIGATED BY G. W. LAMBERT, C.B., AND  
KATHLEEN GAY

ON the 4th August, 1951, two English ladies, Mrs Dorothy Norton (pseudonym) and her sister-in-law Miss Agnes Norton (pseudonym), while staying for a holiday at Puys (Puits) near Dieppe (France), had a remarkable experience in the nature of a collective auditory hallucination. In order to safeguard those concerned against unwanted publicity, the real names of the percipients and the exact address at which they were staying are not given in this report. For the sake of brevity the two percipients are referred to below as D. and A. respectively. Their ages at the time were thirty-two and thirty-three.

D., accompanied by her two young children and their nurse, arrived at Puys on the 26th July, 1951, A. having arrived on the preceding day, for a visit lasting till August 5th following. Puys was chosen for the holiday by D. and her husband during a three-day visit to Dieppe at Easter 1951. Neither percipient had any particular interest in what happened at Dieppe during the Second World War. Both had read in the press about the Dieppe Raid of the 19th August, 1942, at the time of its occurrence, but had not looked up the history of it in connexion with the visit. They have been friends since childhood, but had not previously been on a holiday together.

The percipients' Statements recording what they heard are reproduced below. They are based on notes taken during the experience, which, it will be seen, lasted nearly three hours, from about 4 a.m. till about 7 a.m. The Statements were prepared from the notes partly on the same day (4th August) and partly on the following day, before the party left for England. At D.'s suggestion the notes were written independently. The Statements were posted to the Society by D. with a covering letter dated 9th

August, written after her return. This letter merely described the circumstances in which the experiences took place, and inquired whether the Society had had any other reports of this kind. The writer of this report has seen the notes from which the Statements were prepared, and has checked the Statements against them.

At Puy D. and A. shared a bedroom on the second floor of a three-storey house facing towards the sea, which was about a quarter of a mile away, down a steep path. The house had, so they were informed, been used as quarters for German troops during the war. The nurse and two children, who heard nothing unusual, were in another bedroom on the same floor, two doors away. The times mentioned in the Statements were taken by D. and A. from their wrist watches. Both percipients agree that A.'s watch was keeping better time than D.'s, which was losing slightly. A. was in the Women's Royal Naval Service during the war, and became accustomed to the accurate recording of time. A.'s time is therefore to be preferred, where there is any discrepancy. Both watches were set to 'Single Summer Time' (one hour ahead of G.M.T.), which was also in use on the 19th August, 1942, for service purposes in the Allied Forces and for civil purposes in France. The weather was fine throughout the experience.

1. *Copy of statement by Mrs Dorothy Norton*

Saturday, August 4th, 1951

At 4.20 a.m. A. got up, and went out of the room. I said 'Would you like to put the light on?', but she didn't. She came back in a few minutes. She said, 'Do you hear that noise?' I had in fact been listening to it for about 20 minutes. I woke up before it started. It started suddenly and sounded like a storm getting up at sea. A. said she had also been listening to it for about 20 minutes. We lay in the dark for a little listening to the sound. It sounded like a roar that ebbed and flowed, and we could distinctly hear the sounds of cries and shouts and gunfire. We put the light on and it continued. We went out on the balcony where we could look down towards the beach, though we could not actually see the sea. The noise came from that direction and became very intense, it came in rolls of sound and the separate sounds of cries, guns and divebombing were very distinct. Many times we heard the sound of a shell at the same moment. The roaring became very loud. At 4.50 it suddenly stopped. At 5.5 a.m. it started again and once more became very intense, so much so that as we stood on our balcony, we were amazed that it did not wake other people in the house. By now it was getting light, cocks were crowing and birds were singing. We heard a rifle shot on the hill above the beach.

The sounds became more distinctly that of divebombers rather than the cries and shouts we had heard earlier, although we could still hear them. The noise was very loud and came in waves as before. It stopped abruptly at 5.40.

At 5.50 it started again but was not so loud and sounded more like planes. This died away at 6 a.m. At 6.20 the sound became audible again but it was fainter than before, and I fell asleep as I was very tired.

I was woken by a similar sound on Monday, July 30th, it sounded exactly the same only fainter and not so intense. At the end I seemed to hear a lot of men singing. It ended when the cocks started crowing and I went to sleep. My sister-in-law did not waken.

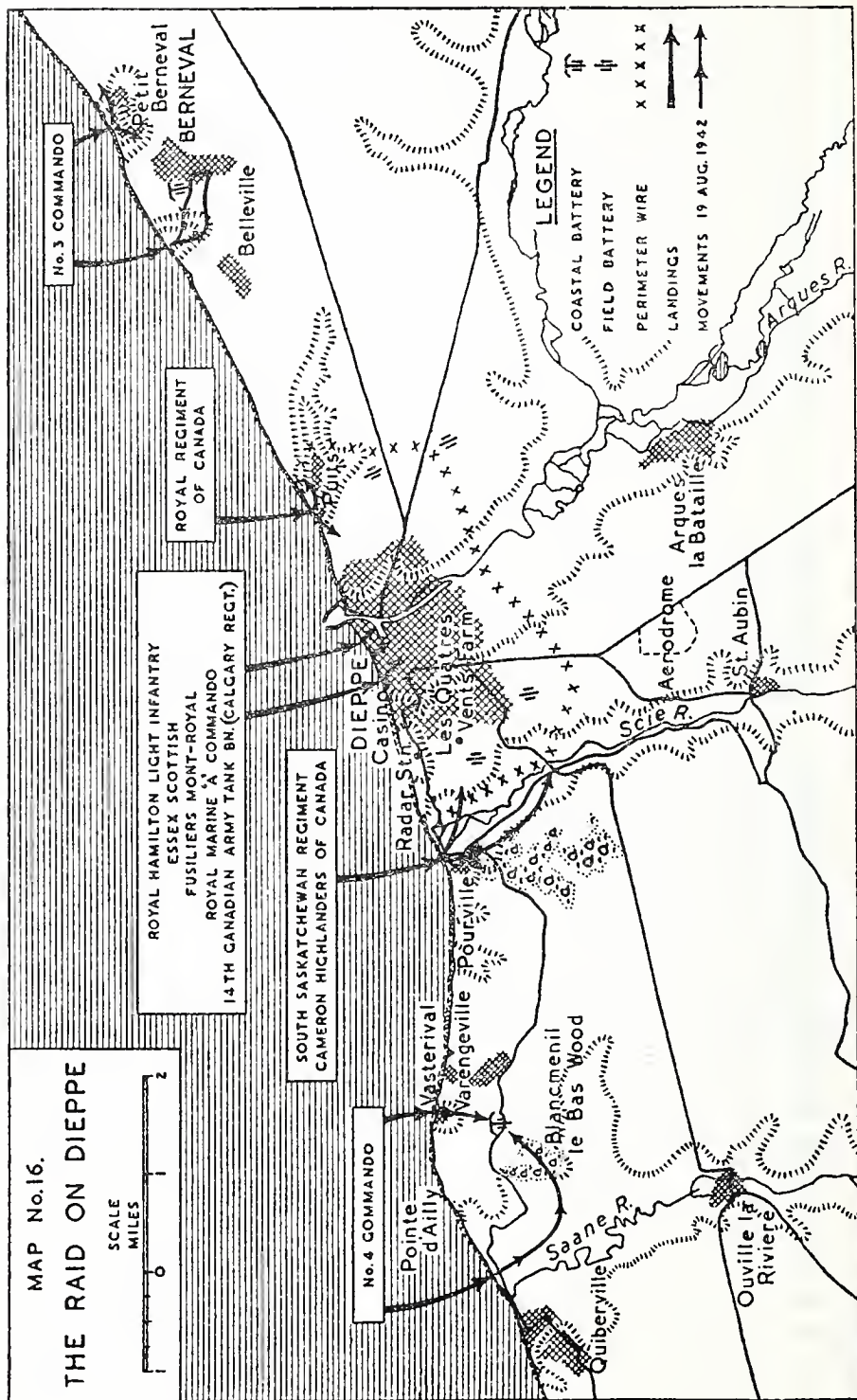
2. *Copy of statement by Miss Agnes Norton, sister-in-law of Mrs Dorothy Norton*

Saturday, August 4th

I woke in what I realized was very early morning although not yet dawn as no birds were singing. I was immediately aware of a most unusual series of sounds coming from the direction of the beach which were cries of men heard as if above a storm.

After listening for about 15 minutes I got up to leave the room and D. spoke to me and asked if I would like to put on the light which I did not in fact do. On my return I asked D. if she heard the noise too, and she said 'Yes', whereupon we put on the light and checked the time as 4.20 a.m. Our next move was out on to the balcony where the sounds intensified and appeared to me to be a mixture of gunfire, shell-fire, dive-bombers, landing craft and men's cries. All the sounds gave the impression of coming from a very long distance, i.e. like a broadcast from America in unmistakable waves of sound. At 4.50 a.m. all noise ceased abruptly and recommenced equally abruptly at 5.07 a.m. At 5.50 a.m. planes distinctly heard in large numbers and other fainter sounds dying away at 6 a.m. At 6.25 men's cries heard again growing gradually fainter and nothing at all heard after 6.55 a.m.

The remarkable feature of this case is the close correspondence between the times of the 'battle sounds' heard by the percipients on the 4th August, 1951, and the times of the actual battle sounds resulting from the operations on the 19th August, 1942. The correspondence is brought out in tabular form below, but the reader will be in a better position to appreciate the comparison if





he is first given a brief outline of the plan and course of the Raid. For fuller accounts, see the section on Dieppe in the volume *Norway—The Commandos—Dieppe*<sup>1</sup> by Christopher Buckley, which was published in January 1952, some five months after the experience of the two percipients, and *The Canadian Army 1939-45*<sup>2</sup> by Colonel C. P. Stacey.

At Dieppe, as will be seen from the accompanying map, the French coast runs, roughly speaking, east and west. The plan was first to land small forces by surprise at places on either side of Dieppe to destroy coastal batteries. These flank landings, as they were called, were directed to Puys, about 1½ miles east of Dieppe, and to Berneval, about 5 miles further east; also to Pourville, about 2½ miles west of Dieppe, and to Varengeville about 3 miles further west. All these flank landings were due to take place at 4.50 a.m., to be followed half an hour later, at 5.20, by the main landings at Dieppe itself. The main landings were to be preceded and covered by a bombardment of Dieppe by the destroyers accompanying the force, and by air attack.

In the event the flank landings west of Dieppe took place punctually. They met with so little opposition on the beaches that they were doubtless hardly heard at all at Puys, on the other side of Dieppe. On the east, the allied vessels closing on Berneval ran accidentally across a small protected German convoy at 3.47 a.m., when about seven miles from the French coast. Firing started immediately after between the opposing vessels, and the noise gave about an hour's warning to the Germans, anyhow on the east side of Dieppe. The accounts of the engagement do not show exactly when this firing stopped, but there is evidence that it lasted till after 4 a.m.

Meanwhile the vessels closing on Puys moved on, arriving 17 minutes later, i.e. at 5.07 instead of at 4.50. (This time, 5.07, has been established as more correct than 5.05, the time indicated for this landing in a report by the Naval Commander, Captain J. Hughes-Hallett, R.N., dated 30th August, 1942, published on 14th August, 1947, as a Supplement to the *London Gazette* of the 12th of that month. That report makes the time 15 minutes late on 4.50, i.e. 5.05.) The Germans, who had had time to man their beach defences, waited until the landing craft had almost touched down and then opened heavy fire on the disembarking Canadian troops. A second 'wave' went ashore a few minutes later, followed by a further landing at about 5.45. Those troops who succeeded in getting ashore were confronted by a high sea-wall which gave little shelter against a murderous enfilading fire from enemy pill-

<sup>1</sup> London, H.M. Stationery Office, 1952.

<sup>2</sup> Ottawa, The King's Printer, 1948.

boxes. Very few penetrated inland, and at 8.30 a.m. the survivors of those pinned to the beach surrendered. Buckley (p. 249) sums up the situation in these words :

In the space of two or three hours the Royal Regiment of Canada had been practically exterminated. Of 26 officers and 528 other ranks who had embarked for the operation on the previous night, all the officers and 496 other ranks became casualties. Two officers and 65 others, half of whom were wounded, were brought away ; no less than eight officers and 199 others were killed ; 16 officers and 264 others, wounded and unwounded, fell into German hands. . .

To the noise of machine-gun and rifle fire from positions commanding the beach there was added the sound of firing from a German howitzer battery, situated a few hundred yards south of Puits, which was directed at the allied shipping lying off the shore. The noise of the shells passing overhead must have been audible to persons in the house in which the percipients found themselves nine years later. German records show that the number of shells fired during the course of that morning by the battery in question was 550.

At 5.12 the destroyers accompanying the main force started to bombard Dieppe, preparatory to the landing there, and, just before that landing started, beginning at about 5.15, Hurricanes made a cannon-firing attack on the sea-front buildings. The landing itself was met by a heavy outburst of firing from the shore. At the same time (5.20) air cover was increased to its maximum, but the increasing noise of aircraft was probably masked for the time being by the naval gunfire.

At 5.40 the naval bombardment stopped, and the sudden drop in the amount of noise audible at Puits must have been very noticeable. On the other hand the sound of aircraft must have become much more pronounced, rising to a maximum at 5.50 a.m., when there was a change-over of the air cover, forty-eight fighters arriving from England to relieve an equal number due to return there. By that time, too, there was a considerable increase in the number of German aircraft overhead.

From 5.50 to 6.55 there was so much activity at several points of attack that one cannot estimate the result in the way of noise, as heard at Puits. It is only necessary to add that there was a considerable amount of noise after 6.55 (when the experience ended), including a good deal of gunfire from naval vessels covering the re-embarkation of allied troops.

In the following table the left-hand column divides the experience into phases (Roman numerals), and under each phase brings together corresponding extracts from the Statements of the two

percipients. The right-hand column shows the officially recorded times of relevant events at the same hour of the day on the 19th August, 1942, and cites sources published before the 4th August, 1951. After 6 a.m., as stated above, the increasingly complex pattern of events makes it hardly possible to continue the comparison. If the reader should feel that, even so, the amount of correspondence between the columns may have been exaggerated by 'unfair' selection of events for the right-hand column, his best recourse would be to read the recently published account of the Raid in *Norway—The Commandos—Dieppe*, bearing in mind that it was not accessible to the percipients, and to study the course of events against the wider background there presented.

## COMPARATIVE TABLE

*Percipients' Statements**Events on 19th August, 1942*

**I. About 4 a.m.** (i.e. about 20 minutes before 4.20).

D. 'It [the noise] started suddenly and sounded like a storm getting up at sea. . . . It sounded like a roar that ebbed and flowed . . . sounds of cries and shouts and gunfire.'

A. ' . . . unusual series of sounds coming from the direction of the beach which were cries of men heard as if above a storm.'

**II. 4.50 a.m.**

A. ' . . . all noise ceased abruptly.'

D. ' . . . suddenly stopped.'

**III. 5.07 a.m.**

A. ' . . . recommenced abruptly at 5.07 a.m.'

**3.47 a.m.** Assault vessels closing on Berneval ran across German convoy. Firing began immediately after, and went on until after 4 a.m.

*Note.* This time (3.47 a.m.) was published in several accounts, e.g. *Combined Operations*<sup>1</sup> and *The Green Beret*<sup>2</sup> by Hilary St G. Saunders (p. 104).

At Puits there was probably shouting by German soldiers manning the beach defences.

**4.50 a.m.** was zero hour for the flank landings, which, at Berneval and Puits, were delayed. There may have been silence at Puits at this stage.

Several published reports mention 4.50 as zero hour but there is nothing to suggest that silence fell just then.

**5.07 a.m.** The first wave of landing craft touched down at Puits in the face of heavy fire.

<sup>1</sup> London, H.M.S.O., 1943.

<sup>2</sup> London, Joseph, 1949.

## Percipients' Statements

Events on 19th August, 1942

D. 'At 5.05 a.m. it started again and once more became very intense. . .'

*Note.* This discrepancy of two minutes is discussed below.

D. 'The sounds became more distinctly that of dive-bombers rather than the cries and shouts we had heard earlier. . .'

## IV. 5.40 a.m.

D. 'It [the noise] stopped abruptly at 5.40.'

(*Note.* A. does not mention this.)

## V. 5.50 a.m.

D. 'At 5.50 it started again . . . and sounded more like planes.'

*Note.* This time was published by inference in *The Canadian Army* 1939-45 cited above. The landing of the first wave is recorded as having been 17 minutes late—i.e. 4.50 plus 17 minutes (p. 71). Buckley (p. 247) also puts the delay at 17 minutes. **5.12 a.m.** Destroyers started to bombard Dieppe.

**5.15 a.m.** Low-flying Hurricanes attacked the sea front buildings.

**5.20 a.m.** The landing of the main force at Dieppe began, in the face of heavy fire.

**5.40 a.m.** The naval bombardment of Dieppe stopped.

*Note.* A press correspondent in *The Times* of 21st August, 1942, stated that the bombardment continued for 20 minutes after the landing had begun (i.e. till 5.20 plus 20 minutes).

**5.50 a.m.** Forty-eight R.A.F. aircraft arrived from England (see p. 612). 'By now, shortly before 6 a.m. . . the noise of aeroplanes had risen to a constant drone, like a net of harsh sound under the sky.' (Same correspondent as under IV above.)

The only account of the Raid which was in the hands of the percipients at the time was one contained in a French guide book entitled *Dieppe*, a copy of which was handed to K. G. by D. They say that they knew of the existence of this account, but had not read it before the experience started. After the noise had begun, they read it on the balcony at about the middle of Phase III. It is therefore necessary to examine carefully passages in the account from which clock-times up to about 7 a.m. might have been obtained. They are as follows :

(1) Le 19 août 1942, la population côtière fut réveillée par une canonnade en mer, au large de Berneval ; à 5h.50, une multitude

d'avions se précipitaient sur Dieppe et ses environs, pendant que des flotilles sous la protection de destroyers s'approchaient des côtes et débarquaient en huit points différents de Sainte-Marguerite à Berneval, des contingents alliés comprenant près de 7000 hommes. (p. 30).

No reader not already acquainted with the facts would realize that the firing off Berneval referred to at the beginning of the above passage took place two hours before the time 5.50 mentioned immediately after the semi-colon. Nor, from the use of the expression 'pendant que' would he be able to infer that the flank landings had already taken place, and that the main landings were well advanced by 5.50.

(2) A Puys, par suite de la fausse manoeuvre d'une flotille, le débarquement fut retardé et fut effectué comme le jour se levait. (p. 41).

This statement is correct, but the reference to the time of the landing is too vague to account for the precise time 5.07 coming out in A.'s statement.

(3) L'attaque principale se produisit à Dieppe. A 5h.20, les destroyers alliés ouvraient le feu. (p. 41).

According to a naval report, it was at 5.12 that the destroyers opened fire. 5.20 was the zero hour for the landings at Dieppe, and they started within two or three minutes of that time.

(4) Vers 7 heures, une vingtaine de tanks du 'Calgary Regiment' . . . débarquèrent, sauf deux. . . qui furent coulés. (p. 43).

These tanks arrived in vessels which touched down between 5.35 and 6.05 and, apart from two which were sunk, were all on shore soon after 6 a.m.

It will be seen that the percipients' time-table could not have been got from the French account, except perhaps the time 5.50 for the sound of aircraft at Phase V. Both percipients are sure that they had never heard of the existence of any of the reports and accounts mentioned above, apart from the French guide book and anything they may have read in the press at the time of the Raid ; and certainly had never read the Hughes-Hallett report or the account in the Canadian volume. A. has also assured us that during her service in the Women's Royal Naval Service she was not in a position to see unpublished naval reports of operations. Between the 4th and 9th August, when D. wrote to the Society, neither percipient appears to have attempted to correlate the times given in her notes with actual battle times.

Even if both percipients had noticed before 5.50 a.m. the statement in the French guide book that a large number of aircraft flung themselves on Dieppe at that hour on the 19th August, 1942,

that knowledge would not ordinarily have caused hallucinatory hearing of aircraft noise by two persons at that particular time. It seems reasonable to attribute the 'appropriate sounds' heard at 5.50 to the same cause as those heard earlier at about 4 a.m. and at 5.07 a.m.

There are some details in the Statements which do not link up with any ascertainable facts about the Raid, and for that reason cannot be attributed to or checked by reference to accounts of the operation. For instance, there is nothing in the accounts to suggest that noise ceased suddenly at Phase II ; nor that noises died away at 6 a.m. ; nor, as A. says, that men's cries were particularly audible at 6.25. There is nothing inherently improbable in any of the statements, if they are read in relation to happenings audible at Puy at corresponding times during the Raid.

Phase III, which furnishes the most remarkable coincidence in the whole series, provides also a very curious puzzle. Both percipients are sure that they simultaneously heard the noise start again, but at this point there is a discrepancy of two minutes in their timing. They attribute this to D.'s watch having been two minutes slow on A.'s watch, which, as stated above, is more likely to have been right than D.'s. All other times noted by both percipients end in 5 or 0, and it may be that A. was reading times to the nearest minute, whereas D. was reading them to the nearest 5 minutes ; also, that 5.07 was the only time 'clocked' which did not, to the nearest minute, end in a 5 or 0. Not to labour the point unduly, the argument favours the conclusion that for the left-hand column of the Comparative Table 5.07 is more accurate than 5.05 for the beginning of Phase III.

Oddly enough, there is a discrepancy of the same two minutes in the reports of the time at which the landing began at Puy on 19th August, 1942. Captain Hughes-Hallett, in paragraph 12 of his report, makes the time 15 minutes late on 4.50 (i.e. 5.05) but admits in effect that his information about that landing was subject to correction in the light of a separate report from another officer who was in a better position to know what happened. The time accepted as correct by the historians of the Raid is 17 minutes late, i.e. 5.07, and the discrepancy is clearly due to the fact that two different observers, one in a better position than the other, judged differently the time at which the landing began. There accordingly appears to be no connexion at all between the discrepancies as such, but as the clearing away of the discrepancies leaves us with a 'corrected' time 5.07, on each side of the Table, we are faced with a coincidence which seems far outside the bounds of chance. No other landing than that at Puy is recorded as having started at 5.07, and that minute stands out with grim

significance in the history of what was known as the 'Blue Beach' landing.

#### CONCLUSION

As many supposed auditory hallucinations turn out on investigation to be cases in which some ordinary noise has been misinterpreted, it is perhaps necessary to point out that that explanation will not serve here. Any theory that the noises heard were due to water in pipes, or to artillery practice a long way off, the sound of which had been carried to Puy by some freak effect, would fail to explain why no one else heard the noises. The percipients say that they inquired during the day (4th August) of several persons whether they had been disturbed during the night by any unusual noise, and received negative answers. In particular, they asked a fellow visitor who had repeatedly complained of being disturbed at night by casual noises, as they had seen her bedroom light on when they were standing on the balcony listening to noises of 'amazing' loudness. She said she had not heard anything unusual. Nor, it may be added, could the noises have come from a cinema running through a film at an unusual hour, for there is no cinema in Puy.

On the other hand, it would, in our opinion, be rash to assume that the sounds heard were a sort of 'sound track' repetition of the sounds of the Raid. The various kinds of sounds heard, gunfire, dive-bombing, planes, a rifle shot, shouts and cries, are all appropriate, but there is not enough detailed information available as to when the several kinds of sound first occurred to enable one to judge whether they are 'phased in' correctly.

It will be seen from the last paragraph of D.'s Statement that she had a slighter and much shorter experience of the same kind on the morning of 30th July, which was not shared by A. who was asleep. D. says she did not mention this to A. till about two days later, as she (D.) was not in the mood to have her holiday interfered with by 'uncanny' happenings, and when she did mention it to A. neither of them thought it sufficiently mysterious to be worth pursuing. This, followed by the much more 'successful' performance on 4th August, suggests that some resistance had to be overcome to 'put across' the sound effects, and they may have been representative, rather than exact repetitions, arranged in a manner that stressed certain clock-times by preceding intervals of silence.

D. had had three previous experiences of a 'psychic' nature, but none of them was a purely auditory hallucination, so she had no reason to expect an experience of that particular kind at Puy. Two of these earlier experiences were visual hallucinations which,

though involving an apparition of a person well known to her, would not necessarily be considered evidential. The third took place one night during her visit to Dieppe at Easter. It, too, was not evidential. Consisting of a peculiarly powerful impression—possibly a dream—of someone being chased through her bedroom and towards the window, it is mainly of interest here because one might possibly associate it with events which took place in Dieppe during the Raid of August 1942, though this does not seem to have occurred to the percipient. A. has never had any other psychic experience, and it seems likely that the presence of D. had something to do with her hearing hallucinatory sounds at all on 4th August.

We have been impressed by the commendable pains taken by the percipients to record the evidence at once, on a day when they must have been busy preparing to return to England, and by the candour with which they have answered questions we have put to them. They both seemed to the investigators to be well-balanced individuals, with no tendency to add colour to their accounts. Neither of them has shown any concern whatever to 'prove' by the experience any preconceived theory of its cause, which would have been likely to determine the form it took. Both as regards form and content we think the experience must be rated a genuine psi phenomenon, of which little or nothing was derived from previous normally acquired knowledge.

The accounts of the Raid in this report are based mainly on information contained in the published sources to which references are given, notably *Norway—The Commandos—Dieppe* by Christopher Buckley and *The Canadian Army 1939—45* by Lt. Col. C. P. Stacey. For advice as to sources the writer of this report is indebted to the Librarian of the War Office, and for certain details about the movements of aircraft to the Librarian and Archivist of the Air Ministry. The Controller of Her Majesty's Stationery Office has kindly given permission for the reproduction of the map and for the quotation on p. 612 from *Norway—The Commandos—Dieppe*.

G. W. L.



## ESP TESTS WITH PSYCHOTICS

BY D. J. WEST

*Summary*

THREE series of tests<sup>1</sup> carried out over the last year are here recorded. The first was exploratory, the second and third were designed to test whether extremely hostile and suspicious attitudes, commonly found in certain types of mental patient, would be conducive to negative scoring. In none of the three series was there found any clear evidence of an ESP effect. Some of the practical difficulties encountered in administering ESP tests to psychotic patients are described.

*Introduction*

It is often assumed that insane or peculiar people are likely to make good psychic subjects. Certainly psychotic patients often believe themselves to be possessed of special psychic gifts. Ehrenwald (1) has gone so far as to base a theory of schizophrenia on the supposition that these sufferers are in fact being overwhelmed by an uprush into their consciousness of a chaotic mass of extrasensory impressions.

Urban (2) has carried out many card tests with psychotic patients and has reported strikingly positive scores as well as significant differences in scoring level between several diagnostic groups. This work awaits confirmation. In my view his results are unreliable on account of inadequate experimental precautions. In 1937 Shulman (3), in some tests on psychotics in the Hudson River State Hospital, obtained an overall score close to chance expectation. The results from one of his fourteen diagnostic groups—the manic-depressive depressed—give a significant positive deviation ( $CR=3.39$ ). This is suggestive of an ESP effect, in spite of the obvious selection factor, but there were not enough patients in his series to demonstrate that the results of the manic-depressive depressed were significantly greater than what might have come about by variation in the patients' scoring levels independently of diagnosis.

In 1938 Margaret Price (4), of the Duke University Parapsychology Laboratory, carried out a large series of ESP card tests with 50 mental patients in the Ohio State Hospital. A report was not published until thirteen years afterwards, since 'the work did not meet the more stringent experimental require-

<sup>1</sup> My thanks are due to the authorities of the Bethlem Royal and Maudsley Hospital, Horton Mental Hospital, Surrey, and Northumberland House, Finsbury Park, where the tests were carried out.

ments of that time, and it had little chance for immediate publication'. In 1700 runs of high aim trials the patients achieved an average score of 5.54 ( $CR=11.05$ ,  $P=10^{-28}$ ) and in 495 runs of low aim the average score was 4.65 ( $CR=3.89$ ,  $P=10^{-4}$ ). The effect was a fairly generalised one, found in patients of all diagnostic types. A breakdown into co-operative, apathetic, and irritable patients showed that both the first two categories scored significantly better than the last one. Thus the attitude of the patients tested had considerable influence upon the results obtained, whereas the nature of their mental derangement did not seem to make much difference.

Dr Betty Humphrey and other American experimenters have recently carried out further ESP tests with mental patients, but their findings have not yet been published.

### *Series I*

The first series of card tests was entirely exploratory, intended to discover any special peculiarities in the results that might be obtained from a group of unselected psychotics. Twenty-five patients were tested, comprising eighteen schizophrenics, one schizo-affective disorder, five psychotic depressions, and one case in which the diagnosis proved doubtful. The patients were selected solely on the basis of their availability and their being neither too disturbed nor too uncooperative to be testable.

Each subject was given four runs of DT trials with the standard ESP cards. The cards were randomised, arranged in packs of twenty-five, and enclosed in numbered envelopes by an assistant. The experimenter presented the envelopes to the subject one at a time, asking him to call twenty-five guesses at the order of the pack from top to bottom inside the envelope. After every twenty-five calls, the target envelope was opened, and the guesses checked in front of the subject. Since there was often no observer present, it was an important condition that the experimenter did not know the order of the target cards until after he had written down the subject's calls, and that the experimenter's record of the target order was later checked against the record that had previously been made by the assistant who randomised the cards.

The results obtained showed a close agreement of total score (both in direct hits and displacement hits) with chance expectation :

	- I	0	+ I
Observed hits	477	512	451
Expected hits	480	500	480

There was no significant scatter of subjects' scores. The largest deviation obtained was by Mr T., who scored negatively. The

next largest negative deviation was produced by Mrs C. These two subjects were exceptional in that both were highly paranoid and both fitted the experimenter into their delusory systems in a persecutory role. Mr T., who was one of the experimenter's own patients, believed that the experimenter was responsible for the 'voices' that plagued him all day long. Mrs C., although she was unknown to the experimenter before the test, 'recognized' him at first sight as someone from South Africa who was concerned in a plot against her. Their scores were as follows :

	TABLE OF SCORES						
	1st run	2nd run	3rd run	4th run	Deviation C.R.		
Mr T	2	3	1	4	- 10	- 2.50	
Mrs C	3	1	5	5	- 6	- 1.50	
Total					- 16	- 2.8	

This result suggested that a highly hostile and suspicious attitude might be connected with negative scoring. It was with this suggestion in mind that a second series of tests was carried out.

### *Series II*

The purpose of this series was to test the hypothesis that hostile, paranoid patients would tend to produce negative scores. Twenty-two subjects were tested. (It was originally planned to try fifty subjects but, owing to the experimenter leaving the hospital, and the experiment proving time-consuming and apparently unproductive, the series was interrupted.) All the subjects were psychotics with paranoid delusions ; most of them were suffering from chronic paranoid schizophrenia. A difficulty was encountered in that the most hostile and most paranoid patients refused to have anything to do with the test, and some of those who were included in the series did not exhibit overt suspicion or hostility in the test situation. The method of testing used was DT clairvoyance, with four runs per subject, exactly as in Series I.

The results were completely null, there being no evidence of any negative trend and no significant displacement effects :

	- 1	0	+ 1
Observed hits (23 subjects)	442	459	433
Expected hits	422.4	440	422.4

Five of the subjects who were noted to be especially hostile did not produce outstanding scores. Series II included a retest of Mr T., who had produced the largest negative deviation in Series I,

but this time his score was close to chance. It must be admitted, however, that in no case in Series II, not even in the retest of Mr T., did the subject express the delusion that the experimenter was a persecutor.

### *Series III*

This series consisted of a group test which, like Series II, was designed to investigate the effect on scoring of marked hostility and suspicion in the subjects. Dr F. K. Taylor of the Maudsley Hospital supplied the subjects. They consisted of six patients who met regularly once a week with a doctor in order to receive psychotherapy as a group. All of them were highly paranoid, several were definitely deluded and psychotic, and the rest were borderline cases. All of them felt persecuted, and tended to be aggressive and suspicious in their dealings with other people. D. J. West sat in on one of their weekly sessions, listening in silence and taking notes while they discussed their intimate affairs with their doctor. At the end of the session, when it may be safely assumed they were feeling peculiarly resentful and suspicious, they were required to take part in an ESP test.

The test consisted of eight runs, DT clairvoyance, under the same general conditions as in Series I and II, except that the subjects wrote down their guesses, all at the same time, and all aiming at the same target. No significant trend was found in the results :

<i>Subject</i>	<i>No. of Runs</i>	<i>Deviation</i>
A	8	+3
B	8	-6
C	8	+1
D	4	+2
E	8	+3
F	8	+3
<hr/>		
Total	44	+6

### *A Note on the Practical Difficulties of Administering ESP Tests to Psychotic Subjects*

In the investigation of possible correlations between the mood of the subject and ESP scoring, psychotic patients provide useful material, since in them moods such as aggressiveness, suspicion, elation, or apathy, occur spontaneously in forms more extreme than are ever likely to be encountered in normal people. Unfortunately, largely because of these violent moods, psychotics are often uncooperative or inaccessible to the experimenter. Even when they appear superficially to agree to co-operate in the ESP test, they often succeed in resisting by indirect means. They

sometimes keep up endless arguments about whether it is right for them to take part in the test, or try to put off the test to another day. Others take so long over their calls, trailing off into such protracted discussion of the pros and cons of each choice, that the experimenter's patience becomes exhausted. Another, and even commoner habit, is to employ systematised guessing. In an extreme case they may say 'Let's have them all crosses' or 'five circles, then five waves, then five stars, then five squares and then five crosses'. More commonly they call the cards in sets of five, e.g. 'cross, square, star, wave, circle', repeating the sequence over and over again either in the same or reversed order. The persistent use of one or other rigid system of calling, after the experimenter's repeated requests to guess the cards at random, is such a common feature of psychotic behaviour in the ESP test situation that it could almost be used as a diagnostic aid.

Systematic card-calling presumably tends to prevent any genuine ESP response, and should therefore be discouraged. It will be remembered that when Dr Soal's subject, Basil Shackleton, adopted fixed guessing habits, he did not score above chance. But no matter how far from random the subject's calls may be, spurious extra-chance scores cannot be produced provided the target cards are strictly randomised.

The chief disadvantage in the use of psychotic patients as subjects is the great amount of time taken up in making contact with them and in conducting the tests. It was not uncommon to have to spend a whole afternoon in obtaining a subject willing to co-operate and in persuading him to complete four runs, which, with a normal subject, could easily be completed in a quarter of an hour.

While the subjects' guesses have throughout this report been referred to as 'calls', in practice it was found necessary to arrange samples of the target cards in front of them and ask them to indicate their choice with a pointing finger. Many psychotics were prepared to co-operate to this extent who would not have been prepared to call out audibly the names of the cards.

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## THE SOCIETY'S NEW PRESIDENT

FOR the year which marks the Society's seventieth anniversary, the Council invited its most distinguished member, Dr Gilbert Murray, O.M., to accept the Presidency. He was formally elected on 23 April.

Dr Murray joined the Society in 1894, twelve years after its foundation, and was President for the year 1915-16. He was a member of the Council for many years, and has been a Vice-President since 1920. His Presidential Address will be delivered at 8 p.m. on Wednesday, 21 May, in the Great Hall of the Caxton Hall, Westminster, S.W.I.

## REVIEWS

THE PSYCHOLOGY OF THE OCCULT. By D. H. Rawcliffe. London, Ridgway, 1952. 551 pp. Illus. 21s.

The main purpose of this book is to discredit psychological research (for which the author prefers to use the emotive term 'occult research') which he says is today 'assuming alarming proportions' and even 'invading the precincts of our most distinguished universities' (p. 9). It has seemed to many of us that it was better that experimental inquiry should be taking place into such matters, and that such experimental inquiry by trained researchers was the most hopeful way of sifting the true from the false. Mr Rawcliffe seems to feel no need for such sifting; it is all 'occultism' and, therefore, all false. All that needs to be done is to think of a 'rational' explanation of any apparently successful psi experiment.

There is, of course, nothing new in this position, and when Jastrow argued twenty-five years ago that the crucial experiments for extrasensory perception were not rigid enough to satisfy the experimental psychologist, he could make out a reasonable case for this opinion. The situation is different now. Experiments have been carried out under conditions which met all reasonable objections and still successes have continued. Unconscious whispering seemed at one time as if it might be a plausible explanation, and Mr Rawcliffe seems still prepared to back it strongly. But psi successes have taken place under conditions in which such an explanation is absurd, e.g. in ESP experiments over long distances, in precognition experiments, and in the Duke DT experiments in which the target cards were known to no one until the guesses were completed.

There is nothing judicial about Mr Rawcliffe's survey of the field of parapsychology. He reminds one rather of a prosecuting counsel with a weak case, concerned to create conviction at all costs. There is, for example, much abuse of the parapsychologist for 'bias and incompetence' (pp. 479 and 486). There are references to the parapsychologists as amateurs; they are contrasted with the psychologists who (according to Mr Rawcliffe) reject the findings of parapsychological experiments. The intention seems to be to suggest to the jury that Professor Gardner Murphy and Dr Schmeidler are amateur psychologists while the author is a professional. A jury sufficiently ignorant of the facts might perhaps be led to suppose that this was true.

Obviously one needs to trouble oneself less about the evidence if the jury can be convinced that all the witnesses on the other side are unreliable. Thus, on p. 323: 'Richet, Myers and Lodge all believed in the unseen spirit world and died steadfast in their belief'. This is true of Myers and Lodge, false of Richet; irrelevant in any case as a means of judging the correctness of their observations. Dalton believed in the atomic hypothesis and died steadfast in that belief, but this is not ground for rejecting his experimental work. On p. 313 we learn that evidence coming from Osty is highly suspect because psychical research was his all-absorbing interest. If all evidence as to paranormal phenomena must be suspect if it comes either from workers who believe in paranormal phenomena or who are strongly interested in it, we shall be left with little evidence. But a similar criterion would leave us with little evidence on any other branch of research.

On p. 323: 'Palladino's investigators were not really investigating her "phenomena" at all. What they were all hoping to do was to prove her "phenomena" authentic.' How does Mr Rawcliffe know this? 'The Soal experiments were first and foremost designed to establish *scientifically* the validity of a purely *meta-physical* concept' (p. 458). My own recollection of Dr Soal's attitude is that, on the contrary, what he was trying to establish was that, under properly controlled conditions, a subject in card guessing experiments could not beat the laws of chance. Being an open-minded man, he gave up that idea when the experimental evidence went against it, but it is absurd to credit him with the opposite pre-conception.

Although Mr Rawcliffe devotes a chapter to discussion of the American experiments in ESP, these are mostly devoted to discrediting the experimenters. There is no need to protect Duke parapsychological investigators against the charge of experimental incompetence. Many competent judges have examined their work critically and come to the opposite conclusion. Mr Raw-

cliffe makes no attempt at a fair appraisal of the evidence from this laboratory by considering the experiments carried out with the fullest precautions against error. There is, for example, no mention of the Pearce-Pratt series of experiments which is claimed by Rhine as the most adequate evidence for the reality of ESP. A sneer at Dr Schmeidler on p. 442, based on a criticism by Dr Soal of a minor detail in her evaluation of results, shows complete ignorance of the point of the criticism.

Dr Soal receives more adequate treatment in the next chapter. The Shackleton results, however, are explained as due to a double system of unconscious whispering by assistant-experimenter and agent. This suggestion is not altogether novel. If it is considered to be a possible explanation of the main Shackleton series of results, it is obviously not applicable to the preliminary Shackleton series in which there was precognitive guessing on a pack of cards, or to the Antwerp-London experiments with Mrs Stewart. For the Antwerp-London series, Mr Rawcliffe has to fall back on the suggestion that the conditions were such that Dr Soal had the opportunity of faking the records.

Mr Rawcliffe does not seem altogether happy about these criticisms of Soal's experiments, since he tries to strengthen them by some statistical considerations which show a curious lack of understanding. On p. 471, for example, he says that even if his other criticisms were not sound, the Shackleton series would be a weak support for Soal's conclusions since their total number is *only* 6,690 guesses which is a small number compared with the American experiments. This, of course, has nothing to do with the case; the anti-chance odds were  $10^{35}$  to 1, which is by no means weak. He is surprised by the 200 guesses with 60 right by Mrs Stewart ten feet from the telephone (p. 472), but remarks that 200 trials is too small a number to be of any significance. This is not the case; the odds against such an excess of right answers arising by chance in 200 guesses is 2,000 to 1 which is amply significant.

If all his criticisms against the experiments fall to the ground, Mr Rawcliffe has a further line of defence in Chapter XXVIII. They could not prove telepathy because this is a metaphysical and not a scientific hypothesis. This is, of course, a mixture of sense and nonsense: the sense is less unfamiliar to parapsychologists than Mr Rawcliffe supposes. What one can do by experiment is to discover whether the knowing of something by one person can be a sufficient condition for an appropriate action with respect to that item of knowledge by another person in the absence of any communication between them through sensory channels. Obviously the success of such an experiment does not tell one that 'tele-



pathy' (or ESP or psi) is the appropriate name to apply to the capacity revealed by such an experiment, nor anything further about the nature of the capacity than is implied in the experimental situation. 'Telepathy' (or ESP or psi) is merely the name given to the capacity indicated by the experiment, and new experiments must be designed to discover anything further we want to know about the capacity. There is nothing metaphysical about that; the concepts of parapsychology are parallel to such concepts as 'energy', 'radiation', etc.

It is rather surprising to find that a book of this kind has a foreword by Julian Huxley which expresses a hope that it may be widely read and taken to heart. However, Huxley expresses disagreement with the main thesis of the book, and says that he cannot follow the author 'in stigmatizing studies on telepathy, clairvoyance, etc., as "occult research", unfit to be admitted to our universities' (p. 6).

The real difference between Mr Rawcliffe and the experimental psychical researcher (or parapsychologist) is that between the Inquisition and Galileo. He knows beforehand what can and what cannot happen; the psychical researcher wants to find out by experiment. We can agree with him that we must find a 'rational' explanation of parapsychological experimental results, but that is not necessarily the explanation that would have seemed to us to be the most rational one before experiment started. If ESP proves to take place, then explanation by ESP is not an irrational one. The question must be settled by experiment, and Mr Rawcliffe gives us no new experimental facts and no new insight into the old ones. Our problems are to be solved by experiment and not by the methods of the debating society.

R. H. THOULESS

THE PSYCHIC SOURCE BOOK. Edited by Alson J. Smith. Introduction by Pitirim A. Sorokin. New York, Creative Age Press, 1951. xii, 442 pp. \$4.00.

This book is described by the publishers as 'a big, exhaustive, factual compendium on the ever-alluring subject of extrasensory perception and psychic research'; and even after applying a necessary corrective to a publisher's natural enthusiasm it can be said the contents cover a wide field and should be a useful primer for newcomers to parapsychology.

It consists mainly of twenty articles culled from the writings of eminent scholars and scientists, about half of whom are from this country. These articles are mostly of the review type and, though necessarily condensed, give a fair account of their authors' views.

Thus we have F. W. H. Myers on 'The Daimon of Socrates', J. W. Dunne's 'An Experiment with Time', G. W. Balfour on 'The Ear of Dionysius', Walter Prince on 'Patricia Worth', J. H. Hyslop on 'Mrs Piper', Mrs H. Sidgwick on Professor Gilbert Murray's 'Thought-transference experiments', the Rev. C. Drayton Thomas on 'Book tests', Dr Soal's Myers Memorial Lecture 'The Experimental Situation in Psychical Research', Dr Rhine on 'Precognition'—all taken wholesale from the original articles and making a rich and varied collection.

Introductions and explanatory notes, mostly quite short, have been added by the compiler, the Rev. Alson J. Smith, and by Pitirim Sorokin, Eileen J. Garrett, and J. G. Pratt. These express a dualist view of mind and body and frankly assume that psychical research has now established the reality of a Spirit of Man transcending his material environment. To some this will detract from the book's scientific value as going further than the presented facts warrant. There are no hints of alternative interpretations.

With so vast a field to cover there must be many omissions even in a compendium of 150,000 words. Thus nothing whatever is said of physical mediumship, nor, rather surprisingly, considering the country of origin, of psychokinesis. The bare word, if a rather scanty index is to be trusted, only occurs twice in the text, with no explanation. PK is not included in a useful glossary, though 'telekinesis' is defined as being 'the alleged supernormal movement of objects'. The use of the cautionary adjective is interesting as it is not used in the parallel definitions of telepathy, clairvoyance, precognition, etc., which are defined as established facts. A bibliography is included with over 300 entries among which are Bunyan's *Pilgrim's Progress*—and *The Koran*. There is also a Biography of forty-six eminent personalities in psychical research, and it is pleasant to see E. J. Dingwall's name jostling for a place in this Roll of Honour with Emanuel Swedenborg, Harry Price, and Sir Arthur Conan Doyle.

G. W. F.

NEW LIGHT ON SURVIVAL. By Roy Dixon-Smith. London, Rider, 1952. 328 pp. 21s.

In this book Mr Dixon-Smith has given us a detailed account of his conversion to Spiritualism after the tragically early death of his wife. It is written with great sincerity and enthusiasm, and with the purpose of bringing the same comfort and assurance to others which he has obtained from his experiences with both trance and physical mediums. It is offered not merely as a narrative but

as a serious contribution towards evidence of survival, and it is from this standpoint that it should be judged.

Part I consists mainly of descriptions of the author's sittings with trance mediums, and in his Explanatory Note (page xvii) he claims that 'the case for survival is at this stage considered to be proved'. The reader's first impression is that the author is unaware how much more difficult it has become to obtain crucial evidence of survival owing to our increased knowledge of ESP. Mr Dixon-Smith, however, gets over this by postulating that the *modus operandi* between a discarnate mind and that of the medium is a form of hypnosis; but that the possibility of this process taking place between medium and sitter is excluded by the latter closing his subconscious mind 'as tightly as possible by will and auto-suggestion' so that no medium 'can hook willy-nilly on to one's subconscious memory' (p. 45). The author's attempts to prove this statement are most unconvincing and can hardly be taken seriously. There is strong evidence that telepathy between sitter and medium does occur. (A typical example can be found in the S.P.R. *Journal* for February 1948 entitled 'Emergence of an apparently Pseudo-Communicator' by Mrs Heywood.) This should not be considered a more 'extraordinary super-human power' (p. 45) than that which the Spiritualist hypothesis entails.

An objective study of Mr Dixon-Smith's sittings reveals that a great deal of the information given to him through mediums was already known to him and was given in private sittings at which he took his own notes (p. 62) and in certain cases had his hand on a ouija board with that of the medium (p. 113). The author's honesty is not being questioned, but this type of evidence does not reach the standard required in psychical research. Proxy evidence is far more valuable and he gives some interesting cases, but he does not draw sufficient distinction between their evidential value. Accurate information given to a proxy sitter is more impressive when neither absent nor proxy sitter are known to each other, and an involuntary leakage of information or an unconscious memory cannot possibly play a part. It is true, however, that it is the *accumulation* of evidence of personality which often brings conviction to a sitter, and such evidence cannot be entirely disregarded even though it may not come up to scientific standard.

Mr Dixon-Smith rightly reminds us of the famous cross-correspondence cases, and others of high evidential standard published by the S.P.R., where an explanation based solely on ESP is inadequate and postulates a degree of that faculty which is at present beyond our knowledge. It is the existence of cases of this standard which makes further and wider research of such importance, but it is imperative that it should be carried out under

conditions of the most carefully devised control (however unassailable the good faith of the experimenter may be) and if possible in conjunction with modern psychological research.

This applies with even greater force to the physical phenomena described in Part II. They cover every form of manifestation and are so remarkable that it is to be regretted that more details are not given regarding the methods of control. This branch of inquiry is far more subject to fraud and malobservation than the author appears to realize, and it is to be hoped that Mr Dixon-Smith will give us more detailed and corroborated descriptions of his sittings in the next book which he has promised us.

The absence of an index is a great inconvenience.

K. A. G.

## CORRESPONDENCE

### QUALITATIVE MATERIAL AND THEORIES OF PSI PHENOMENA

SIR,—Many members of the Society may, like myself, doubt their ability to co-ordinate the facts of psychical research with physiological science or to bring them within any metaphysical scheme. If so, they will share my gratitude to Dr J. R. Smythies for his paper in the *Journal* for September 1951, and to their fellow-members who have commented on it in the issue for January 1952. The kind of theory which would be most helpful to us would be one that covered as many as possible of the facts of psychical research which have been established, without bothering about material that at the present time needs support from the results of further research.

The literature of psychical research, in this and other countries, is enormous. It would be impracticable for any intending theorist to read every report in every publication with the attention necessary to form an opinion whether the occurrences there described were properly attested, and whether in each case there were adequate grounds for dismissing sensory clues, chance, and other 'normal' explanations. Should he succeed in completing such a task he would have little leisure for theorising.

Fortunately, in assembling material to be brought under his survey, he can profit by the labours of his predecessors in this Society. His concern will not be with separate, individual, occurrences, least of all with instances, whether experimental,

mediumistic, or spontaneous, which present peculiarities that cannot readily be paralleled from other reported instances, themselves well attested. Exceptional instances are treacherous ground, and he will be safer if he keeps to classes or types of phenomena that are represented by several examples published in S.P.R. *Proceedings*.

The long life of the Society, its freedom from corporate dogma, and the enterprise of its members in research are a pretty good guarantee that every sort of phenomenon will be investigated by a member and reported to the Society. But no report will appear in our *Proceedings* until it has been screened by the Committee of Reference, which dates back to the very early days of the Society and has numbered many men and women of outstanding ability, critical judgment, and varied opinions.

It would be absurd to claim that our investigators or the Committee of Reference have made no mistakes. A few hoaxes have succeeded, and examples of carelessness in experiment, observation, or record are to be found. But when several examples of about the same type of supposedly paranormal occurrence are to be found reported in our *Proceedings*, that type has, I suggest, a *prima facie* claim to be included in any theoretical scheme.

Should the intending theorist share the doubt, which is sometimes expressed, as to the possibility of deciding whether such qualitative material as spontaneous cases, mediumistic 'communications' and so on can be attributed to chance or not, and does not trust his own judgment, he can always seek the guidance of notable sceptics, such as Mrs Sidgwick and Miss Alice Johnson, whose critical abilities were exercised over most of the field of psychic experience. I do not believe he will anywhere find a more objective criterion for matter worth including in a theoretical scheme than publication in *Proceedings* combined, where qualitative material is concerned, with acceptance as probably paranormal by these experts. This will give him quite as much material to work on as he is likely to need or desire.

Now, if this yardstick be applied to the papers of Dr Smythies and his critics, it will be noted that, while in some respects the material discussed by them lies outside the limits suggested above, there is a very large quantity and variety falling within those limits which most of the writers neglect. With few exceptions they confine their remarks to quantitative experiments. Some of these, notably the Soal-Goldney experiments, thoroughly deserve their attention. Others might conveniently wait until we have a larger quantity of positive results obtained under good conditions.

If the results of quantitative experiment were typical of psychic phenomena as a whole, there would be no occasion to press a

theorist to travel outside them. But for reasons too well known to readers of the *Journal*, arising from their being experimental and quantitative, that is far from being the case. If, on the other hand, there were no points of contact between the quantitative and the qualitative, it might be simplest to get two teams on the job in the assurance that their theories would not conflict or overlap. But as things are, attempts to frame a theoretical scheme exclusively out of quantitative material may be quite misleading if it is assumed to be applicable to other types of phenomena, and even misleading as regards the quantitative material itself, as obscuring its connexion with and partial resemblance to other material left outside the scheme.

It is gratifying to note that our American colleagues are alive to the importance of qualitative material. I would refer in particular to the short paper by Dr Emanuel Schwartz in the American Society's *Journal* for January 1952, 'The Psychodynamics of Spontaneous Psi Experiences'. Referring to a previous article by him (October 1949) he writes (p. 3) :

I suggested that over the period of the last seventy-five years the original emphasis in psychical research [a footnote here mentions Myers's *Human Personality*] had been shifted from the *experiencers* to the *experiences*. It seemed to me that psychical research divorced from the study of human personality was a meaningless undertaking and that the cul-de-sac into which psychical researchers have been led might have been anticipated.

The general purpose of the article is to stress the need for the study of motivation in psychic experiences and of the 'interpersonal field', to use Professor Gardner Murphy's phrase. With this object and with several passages in Dr Schwartz's article I am in full sympathy. When, however, he speaks (p. 3) of the need to shift the emphasis 'away from the "phenomena" and their veridicality back to human beings', I think a source of possible misconception is to be noted.

Let us take two imaginary cases. (A) Jones has a waking vision of Smith being involved in a motor smash : Smith has not in fact been so involved. (B) Jones has a similar vision at the moment when Smith was in a motor smash a hundred miles away, the attendant circumstances being so accurately reproduced in the vision as to make an explanation by chance-coincidence highly improbable. (How convincing these imaginary cases are!)

It would not, I suggest, be at all safe to assume that the interpersonal relation between Jones and Smith was the same in the two cases. It might be that the motivation of case A was an emotion of Jones regarding Smith that was not reciprocated, and

in case B an emotion that was. Whether that could be shown or not, the difference between the two cases suggests a probable difference in the psychological situation. It seems to me therefore that an analysis of the psychology of spontaneous cases (and of mediumistic 'communications' also) will lose much of its value unless the laborious first step be taken of sorting out the material under examination into veridical and non-veridical. From his praise of the early investigators, who tested each case very thoroughly to see how far it could be counted as veridical, I should infer that Dr Schwartz himself is under no misconception as to the need for this, but some of his remarks might perhaps mislead incautious readers of his article.

W. H. SALTER

#### THE SHACKLETON EXPERIMENTS: MR RAWCLIFFE'S THEORY OF DOUBLE WHISPERING

SIR,—In *The Psychology of the Occult*, Mr D. H. Rawcliffe puts forward a theory of double whispering to account for the results of the experiments with Basil Shackleton. Students of psychical research will realize that the theory is so preposterous as to be scarcely worth refuting, but for the sake of uninformed persons who may read the book it may be advisable to make a few comments.

The author supposes that the experimenter, in addition to presenting the random numbers at the aperture in the screen and calling in a loud voice the serial numbers 1-25, involuntarily whispers the *next* number on his list or tags on its initial consonant to the serial number. The Agent sitting on the other side of the screen picks up this whisper, decodes it into the corresponding animal's name, and whispers the name or its initial letter, and the whisper is picked up by Shackleton sitting 18-20 feet away in the next room. All this complicated series of exchanges is supposed to be packed into the  $2\frac{1}{2}$  seconds between successive calls. When, however, the experiment is speeded up to twice the normal rate, the experimenter is supposed instead to whisper the *next number but one* on his list, and this is said to account for the (+2) displacement. This double whispering and decoding has now to be compressed into an interval of  $1\frac{1}{2}$  seconds or less!

Shackleton succeeded brilliantly with no less than seven different experimenters. All these, according to Mr Rawcliffe, must have been whispering the number one or two places removed from the one they were actually showing at the aperture, and all of them must have been completely unaware of it!

In any case, in putting forward this theory Mr Rawcliffe omits to say that true involuntary articulation occurs as a reinforcement effect and not as a deliberate separate process. He is therefore in effect saying that the experimenter (1) looks up at the appropriate random number on the list, (2) deliberately shifts his glance to the next number or the next but one, (3) looks down to pick up the card bearing the appropriate number, (4) presents it at the aperture, (5) calls out the next serial number, and (6) deliberately whispers the next random number or the next but one. All this, plus the picking up and decoding of the whisper by the Agent, and the latter's own whispering, would indeed constitute a phenomenon of the first order, even if not compressed into a maximum of  $2\frac{1}{2}$  seconds! Of the implication of fraud, I will only say that ten different people—seven experimenters and three Agents—would have had to be party to it!

There are other phenomena—e.g. the success of one Agent and the total failure of the other when two agents were employed simultaneously, either with the same or with different targets—which Mr Rawcliffe's theory is incapable of accounting for, but to deal with these would be a waste of your space and of the reader's time.

I shall scarcely be expected to comment here on Mr Rawcliffe's remarks on the conduct of the Antwerp-London experiments with Mrs Stewart.

S. G. SOAL

#### ESP AS GUESSWORK

SIR,—In two recent broadcast talks on 'The Significance of Parapsychology', and in the conflated version of these which I gave to the Society in February, I suggested that if we thought of ESP not as a species of either cognition or perception but as a kind of guesswork, we might be led to investigate ordinary guesswork; and that what we found out might throw into relief significant peculiarities of paranormal guesswork. I have been asked to expand and explain this suggestion. I am glad to be driven to try to do this: for what I originally had in mind was an extremely hazy and tentative idea; and it is good to be forced to find what—if anything—can be made of it.

Obviously the first difficulty lies in the notion of ordinary guesswork. For—at least in the present state of our ignorance—there is no way to distinguish decisively in any *single* case between a guess (or hunch or dream or vision) which just happens to be right and a correct guess (or hunch or dream or vision) in which the



ESP factor is operating. This is because all we can at present mean by the term 'ESP factor' is the factor which gives rise to significant deviations from pure chance expectation in a *series* of guesses (or whatnot). If we use the term to mean more than this, then the evidence at present available has not established the presence of an ESP factor: if 'ESP' is taken to refer to the putative unknown means or mechanism by which successful ESP runs are achieved, then there are no sufficient grounds for thinking genuine ESP occurs; for though conjuror's means and mechanism can produce *bogus* ESP effects, we have no reason to suppose *genuine* ESP runs are produced by any means or mechanism; and every reason to suppose the contrary. (What means or mechanism *could* operate backwards to produce genuine precognitive ESP?) The difficulty could be got round, at least temporarily, by prescribing that guesswork (and whatnot) was ordinary when and only when it was done by someone who has always failed to score significantly in ESP tests. This is no doubt excessively rigorous; but it's as well to be on the safe side, by thus excluding the dud runs of the sometimes successful.

Now what I mean by studying ordinary guesswork (and whatnot) is finding out why people guess one thing and not another (dream one thing and not another, have one particular hunch or vision and not another) when no ESP factor is involved. If we knew more about this it might be easier to see more of the nature of this mysterious ESP factor, which appears occasionally to upset the ordinary mechanisms and motivations of the normal. What leaps to mind here are Freud's masterpieces: *The Psychopathology of Everyday Life* and *The Interpretation of Dreams*. But a great deal of other work has been done; and much more needs to be done.

Now I have developed the idea, I am no more sure than I was when I first made the suggestion that there really is anything in it. But perhaps, even if this is just one more dead end, it will have been worthwhile to have reminded psychical researchers—especially those working on putative spontaneous phenomena—that Freud did show that very many apparently paranormal predictive omens and dreams could be sufficiently explained in terms of the motives of the dreamer.

ANTONY FLEW

## 'THE CLAIRVOYANT THEORY OF PERCEPTION'

SIR,—I should like to make two comments on Captain Moncrieff's letter in the March issue of this Journal.

(1) I did not say, as Captain Moncrieff suggests, that his theory cannot account for disordered vision in cases of cataract. In fact, I expressly said that it could account for this but not for disordered vision in cases of disease of the optical apparatus *proximal* to the retina, i.e. in the optic tracts, optic radiations, and occipital and parietal cortex. Captain Moncrieff explains the various phenomena and syndromes which I mentioned thus: '. . . if clairvoyance is a mental faculty, such abnormal cerebral conditions as above would naturally be expected to produce them. . . . Thus, in case of "vascular lesions of the parietal lobe" we would hold that their effect on vision may possibly be due to the lack of integration or correlation in respect to the cerebral processes.'

Now the disorders of vision caused by such vascular lesions are fairly extensive, and the visual field is grossly disorganized. Thus the integrative and correlating processes, which are held to fail in these cases, must be very greatly responsible for the orderly presentation of the normal visual field. Furthermore, experimental psychology has shown that the actual size of objects in the visual field and the spatial relations between these objects is quite different from the size and relationship of the objects of the common physical world. This is particularly so in the case of distant objects. If the actual nature of the visual field is borne in mind, together with the evidence as to its means of presentation that we can actually inspect—after-images and the stroboscopic phenomena—it seems that these integrative and correlating processes in themselves provide an adequate explanation of the missing link in perception. The visual and auditory fields and the somatic sensory field making up the perceived body must actually be constructed by mechanism—that is by process in extension. Whether this mechanism is in the brain or in the psyche is not immediately relevant. The problem of externality of *sensa*, which Captain Moncrieff's theory attempts to solve, is an entirely false problem due primarily to the confusion in common sense between the perceived body and the physical body and to the confusion in neurology between the perceived body and the body-image. All *sensa*, including the visual fields and the perceived body, are wholly *inside* the human organism—geographically, anatomically, and physiologically *inside* it.

(2) I would not agree that 'no theory can be expected to provide a full and adequate explanation both in respect of normal and abnormal conditions of vision'. There is no clear-cut

division in nature between the normal and the abnormal, and any theory which cannot account for both is inadequate.

In conclusion, I would like to suggest for further reading Paul Schilder's book *The Image and Appearance of the Human Body* (London, Kegan Paul, 1935). Further advances in philosophy depend largely upon the integration into the existing philosophical structure of the large body of evidence contained in the special humanistic sciences of neurology, neurophysiology, experimental psychology, and neuro-anatomy.

J. R. SMYTHIES

## OBITUARY

### J. G. PIDDINGTON

WE regret to record the death, on 1 April at the age of 83, of Mr J. G. Piddington. A member of the Council for many years, he was elected a Vice-President in 1921 and President in 1924. He contributed several papers to the *Proceedings* on the Cross-correspondence scripts, and was Trustee of the Research Endowment Fund for many years. An obituary notice will be published in the *Proceedings*.

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THREE SCORE YEARS AND TEN

THE S.P.R., 1882-1952

SEVENTY years would be a fairly long period for the life of any research society that was dependent entirely on the financial support and energy of its members and had no backing from professional or academic interests. For such a society to be engaged for all that time on an investigation as full of difficulties and pitfalls as psychical research, to have survived two wars and three decades of almost uninterrupted financial crisis, and at the end to have emerged with a roll of full members much larger than at any time before the Second World War, is a truly remarkable feat. It proves that in 1882 its foundations were well and truly laid, and suggests that this is an appropriate moment to recall to memory the men and women who laid them, the objects they had in view, and the principles by which they wished themselves and their successors to be guided.

'Psychical research' was a new term intended by our founders to mean systematic inquiry into several debatable questions having little in common except that they did not seem capable of a solution within the generally accepted scheme of natural science. There were, for example, stories of apparitions and haunted houses, too numerous and apparently too well authenticated to be lightly dismissed as old wives' tales: how much fact underlay the accumulations of hearsay and credulity? What light would a scientific investigation of hypnotism throw on the structure of the mind, and on those extensions of perceptive faculty reported by earlier inquirers? Could recent experiments in thought-transference be confirmed, and, if so, could the *modus operandi* be defined? Was it true that spirits could 'materialize' themselves in visible and tangible form? Were there forces, unrecognized by Science, capable of moving material objects?

The solution of these and many similar questions clearly depended on the conduct of a more sustained and better co-ordinated inquiry than any that had hitherto been, or could be, pursued by individuals or small groups of friends. A conference of interested persons was accordingly convened in London in January 1882 by Professor William Barrett, the Dublin physicist, at which a project was submitted for a society to conduct systematic investigations. This resulted in the formation a month later of the present Society.

Henry Sidgwick, who in the following year became Professor of Moral Philosophy at Cambridge, was elected as first President, and it was fortunate that the nascent Society should have been guided by a man of his character and intellectual gifts, his rare union of cautious judgment with tireless determination, and his experience of many forms of supposedly psychic phenomena gained in active association with Frederic Myers and Edmund Gurney. Both were men of exceptional ability. They had made many experiments and observations, the results of which their wide reading and personal contacts with American and Continental psychologists enabled them to correlate with the significant developments of the time, especially in medical psychology. Of the two, Gurney had the greater aptitude for experiment, and could give more time to it; Myers had no rival as an exponent of facts and ideas, which are now through the Society's work familiar to all, but were then startlingly novel.

The new Society attracted men and women of eminence in many branches of science, in philosophy, scholarship, literature, and politics: to name a few, William Crookes, Lord Rayleigh, Oliver Lodge, A. R. Wallace, William Bateson, W. E. Gladstone, A. J. Balfour, John Ruskin, Alfred Tennyson, R. L. Stevenson, 'Lewis Carroll', Leslie Stephen.

Opinions of the most diverse kind flourished among the members on such questions as the reality of various kinds and instances of supposedly psychic occurrences, and especially as to their bearing on the problem of human survival. Some were already convinced Spiritualists, others hard-shell sceptics. All, however, agreed to accept the principle laid down in the manifesto printed in Volume I of the Society's *Proceedings*, to approach the problems of psychical research 'without prejudice or prepossession of any kind, and in the same spirit of exact and unempassioned enquiry which has enabled science to solve so many problems, once not less obscure nor less hotly debated'.<sup>1</sup> This has been ever since, and still is, the foundation of the Society's policy.

Attempts have indeed been made to turn the Society into a

<sup>1</sup> *Proc.*, i, 4.

doctrinaire body, but these have always been frustrated by the resistance of an overwhelming majority of the members. Secessions there have been from both wings. A few years after the Society's foundation a handful of Spiritualists resigned in disapproval of the critical attitude of some prominent members towards physical phenomena. A dozen years later a few sceptics left because they thought they detected a Spiritualist bias in the Council. But there have been no splits or secessions of importance. Anyone with a fairly long practical experience of S.P.R. work would probably agree that differences of opinion are no obstacle to harmonious co-operation among researchers of ordinary intelligence who make objectivity their aim.

The inaugural manifesto, while attempting no precise definition of psychical research, gave a list of subjects which it was proposed to investigate. This is of historical interest owing to the curious phrasing of some of the items and the way in which they were grouped. The list was comprehensive and may be briefly described as a programme for the investigation of all those aspects of human personality, and the faculties, states, and phenomena connected therewith, that were not recognized by general scientific opinion and were not at the time being fully investigated by any other science. That puts the matter negatively. A positive definition might be harder to formulate, but the full exploration of the subconscious mind (not to be confused with the Freudian 'unconscious') might not be far off the mark.

In practice the new Society was in its earlier years mainly engaged in pursuing those lines of inquiry which several of the members had been following before its foundation, introducing order and method where necessary. Some of the lines of inquiry led nowhere. Others were successfully followed up to a point at which it became profitable to leave further progress to some other organization. Thus, much of the pioneer work in this country relating to hypnotism and medical psychology in general was done by members of the Society, at first separately, but later grouped into a Medical Section. When, however, after the First World War the British Psychological Society formed a Medical Section of its own, there was no point in continuing the Medical Section of the S.P.R.

But there is no other organization to which as yet the main departments of psychical research could be transferred with the assurance that they would receive the thorough investigation that they need. Two main departments have long been recognized. One is concerned with the so-called 'physical' phenomena, such as the movement of objects without muscular or mechanical force ('telekinesis'). The other relates to 'mental' phenomena: for

example, the transmission of impressions otherwise than through the normal channels of sense ('telepathy'). (Needless to say, neither of the above descriptions should be regarded as an assertion that the subject-matter of the description has been proved to exist). As a rough-and-ready classification, which is easily understood, the division into 'physical' and 'mental' may be conveniently retained, even though it suggests a sharp distinction which is no longer fashionable.

As regards physical phenomena, although much good investigation had already been done by Henry Sidgwick and his wife, Myers and Gurney, the general level of investigation was low. It was urgently necessary to raise it by showing inexpert sitters how easily they could be deceived through their inability to observe accurately what happened in the seance-room under the conditions usually prevalent. This was effectively done by Richard Hodgson who arranged for a friend, Davey, to duplicate by 'normal' means, i.e. conjuring, some physical phenomena that were then astounding the public before groups of sitters who were asked to write down what they thought they had seen. They were then told what in fact they had seen and how and why they were in error.<sup>1</sup> When, however, conditions were formulated that would effectively exclude deception, great difficulty was experienced in finding mediums willing to accept them. In consequence the field of investigation into physical phenomena has proved disappointingly barren, and there are few records of positive results obtained by competent researchers under good conditions. The best example is perhaps the report by the Hon. Everard Feilding, W. W. Baggally, and Hereward Carrington, a very strong team, of their sittings in Naples with Eusapia Palladino.<sup>2</sup>

Research into the 'mental' phenomena was actively pursued along two lines. There were first the collection and analysis of spontaneous occurrences of the kind that are the basis of the traditional ghost story. Two large-scale inquiries were initiated by the Society, the first conducted by Gurney, Myers, and Frank Podmore, and resulting in the publication in 1886 of *Phantasms of the Living*. The second was the 'Census of Hallucinations', the report on which was published in 1894.<sup>3</sup> It was mainly the work of Mrs Henry Sidgwick and Miss Alice Johnson, later Research Officer of the Society, both of whom were familiar with scientific method. From *Phantasms* and the Census it was apparent that narratives of persons being 'seen' by their friends, at a time when they were involved in some sudden and unexpected crisis occurring at a distance, a fatal accident, for example, were too

<sup>1</sup> *Proc.*, iv, 381-495, and viii, 259-310.

<sup>2</sup> *Proc.*, xxiii, 306-569.

<sup>3</sup> *Proc.*, x, 25-422.



well authenticated and too numerous to be explicable by mistakes of memory or by chance. A strong case was made out for these so-called 'crisis-apparitions' being due to the 'externalization' by the percipient as a vision of telepathic impressions received by him subconsciously.

It is difficult, many would say impossible, to *prove* through the analysis, however skilful, of spontaneous incidents that mental impressions can be transmitted from one person to another otherwise than by the recognized channels of sensory perception. The elimination of chance as a conceivable explanation cannot be made absolutely certain. These cases can, however, throw much light on the working of telepathy, if confirmed by the results of experiment.

This was the second line of investigation. It was pursued, not perhaps as systematically as it might have been, in the early days of the Society, and is now the main preoccupation of psychical researchers both in this country and in the United States. Several different types of experiment have been tried. Sometimes the subject-matter has been of a more or less complex kind, such as incidents of real life, scenes from fiction, or pictures. In other instances the 'targets' have been cards with a limited number of designs on them to choose from. The former type, of which Dr Gilbert Murray's experiments<sup>1</sup> are a well-known example, is, like the crisis-apparitions and for similar reasons, not conclusive to all minds, although highly informative as to process. It is on the second type that interest is now mainly concentrated, since controlled quantitative experiment not only enables the investigator to determine precisely the extent to which a particular result may be expected to occur by chance, but by the refinements of modern statistical technique to distinguish between different modes of 'paranormal cognition', or 'extrasensory perception' (ESP), as it is now more generally called.

Experiments carried out in recent years by a large number of investigators, among whom Professor J. B. Rhine of Duke University, the late Whately Carington,<sup>2</sup> and Dr Soal deserve special mention, show that telepathy is not the only paranormal 'mental' phenomenon requiring study. There is evidence that some subjects have a grasp of future events exceeding the powers of rational inference ('precognition'),<sup>3</sup> and cases are on record of the direct apprehension of facts about physical objects (e.g. the order of cards in a shuffled pack) not normally known either to the percipient or anyone else ('clairvoyance').<sup>4</sup> There may be other paranormal faculties to be explored in the same way.

<sup>1</sup> *Proc.*, xxix, 46-110 and xxxiv, 212-74

<sup>2</sup> *Proc.*, xlvi and xlvi.

<sup>3</sup> *Proc.*, xlvi, 21-150.

<sup>4</sup> *Proc.*, xlvi, 1-28.

The question whether human personality survives the death of the body is one that falls within the scope of the Society's objects, although it is not specifically mentioned in the founders' manifesto. No reasonable person can be indifferent to this problem, and to many psychical researchers, including Myers, it has seemed the fundamental problem, to which all others are subsidiary. But Myers, as his great work *Human Personality and its Survival of Bodily Death* shows, was well aware that the problem must be approached through careful inquiry into the personality of man as we are familiar with it in the flesh, its powers and possibilities. No one, whatever the importance he attaches to the survival issue, ought to regard as mis-spent the time, labour, and skill now being devoted to a thorough exploration of ESP.

The direction taken by psychical inquiry at any given time has always been determined by the human material available for study. The psychical researcher should be prepared to deal with whatever kind of phenomenon he can bring within his net, whether mental or physical, experimental or spontaneous. He cannot plan the order in which problems will present themselves for solution.

A few years after its foundation the Society had the good fortune to contact Mrs Leonora Piper, the most remarkable of all 'mental' mediums. She was under close observation for about a quarter of a century, and when her powers were beginning to decline, another 'mental' medium, Mrs Osborne Leonard, whose powers were little inferior, became available for study. Through these two mediums, both of whom were most co-operative and of unimpeachable integrity, 'communications' were received which impressed as paranormal all the trained investigators with extensive experience of them, and were regarded by many as proof of the survival of the individuals from whom they ostensibly came. The difference of opinion rested on doubts as to the scope of telepathy between medium and sitter, and possibly between the medium and some third party unknown to her.<sup>1</sup>

In 1901, a few weeks after Myers's death, began the scripts of 'the S.P.R. group of automatists', which lasted for over thirty years. About a dozen automatists, many of them members of the S.P.R., produced scripts which taken separately often seemed to have little meaning, but when put together were found to convey a coherent message, ostensibly coming from a group which included Myers, Sidgwick, and Gurney. The significant point about these 'cross-correspondences', as they were called, was that they embodied an elaborate pattern divided among the scripts of the different automatists, certainly not attributable to the conscious mind, nor apparently to the subconscious mind of any one of

<sup>1</sup> W. H. Salter, *Trance Mediumship*. (S.P.R., 1950).

them.<sup>1</sup> Something more than telepathy between the living seemed, therefore, to be involved. For this reason, and on account of other features deserving study in detail, the scripts are generally regarded as among the most important pieces of evidence for survival. But once again the question arises, Do we know enough about the paranormal powers of incarnate minds to say with confidence that such or such occurrences can or cannot be assigned to them?

The Society was formed to undertake investigation on a wide front, neglecting nothing that could promote knowledge of human personality. It has until now held fast to this purpose, and should continue to do so. No line of research in which progress has been made should be abandoned. But there is one form of research which did not in the past attract as much attention as it might have done: controlled experiment into ESP. This is a line which now needs, and is likely for some time to need, vigorous prosecution. The planning and conduct of experiments of this kind and the analysis of the results are an extremely technical business calling for highly trained researchers supported by assistants handling the heavy incidental routine work, as is customary in an ordinary psychological laboratory. This task would not in any way prevent the Society from continuing its traditional, qualitative researches, as and when opportunity offered, but would rather advance them by enabling some elements of the problems to which they are directed to be more precisely defined.

W. H. S.

## THE SOLUTION OF PROBLEMS IN DREAMS

BY G. F. DALTON

### *Summary*

Brief accounts are given of a number of dreams in which problems are solved, or (as a special case) lost articles found. A general description of such dreams is given. All of them are wish-fulfilment or anxiety dreams, and the dream-figures which appear in some do so as a part of the wish or the anxiety. The wish-fulfilment principle is further used to account for the fact that the lost-article dream usually shows the article as at the time

<sup>1</sup> H. F. Saltmarsh, *Evidence of Personal Survival from Cross-correspondences*. (Bell, 1938).

of finding, not of losing. In some cases there is a precognitive element, although the essential information is obtained through retrocognition; to explain these the hypothesis of a double dream is put forward. In some cases where the dreamer is not the loser of the article (Squires, Chaffin) there must be a telepathic communication; the difficulty of locating this at any moment of ordinary time is considered, and it is concluded that communication takes place between four-dimensional entities. It is shown that clairvoyance is not a necessary hypothesis, retrocognition, telepathy, and precognition being sufficient to explain all the facts. In cases not concerned with lost articles (Lamberton, Hilprecht) the *prima facie* conclusion is that much intellectual work must have taken place in sleep. The incongruity of this with the general habits of the dreaming mind is pointed out, and it is suggested that here too there may be a precognitive element. In conclusion, the 'solution' dream is compared with other psychological events, including religious conversion.

FROM the earliest times a type of dream has been known in which a problem is solved, or (a special case of very frequent occurrence) a lost article found. Professor Dodds<sup>1</sup> gives examples from Greek temple records. Synesius, in his treatise on dreams (A.D. 404 or 405), considers the question in the light of his own experience. '... Of many of the things which present difficulties to us when awake, some of these it [the dream] makes completely clear while we are asleep, and others it helps us to explain. . . . It has frequently helped me to write books, for it has prepared the mind and made the diction appropriate to the thought. . . . Moreover when I am engaged in the chase, it has suggested to me stratagems of the hunter's art. . . .'<sup>2</sup>

Although he gives no details, it is probable that these revelations did not take a dramatic form; the Muses did not give him advice about his style, nor Artemis about hunting, or he would surely have mentioned the fact.

The first reasonably detailed and trustworthy account comes from St Augustine. As with most of the following cases, I give a condensation.

1. *Eulogius*.<sup>3</sup> A rhetorician named Eulogius, an ex-pupil of Augustine's, found himself in difficulties with a passage of Cicero which he had to expound to his class the next day. He was greatly troubled over this, and could scarcely sleep. In the night he dreamed that

<sup>1</sup> 'Telepathy and Clairvoyance in Classical Antiquity.' *Journal of Parapsychology*, December 1946.

<sup>2</sup> *Treatise on Dreams*, sect. 9; trans. A. Fitzgerald, 1930.

<sup>3</sup> *De cura pro mortuis gerenda*, sect. 11.

Augustine appeared to him and explained the passage. Augustine heard this story from Eulogius on his return to Africa.

Augustine quotes this for comparison with the celebrated Milan case, to show that dream-figures may be subjective. The latter, though third-hand and anonymous, can scarcely be left out of consideration.

2. *Milan*.<sup>1</sup> A young man in Milan was proceeded against for a debt incurred by his father, who had recently died. The creditor produced the father's promise to pay. The son was sure that the debt had been paid, otherwise the father would have made some mention of it in his will; however, he had no evidence, and was much distressed. In a dream the father appeared to him and told him where the receipt was. The son found it, and was able to prove that the debt had been paid.

A third dream, of a decidedly different type, is given in Augustine's *Confessions*.<sup>2</sup>

3. *St Monica*. ' . . . For whence else was that dream of hers, by which thou comfortedst her; after which she allowed me to live with her, and to eat at the same table in house with her, which she had already begun to be unwilling to do, refusing and detesting the blasphemies of my error. [Augustine was at this time a Manichæan.] For she saw, in her sleep, herself standing upon a wooden rule [*in quadam regula lignea*], and a very beautiful young man coming towards her, with a cheerful countenance and smiling upon her, herself being grieved and far gone with sorrowfulness. Which young man when he had demanded of her the causes of her sadness and daily weepings . . . and she had answered that it was my perdition that she bewailed; he bade her rest contented, and behold, that where she herself was, there was I also. Who when she looked aside, she saw me standing by her upon the same rule.'

Nine years later, Augustine was converted to Christianity.

This is practically a first-hand case, for St Monica told the dream at once to Augustine. Its genuineness is, I think, proved by the play on the word 'rule', which is as intelligible in Latin as in English. Neither Monica nor Augustine would in their waking lives have put a pun into the mouth of a divine messenger; on the other hand, the tendency of the dreaming mind to puns is well known.

I have not come across any well-attested cases occurring during the next thirteen hundred years. It is possible that they were not considered remarkable enough to be worth writing down. Mme de Marteville's experience has survived through the accident of a Swedenborgian legend having formed round it.

<sup>1</sup> *De cura pro mortuis*, sect. 11.

<sup>2</sup> Book III, trans. W. Watts.

4. *Mme de Marteville*. This lady, whose husband had recently died, was called upon to pay a debt said to have been contracted by him. Knowing him to be methodical in money matters, she was sure that he had paid the debt, and kept the receipt; the latter, however, could not be found. She was very worried, and applied to Swedenborg, who she thought might be able to get in touch with her husband. 'Eight days after she had made her request to Swedenborg, Mme de Marteville saw her late husband in a dream, and from him learned the place where she would find the desired receipt. She woke up at 2 a.m., satisfied herself at once of the truth of the dream, and went to sleep again. Towards eleven o'clock Swedenborg called, knowing nothing of what had happened. He also, as he said, had seen M. de Marteville last night. He wished to talk with him, but the latter would not enter into the subject, because he had to go to his wife in order to let her make an important discovery.' (According to the legend, of course, the discovery was made by Swedenborg.)

The latter part of the foregoing story is from a letter written in 1775 by a Danish 'General von E.', the second husband of Mme de Marteville. Professor R. Hennig, who quotes it,<sup>1</sup> concludes that the story is now cleared up 'in a quite harmless and natural way', apart from the last sentence, which may be considered as 'a scarcely believable embroidery'. But even taking Mme de Marteville's dream by itself, it is surely unduly disparaging to say, as Hennig does, that it is no more wonderful than dozens of similar stories. He gives a case, from 'the quite uncritical Perty', which concerns a clergyman's widow, a secret drawer, and a red velvet bag. This same case is quoted by du Prel and Büchner, the original source, it seems, being J. C. Hennings's *Traüme und Nachtwandler*, published in 1784. If there are 'dozens' to choose from, why is this anonymous and doubtful tale bandied about from author to author?

I pass over the affair of Mr R—d, narrated by Scott in a note to *The Antiquary*. It is third-hand and anonymous, it has obviously been 'written up', and it has been given already by half-a-dozen authors. Its chief point of interest is that at the time of the search Mr R—d had given up the search for the missing document. This, as will be seen, is a very frequent feature of these cases; it occurs also in the following:

5. *Lady Miller*.<sup>2</sup> Sir John Miller died. His wife had always understood that he was to leave her the house and a large jointure; but no will could be found. The heir-at-law allowed Lady Miller to stay for six months in the house in order to search for the missing will. At last

<sup>1</sup> 'Das Urteil über Swedenborg im Lichte der heutigen Wissenschaft.' *Zeitschrift für kritischen Okkultismus*, 1927.

<sup>2</sup> Miss F. P. Cobbe, 'Unconscious Cerebration', in *Darwinism in Morals and Other Essays*, 1872.

she came to the conclusion that her memory must have deceived her. On the last night of the six months Sir John Miller appeared to her, standing beside the bed, and said solemnly : 'There *is* a will!' Extra time was allowed for a further search, and in the end the will was found.

There appears to be no reason to doubt the authenticity of this ; but it might be objected that it is a waking hallucination rather than a dream. However, an apparition to a person in bed, who has just awakened or is just about to go to sleep, is so close to the region of dream that it would be pedantic to exclude it.

6. *Cashier*.<sup>1</sup> In an Edinburgh bank a customer one day insisted on being served before his turn. He was a stammerer and noisy, and caused so much annoyance that the cashier agreed to serve him first in order to get rid of him. Eight or nine months later, the books of the bank could not be made to balance ; there was a deficiency of £6. Several days and nights were spent trying to trace the error, but without success. One night in a dream the whole affair of the stammerer was re-enacted, and on waking the cashier (who had completely forgotten the incident) found that the amount paid to the stammerer, £6, had not been entered in the book of interests.

This account is anonymous, but was given to Abercrombie at first hand. Although it contains two unusual features, there is no special reason to doubt its genuineness.

Among over 700 cases set out in *Phantasms of the Living*, only one, No. 66, can be included in the class under discussion. It is another borderland hallucination.

7. *F.R.C.P.*<sup>2</sup> A doctor was attending a delirious woman. Her symptoms puzzled him ; he suspected alcoholism, but could not confirm this. He wished he could get into the house late at night. One night in bed he had a vision of the proprietor of the patient's house standing under a lamp post in the street, talking to another man. He rose at once, went to the spot he had seen in his vision, and found the two men there. He accompanied the landlord back to the house, went in, and found the maid giving drink to the patient.

This case was twenty years old at the time of recording, and no confirmation could be obtained. It is no doubt genuine, since it satisfied Gurney, Myers, and Podmore ; but one would have liked a great deal more detail.

With Myers's papers in *Proceedings* we are on firm ground. The cases are first-hand and detailed. Eighteen cases are given in Volume viii, and four more in Volume xi. Most of these are straightforward stories of articles lost by the dreamer, and

<sup>1</sup> Abercrombie, *On the Intellectual Powers*, 1831.

<sup>2</sup> *Phantasms of the Living*, i, 267.

recovered by a direct vision of the article without the help of any dream-figure ; but the following present special points of interest :

8. *Mrs Stuart*.<sup>1</sup> A friend lost a stone from a ring. 'I would not have done this for the world ; it belonged to my father,' he said. All searched, but without success. Mrs Stuart that night dreamed of the stone lying on the lawn under a fallen leaf, with dewdrops sparkling in the sun. She woke, went out at once—it was 6 a.m.—and found the stone just as in the dream.

9. *A.v.S.*<sup>2</sup> A little girl, eight or nine years old, lost a knife. 'The loss nearly broke her heart.' Next night she dreamed that her dead brother appeared and showed her the place where the knife was. It was found there next day.

10. *Wilmot*.<sup>3</sup> Mr Wilmot, a gardener, lost his wages (fifteen shillings) on his way home. He made every enquiry, but without success. During the next night he dreamed that he went to a house he had visited the day before, and, crossing the road after he had left it, walked into a mud heap ; that his foot struck the paper containing the money, the half-sovereign rolled away and the five shillings remained under his foot. The dream was repeated. Next day he went to the place indicated, and the dream fulfilled itself exactly.

11. *Peterson*.<sup>4</sup> Some crucial evidence was missing in a case in which Mr Peterson was engaged as a barrister. He dreamed that he was addressing the court, and declared that he had found in an account-book an entry which cleared up the mystery. The opposing counsel then denounced him as a liar, Peterson in reply threw an inkstand at his head, and in short the dream proceeded in a dream-like manner. On waking he at once went to his client and found the entry as in the dream.

12. *Yates*.<sup>5</sup> Mrs Yates lost some photographs. A year later she dreamed of taking out a certain drawer and finding them ; but on waking she did *not* look there.<sup>6</sup> Some months later the drawer was taken out for another reason, and the photographs were found.

13. *Squires*.<sup>7</sup> The dreamer's friend, Davis, lost his watch. Both men were at the time employed on an American cattle-farm. Squires was sympathetic ; 'could not keep his mind off the watch, and, after two or three days' thinking of it, went to bed one night still thinking of it.' In his dream he saw the watch. The dream vision is described in detail ; the watch was lying in long grass, with its chain in a half circle. Next day Squires went to the place and found the watch. At the time of losing it he had been some distance away, and could not have seen it fall.

<sup>1</sup> *Proc.*, viii, 384.

<sup>2</sup> *Ibid.*, 387.

<sup>3</sup> *Ibid.*, 388.

<sup>4</sup> *Ibid.*, 396.

<sup>5</sup> *Ibid.*, 383.

<sup>6</sup> This unenterprising behaviour contrasts with the splendid zeal of Mr Peterson, who started from his bed at 2.30 a.m., called for boots and saddle and relays of horses, knocked up his client after a three hours' ride, had the books brought before him, and *to his surprise* found that the dream was correct!

<sup>7</sup> *Proc.*, xi, 397.



14. *Howe*.<sup>1</sup> Judge Howe had bought a house, and, naturally, had made enquiries as to title, liens, etc. He dreamed that he met the sheriff, who told him that he was going to sell the house on account of a debt of \$446.50, in a claim of *R. M. v. J. Chaffe*. Judge Howe next day told his dream to Chaffe junior, from whom he had bought the house (the Chaffe mentioned in the dream being dead). Mr Chaffe went to his ledger and turned up an entry of a debt of \$444.50 due to R. M. from his father, which with interest came to \$446.50, the exact amount mentioned in the dream. The debt had however been settled before the death of Mr Chaffe senior.

Mr Chaffe gives his own statement confirming Judge Howe's, and making clear that he knew of the debt, and merely referred to the ledger for confirmation. This was not the case as regards the interest, however. According to Howe, Chaffe was surprised to find in the ledger a pencil entry giving the amount of the interest; Chaffe himself says that he reckoned it there and then.

Professor W. Romaine Newbold's paper on *Subconscious Reasoning*<sup>2</sup> contains two interesting examples.

15. *Lamberton*. Professor Wm. A. Lamberton tried to solve a mathematical problem by algebraical means. After a week or two, 'I came to the conclusion that I was bogged'. He dismissed the problem from his mind. In his bedroom there was a disused blackboard; and one morning a week later, when waking, he saw on this blackboard a diagram giving a geometrical solution. The diagram vanished at once, but he remembered it, and it proved correct.

Newbold's second case is so well known that it need not be quoted. It is that of Professor Hilprecht, whose archaeological problem was solved by the help of a dream-figure of an Assyrian priest.

Hilprecht's dream, with the R—d, Eulogius, and Milan cases, are quoted by Andrew Lang.<sup>3</sup> He adds two others coming within his own knowledge—lost articles recovered without dream-figures. Havelock Ellis<sup>4</sup> also discusses the question, and gives a modern parallel to the Eulogius case in which the mentor is a chemical expert and the dreamer an inventor. Carpenter<sup>5</sup> devotes a chapter to 'Unconscious Cerebration', but (apart from extracts from Miss Cobbe's paper) his examples are concerned with somnambulism, and are in a different class. For the same reason I have not included accounts of the numerous lost articles recovered by 'Dreaming Joe' (Johannes Jonsson), as described by Professor A. Bjarnason.<sup>6</sup> 'Dreaming Joe' was questioned in his sleep, and replied giving information about the objects. Clearly

<sup>1</sup> *Proc.*, xi, 403.<sup>2</sup> *Proc.*, xii, 11.<sup>3</sup> *Dreams and Ghosts*, 1897.<sup>4</sup> *The World of Dreams*, Constable, 1911.<sup>5</sup> *Mental Physiology*, 1874.<sup>6</sup> *Journal*, xvii, 53-82.

this is not ordinary sleep, and the cases are not spontaneous and cannot properly be included here.

Mrs Sidgwick's collection *Phantasms of the Living*<sup>1</sup> contains two cases of 'solution' type. One of these (Wilson, p. 71) is a waking impression, but is otherwise similar to the Squires case; the other is a dream. I give Mrs Sidgwick's summary.

16. *Hodgson*.<sup>2</sup> 'A burglary had occurred at Mrs Hodgson's house on September 30th, 1897, and among other things a small papier-mâché box which she valued had been taken. About a week later she dreamed she went into the coal cellar and found it hidden among the fine coal. She told her daughter of the dream the next morning, but it seemed so absurd that no search was made. In August 1898 Mr Hodgson went into the cellar to see how much coal there was, and found amongst the fine coal the lost box wrapped in a newspaper of the date of the burglary.'

The next case is as well known as Hilprecht's, but it is a crucial one.

17. *Chaffin*.<sup>3</sup> J. L. Chaffin in 1905 made a will leaving his farm to the third of four sons. In 1919 he made a new will leaving everything equally between his sons. This will was unwitnessed, but, being holograph, was valid under the local State law. Its existence was not known to anyone else. J. L. Chaffin died in 1921, and the earlier will was proved. In June 1925 the second son, J. P. Chaffin, had vivid dreams of his father, in one of which the latter appeared and said, 'You will find the will in my overcoat pocket.' The overcoat was found. In its lining was not the will but a paper referring to an old Bible. The will was discovered between the pages of the Bible. (In this case also it is uncertain whether the percipient was awake or asleep.)

In a series of articles on 'Remarkable Dreams',<sup>4</sup> Dr W. H. C. Tenhaeff includes a discussion of the 'solution' type, with numerous examples. These are first-hand and recent, but confirmatory statements and fuller details would be useful in some cases.

18. *W. H.* Mrs W. H. thought she had left her bicycle as usual in a garage, but when she came to claim it, it could not be found. That night she dreamed that she saw the machine standing in a yard of a grocer's shop, and that on coming out on to the street she saw nearby another shop which she had visited on the previous day. The next day she went to the place, looked into the yard, and saw the bicycle. She had left it standing in the street and forgotten about it, and it had been put into the yard overnight. The shopkeeper brought it out for her, so that she never saw the yard from inside as in the dream.

<sup>1</sup> *Proc.*, xxxiii.

<sup>2</sup> *Proc.*, xxxiii, 351.

<sup>3</sup> *Proc.*, xxxvi, 517.

<sup>4</sup> *Tijdschrift voor Parapsychologie*, x.

19. *L.P.E.* The solution of an inventor's problem was revealed by the dream-figure of his mother.

20. *J. v. M.* A valuable ring was lost. J. v. M. feared it had been stolen by a maid who had lately been dismissed. In a dream he heard a voice saying: 'What you thought has not happened, and what has happened is what you would never have thought. Be at ease; the ring is not stolen and will come back.' As a result of the dream, he felt easier in his mind, and took no action in the matter. A few days later the ring was found. His daughter had put it in one of the decorations of a Christmas tree, and forgotten about it.

Since the publication of Myers's papers, a number of fresh cases have been printed in the *Journal*. Of these the following are most noteworthy:

21. *Smith*.<sup>1</sup> At a church in Sydney, N.S.W., a hymn tune was sung which Mr Smith had never heard before (although he had seen the name of the tune in an old collection at his house in Aberdeen). He was struck by the tune, and tried to learn it while it was being sung, but forgot it soon afterwards. A month later he left for London. 'On the passage home I often used to try to recall it, as it still haunted me, but I could not do so, try as I might. One night while on the passage home (the passage lasted 116 days from February 3rd, but I cannot even approximately give the date of the night I refer to) I dreamt that I was at the same church in Balmain, that the same hymn was sung to the same tune and that I resolved that this time I should not forget it . . . Just as it finished, I awoke with the tune ringing in my ears and I knew it correctly.' He never afterwards forgot it.

In May of the same year a case is given which concerns two sisters. One of them was puzzled as to whom a certain person resembled; the dream which gave the answer was dreamed by the other sister, who had seen neither the person in question nor the other person whom he resembled. Apart from occurring to the wrong sister, the dream is of normal type.

The next case of interest is:

22. *Blaikie*.<sup>2</sup> 'In April, 1892, I had given to me an old cross of very good old paste and good design, which I shortly afterwards wore, for the first time, at the theatre. On reaching home in the evening I found that I had dropped it or had it stolen, and went to bed mourning its loss.

'On falling asleep I dreamed what actually had happened—that I had lost it on my way from the theatre—but then fancied that I found myself, in broad daylight, in the drawing-room of the house in — Place, W., where I was then staying, and which overlooked the front door. In my dream I looked out of the window and saw, lying in the gutter immediately in front of the door, my cross, and rushed downstairs and into the street and picked it up.

<sup>1</sup> *Journal*, viii, 29.

<sup>2</sup> *Journal*, x, 30.

'In the morning I had quite forgotten my dream, but in the afternoon when I was having tea in the drawing-room with my (hostess), she spoke of the cross I had lost and the unlikelihood of my ever seeing it again. I then remembered the dream, and told her about it.

"I went forward to the window", I said, "and looked out, and there it was, lying in the gutter, close to the kerb."

'She laughed, and we both went to the window and looked out, and just then the sun caught on something lying in the gutter, just at the kerb.

'It was the old paste cross.'

Miss Blaikie's account is corroborated by her hostess.

Several cases of bodies being found through dreams are on record. In the following the 'solution' aspect is especially noticeable :

23. *Blunt*.<sup>1</sup> James Blunt, a coal-miner of New Lambton, Co. Durham, and Police-constable Egleton, stationed in the same district, were old friends. One Saturday night in January 1902 Egleton disappeared. A search was organised over the whole district, and carried on energetically for four days by parties which included thirty or forty constables ; but it proved fruitless. Blunt also assisted in the search. 'He talked a lot about him, saying he wished he could be found.' In the course of the search a certain stream running through Brecon Hill Wood had been explored by Blunt and others. On Thursday morning Blunt told five people (who confirm the story) of a dream he had had in the night. 'I dreamt that I saw the burn running through Brecon Hill Wood, and in the stream by a stump of a tree P.C. Egleton was lying with his head to the stump, and I saw him in the dream lying in two feet of water.' He went to the spot at the first opportunity, later in the day, and found the body at the place seen in the dream. There was afterwards noticed a mark on the bank, which may have been made by Egleton falling or throwing himself in. These facts came out at the inquest, at which an open verdict was returned ; but Egleton had been very depressed before his disappearance, and had had a breakdown in health and family troubles as well.

Another 'lost body' case, Mme Clarinval's, can scarcely be included here, as it is a daylight hallucination ; it is discussed later.

24. *Wild*.<sup>2</sup> Miss Ida Wild lost a hammer. 'I hunted everywhere likely for it, and had it constantly in mind.' Some weeks later, 'I dreamt I was walking in the long rough grass near the beehives, and saw my hammer, fine and bright, on the top of the grass.' The hammer was found there, but entirely covered by grass which had to be cut away to find it. It was not bright, but very rusty, and the haft sodden.

<sup>1</sup> *Journal*, x, 298-303. For some extra information bearing on this case I am indebted to Mr D. B. Cameron of the King's College, Newcastle, Psychical Research Society.

<sup>2</sup> *Journal*, xxii, 28.

The case of Mrs X. (*Journal*, xxix, p. 272) is of the Squires type ; a key lost by Mr X when out fishing was found by Mrs X through a dream, although she had never been to the spot. Of the same type is a case described by Major Günther,<sup>1</sup> Frau Günther being the loser and her daughter the dreamer. He gives two other cases of lost articles recovered ; but the persons concerned are professional mediums, and the present paper must limit itself strictly to spontaneous cases.

These examples have been chosen from some ninety cases.<sup>2</sup> Doubtless a more thorough search would disclose many more, but it is clear that dreams of 'solution' type are rarer in the literature than telepathic or precognitive dreams of the ordinary kind, which can be counted by the hundred. (In the present paper, both telepathy and precognition will be taken as established.) However, enough have been recorded to distinguish the type. Some problem is presented to the waking mind which it cannot answer. The subject is very much concerned over his failure.<sup>3</sup> This is expressly stated in most cases ; in others, such as Chaffin's (17), it may be presumed. Sometimes he dismisses the question from his mind, and abandons the search ; in others, that he was not worrying acutely at the time of the dream may be presumed from the simple fact that he was asleep. In a dream the solution of the problem is presented, sometimes by a direct vision, sometimes through the mouth of a dream-figure. On waking the solution is tested and found correct.

In contrast, the ordinary precognitive or telepathic dream arrives out of a clear sky. In some examples (such as the Terriss murder, quoted later) the dream may be the fulfilment of a subconscious wish, but not the solution of a problem consciously brooded over. It is this, rather than the action taken as a result of the dream, which marks off the class. It is true that these dreams are almost invariably, and other veridical dreams rather rarely, followed by action<sup>4</sup> ; but this is simply because unless action was taken the

<sup>1</sup> *Zeitschrift für metapsychische Forschung*, December 1936.

<sup>2</sup> They include most of the cases with dream-figures, and every well-evidenced case which appears to be an exception to the generalisations which follow. The cases not mentioned are mainly simple lost-article dreams, or are insufficiently detailed or authenticated.

<sup>3</sup> One of Dr Tenhaeff's correspondents, who frequently solves problems in sleep, says that the more he concentrates on the problem when awake the more easily does he dream about it.

<sup>4</sup> The exceptions are St Monica (3) and J. v. M. (20), where inaction was enjoined by the dream itself ; Yates (12), Hodgson (16), and possibly Blaikie (22), where the article was found by chance ; and Smith (21), where no action was necessary.

lost article would not be found, or the problem would not be solved, and the dream would never be revealed as veridical at all.

There is an interesting group of cases which, though they cannot be included with the 'solution' type, should not be left unmentioned; they may be described as 'professional anxiety' dreams. This group includes 'Father Brompton' and the dying woman<sup>1</sup>; the coachman and the mare<sup>2</sup>; the surveyor and the bridge<sup>3</sup>; the insurance official and the fire<sup>4</sup>; and the biographer and her subject.<sup>5</sup> The information is in most cases acquired through precognition, but the first case is possibly telepathic, and the last probably retrocognitive. An interesting feature is the large proportion of auditory dreams or hallucinations.<sup>6</sup>

Almost all these dreams, it is clear, are wish-fulfilments. The one exception—Howe's (14)—is an anxiety dream, which is a wish-fulfilment reversed. They differ from the majority of the class in that the dream is not a substitute for the real-life event, but a stepping-stone to it; but this does not affect the structure of the dream itself. When the figure of some other person supplies the information which solves the problem, it is to be presumed that his appearance is a part of the wish: 'I wish F. were here to tell me about this.' F. must therefore be a person to whom the subject would naturally turn. He must be believed to be able and willing to supply the information, and he must be known to the dreamer. This does not exclude Hilprecht's priest, whose appearance would be quite familiar to the Professor, or St Monica's 'beautiful young man', the traditional divine messenger; but it explains why the

<sup>1</sup> *Proc.*, xxxiii, 287.

<sup>2</sup> *Ibid.*, 138.

<sup>3</sup> *Proc.*, viii, 397.

<sup>4</sup> *Ibid.*, 396.

<sup>5</sup> *Journal*, v, 253.

<sup>6</sup> The following story has no evidential value, having been handed down through three generations, but on internal evidence appears genuine. I take it from Dr C. Dickson's *Life of Michael Dwyer* (p. 180). It dates from about 1800, when Dwyer was being hunted through the Wicklow mountains, with a price on his head. A year or two before, McAlister, one of his followers, had saved his life at the cost of his own. 'One night as Dwyer had retired to a cave he had made in a double ditch in Leitrim (Glen Imaal) and which was unknown to anyone except McAlister and himself, McAlister's spirit appeared, called him and saved his life, he said. Before he was long in, he lay down tired after his watchful day and soon was in slumber, when to his great astonishment he was awakened by a voice calling him. He sat up, listened for some time and heard nothing; but thinking he was only dreaming, lay down again in silence to listen, whereupon he distinctly heard McAlister's voice calling him in stern tones to "get up". He at once recognised the voice, leaped up and peeped out just in time to find that he was almost surrounded by redcoats . . .' and succeeded in escaping. It should be added that McAlister was the only Ulsterman in Dwyer's party, and his Northern accent would be quite unmistakable. As a dream-figure he is obviously highly appropriate.

loser does not appear as F. in cases of the Squires type (13), why Peterson (11) did not imagine the opposing counsel or his client as giving him the clue to the missing evidence, and why Mrs W. H. (18) did not see the grocer in her dream. In Howe's case (14), similarly, the sheriff is part of the anxiety, and so comes in more aptly than Chaffe senior or junior, either of whom, if he had the information, might not be willing to give it. The sheriff, also, would be a familiar figure to the Judge.

The deceased person is obviously the only possible dream-figure in cases of the Marteville type (4), and he never fails to make his appearance. That the pious Monica sees angels, and the free-thinking Synesius sees none, needs no lengthy explanation. Two cases, however, remain a little anomalous—A. v. S. (9) and L. P. E. (19). The little girl's dead brother knew no more of the knife than anyone else, and his entry into the dream seems inappropriate. Similarly, there is no reason to suppose that the inventor's mother knew anything of mechanics or chemistry. But both these are persons to whom the dreamers were accustomed to turn in *any* difficulty; their use in the dream is inartistic, perhaps, but not unnatural.<sup>1</sup>

The only case which cannot be brought under the formula 'I wish F. were here to tell me' is F. R. C. P.'s (7). The landlord, however, is not a dream-figure in the sense in which we have been using the term. He is not the bringer of news, but is himself the sought-for object. The message conveyed is: 'The landlord is coming home late; you can meet him and get into the house.' This is conveyed in the simplest possible way.

There are a number of cases in which the dreamer has no reason to suppose that anyone can help him. In nearly all such cases the dream is in the form of a direct vision. Sometimes, however, the information is difficult to express in images. This difficulty is surmounted very neatly in Peterson's case, in which he is himself the dream-figure. In J. v. M.'s an impersonal voice is heard, and the same device is used in another dream of Peterson's<sup>2</sup> which I have not quoted. This difficulty in direct presentation is,

<sup>1</sup> The 'Devil's Sonata' dream of Tartini might be cited as an example of an inappropriate dream-figure. However, the dream is not of 'solution' type. Tartini was under no obligation to compose a sonata; there was no problem, and no anxiety. I am not therefore compelled to explain the Devil. However, it may be of interest to note that Tartini was destined by his parents for the Church, but expressed such a repugnance to the idea that they were compelled to give it up. From an early age, music and the Church must have been opposed to each other in Tartini's mind; that the Devil, the enemy of the Church, should appear as the provider of music, is quite in accordance with the logic of dreams.

<sup>2</sup> *Proc.*, viii, 395.

perhaps, a contributory cause of the use of imaginary figures like Hilprecht's priest.

In studying spontaneous cases, the only form of experiment possible is the construction of fictitious cases, illustrating or contravening the rules derived from the real cases. If the passage of time, or a more thorough examination of the literature, supplies real cases corresponding to the first fictitious class, but not to the second, these rules will be established more firmly ; if not, they must be reconsidered. Thus, the conclusions arrived at above may be summed up by saying that the following should *not* occur :

*Fictitious Case I.* D. is much concerned over a certain problem. In a dream he sees F., a person quite unknown to him, or an acquaintance whom he has no reason to associate with the problem ; this person tells him the solution. D. wakes, tests the solution, and finds it correct.

It appears, then, that the conclusions of Augustine in the Milan case<sup>1</sup> are fully justified. The dream-figures are in all cases constructs of the dreamer, and have no relation to the objective situation. The appearance of Chaffin senior to Chaffin junior (17) does not prove survival ; the non-appearance of Davis to Squires (13) does not disprove telepathy. The sole means of judging is the information itself.

It is indeed remarkable that this information shows so little sign of its origin. It will not be disputed that in most lost-article cases the source of the dream is the revival of a forgotten memory. That memory must be of *the act of losing* ; yet this act itself hardly ever appears in the dream. The following seems a very plausible sequence :

*Fictitious Case II.* D. loses a valued article. He dreams of putting it in a certain place. On waking he looks there and finds it.

Or, in the Marteville type :

*Fictitious Case III.* F. dies. D. cannot find his will. He dreams of F. going to a secret drawer, etc., and putting the will there. It is found there on waking.

Nothing exactly like either of these is known to me. There are a few special cases discussed below ; but in general the dream concerns itself with the finding, not the losing. Mrs Yates (12) does not dream of putting the photographs in a drawer, but of taking them out. Squires does not see the watch fall ; he sees it fallen. Mrs Hodgson (16) does not dream of the box being hidden in the cellar, but of the box being discovered ; and so on.

Here again the wish-fulfilment principle must be invoked. The dreamer's thought is not 'I wish I knew how I lost the article',

<sup>1</sup> And of Mr Tyrrell (*Proc.*, xlviii, 81 ff.).



or even, in most cases, 'I wish I knew where it is', but 'I wish I had it now'. The dream, therefore, satisfies the wish most completely by looking forward to the point of application of the information. Peterson, for instance, sees himself making his point in court. It is the same in anxiety dreams; Howe's vision of the sheriff about to sell the house is a long leap forward from the (erroneous) information of Mr Chaffe's debt.

Miss Wild's case is an exception which, in the true sense, proves the rule. She sees the hammer clean and bright, lying on top of the grass, as it was when lost. But clearly it was no part of her wish that the hammer should be rusty and sodden, and only extricated with difficulty from the long grass which had grown over it. Even in this case, it is to be noted, the action of losing the article is not reproduced in the dream.<sup>1</sup>

It is reproduced in Smith's case (21); but a hymn tune is no ordinary lost article; the moment of waking from the dream was the moment of finding. Smith was not looking forward to any special future occasion on which he was to sing the hymn, and he had not been taught it by anyone who could serve as a dream-figure. In the circumstances it is difficult to see what else the dream could have done.

The same applies to the cashier's dream (6). This is not a lost-article dream; the missing amount had not been recorded anywhere, and the recollection of the event was the only way in which the problem could be solved. This case is also interesting in suggesting a possible unconscious motive for the forgetting—the wish to be rid of an unpleasant and embarrassing scene. It may be suggested that an unconscious motive is at work in some other lost-article cases; but in the majority the loss seems to have occurred very naturally.

The dream which looks forward to the point of application of the information provides favourable conditions for precognition; and precognitions are in fact found. The cases of Stuart (8), Squires (13), Blaikie (22), Hodgson (16), W. H. (18) and F. R. C. P. (7) are *prima facie* either clairvoyant or precognitive; precognition is possible in those of St Monica (3) and J. v. M. (20); and the precognitive element in the Wilmot case (10) is inescapable.

The chief difficulty about precognition is, of course, the apparent reversal of causation. The first serious attempt to solve

<sup>1</sup> In one of Dreaming Joe's dreams the action of losing was reproduced, but the case was denied by Joe himself and could not be confirmed. In another, Joe dreamed of a thief putting a missing purse in a certain place, *but it was not found there*, nor was the thief ever identified. This suggests that with professional 'clairvoyants' (Joe was almost a professional) the desire to impress the client may have its effect on the form of the vision.

this problem was made by J. W. Dunne.<sup>1</sup> In Dunne's theory there are an infinite number of Time dimensions, of which, however, only the first two need concern us here. His 'Time 1' is 'ordinary' time; an observer to whom this is Time sees a three-dimensional world in which various events occur in succession. But there is also a 'Time 2', and a corresponding 'Observer 2'. To the latter, Observer 1's three-dimensional field of view appears as a moving section of a four-dimensional world. By concentrating his attention on Observer 1's view, Observer 2 perceives the three-dimensional aspect of things. If this attention is relaxed, however, or if Observer 1 is not functioning—that is, in states of abstraction or unconsciousness—Observer 2 reverts to his proper four-dimensional view. He then sees Time 1 as an extra dimension of space, and all its length is equally present (though not equally accessible or equally interesting) to him. He can thus perceive an event which is future to Observer 1; but to him it is neither future nor an event, but a mere static configuration. Observer 1's impression that an event is happening is an illusion caused by the 'present moment' which moves along Time 1—an illusion similar to that of a cinematograph. The only events which Observer 2 sees as really happening are, first, the steady movement of the 'present moment' along Time 1, and, secondly, occasional changes in the Time 1 'future' due to intervention—of which more must be said later.

It follows from the above that the Freudian 'unconscious', or the 'subconscious' or 'subliminal mind' of other authors, which is dominant in dreams or states of abstraction, must contain a large element of Observer 2.

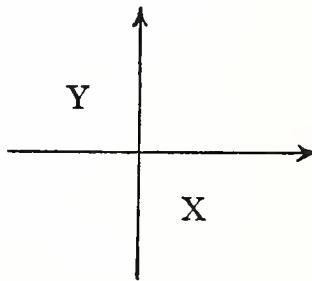


FIG. 1

Professor C. D. Broad<sup>2</sup> has also produced a theory of pre-cognition using two Time dimensions. These are not, however, connected by any law as in Dunne's theory. For illustrative purposes they are conceived as being at right angles (Fig. 1).

<sup>1</sup> *An Experiment with Time*, 1927.

<sup>2</sup> *Knowledge and Foreknowledge*. Aristotelian Society, Supplementary Vol. xvi, 1937.

An event X which is after Y in Time 1 may therefore be before it in Time 2.

Broad's theory is much less elaborate than Dunne's; indeed it is a mere suggestion, put forward in the most tentative manner. It can be seen, however, that its solution of the problem of reversed causation differs radically from Dunne's. Dunne asserts that the supposed future event is really present, Broad that it may be both past and future. It is thus theoretically possible in Broad's theory, but not in Dunne's, for an event to be the cause of its own cause. Broad's two times exist together; an event which is before another in Time 1 may be subsequent to it in Time 2, and causation may operate in either Time dimension. To any of Dunne's observers, however, there is only one real Time, and all inferior times are dimensions of space. Observer 2 may see an apparent cause-and-effect sequence running along Time 1, but it is merely apparent, frozen as it were; events, to him, happen in Time 2, and it is only in Time 2 that there can be causes and effects.

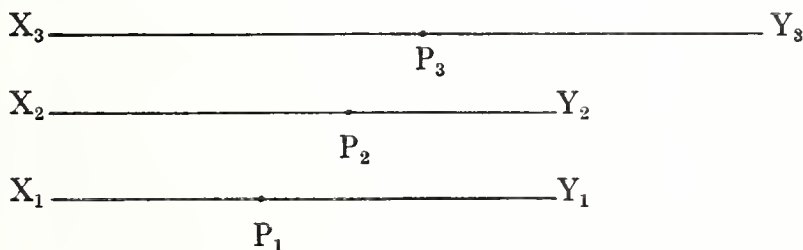


FIG. 2

It happens that one case in the present series raises this question sharply. It is Wilmot's. The dream contains a precognition of a future event—it is therefore presumably caused by that event; but the event itself—the finding of the money—is the result of the dream! This appears to be an example of circular causation, and hence tells strongly in favour of Broad's theory as against Dunne's.

Dunne's theory, however, contains an important feature which so far has been barely mentioned—the conception of 'intervention'. Observer 2 is not merely a passive spectator. He can intervene to alter the Time 1 future, and there are many cases on record to suggest that he sometimes does so. Fig. 2 represents such an intervention.

XY represents the subject's life, and P the 'present moment'.  $X_1Y_1$ ,  $X_2Y_2$ , etc., are states of the life which are successive in Time 2. The 'present moment' moves along XY, occupying the successive positions  $P_1$ ,  $P_2$ ,  $P_3$ .

At the moment of Time 2 represented by the bottom line, Observer 2 looks along  $X_1Y_1$  and observes the point  $Y_1$ . The result is, let us say, a dream about an aeroplane crash. As a result of the dream he takes action—for instance, he cancels his intended flight. This action results in an alteration of the Time 1 future, including a lengthening of the subject's life as shown by  $X_3Y_3$ . Here we have three events—the dream, the cancellation of the flight, and the lengthening of the life—which are linked in a causal sequence. Now, starting from the latter end, the sequence can be traced back in Time 1 without difficulty up to a certain point. The lengthening of the life is caused by the cancellation of the flight, which is caused by the recollection of the dream. But this last item has no causal ancestor in Time 1. In that dimension it appears to spring from nowhere. To trace the chain further we must employ the outlook of Observer 2; we can then say that the dream resulted from observation of  $Y_1$ . (It may be remarked that  $Y_1$ , the event precognized, is not future to any observer; for Observer 1 will experience quite a different event when he comes to the point in  $X_3Y_3$  corresponding to  $Y_1$ . *No observer in the Dunne series can foresee the real future.* In particular, no observer can foresee the results of his own intervention.)

If Observer 2 intervenes, therefore, the causal sequence must be traced first in Time 2 and then in Time 1. The moment of change from one Time to the other is, presumably, the moment when the dreamer wakes. He gets up, goes about his business, and the next night goes to sleep again. But now the Time 1 length open to his inspection is not  $X_1Y_1$  but  $X_3Y_3$ ; in other words he may now precognize the events which will (in Time 1) result from his recent intervention. This may result in a new intervention, and so on indefinitely; the chain of cause and effect zig-zags across the diagram. But it cannot turn back, either in Time 1 or Time 2.

We may now return to the Wilmot case. Let us suppose that Wilmot has a dream in which his memory of the losing of the money is revived. This, just as much as a precognition, is the work of Observer 2; the dream is derived from inspection of a part of  $XY$  between  $X$  and  $P$ . He wakes up with the memory of the dream. This, given his character and all other circumstances, determines that next day he will search for the money and find it in the place dreamed of. In other words, the recollection of the dream is intervention, and alters the Time 1 future. As he still lies in bed, the finding of the money has become part of that future. There is therefore no reason why, if he goes to sleep again, he should not have a precognitive dream of the finding; and this will be the case however short or partial the waking may be. If now the second dream obliterates the memory of the

first, it will make no difference, for the second dream contains the same essential information as the first, plus some extra details derived from precognition. The one-way sequence of causation is thus preserved.

It remains to be seen how this hypothesis will apply to other cases. It has already been noted that the act of losing the article very seldom appears in the dream, although it must have been the dream's starting-point. This missing link in the causal chain must have existed somewhere, and will fit very neatly into the hypothetical first dream. It may appear surprising that the first dream is so completely forgotten; but after all the remembering of a dream, not its forgetting, is the unusual event, and in nearly all cases the two dreams may be presumed to be so alike that they would become fused in recollection. Secondly, the first dream *does* sometimes leave a trace in memory. In the Wilmot case itself, we are told that the dream was repeated. It is quite possible that on its first occurrence it lacked the details about the money rolling away. In Miss Blaikie's case (22) the whole mechanism can be seen. The dream first refers to the losing, and then passes to the act of finding. That the first part of the dream is preserved on this occasion is, perhaps, due to the circumstance that Miss Blaikie feared her ornament had been stolen. By re-enacting the loss, the dream reassures her on this point. The message of the dream is in fact in substance the same as J. v. M.'s, although so differently expressed.

If this theory of the Wilmot case be accepted, it can be applied also to the touches of apparent clairvoyance in other cases. Mrs Stuart saw the stone lying under a fallen leaf, with dewdrops sparkling in the sun—just as it was at the time of the dream, but also just as it was when she found it a few minutes later. Squires saw the watch among tall grass, 'the steel chain lying in a half-circle'—a picture that could not have been derived from the mind of Davis, the loser; but Squires himself saw it so on the following day. Mrs W. H. leaves her bicycle in the street, and finds it in a yard; she dreams of it in the yard. F. R. C. P. finds the landlord talking to a friend, just as he had seen him in the vision; he did not enquire, apparently, if the friend had been there at the time of the vision some minutes earlier. Howe dreams, not of the amount known to Chaffe, but of the amount plus interest, which he did not find out till later. Blunt sees the body of Egleton as nobody—including Egleton himself—had seen it at the time, but as he himself saw it next day.

The precognized details may on this hypothesis be set aside, as the dream-figures may be set aside, in the search for the source of the dream. What remains is the bare minimum of information

necessary to cause action. How does this information find its way into the dream? There are several possibilities.

I. *Chance*. The hypothesis of chance coincidence has been given very little attention in this connexion. It implies that there are many such dreams, and that the non-veridical ones are not recorded. It is true that these would naturally be rather elusive. If Peterson, for instance, after his Paul Revere ride, had *not* found the missing evidence, it is pretty certain that he would not have told the story to Myers or anyone else! But the probability of finding the object by pure chance is so low in such cases as those of Squires and Mrs Stuart that these cases would require to be balanced by thousands of falsidical ones. It is exceedingly improbable that all these cases would have escaped the attention of all the authorities who have considered the subject.

Moreover, since the publication of *Die Traumdeutung* one does not talk so lightly of random dreams. A dream impressive enough to lead to action is almost certainly *not* random, whether veridical or otherwise. If in such a dream a lost object is seen in some place, there must be *some* association between the place and the object. If there is a chance coincidence, it must be between two such associations; for instance a missing person may, in life, have frequented a spot where his body is later discovered. In Blunt's case, however, this does not seem very likely. Or the association may be of the symbolic type; the connexion between money and dirt, noticed by Freud, may have led Wilmot's dreaming mind to the consideration of mud-heaps; but we have still to explain why he dreamed of the one particular mud-heap which actually contained the money, out of all the mud-heaps which he may have seen. And what association other than real contiguity could exist between Davis's watch and a particular clump of grass?

These considerations apply chiefly to dreams of the true 'solution' type, preceded by anxiety over a particular problem, and followed by action. As regards wish-fulfilment and anxiety dreams in general, it is undeniable that the great majority come out of the gate of ivory. The St Monica type, in which the dreamer's action does not cause the *dénouement*, is quite easily explicable on a chance basis. The event prophesied is, in each of our two cases (3, 20), not very improbable. But even here the hypothesis of chance coincidence would be greatly strengthened if there were a large number of cases on record in which divine messengers, or impressive voices heard in dreams, gave *false* information.

II. *Retrocognition*.<sup>1</sup> In most of the cases it is probable, or at

<sup>1</sup> Following Myers, I use this term rather than 'hypermnnesia', to mark its similarity to precognition and its difference from ordinary memory.

least possible, that the information was at one time in the dreamer's possession, in which case its revival in the dream is understandable. It is not necessary that he should have consciously known it, for there is ample evidence that incidents ignored by consciousness may be noted unconsciously.<sup>1</sup> This explanation will not serve, however, for the dreams of St Monica, J. v. M., Squires, Howe, Hodgson, Chaffin, and the vision of F. R. C. P. ; and in the Marteville case it is unlikely.

III. *Precognition*. There is no difficulty in accepting Mrs Hodgson's dream as precognitive, and the same explanation is applicable in any of the cases in which there is no intervention by the dreamer : J. v. M., St Monica, and Yates. The nine years' interval between dream and event in St Monica's case is unusually long, but intervals of twenty years and more are known. In Blunt's dream (23) it must be borne in mind that the body of Egleton would almost certainly have been found sooner or later, even without the help of the dream. Precognition is thus a possibility here ; but so is retrocognition ; and even a *post mortem* telepathy from Egleton is not out of the question. In fact this case runs the whole gamut of hypotheses, and it is best to follow the jury's example and return an open verdict.

IV. *Clairvoyance* is a possible explanation in every lost-object case. The form of the vision in such cases as Miss Wild's is no argument against this, just as the form of Mrs Stuart's vision is no argument in its favour. The objection to clairvoyance is that it explains all cases, both those which occur and those which do not. Consider the following :

*Fictitious Case IV*. A house is bombed, or a ship is wrecked, and a valued possession is lost. The dreamer sees it among the ruins, or on the seashore, in some place in which it would not have been discovered without the aid of the dream.

The antecedent conditions here must have existed in many cases ; but as far as my acquaintance with the literature goes, the

Of late there has been a tendency to use 'retrocognition' for the Versailles and similar cases, in which the percipient seemingly experiences events of a remote past. These events, however, are not part of the *subject's* past. On the hypothesis that he is re-living the past of someone else, the case is one of retrocognitive telepathy ; otherwise, of retrocognitive clairvoyance.

<sup>1</sup> Secondary personalities of the 'Sally Beauchamp' type might be described as the nearest possible approach to a conscious unconscious. They often claim to be able to 'look out of the corner of the eyes' when the primary personality is looking elsewhere. This has been very interestingly described by 'B' of the BCA case, and verified by Morton Prince in experiments with the same subject. (*Journal of Abnormal Psychology*, iii and xiv.)

dream has never resulted. To establish clairvoyance in such a case, it must be shown (i) that no human eye saw the lost object in the place in which it was found (this excludes retrocognition and telepathy); (ii) that the discovery would not have been made except for the dream (this excludes precognition).<sup>1</sup> Two cases in the *Journal* come close to this; neither is a dream case, and in neither is the evidence conclusive. Mrs 'Watson'<sup>2</sup> lost her wedding ring, and was directed by automatic writing to look for it in a sack of waste paper. It was found there, and was supposed to have been carried in by rats—the ring was lost in 1944, and the sack was filled with waste paper in 1945. It is unlikely that the ring slipped in along with the paper; however, it is not at all impossible.

The other case is from the *Revue Spirite*, April 1921.<sup>3</sup> Mme A. Clarinval's son René, an airman, was killed in 1916, and buried by the Germans. After the armistice Mme Clarinval searched for the grave, but it could not be found. Some months later she had a waking vision of her son, apparently alive, between two unknown men. The one on the left seemed to be a German, and the other a Russian. A further search was made three months later (not in consequence of the vision), and it was established that the body had been buried in the German cemetery as unknown. The officer in charge refused to let the bodies be taken up for examination. 'But I was determined. We returned to Verdun . . . and sought out the officer in command of the graves commission. After a long discussion . . . he yielded, and gave us authority to make a search. Next day at 5 in the morning we were at the cemetery . . . By 12 o'clock 20 coffins had been opened without any result. The men went to dinner, and my husband and I stayed there, greatly depressed; for we were beginning to lose hope . . . Suddenly *I thought of my vision* . . . I said, "Yes, we shall find him; he is between a Russian and a German. There was a Russian in the cemetery at Dieppe; let us look for him." The men came back and resumed their work, whilst we ourselves looked for the Russian . . . At last, at 4 o'clock, I found the Russian. To his left there was an unknown soldier, and to the left of the unknown a German.' The unknown corpse was exhumed, and proved to be that of René.

Camille Flammarion, who published the case, was perplexed by the form of the vision, which does not correspond with the reality.

<sup>1</sup> The possibility of such a case being precognitive is denied by Dunne's theory, since no observer can foresee the result of his own intervention. Their non-occurrence is therefore a point in favour of the theory.

<sup>2</sup> *Journal*, xxxiii, 230-9.

<sup>3</sup> Quoted in *Journal* xx, 244-9.



After a comparison with others, and especially with the Wild case, it can be seen that there is no difficulty here. Mme Clarinval's vision is the result of a very powerful wish. But that wish is not for a grave, nor for a corpse—the mother wants *her son*.

The first criterion of a clairvoyant case is here well satisfied. No human being knew that René Clarinval was buried between a Russian and a German. The second, however, is more doubtful. It seems clear that Mme Clarinval's actions were not affected by the vision during the early part of the search. The question is, if she had not recollected it, would she have given up the search in despair? On the one hand, the cemetery contained more than 2,000 graves. We are not told how many of these were of unknown soldiers—an important point—but there may have been several hundred. Without the vision, the search would have taken some days, and possibly weeks. On the other hand, we must consider the intense determination of Mme Clarinval, which broke down bureaucratic opposition, and sustained her through the gruesome examination of dozens of corpses. On the whole, it seems likely that even without the vision she would have persisted in her search, at least for some time. It is therefore possible that she would have achieved her object without the help of the vision; the latter, consequently, may be precognitive.

I should add that both these cases are ascribed to intervention by spirits. There is no need to discuss this hypothesis, however, because, as Mr Hubert Wales pointed out in the Clarinval case,<sup>1</sup> the dead persons knew no more of the facts than anyone else, and could only have acquired them through clairvoyance. This is contrary to the principle of the economy of hypotheses; if clairvoyance is a fact at all, there is no reason to suppose that a dead person is more clairvoyant than a living person. The sole basis for the spiritist theory is the form in which the information is conveyed; and this form, it has been shown, is moulded to suit the wishes of the percipient.

V. *Telepathy*. If clairvoyance is rejected, Squires's dream must be considered as telepathic; and, if we include post-mortem telepathy, Chaffin's also. Whatever is said about Squires will apply also to the other cases of the same type, and (although no certainty can now be reached) the Marteville and Milan cases may be parallel to Chaffin's. Others in which the telepathic explanation must be considered are Howe, F. R. C. P., and J. v. M.

It has already been suggested by several writers<sup>2</sup> that telepathy is from subconscious to subconscious. This agrees with Dunne's view<sup>3</sup>, according to which, if A is the agent and P the percipient,

<sup>1</sup> *Journal*, xx, 347.

<sup>2</sup> For instance, A. Glardon, *Journal*, ix, 9.

<sup>3</sup> Note on telepathy in *The New Immortality* (1938).

the information is passed from A<sub>1</sub> to A<sub>2</sub>, from A<sub>2</sub> to P<sub>2</sub>, and finally from P<sub>2</sub> to P<sub>1</sub>. We may therefore expect to find, on the one hand, four-dimensional effects—precognition or retrocognition; and on the other hand, evidence of subconscious activity on the part of agent and percipient.

Among the 'solution' dreams there are no cases of precognitive telepathy, which is not surprising, as will be seen if an attempt is made to devise a fictitious case. Retrocognitive cases, however, are comparatively common. They may be expressed diagrammatically as in Fig. 3.

BD represents a period of the agent's life, and CE the corresponding period in that of the percipient. At B the information becomes known (consciously or otherwise) to the agent; at E it appears in the consciousness of the percipient. Discussion of such cases<sup>1</sup> has hitherto turned largely on the question whether the telepathic message followed the path BCE, implying a latent period

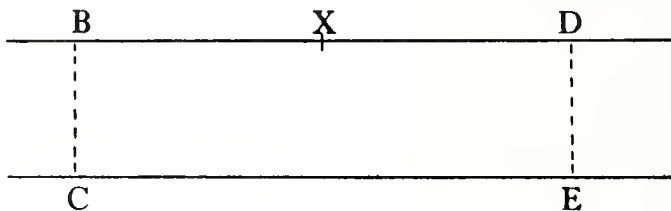


Fig. 3

in the percipient's mind, or the path BDE. In the Chaffin case the agent dies at the point X, and the BDE path therefore implies the hypothesis of survival. But if it is conceded that both parties to the telepathy perceive four-dimensionally, then the time in which the telepathy occurs is Time 2, and the discussion loses its point. To ask whether the communication was sent before or after Chaffin's death is meaningless, since the death is not an event in Time 2; one might as well ask whether Euston station happens before or after six o'clock. There is, of course, a sense in which the terminus is an event to a railway passenger; but the ending of his career as a passenger does not affect his existence as a human being; similarly, the arrival of Chaffin 1 at the end of his journey does not imply the extinction of Chaffin 2. In the same way, the idea of a 'latent period' refers to Time 1, and has no application to Time 2.

Turning to the question of subconscious activity, we find that there is no difficulty at the percipient's end. In every case he experiences a dream or a hallucination, which is unquestionable evidence of such activity. The agent's part must be examined in

<sup>1</sup> e.g. *Proc.*, xl and xlvi, 'Is Proof of Survival Possible?'

more detail. In the Squires type, the information was never known to the conscious mind of the agent ; if these cases are telepathic at all this information must have been present subconsciously. In the Marteville type the information was consciously known to the agent before his death, and in each case he had the opportunity to pass it on to the percipient by normal means. Why did he not do so? Either he forgot, or he wished to conceal it. M. de Marteville, a methodical man of business, would not have left a receipt for a large amount lying out of its proper place unless he had forgotten or lost it. Chaffin's is a case of concealment, and here a curious internal struggle may be observed. 'Chaffin A' draws up a will ; 'Chaffin B' makes another contradicting the first. 'A' contrives that the later will is unwitnessed, and hides it away ; 'B' writes out a clue to the hiding-place ; 'A' conceals the clue. There is ample evidence here of a suppressed wish, which may easily have sunk into the subconscious.<sup>1</sup>

In the remaining two cases the identity of the agent is unknown. F. R. C. P.'s is put down to the landlord ; but this gentleman possesses no qualifications for the part beyond the fact that he knew of his own intention to return late. There is another person in the case who was in a good position to know this, and also to know the doctor's problem ; who was vitally interested, and who can easily be supposed to have a repressed desire to communicate the information—the delirious patient, struggling against her craving.

In Howe's case there are three possible agents—the two Chaffes and R. M., the creditor. The last-named was well known to Judge Howe, but we have no details of their acquaintanceship ; of Chaffe senior we know only that he was dead. In connexion with Chaffe junior, however, we have on important point. He had sold the Judge a house. What price did he get for it? This question, irrelevant as it may appear, is perhaps the key to the problem. Let us suppose that the price was rather low. This explains, on Howe's side, the suppressed feeling that there must be a snag somewhere, which appears in the form of an anxiety dream. But what of Chaffe? He knows that there are no snags. Howe has a good bargain—too good a bargain. The old debt has been paid off—but suppose Howe thought it had not been paid off?

It will be objected that the train of thought I attribute to Chaffe is childishly spiteful. Such ideas would certainly find no place in

<sup>1</sup> What was the 'particular reason' which made Mr R—d's father employ a different lawyer for his purchase of the title-deeds? Why did Hennings's clergyman put the receipt in a secret drawer? There is something decidedly Chaffinesque about such behaviour, the reason for which, unfortunately, we shall never know.

his conscious mind ; but they are quite in keeping with the ways of thinking of the subconscious ; and the combination of paranormal powers with a childish mentality is encountered in many departments of psychical research.

In this and the preceding case the explanations have been largely speculative, but this does not affect the chief point, which is that in no case do we find on the part of the agent both conscious knowledge and a conscious desire to communicate it. The obstacle to communication is always psychological, never merely physical.

It may be concluded, then, that in the great majority of cases the essential information is derived from retrocognition, telepathy, or precognition. Chance coincidence is possible in the St Monica type, but clairvoyance is an unnecessary hypothesis,<sup>1</sup> and the idea of a spirit message as usually conceived is inapplicable.

I have now surveyed all the various types of 'solution' dream, with the exception of that which at first sight seems the easiest to explain—the purely intellectual problem, such as those of Lamberton and Hilprecht. In the cases hitherto discussed, the dreaming mind has been called upon for a momentary effort of retrocognition, telepathy, or precognition. This is well within its capacity, as shown by many other cases ; but sustained logical thinking is quite another matter.

I do not deny that reasoning takes place in dreams ; but a distinction, not hitherto important, must now be drawn between the part of the mind that composes the dream—the 'dramatist'—and the part that watches the story as it unfolds—the 'spectator'.<sup>2</sup> The distinction is clear in most dreams ; a specially obvious case is that of dreams with surprise endings, as Stevenson<sup>3</sup> and Greenwood<sup>4</sup> have observed. The difference of character between these entities is often very striking, as in a dream of Havelock Ellis's<sup>5</sup> ; ' . . . I dreamed that I was in a drawing-room and saw a beautiful and attractive woman with an unusually low evening dress entirely revealing the breasts ; then, between the breasts, three additional nipples appeared, and I realised in my dream that here was a case of supernumerary breasts of sufficient scientific interest to be carefully examined later on ; and then, as I gazed, I saw a number of little fleshy nipple-like protuberances on the

<sup>1</sup> There is of course, much evidence of clairvoyance in other situations : for instance, some of the Duke University experiments.

<sup>2</sup> In Dunne's terminology the 'dramatist' is Observer 2 and the 'spectator' Observer 3.

<sup>3</sup> Essay on Dreams in *Across the Plains*, 1909.

<sup>4</sup> *Imagination in Dreams*, 1894.

<sup>5</sup> *The World of Dreams*, 1911.

body, and thereupon I realised that I was really looking at a case of the rare skin disease termed *molluscum fibrosum*.<sup>1</sup> Here the erotic propensity and the rapid, illogical transitions of the 'dramatist' are in strong contrast with the cool, detached, scientific attitude of the 'spectator'.

The 'spectator' reasons well, but cannot carry on a long train of thought because the 'facts' he has to go on—the dream images—are perpetually changing. The 'dramatist's' logic, where it can be recovered, is of a most primitive type. The very structure of a dream is based on the 'undistributed middle'. Thus, the reasoning behind Freud's dream of 'Uncle Joseph'<sup>1</sup> is (in part) as follows : (i) N. has been found not guilty of a crime, therefore N. is a criminal. (ii) My uncle Joseph was a criminal. (iii) Therefore N. is my uncle.

A poetic metaphor, it may be remarked, is also an undistributed middle ; 'There is a garden in her face' is much the same sort of thing as 'N. is my uncle'. There is evidence of artistic ability in dreams—consider, in these very cases, the appropriateness of the Assyrian priest, and the economy of the hallucinatory diagram on the real blackboard—but not of sustained reasoning.

One would expect, then, that in 'solution' dreams only the starting-point would be provided by the 'dramatist'—that, for instance, in Lambertson's case (15) the dream-image would be a combination of the blackboard and the algebraical form of the problem ; that the 'spectator' would then realize that the attack could be made along these lines ; that the dreamer would wake with this idea, and proceed, in his waking hours, to the working-out. This does not happen. The dreamer is presented with a *fait accompli*, apparently the unaided work of the 'dramatist', and his waking task is merely to check the result.

But, if we leave aside these dreams, and a few other special types such as the battle-dream, we have an overwhelming mass of evidence as to the character and capacities of the 'dramatist'. It is naïve, childish, changeable, following associations of form rather than of idea, a lover of the obscene, erotic, coprolalic, violent, a phantast, sometimes nearly a poet, sometimes almost a maniac—and this even in the most chaste and the most intellectual of mankind. We are now asked to believe that this same entity, in Lambertson's case, set itself soberly to the task of drawing tangents, dropping perpendiculars, and the detailed working-out of a problem in geometry ; and in Hilprecht's, to the laborious deciphering of an Assyrian inscription.

In this dilemma it may be helpful to refer to the 'lost-article' cases. It was found that the article was frequently seen in the

<sup>1</sup> *The Interpretation of Dreams*, 6th English edition, p. 143.

dream as at the time of finding, not that of losing. This could be accounted for as to the motive by wish-fulfilment, and as to the means by precognition. In the cases now being considered, we have an analogous effect, and the same motive ; may not the means, too, be the same?

This may appear far-fetched. But if Lamberton, *after solving his problem*, had had a dream vision of the completed figure ; or if Hilprecht had dreamed of the Assyrian priest *on the next night*, no one would have thought it strange. The remarkable thing about these dreams is their displacement in time. But this is the very definition of a precognitive dream.

If it be asked : when was the detailed work on the problem carried out? the answer is that it was done on the day after the dream. The case is in fact parallel to Wilmot's. The dreaming mind, by bringing together elements which the waking mind had failed to connect, gave the essential 'flash of inspiration' which made the solution possible ; the solution itself thus became a fixed point in the future, and this reacted on the dream, giving it its final form. But the interaction of dream and event did not stop here, for the working-out itself, owing to the interposition of the dream, became much less laborious, and was in fact reduced to a mere process of checking.

The motivation of the 'solution' dream is straightforward. The conscious and subconscious mind are here working in co-operation, and not, as so often, in opposition. The subject is concerned over his problem. He brings all his conscious faculties to bear on it, but without success. After a while he gives it up and 'puts it out of his mind'. The problem is thus handed over to the subliminal mind ; and it exerts *its* very different faculties—including retrocognition, precognition, and telepathy.

When playing tennis, you lose a ball. After much search you give it up, get another and go on with the game. Your dog meanwhile has been watching ; and some time later, when you have forgotten all about it, he comes frisking up with the lost ball in his mouth—having smelt it out.

The subliminal mind, too, must handle its material in its own way. It dramatises, symbolises, condenses, and distorts just as it does with any other waking experience. Sometimes it comes up (wagging its tail, as it were) with something quite inappropriate. Take the Howe case ; the conscious mind asks anxiously, 'Are there any unsettled claims on this property?' The dreaming mind responds by producing a claim that has been settled some time ago. Another example (though not a 'solution' dream) is the well-known Terriss case. Mr Lane, a subordinate actor, dreamed of the murder of Terriss, the star, the night before it happened.

Lane was Terriss's understudy. He must therefore, like all understudies, have been longing for his principal to fall ill, so that he could have a chance of making a name for himself. Probably he tried to repress these longings. In sleep, however, the dreaming mind set off in search of such a situation, and came back with the nearest it could find—Terriss's approaching murder. This, however, was not at all what Lane wanted, as of course the theatre was closed and did not re-open until a new star had been engaged.

The 'solution' dream may be classed among a series of mental processes which have the common feature that the attention is first concentrated and then relaxed. The simplest case—cited by William James<sup>1</sup>—is the everyday one in which a forgotten word or name, for which one racks one's brains in vain, comes back 'of its own accord' some time later. From this we may pass to the solution of puzzles, and from them to scientific discovery and invention; in each of these the same sequence of mental events is found. Artistic inspiration is somewhat different, but may still be brought under the same formula. When Wordsworth saw the daffodils, his senses were keenly stimulated; his attention was directed strongly outwards. Later, lying on his couch 'in vacant or in pensive mood', the image recurred spontaneously, bringing with it the beginnings of a poem.

Finally, the most striking phenomenon of this class is undoubtedly religious conversion. The analogy between sudden conversion and the 'solution' dream is close. We have first a period of mental distress, often very severe and prolonged; during this time the problems of sin and salvation are unceasingly brooded over. Then there often intervenes a period of tranquillity during which the problem is put aside; finally, in a sudden uprush of subliminal energy, often accompanied by hallucinatory visions and voices, the answer is found. Whether, on such occasions, any objective information is brought up, is a question which can scarcely be discussed here. However this may be, the subject's problem is solved.

<sup>1</sup> *Varieties of Religious Experience*, 1902, p. 205.

## WATER DIVINING

SUMMARY OF AN EXCHANGE OF VIEWS BETWEEN  
P. A. ONGLEY AND J. C. MABY

WE give below a précis of articles by P. A. Ongley and J. C. Maby which appeared in the *Journal of the British Waterworks Association* (Vol. xxxi, August 1949, Vol. xxxii, April, June, October 1950).

P. A. Ongley, M.Sc., is a New Zealand research chemist. J. C. Maby, B.Sc., is a full-time water diviner who uses instrumental methods.

The first article 'Divining in New Zealand' is from Ongley's pen. He begins by quoting an opinion of Ellis in 1917 that dowsing had been thoroughly discredited. He then quotes the 1920 figures of the New South Wales Commission for Water Conservation and Irrigation :

	DIVINED		NOT DIVINED	
	No. sunk	%	No. sunk	%
1. Serviceable bores giving more than 100 gals. per hour	1,294	70·4	1,516	83·9
2. Serviceable bores giving less than 100 gals. per hour	185	10·1	96	5·3
3. Unserviceable bores	87	4·8	61	3·4
4. Absolute failures	269	14·7	133	7·4
Totals	1,835	100	1,806	100

In 1947 a well-borer was found negligent in that he did not take a diviner with him when inspecting a proposed well site. To condemn divining on inadequate evidence would be unfair. It is almost impossible to get comparative figures for the success of diviners and non-diviners in selecting well sites.

It is often claimed that where well A (undivined) went dry, well B (divined nearby) was successful. Ongley gives a diagram to show a possible explanation in terms of geology. Successful water divining is often due to common sense. Other claims depend on half-truths and deliberate untruths on the part of diviners ; an example is given of each.

*Ongley's Experiments.* Seventy-five New Zealand diviners were tested. They included clergymen, ex-nurses, motor-mechanics, mill-hands, farmers, etc. Ten were women. In all tests care was taken to test only what the diviner claimed to do. Rigorous control experiments were done to make sure : that (a) there were no 'interfering rays' ; (b) containers etc. did not cut off 'divinable



rays' ; (c) there was enough material to be divined ; (d) 'reminiscences' (divinable traces allegedly left behind when a substance is moved) could not account for wrong results.

Sixty diviners were divining for water, the remaining fifteen for miscellaneous substances. Some claimed to find only moving water, the rest to find water anywhere.

The first test was to see how accurately the diviner could locate the edge of an already divined stream on repetition. If his eyes were open the diviner always 'found' his 'stream' at the same place, but blindfold he would be up to ten feet out. In control tests blindfold normal subjects did rather better than the dowzers in locating a predetermined mark.

Four diviners were tested to see if they would declare pegs in the ground already dubbed by them as 'wet' or 'dry', to be in the same state on repeat attempts. Observed : 113 right in 210 trials (expected 105).

Ongley quotes instances of diviners contradicting each other in reporting streams crossing a given line. Diviners who could 'find' only running water were asked to tell whether a mains supply was on or off. Eight diviners scored 80 out of 170 (expected 85). Those who could only find static water were asked to tell water from an organic liquid or to say whether a bottle was full or empty. The 7 diviners so tested scored 116 out of 210 (expected 105). All the dowzers were badly astray about depth of water.

Examples of dowzing for miscellaneous objects are given. The results are described as 'ludicrous'.

'Although, unfortunately it has not been possible to test every diviner on all his claims, nevertheless each has conclusively been proved totally unreliable.' Digging wells on divined sites is impracticable and not as decisive as the results of so-called artificial tests, in defence of which may be noted that (1) diviners were asked to suggest tests ; (2) in many cases, e.g. in identifying metals, these tests had been used by diviners themselves ; (3) careful tests were always done to see that the tests were both fair to the diviner and foolproof ; (4) one diviner said, 'Your tests are perfectly fair. Until I tried I would have bet £5 to a penny I would have been successful.'

Ongley also defends the blindfold tests. He concludes his article with a discussion of the mechanism of movement of the diviner's rod, and applies a mathematical treatment to the problem. He concludes that the bending of the rod is due to the unnatural position of the hands producing a torsional couple on it in a vertical plane.

Summarising, Ongley maintains that 75 diviners would have included some, at least, of the reliable diviners had any existed.

Secondly, ' . . . the nuisance value and the menace of dowsing is not sufficiently realised. A water or mineral witcher can cause an awful waste of private and public money. The medical witcher can cause a waste of human life.'

Maby in his reply complains ' . . . Ongley shows no sign of having properly examined the very large body of positive instrumental and physiological data, or the excellent records of a few accredited dowzers, before drawing dogmatic conclusions.'

Ongley is prejudiced ; his intentions are sincere but misguided. Maby accepts his results, ' . . . so far as they go, at their face value. But the matter does not end there, since a vast body of evidence by numerous competent engineers, physicists, physiologists and others has securely established the basic facts of radiesthesia and dowsing (excluding interfering psychological factors) in recent years, despite the negative results of Ongley, Macfayden and one or two other sceptics.'

Neuro-muscular reflexes to living organisms etc. can be demonstrated in the laboratory (Maby refers to Tromp's *Psychical Physics* and to Maby and Franklin's *Physics of the Divining Rod*), yet 'the practical application of dowsing reflexes in accurate surveys and analyses is problematical'.

'There have never been more than a few tolerably expert, self-critical, reliable diviners in any one country.' Maby agrees with Ongley that enthusiastic amateurs have a high nuisance value. This does not disprove true dowsing, cf. the work of Tromp, Pericas, De Vita, Von Pohl, Dannert, Regnault, Jemma, Gorceix, Chicca, Creyke, Budgett, Turenne, Maby, Franklin, and others ; also the commercial success over years of Pericas, De Vita, Creyke, Franklin, and Maby.

There is nothing supernatural about genuine divining. [Maby belongs to the school of thought which holds that dowsing is solely physico-physiological and not psychic.] Modern dowsing has shown that there are many complicating physical factors to contend with : 'Hence the frequent errors and misinterpretations of quite objective physical effects by both dowzers and instruments used.' Few dowzers realize these sources of error. Examination of a mixed bag of dowzers and sites is useless. Genuine successes will be lost in a 'jungle of guesswork, auto-suggestion and chance coincidence . . . In any case, geological conditions should be taken into account . . . one must be sure that : (a) a really competent dowser is employed ; (b) he works under fair and proper conditions, free from geophysical, meteorological, physiological and even psychological sources of interference . . .'

In conclusion Maby says Ongley has shown that New Zealand

dowers have no commercial value. He cannot accept that all dowsing is unreliable as the reverse has been proved many times. Dowers should be more cautious and self-critical and follow the precepts of Tromp, Franklin, and Maby. The general public should be more objective in its approach, scientists and engineers less prejudiced.

In his second article Ongley replies that in fact he is quite familiar with the classical literature on dowsing, and with Maby's book and papers. He repeats his contention that many of the 'excellent records' of dowers are due to half-truths and untruths. Ongley does not agree that he only tested duds, contending that he tested all comers; among the 75 there should surely have been some of ability. All care was taken to see that no diviner attempted anything beyond his alleged powers and that all tests were both fair and foolproof.

In a six-page paper he had no room for detailed results. These are described more fully in the *New Zealand Journal of Science and Technology*, 1949, 30B, pp. 38-54. Ongley concludes with a challenge: 'I am willing to go anywhere to meet up to say half-a-dozen diviners in the field on the following conditions: (i) I am able to come from and return to Glasgow within the weekend; (ii) Before we start trials we agree on what we are going to try to do . . . (iii) [not relevant to this précis]; (iv) If the diviners are unable to substantiate their claims, I would expect a refund of travelling expenses.'

Maby, in his second article, says Ongley is clearly fixed in his views though otherwise fair; he again recommends studying the literature, watching a good commercial dowser at work, and asking for his records of a typical year's working, e.g. some of Maby's own results which he gave in the *Journal* of the British Society of Dowers for March, 1949.

In fairly homogeneous strata, average weather, open rural country, and absence of thick clay or shale beds, depths can often be predicted to one per cent and yields to ten per cent. The reverse conditions may lead to serious errors. There is no question of ' . . . "clairvoyant perception"', but merely a straightforward electrical and geophysical routine (incorporating both the divining rod and a few simple electromagnetic accessories) as roughly described in *The Physics of the Divining Rod*. But errors arise readily from complicating physical and even psychological factors.

Ongley assumes undue authority because he is a *chemist*, not a physiologist or geophysicist or electronic engineer. Many very

eminent physicists, physiologists, doctors, and engineers have already agreed to both the main facts and also some of the tentative explanations of dowsing.

Moreover, tests of the type Ongley carried out require both a special technique and a very considerable knowledge of human psychology. Sceptics or hostile investigators invariably cause failure because of the psycho-radiant output of short waves from their bodies which can be picked up by dowsers and by certain new electro-mechanical detectors. Perhaps Ongley is an inhibitor.

Maby is unable to accept Ongley's challenge because (i) 'there are too many . . . almost threatening conditions attached' ; (ii) he and other experts are too busy ; and (iii) there is ample positive evidence on record. Anyhow, what would be the use of convincing Ongley, whose verdict would not be likely to result in universal acceptance?

The exchange of views concludes with a rejoinder from Ongley, who claims that, like many others before him, he has investigated dowsing as fully as the diviners would allow him. 'there is not a shred of unequivocal evidence in favour of divining . . . In contrast to the formidable literature that has accumulated against divining the only reputable literature that might possibly be interpreted as favourable to the art is the paper of Barrett who, after meandering indecisively for 250 pages, concludes by saying more or less that he does not know what to think. That Maby needs to rely on this 50-year-old paper as his mainstay is in itself sufficient condemnation.'

Ongley quotes Barrett in support of his views that a dowser's own records cannot be relied upon. The 'eminent physicists, physiologists, doctors and engineers' supporting dowsing exist only in Maby's imagination. If they do exist it proves nothing. 'In science, what count are not authority, but facts, and the logic of the conclusions drawn from these.' Ongley says that although a chemist, he has a working knowledge of physics, biology, physiology, and statistics, and has had the assistance of expert geologists, physicists, medical men, farmers, and well-borers. As an ex-teacher he probably knows all that is needed about setting examinations and handling examinees. Maby's excuses for not accepting his challenge are so farcical as not to need rebuttal. Evidence for dowsing should not comprise statements of prowess, but actual results in foolproof tests in the presence of impartial observers. If Maby and the dowsers are unwilling to supply the evidence one can only assume they cannot.

## REVIEWS

HOMO FABER : A Study of Man's Mental Evolution. By G. N. M. Tyrrell. London, Methuen, 1951. viii, 205 pp. 15s.

Mr Tyrrell here defends and extols the fundamental importance to modern man of a broad comprehensive *non-specialist* approach to the problem of his own nature and place in the universe. Science, he believes, represents progressive specialization in techniques for the control of the material world, with a resulting mastery of *immediate* reality and an increasingly hopeless removal from the apprehension of *ultimate* reality.

The philosophical tools employed in this analysis are derived largely from Bergson and from Eddington. It is Bergson's *Homo Faber* in the *Creative Evolution*, with a mind structured by the adaptive necessity to cope with the physical world, who supplies the title ; and who also supplies much of the groundwork for the description of a practical mind working, in geometrical and mechanical terms, to master the practical obstacles which material Nature presents. It is Eddington's brilliant description of the scientists' dilemma (in the *Philosophy of Physical Science*) that guides Mr Tyrrell through the epistemological quicksands of modern positivism. From Bergson we learn that our minds were evolved to *act*, not to *grasp ultimate realities* ; and from Eddington we learn (if Kant had not already persuaded us) that the objective and the mathematical take on a certain form and present a certain solid façade of unquestionable truth because we have in the first place determined, by the very structure of our minds, what manner of things there are in the world. Truths which belong to this objective system are 'convergent', and can be brought into more and more refined formulation ; those which transcend it lead to 'divergent' difficulties, out of range of Homo Faber's sight, which 'paralyse the mind and dishearten it' (p. 58).

In the light of this analysis, physical science, biological science, and psychology can all win tactical success against immediate local objectives, and philosophy can achieve rationalistic successes through improved clarification of terms ; but all specializations lead us away from the intuitive grasp of our own natures and of the cosmos to which we belong. Specialization actually tends, Mr Tyrrell believes, to disturb our capacity for self-understanding and self-fulfilment.

In reading these earnest arguments from a man of such highly specialized and such brilliantly creative analytic powers, the reviewer confesses to a considerable bewilderment. Mr Tyrrell's ingenious experiments in paranormal perception exemplify all the characteristics of the astute planner and controller of the conditions

under which a fact has to be observed if it is to be known to exist ; all of Homo Faber's adaptive resourcefulness was poured into them. It is not true that he concerned himself as scientist only with the external and objective phases of his subject's mind ; he was just as much a specialist when he was considering subjective as he was in considering objective aspects of his problem. He is the same Mr Tyrrell who gave us the magnificent study *Apparitions*, involving a highly disciplined and highly specialized study of very complex and bewildering psychological phenomena, including the analysis of unconscious dynamics and interpersonal interactions. Is it just possible that Mr Tyrrell in the present volume is not really attacking the *specialist* at all, but the dogmatic materialist who uses the sciences as weapons in his ideological battle?

Perhaps this short volume is not the place for me to hope for an answer to my difficulties, but I am also worried by the lack of definition of two terms which are central in the present effort : instinct and intuition. Throughout the book the innate predisposition to observe and to think in a given manner (a predisposition to which traditionally the term 'intuition' has sometimes been applied) is referred to as an instinct ; we even learn that those who uncritically hold to such an instinct 'will go on holding it, just as birds will go on building nests and rabbits will go on digging holes' (p. 173). On the other hand the term intuition is applied to a process transcending the sensory processes (and apparently also the ordinary rational operations) ; it is compared with religious and at times with creative activities in such a way as to suggest that these are vehicles by which it is carried ; it is said to carry us to a kind of self-understanding which will free us from the specialist's errors ; but it remains undefined. Light on the process is, despite all this, shed in some degree by reference to Indian mysticism ; and I must confess that Mr Tyrrell has done much to convince me that we need far more competent and thorough studies of Indian practices than we have, by specialists who are themselves in the thick of the phenomena, deeply sympathetic and capable of identifying with the adepts, and also equipped with tools for careful observation and analysis. I am sure that if I could talk with Mr Tyrrell he would agree. It is surely not the specialist that he decries, but the specialist who specialises before he exposes himself to the ABCs of human nature, in which always much of the non-geometrical is found.

The trouble with the reviewer is very likely the fact that he is a sort of a specialist. His own bias is to think that Mr Tyrrell really believes in organized specialized science just as much as he himself does, and that time will bring him back to another series of

brilliant contributions to psychical research, in the best tradition of science, with all the appurtenances of Homo Faber's analytic mind and all the humanity of one who can see deeply into *The Personality of Man*. Let Mr Tyrrell, and all good psychical researchers, go on attacking the stupid and narrow specialists, but outspecialise them all in the technical competence of their observation and their analyses!

GARDNER MURPHY

NEW APPROACHES TO DREAM INTERPRETATION. By Nandor Fodor. New York, Citadel Press, 1951. xvi, 363 pp. \$5.00

Dr Nandor Fodor was at one time a well-known figure in the field of psychical research—though, so far as the present reviewer is aware, he was never actively connected with the S.P.R. In more recent years he has turned to psycho-analysis, and he can now speak of 'my psychical research days' as being of the past. Nevertheless his interest in what might be called borderline phenomena has persisted, and in the new field has manifested itself in a considerable preoccupation with some of the more mysterious problems which present themselves to the psycho-analyst, so that he has to some extent constituted himself a liaison officer, calling attention on the one hand to the possible importance to the psycho-analyst of certain recognized problems in parapsychology and on the other to the interest which some of the more curious phenomena revealed by psycho-analysis might quite naturally present to the student of psychical research.

As regards the first of these he justifiably reminds psycho-analysts that (with a very few exceptions) they have not followed up Freud's own suggestion that psycho-analysis 'has prepared the way for the acceptance of such phenomena as telepathy'. Like Freud himself, Fodor is inclined to believe that telepathy manifests itself between the unconscious minds of the persons concerned and that this accounts for much of the apparently erratic and elusive nature of the relevant phenomena, which makes them often so exasperatingly unamenable to experimental or other conditions imposed by conscious intention. Being by its nature an affair of the unconscious, telepathy also partakes of the other characteristics of unconscious mental processes as revealed by psycho-analysis. As with dreams and other mental events largely affected by the unconscious, the 'meaning' of telepathic impressions is frequently not immediately obvious to the conscious mind and may require 'interpretation' of the kind familiar in psycho-analysis—and therefore sometimes apparently fantastic or 'far-fetched' in its

nature—before the full extent and nature of the telepathic occurrence becomes clear.

This process of interpretation, however, naturally increases the difficulty of scientific proof, since (as with psycho-analytic interpretations generally) there is often room for doubt as to whether the interpretation in any given case is correct or adequate. The author's own interpretations in this book range from those which would be acceptable to almost anybody who is willing to grant that a symbolic process is at work to others as regards which even many fellow psycho-analysts might be sceptical, especially as, in the interest of speeding up the therapeutic and interpretative process, Dr Fodor is admittedly prone to a somewhat active interference with the analysand's associations.

In the present book the author deals (amidst much other material) with some twenty cases in which telepathy seems to have manifested itself in the dreams of two or more subjects, usually but not always on the same night, a number of them occurring during or in connection with psycho-analytic treatment. Some of these are striking, there being in one case no less than eight 'elements in common' between the 'shared dreams'. In another pair of dreams it would seem that the cognitive content had got dissociated from its affective context, so that one dreamer can subsequently say to another: 'You were not afraid of the fire. You could afford it, because I got your fear' (p. 198). Displacement of affect is of course well known in psycho-analysis since its early days, but here the displacement is from one dreamer to another. As one criterion of telepathic influence in dreams, the author suggests that a 'shared dream' cannot be adequately explained or interpreted even by the most assiduous psycho-analytic procedures unless the corresponding dream of the other person is taken into account. But as to the circumstances which tend to produce such 'shared dreams' he is cautiously reticent, except for suggesting that there may be present some element of sympathy or 'need for companionship, for sharing warmth and affection, as well as the need for protection and reassurance in the case of fear and anxiety' (p. 203).

In the absence of some compelling or quantitative criterion (such as can usually only be supplied by rigorous experimental procedure) readers' judgment concerning the evidence submitted will inevitably vary, but it can certainly be said that the author has here provided a good *prima facie* case for further investigation along these lines. Furthermore, his data suggest the reflection that, if telepathic dreams occur, they would in the vast majority of instances be overlooked, so that there would seem to be a strong case for a detailed comparison of dreams over a given period as



between persons possessing some common bond which might seem to make their occurrence likely. (Would that we knew more about the nature of such common bond!) Ideally, perhaps, these dreams should in the first place be submitted to some third person with a knowledge of the personalities and circumstances of the two dreamers, with psycho-analytic training and with opportunity for interviewing them when necessary. The introduction of such a third person, though in practice it might often be far from easy, might reduce difficulties arising from the dreamers' own 'resistances' and at the same time minimise the dangers of mutual suggestion during subsequent discussion, such as is almost bound to occur if the dreams are first discussed and 'interpreted' by the dreamers themselves. Some further description of the author's own precautions to avoid such suggestive influences would in certain cases have been welcome.

Dr Fodor also believes in the reality of 'prophetic' dreams, though here the evidence presented is less extensive. As in the case of 'shared dreams', psycho-analytic interpretation is usually necessary. Thus in a dream of his own he contends that there is precognition of a quite unanticipated outcome of a libel action. To mention the most impressive element, he dreamt that he was presented with £4 for a suit (of clothes), with which sum he was dissatisfied, as he expected the suit would cost £8 (this was early in 1939). In the subsequent (law)suit, the judge quite unexpectedly divided the grounds of libel into four groups. The jury found libel on two of these counts, but not on the other two, whereupon the judge divided the costs, so that Dr Fodor received only half of the full cost of his 'suit' (p. 314).

In the other of the two fields in which, as was suggested above, Dr Fodor has constituted himself a sort of liaison officer, he is particularly concerned with the influence of birth and intra-uterine fantasies and the anxieties connected with them. In this he follows up the theme of his earlier volume *The Search for the Beloved*. Ever since Freud first pointed out the existence of dreams and fantasies of this kind, they have constituted a problem which most psycho-analysts seem to have been shy in tackling. Otto Rank's book *The Trauma of Birth*, which appeared in 1924, has been cold-shouldered as supposedly guilty of exaggeration and as manifesting a flight from the more plausible and more easily verifiable factors of later life revealed as important by psycho-analysts. The possible influence of the birth trauma is complicated by the fact that birth fantasies are undoubtedly used to symbolise (perhaps we should say to conceal or cover) incestuous tendencies connected with the Oedipus complex or indeed sexual or other desires of a more general nature (the latter including the

widespread notion of 'spiritual rebirth'). Dr Fodor recognizes the reality of this tendency (which he calls 'retrojection'), but nevertheless is firm in his insistence on the importance of birth fantasies in their own right and maintains that genuine birth dreams can often be detected by an element of 'fatality' (in the sense that something *must* be done), finding a further justification in the therapeutic success in his hands of what he calls 'birth therapy'.

When we turn to the other obstacle to the serious treatment of birth fantasies, which lies in the difficulty of imagining that we can have any memory of our own individual birth, the author falls back on the admittedly vague concepts of 'organismic mind' and 'organismic memory', which he holds can produce fantasies which have a rough but real resemblance to the actual biological facts of the birth process, even though the symbolism makes use of the most varied elements of experience from later life. In judging this position we must remember that Jung has insisted upon the existence of psychic 'archetypes' and that Freud held, though perhaps more hesitatingly, a similar view as regards certain primal fantasies—both of which were supposedly independent of what we ordinarily recognize as individual memories and which might seem to be mediated by what might not unreasonably be called organismic mind. We recognize that anatomical and physiological development both in man and animals is determined by hereditary or 'organismic' tendencies, that reflex and instinctive behaviour is similarly determined, but we boggle at the notion of conscious *ideas* (as distinct from conative *tendencies*) having a parallel origin. If Jung is right (not that Dr Fodor makes any appeal to Jung) this attitude is unjustified, and if we grant that fantasies may sometimes be determined by what we might broadly term racial memories, then there is *a fortiori* also a case for some dim kind of memory underlying birth and uterine fantasies, for they at least correspond to events or states which have undoubtedly occurred in the *individual* life, even though they belong to a period usually covered by the deepest infantile amnesia. What is badly needed here is some attempt to correlate the nature or vividness of natal or prenatal fantasies with the actual individual history of birth or gestation. Some thirty years ago the present reviewer (in *The Psycho-Analytic Study of the Family*, 1921) suggested a line of research which might possibly throw light upon the subject, but in the interval neither he nor any other investigator has been enterprising enough to face the difficulties (including the difficulty of incredulity) involved. At least two cases mentioned by Dr Fodor appear to show the existence of such a correlation: the first, a patient whose (neurotic) pains in the navel were associated

with a clumsy tying of the umbilical cord, the second that of a patient whose nightmares of strangling were connected with the fact that she was born with the cord tightly wound round her neck. In both these cases the patients had been told of these conditions attending their birth, and this subsequent information may have been the sole factor in producing the mental phenomena observed ; on the other hand it may just possibly have acted merely as a reinforcing agent, and taken as a whole Dr Fodor's data certainly strengthen the case for taking the 'birth trauma' seriously, whatever may be its origin, and for studying it by all methods which may be available.

Not content with the fantasies of birth and of the prenatal state, Dr Fodor goes further in suggesting what he calls fantasies of the pre-maternal state and the trauma of conception. In some of what he says under this heading he was interestingly anticipated by Herbert Silberer who in 1912 (*Jahrbuch für Psychoanalytische und Psychopathologische Forschungen*, iv, 141) called attention to the occurrence of 'spermatozoa dreams', which he thought might symbolise a desire for impregnation (the dreamer had a knowledge of biology) but which also, according to him, involved a desire for 'infinite regression', including return to the *father's* body, much as in some of Dr Fodor's interpretations (though the latter author does not actually refer to this anticipation of his views). Dr Fodor is no more afraid of facing an element of mysticism than was Silberer in an earlier psycho-analytic generation, though he admits that such pre-maternal fantasies (insofar as they are anything more than mere biologically interpreted fantasies expressing the common idea of life before conception or incarnation) 'must be so alien to our comprehension as to defy all attempts at verbal expression. In feelings we may rise into it [pre-maternal existence] but we may not be able to translate this feeling into knowledge. . . . As to the reality of such existence science has nothing to offer' (p. 94).

There are various other aspects of the present work (the several chapters of which originally appeared in article form in various periodicals between 1942 and 1951) which are of interest primarily to the psycho-analyst or psychopathologist. Among these is a very striking account of some cases of long-persistent hysterical colour blindness which yielded rapidly to psychological treatment. We should mention, further, the author's interesting discussion of the unconscious significance of numbers, which is probably the most complete which has yet been attempted. The contrast between the conscious (or perhaps we should say 'realistic') mathematical treatment of numbers which are regarded as entirely abstract and the unconscious treatment which immediately invests

them with some concrete meaning (often of a 'magical' and extremely fantastic kind) is here particularly impressive and is well calculated to bring out the special peculiarities of 'autistic thinking' (e.g. 15 = house or home, because 15 minutes = a quarter of an hour, and 'quarters' = living quarters, (p. 218); or again, in what the author calls the 'evocation of the undreamt', when the significant number may, for example, be obtained by subtraction, as when '24 and 100' = 'independence', because  $100 - 24 = 76$ , and 1776 is the date of the Declaration of Independence (though the figure 76 itself never enters consciousness), (p. 348).

As the author says, 'logically or illogically the unconscious may stop at nothing in search of meaning' (p. 263), and he reminds us in this connection of the one-time humorous description of the sexologist Baron Schrenck von Notzing as 'Baron Shrink from Notzing (nothing)'. Some interesting examples are given of such unconscious processes which have found their way into the conscious mind and there become superstitions which have exercised a potent influence on belief and behaviour. This raises the disquieting question how far psycho-analytic or parapsychological interpretations (including some of the author himself) may represent similar irruptions of the unconscious into our would-be scientific attitudes and findings. The question has to be faced, but should not deter us from a due scientific regard for the real influence of the unconscious on many (we hardly know yet how many) of the difficult problems with which psychical research is concerned.

J. C. FLUGEL

THE GREEKS AND THE IRRATIONAL. By E. R. Dodds. Berkeley and Los Angeles, University of California Press; London, Cambridge University Press, 1951. ix, 327 pp. 37s. 6d.

The purpose of this book, which is the outcome of lectures delivered in 1949 at Berkeley, California, is to consider whether the Greeks were 'in fact quite so blind to the importance of non-rational factors in man's experience and behaviour as is commonly assumed both by their apologists and by their critics'. The author's conclusion is that they were *not*. The book should be considered primarily as a contribution to classical scholarship, and has little direct bearing on the problems of modern psychical research, but there is much in it that will interest the student of these problems.

A chapter on 'The Blessings of Madness' includes a discussion on 'Prophetic Madness' as exemplified by the Pythian priestess at Delphi, into whom, it was alleged, the God Apollo entered and

used her vocal organs in the same way that 'the so-called "control" does in modern spirit mediumship', and there is also (p. 71) an account of 'belly-talkers'. It would appear that these people went into trance during which a hoarse 'belly-voice', assumed to be that of a 'daemon', was heard issuing from their lips. The analogy with the 'direct voice' of modern days is obvious.

In the chapter on 'Dream-Pattern and Culture-Pattern' Professor Dodds explains (p. 103) that he is mainly concerned not with the dream-experience of the Greeks, but with their attitude to dream-experience, but he points out that we may have to reckon with variations in the character of the experience itself: 'In many primitive societies there are types of dream-structure which depend on a socially transmitted pattern of belief, and cease to occur when that belief ceases to be entertained'. He quotes a statement by Jung that a medicine-man had confessed to him that 'he no longer had any dreams, for they had the District Commissioner now instead'. The chapter includes an account of the various methods, mainly incubation, used to induce 'divine', that is, significant dreams, as opposed to non-significant dreams. The practice of incubation still persists among the Greek peasants of today. The Hippocratic treatise *On Regimen* (p. 119) relates many dreams to the physiological state of the dreamer, which they express in symbolic form. Although the fourth-century author's interpretation of these symbols would not be accepted today, it is interesting to find him to this extent foreshadowing Freud, whom he also anticipates in accepting 'the principle that the dream is always egocentric'.

In Appendix I on 'Maenadism' there is an account of the Oreibasia, the mountain dance, which seems to have originated as a means of inducing religious experience and ecstasy; dancing as a means to this end has many parallels. 'The ritual oreibasia at a fixed date may have originally developed out of spontaneous attacks of mass hysteria. By canalising such hysteria in an organised rite once in two years, the Dionysiac cult kept it within bounds and gave it a relatively harmless outlet' (p. 272).

Appendix II contains an account of Theurgy, about which much is still obscure. The practice would seem, however, to have involved in some cases the employment of a trance medium. Sometimes the trance would be a light one, the medium's consciousness still persisting; sometimes it would be deep, resembling apparently the trance observed in Mrs Piper's mediumship. The phenomena alleged to occur in the presence of these Theurgists offer many analogies to the phenomena associated with modern Spiritualism, paranormal knowledge, elongation and possibly levitation of the medium's body, luminous forms and apparitions

seen entering and leaving his body, with which we may compare 'the so-called "ectoplasm" or "teleplasm", which modern observers claim to have seen emerge from and return to the bodies of certain mediums'.

H. DE G. S.

JOURNAL OF PARAPSYCHOLOGY. Durham, N.C., Duke University Press. \$1.50

VOL. 15, NO. 3, SEPTEMBER 1951.

In an editorial article, Professor Rhine discusses the outlook in parapsychology. He notes the signs of maturity in parapsychological experimentation and its consequent increasing technicality. Of future trends, he prophesies increasing use of spontaneous cases for giving fresh orientation in experimental work and increased integration with neurophysiological and biological researches.

Mrs Rhine, in an article on 'Conviction and Associated Conditions in Spontaneous Cases', points out some of the differences between spontaneous cases of psi knowing and the experimental results of card-guessing experiments, particularly the fact that the former are very often accompanied by a conviction that the percipient knows that the experience he is getting is giving him information about a real event.

An article on 'ESP Performance and Target Sequence' by S. G. Soal and J. G. Pratt contains an account of a new analysis of a series of 33,500 ESP trials by Mrs Stewart in which the attempt is made to discover whether there is any relationship between success and the pattern of target sequences. Highly significant relationships were found.

Professor Broad reviews a symposium by three philosophers on the relevance of psychical research to philosophy. He agrees that philosophers may make a useful contribution to parapsychology by analysing its terminology, but suggests that they should prepare themselves for this task by a careful study of the relevant literature.

VOL. 15, NO. 4, DECEMBER 1951.

In an article on 'The Present Outlook on the Question of Psi in Animals', Professor Rhine makes a wide and well-documented study of the research done in this field and of its unsolved problems. He deals with investigations that are being made of migratory and homing behaviour in birds, and of experimental work on homing in mice. He also points to the very considerable body of evidence on spontaneous homing behaviour in dogs and cats, and the still more puzzling cases of trailing behaviour in which families have travelled to a distance by car or train and found that their pets which were left behind have turned up at their new homes.

In an article on 'Introversion-Extraversion Ratings in Relation to Scores in ESP Tests', Dr Betty Humphrey finds a significant difference between the psi scoring of those who are rated as extraverts by the Bernreuter Personality Inventory and those who are rated as introverts, the former scoring positively and the latter negatively. It would be interesting to know whether this is an intrinsic difference in introverted subjects or whether it is determined by the personality type of the experimenter. It is possible that an introverted experimenter might find that his introverts scored positively and his extraverts negatively.

A minor article by Paul and Christiane Vasse compares the scoring rate of these two experimenters in PK tests. C.V. showed significant positive effects while P.V. scored at chance level. A previous study had suggested that C.V. was successful in influencing the rate of germination of seeds while P.V. was less so.

In 'An Experimental Study of ESP Capacity in Mental Patients' by K. E. Bates and Marietta Newton, it is reported that although co-operative patients scored better than those who were non-co-operative, there were no significant differences found between patients suffering from different mental disorders. The experiment seems to have been of somewhat defective design since differences were calculated only between total scores of different classes of patients, instead of separate patients being treated as individuals in a contingency table.

This number of the *Journal* concludes with 'Reflections on Parapsychology, Psycho-analysis, and Atomic Physics' by Professor P. Jordan, the German physicist, and a review by Professor Ducasse of Mr Moncrieff's book *The Clairvoyant Theory of Perception*.

VOL. 16, No. 1, MARCH 1952.

The Editorial by Professor Rhine is a discussion of the present position of our knowledge about dowsing. It contains an informative account of experimental work in the past with valuable suggestions of how it may be made more fruitful in the future.

Esther Foster gives an account of ESP experiments in which (as in guessing playing cards) there is more than one aspect of the target object on which success can be obtained. The problem is whether there is a tendency in successful experiments to score separately on the different aspects or whether success means that the card is guessed right as a whole. In most experiments it appears that the card is guessed right as a whole, but there are indications in one experiment that the subject may be more successful in some aspects than in others.

An address on 'Thought Transference and Related Phenomena' given by R. H. Thouless to the Royal Institution in 1950 is reprinted from the *Proceedings* of the Royal Institution.

A minor article of great interest and importance is contributed by Miss E. A. G. Knowles on a psi effect obtained in stopping a manually operated random selector in a determined position when the apparatus is not seen by the operator. She points out that this is an operation of a different character from that ordinarily covered by the term PK, and suggests that it should be provisionally labelled PD (psi dexterity). She also points out that PD may enter into success with dice when these are thrown manually.

A placement PK experiment by H. Forwald, following a highly successful experiment reported earlier, did not yield significant deviations from mean chance expectation. The combined result of the two series remains, however, highly significant.

R. H. THOULESS

JOURNAL OF THE AMERICAN SOCIETY FOR PSYCHICAL RESEARCH.

Vol. 46, No. 2, April 1952. New York, A.S.P.R., \$1.50.

In 'Current Developments in Psychical Research', Dr Gardner Murphy reviews modern trends and feels that real progress is being made in the field of extrasensory perception. He makes a six-part appeal to members of the A.S.P.R. which our own members would do well to heed : (1) To record every spontaneous case that comes their way ; (2) to take part in home experiments ; (3) to contribute financially ; (4) to let others know about the aims and work of the Society ; (5) to come to meetings or form Study Groups ; (6) ' . . . you can always ask questions which will put a bee in our bonnet . . . . There is nothing more glib, more garrulous, and more hopeless than a research man who thinks he can get along without other people's suggestions. It is a completely nonsensical idea . . . . What we research people want is to be understood, to be helped . . . to be criticized. We want to be told what we should do that we are not doing, and told how we should do it. If you know of sensitives, if you know of methods, if you know of problems, let us hear about them ; let's find a way. Let's do the best we can do in a group enterprise.'

J. L. Woodruff and Mrs L. A. Dale present a report on a study of the relationship between ESP function and the psychogalvanic response. The results were disappointing.

W. H. W. Sabine stresses the value of reports of spontaneous precognition. He suggests that selected remarkable cases 'will not help so much in its study as a number of cases, however trivial the circumstances, which have happened to the same



individual, provided always that he is capable of exact observation and narration.'

An elaborate PK research is being carried out by Dale and Woodruff using the rotating cage with camera with which Professor Robert McConnell, of the Physics Department of the University of Pittsburgh, is said to have achieved spectacular results.

D. P.

## CORRESPONDENCE

### THE DIEPPE RAID CASE

SIR,—The statements made by D. and A. in The Dieppe Raid Case contain unusual features and are in certain respects ambiguous.

(a) Until A. had returned to the room (assuming A. was absent from the room for 5 minutes) neither A. nor D. had mentioned to the other the noises that they had heard for 15 minutes.

(b) There is no conclusive evidence that A. continued to hear those noises during the 5 minutes that she was absent from the room. If she did not, what significance (if any) should attach thereto? There is no evidence that A. suddenly (or otherwise) ceased to hear those noises upon leaving the room and suddenly (or otherwise) commenced to hear those noises upon returning to the room. If she did, what significance (if any) should attach thereto?

(c) There is no conclusive evidence (apart from the fact that in their respective statements D. says that she had been listening to it 'for about 20 minutes' and A. says that she had been listening to it 'for about 15 minutes') whether D. continued to hear those noises during the 5 minutes that A. was absent from the room.

(d) There is no evidence (apart from the fact that in her statement D. says that 'A. said she had also been listening to it for about 20 minutes') whether D. or A. (and at what time) first mentioned to the other the length of time that D. or A. had heard those noises.

(e) Although A. in her statement says that she had been listening to the noises 'for about 15 minutes' D. in her statement says that 'A. said she had also been listening to it for about 20 minutes'. When did A. make this observation to D.?

(f) If it is correct that D. and A. had heard those noises for 20 minutes and 15 minutes respectively, before A. left the room, how is the discrepancy of 5 minutes accounted for?

(g) Since it is not known for how long D. had been awake, D. may have been mistaken when she says that she 'woke up before it started'. While A. suggests (unless her awakening and the commencement of the noises were coincidental) that she (A.) was awakened by such noises, it may be (although such noises may have commenced in the sense that had A. been awake she would have recognised them) that she was not.

(h) Is there any evidence that D. and A. (alone or jointly) had recently attended a film of, or discussed with others, the Dieppe landing, or some other subject that might be suggestive thereof? Whether such evidence is, or is not, available, it is apparent from the nature of the statements made by D. and A. and from the fact that D. and A. occupied the same room, when they heard those noises, that collaboration cannot be excluded.

DENIS CHESTERS

SIR,—The Dieppe Raid Case, investigated by G. W. Lambert and Kathleen Gay, must recall to the minds of many the case of *An Adventure*, in which also anonymity was, at the outset, preferred by the percipients. In both cases the hallucinatory experience occurred to two English ladies while on a holiday in France. Both dates were in the month of August, and on a Saturday (August 10, 1901, and August 4, 1951). The Dieppe Raid case was, indeed, almost on the fiftieth anniversary of the Trianon one. In both cases additional experiences were reported by one only of the percipients: Mrs Dorothy Norton in the one case, Miss Jourdain in the other.

A less obvious but very important point of resemblance arises out of the possession by Mrs Dorothy Norton and Miss Agnes Norton of a guide book entitled *Dieppe*. Mr Lambert appears to have shown satisfactorily that the information about the raid in this guide book could account for but a small part of the knowledge evinced by the percipients in their statements; and moreover they said they had not read the book before the experience started. None the less, it is undeniable that the word 'Dieppe' is alone sufficient to suggest to the mind of any English person the famous raid of August 19, 1942.

Miss Moberly and Miss Jourdain, the percipients in the Trianon case, also had a guide book—Baedeker's *Paris*. Turning to the description of the Petit Trianon, the actual scene of their 'Adventure,' in the 1900 edition, we read: 'A visit should be paid to the Jardin du Petit Trianon, which is laid out in the English style and contains some fine exotic trees, an artificial lake, a 'Temple of Love', and a 'Hamlet' of nine or ten rustic cottages, where the court ladies played at rustic life.'

When the eye of the English-speaking reader lights on the word 'Hamlet' (so printed), it is apt to suggest to him the tragedy, although a moment later he realises that the celebrated *Hameau* is intended. But an image of Hamlet has been called up, however fleetingly, a picture of the solitary and melancholy man in conjunction with the 'Temple of Love.' These images may well account for the sinister cloaked figure which Miss Moberly and Miss Jourdain believed they had seen sitting close to the pillared kiosk.

That which is suggested by the word 'Dieppe' to two modern young Englishwomen who know what the sounds of war are, and that which is suggested by the words 'Temple of Love' and 'Hamlet' to two middle-aged spinsters of the year 1901 are, beyond all question, very different. But is there not an indication of the same principle lying behind their respective hallucinations? May we not have in both cases illustrations of the power of the mind to be cognisant of the contents of books and other sources of information which have not yet been seen, and, under rare and exceptional circumstances, to share this experience with another and bring it to consciousness by means of a visual or auditory hallucination or both?

I am well aware that Miss Moberly and Miss Jourdain displayed a sad ignorance of psychical research, scarcely excusable in women of their opportunities; but their severest critics may see in the experience of Mrs and Miss Norton some reason to reconsider whether the Trianon case did not have, at the outset, a genuinely paranormal basis.

It may also be thought desirable to enquire whether the percipients in the Dieppe Raid Case have ever read *An Adventure* or extracts from it.

W. H. W. SABINE

MR G. W. LAMBERT writes :

The Editor has shown me the above letters with the authors' permission, and I am glad to have the opportunity to make the following observations.

Mr Chesters is quite right in calling attention to a discrepancy between D.'s and A.'s statements as to the earliest phase of the experience. I noticed the discrepancy and asked the percipients questions with a view to resolving it but was not entirely successful. The discrepancy is unsatisfactory, but relates to a period of time which is small (5 minutes, at the outside) and is not of crucial importance, considering the margin of inaccuracy one must allow for in judging how long one has been awake in the dark before looking at the time.

In the course of my questioning I ascertained

(1) that the percipients took the time from their respective wrist-watches, which had *not* got luminous dials. It is to be inferred, therefore, that neither knew the time was 4.20 a.m. till the light was turned on. It is clear that the light was not turned on till A. returned to the room. I therefore inferred that D. heard the noise start at 4.20 a.m. minus 20 minutes—but this 20 minutes is necessarily a guess and, for all one can know, it may have been 3.47 or so, when (on 19 August 1942) the firing started, or some minutes after 4 a.m.

(2) that A. did (quite certainly) hear the noise while she was out of the room, just as she continued to hear it out on the balcony later on. (I was concerned to discover whether A.'s hearing of the noise was dependent on the close physical presence of D.; apparently it was not). I made no attempt in the report to draw out the full significance of the incident. I did not ask D. whether, during A.'s absence from the room, she also continued to hear the noises, but it is tolerably certain that she did.

The above supplementary information goes a long way to answer the points (a) to (g). It means that the time '4.20 a.m.' from the beginning of D.'s statement cannot be taken as exact, as she did not know the time was 4.20 till the light was turned on 20 minutes later. The existence of the discrepancy between the statements at this point is tantalizing, but is rather strong evidence against the theory of collaboration.

As to point (h), neither D. nor A. was 'interested' in the Dieppe raid, and had neither 'read it up' nor had it brought forcibly to her attention shortly before the experience. The possibility of collaboration in making up the whole story cannot be excluded, any more than can the possibility of 'faking' in what purports to have been a scientific experiment of a kind that cannot easily be repeated. The good faith of the percipients must either be accepted or not. In this case the investigators, who are not without experience in these matters, have more than usually full information affording grounds for accepting the good faith of the percipients.

In his very interesting comparison of the Dieppe Raid Case with the Trianon case of 1901, Mr Sabine asks whether the percipients in the former case had ever read *An Adventure* or extracts from it. Mrs Norton (D.), on being asked whether she had read that book, did not recognize the title, but, when Versailles was mentioned, had a vague recollection of it. She seemed to know the outline of the story, without any detail. It is, of course, possible that her unconscious memory of the Trianon case is much fuller than her conscious

recollection of it. But if the earlier case played any important part in suggesting the 'framework' of the later case, it is surprising that the later experience was not a visual hallucination, like the Trianon case, and like the earlier experiences of Mrs Norton herself, before her visit to the Dieppe neighbourhood. I would judge that both percipients had enough normally acquired knowledge of the Dieppe Raid, and experience, acquired doing the war, of noises made by guns, shells, etc., to 'explain' the occasion and general content of the experience of the 4th August, 1951, and that it is hardly necessary to call in aid forgotten details of the Trianon case. What the memory of past reading or experience cannot explain in the Dieppe Raid Case is the time coincidence factor which characterized an abnormal experience (prolonged *auditory* hallucination) unique in the lives of both percipients. If one admits that any feature of the experience came *ab extra*, where is one to draw the line?

#### ESP AS GUESSWORK

SIR,—When Mr Flew first put forward his suggestion about ESP and guesswork I failed to see the connexion, and his recent letter to the *Journal* does not make it any clearer. Certainly the process of 'guessing' deserves psychological study. It has long been realized that guesses do not come mysteriously out of the blue in strictly random fashion. In card-calling tests most subjects change their call from one trial to the next more frequently than occurs in a chance series, and every subject has his own symbol preferences and sequence habits. While it would be interesting to know the inner reasons for these habits, it is difficult to see how such knowledge could help towards an understanding of ESP.

In a successful ESP test, there is a correlation between cards and calls which shows that one of the associations available to the subject, and effective in determining his choices, is the target order. No amount of knowledge of the ordinary associations which influence choices seems likely to explain how the concealed target becomes available to the subject.

The same consideration applies to spontaneous cases. It is interesting to know, for example, that a dream of a relative's death is due to unconscious hate, but this in no way explains the fulfilment of such a dream. Unlike Mr Flew, Freud seems to have been well aware of this when he put forward the theory that mediums are telepathically sensitive especially to the repressed complexes of their clients.

D. J. WEST

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# Society for Psychical Research

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SUPPLEMENT

TO

JOURNAL

Volume XXXVI No. 671 JULY-OCTOBER 1952

FOR MEMBERS AND ASSOCIATES

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## THE SOCIETY'S ROOMS

THE Society's Rooms, including the Library, will be open throughout the summer.

## ANNUAL GENERAL MEETING

THE Annual General Meeting of the Society was held at 31 Tavistock Square, London, W.C. 1, on Wednesday, 23 April 1952, at 3 p.m., under the chairmanship of Mrs W. H. Salter.

The Secretary having read the notice convening the Meeting, the Report of the Council and the Accounts were presented. After a discussion in which Mr E. R. Brown, Dr E. J. Dingwall, Mrs K. M. Goldney, Mrs F. Heywood, Mr P. I. Price, Mr P. S. Seward, and Mr B. Smith took part, the Hon. Secretary, Mr W. H. Salter, moved the adoption of the Annual Report and Accounts which was seconded by Miss I. Jephson and carried unanimously.

The Chairman announced that there were no candidates for election to membership of the Council other than the six members who retired by rotation and who offered themselves for re-election, and the following six members were accordingly unanimously elected: Mrs K. M. Goldney, Lord Charles Hope, Mr G. W. Lambert, Mr W. H. Salter, Mrs W. H. Salter, and Admiral the Hon. A. C. Strutt. On the proposal of Admiral Strutt, seconded by Dr E. J. Dingwall, Messrs Miall, Harper & Co. were re-elected Auditors for the forthcoming year.

## OBITUARY

We regret to record the death of Dr William Brown, a distinguished member of the Society. Dr Brown was Wilde Reader in Mental Philosophy in the University of Oxford from 1921 to 1946, and founder and first Director (1936-45) of the Institute of Experimental Psychology in that University. He was President of The British Psychological Society, 1951-2.

## MEETINGS OF THE COUNCIL

Meetings of the Council were held as follows :

473rd	27 Feb. 1952	Chairman : The President, Dr S. G. Soal.
474th	16 Apl. 1952	Chairman : Admiral the Hon. A. C. Strutt.
475th	23 Apl. 1952	Chairman : Admiral the Hon. A. C. Strutt.
476th	23 Apl. 1952	Chairman : Admiral the Hon. A. C. Strutt.

At the meeting of the Council held on 16 April 1952 the following co-optations to the Council were renewed for the current year : Mr G. W. Fisk, the Hon. Mrs C. H. Gay, Professor A. C. Hardy, Mr J. Fraser Nicol, Mr Edward Osborn, Dr D. J. West, and Dr Richard Wilson.

At the meeting of the Council held immediately after the Annual General Meeting on 23 April 1952, the following were elected :

## PRESIDENT

Dr Gilbert Murray, O.M.

## HONORARY OFFICERS

*Hon. Treasurer*, Admiral the Hon. A. C. Strutt.

*Hon. Secretaries*, Mr W. H. Salter and Mr Denys Parsons.

*Hon. Editor of Proceedings*, Mrs W. H. Salter.

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MEETINGS OF THE SOCIETY

1. *Private Meetings at 31 Tavistock Square*

- 249th Wednesday, 19 March 1952, at 6.30 p.m. Dr E. J. Dingwall :  
'Some New Light on D. D. Home'.  
250th Wednesday, 16 April 1952, at 6.30 p.m. Mr A. T. Oram :  
'The Trend of Experimental Technique in ESP and PK'.

2. *Public Lectures at Caxton Hall*

- Wednesday, 21 May 1952, at 8 p.m. Dr Gilbert Murray, O.M.  
Presidential Address.  
Wednesday, 4 June 1952, at 8 p.m. Dr R. H. Thouless. 11th  
Myers Memorial Lecture.

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ESP EXPERIMENTS WITH SIMULTANEOUS  
ELECTRO-ENCEPHALOGRAPHIC RECORDINGS

BY S. C. WALLWORK

THE intention in performing these experiments was to discover whether it is possible to distinguish between correct and incorrect ESP calls by changes in the electrical activity of the brain of the percipient. If such a distinction could be made, it would not only give some insight into the ESP process, but it might enable a weak ESP faculty to be detected without the necessity for carrying out a large number of experiments for the application of statistical methods. It is reasonable to suppose that a call which is correct by chance would be accompanied by a similar type of brain activity to that of an incorrect call, but one which is correct by genuine ESP faculty might be accompanied by a different type of activity. If these two types of brain activity could be unambiguously distinguished, then every single ESP hit could be detected.

As soon as a few preliminary runs had been completed, it was realized that it would be very unlikely that any unambiguous difference could be recognized on the EEG record between ESP hits and misses. These runs were, nevertheless, very instructive and served as a guide to further experiments, so the procedure will be described briefly.

The percipient generally wore four pairs of electrodes, two in fronto-parietal and two in temporo-occipital positions, and the potentials developed across each pair were recorded on separate channels on the EEG. The tests were carried out under GESP conditions, using a home-made pack of twenty-five cards containing five of each of the symbols O, L, S, V, Z. As each card was exposed by the agent, he tapped with a pencil directly on to the EEG record in a position close to the recording pens, thereby making a mark which synchronised the card exposure with the record of brain activity. The tap could be heard by the per-

percipient in an adjacent room through an open communicating hatch, but percipient and agent were not visible to each other. On hearing the tap, the percipient made his guess at the target card.

Three methods of recording the guesses were tried. In the first, the percipient wrote them down himself while in a semi-reclining position and with his eyes open. This method was rejected because regular, large-potential jumps, particularly in the temporo-occipital channels, indicated that movement of the eyes after the recording of each guess was causing interfering muscle potentials. In the second method, the percipient indicated his guess by depressing one of five keys on a key-board, so causing the illumination of the corresponding symbol in a box watched by the experimenter. Unfortunately, electrical interference from the keyboard completely obliterated the EEG record, and this method was abandoned also, though the interference might have been overcome by screening the keyboard. The third method, which was adopted for all subsequent experiments, was simply that the percipient spoke his guesses softly from a reclining position with eyes closed and with the minimum of muscle movement, while the experimenter recorded the guesses. These softly-spoken calls could not be heard by the agent in the adjacent room because of the background noise made by the mechanism of the EEG.

It was soon found that there was no obvious change in the EEG pattern at any point in a GESP run, and it could no longer be expected that any prominent feature could be associated with either a correct or an incorrect ESP call. It should be mentioned here that the percipient had shown significant results in previous GESP tests. Table 2 (*a*) shows the results of all the tests carried out under GESP conditions with the same agent during the first three months of experiments with this percipient. From the ESP point of view, the experimental methods and conditions were the same as for the EEG experiments, except that the agent and percipient were seated at separate tables in the same room in such a way that the percipient could not see the agent and the agent could only see the back of the percipient. Also, in some of the experiments, ESP instead of letter cards were used, but in all cases they were in 'closed packs' of twenty-five cards. After this significant series of tests, there had been an interval of about two months before any further GESP experiments had been carried out, and when they were resumed no further significant scores were obtained. The EEG experiments were therefore commenced at a time when good scores could not be expected. Nevertheless, it was reasonable to suppose that the percipient

would still be capable of making at least an occasional correct guess by ESP.

It was then considered that a more sensitive test for extra-sensory communication between agent and percipient might be the recognition of similar trends in the EEG records made from both persons at the same time. Such similarities might be detectable even if differentiating features could not be found in the record from the percipient alone. This possibility was tested by taking simultaneous records from both percipient and agent, each wearing two pairs of electrodes in posterior positions and feeding into the four channels of the one machine. No obvious similarity between the two sets of records could be found at any point. The conditions were not good, however, because the agent had his eyes open and both he and the percipient were in a sitting position.

After these preliminary experiments, a different procedure was adopted, for the suggestion of which the author is indebted to Dr J. R. Smythies. The electrode arrangement and the method of conducting these experiments were the same as in those already described in which the percipient and agent were in adjacent rooms. The agent recorded the order of the target cards on a score sheet which was kept separate from the experimenter's list of the percipient's calls until the experiment was over. Before comparing the calls with the target cards, the EEG record was examined for the extent of alpha-rhythm. Each period during which a card had been exposed was rated as 'strong alpha', 'average alpha', or 'weak alpha', on the basis of the previous records made by the percipient. A more quantitative rating could not be carried out because a wave-analyser was not available, but this qualitative classification was made by Mr E. G. Williams, the electro-encephalograph technician, assisted by the agent and the experimenter. Only after this examination was complete were the guesses and target cards compared and the positions of correct and incorrect calls correlated with the EEG.

This correlation was carried out as follows. Of the total number of periods during which cards had been exposed, the fractions having each of the three ratings were found. These fractions represent the probabilities of any one guess being associated with 'strong alpha' or 'average alpha' or 'weak alpha' activity. Also, the number of hits and the number of misses associated with each type of activity were found. By comparing these with the total numbers of hits and misses, they are converted into the proportion of hits with each rating and the proportion of misses with each rating.

If there were no correlation between success and alpha-rhythm

activity, these two proportions should be the same for each type of rating, or the difference between the proportions should be zero. This difference is not usually zero, and to test whether it is significant it is compared with the standard error of the difference, calculated from the formula  $\sqrt{pq(1/n_1 + 1/n_2)}$ . Here,  $p$  is the probability of a guess being associated with the particular type of activity under consideration (found as described above),  $q$  is the probability of its not being so associated and is therefore equal to  $(1-p)$ ,  $n_1$  is the total number of hits, and  $n_2$  the total number of misses. The ratio of the difference between the proportions to the standard error of the difference is the critical ratio which is the measure of the significance of the difference. To be indicative of a definite tendency for hits to be associated with one particular type of alpha-rhythm activity, the critical ratio must be at least three. This analysis has been carried out for  $+1$  (precognitive) and  $-1$  (postcognitive) hits as well as for 0 hits on the actual target card, and the results are shown in Table 1.

From the small values of the critical ratios it is clear that no correlation has been found between the distribution of hits and the variation of alpha-rhythm activity in the brain of the percipient. Unfortunately, the analysis of scores shows that these experiments are also insignificant from the ESP point of view. This is probably due largely to the general decline in scores shown by the percipient, but it may also be due partly to bad ESP conditions caused by the discomfort of wearing the electrodes. A comparison of the scores in this series of experiments with the GESP scores in the previous significant series of experiments, with the same agent is shown in Table 2.

Because of the insignificance of the ESP scores in these experiments with the EEG, it is impossible at the moment to see whether there is any correlation between the correctness of an ESP call and the nature or extent of electrical activity of the brain of the percipient at the time when the call is made. It is hoped, therefore, that it will be possible to repeat these experiments with other percipients.

The author would like to thank Mr J. Parsons for being a very co-operative percipient under rather trying experimental conditions and Mr R. B. Joynson, Lecturer in Psychology at Nottingham University, for advice and assistance. Thanks are also due to Dr D. Macmillan, Physician Superintendent of Mapperley Hospital, Nottingham, for permission to use the electro-encephalograph, and to Mr E. G. Williams for assisting in the experiments and in the evaluation of the results.

TABLE 1

## CORRELATION OF ALPHA-RHYTHM ACTIVITY WITH DISTRIBUTION OF HITS

(a) Number of guessing periods having each of the three alpha-rhythm ratings

Alpha-rhythm rating	First expt., 28-8-51			Second expt., 4-9-51		
	-1	0	+1	-1	0	+1
Strong - - -	46	48	47	48	50	45
Average - - -	94	98	92	99	102	101
Weak - - -	38	40	39	45	48	46

(b) Analysis of correlation of hits and misses with the three alpha-rhythm ratings

	Rating	Number of hits with each rating			Number of misses with each rating			Difference between proportions of hits and misses with each rating			Critical ratios		
		-1	0	+1	-1	0	+1	-1	0	+1	-1	0	+1
First experiment, 28-8-51	Strong	11	10	12	35	38	35	0.030	0.080	0.080	0.38	0.19	0.70
	Average	20	19	16	74	79	76	0.019	0.016	0.136	0.21	0.17	1.51
	Weak	8	8	11	30	32	28	0.011	0.001	0.080	0.15	0.01	1.07
Second experiment, 4-9-51	Strong	7	9	8	41	41	37	0.046	0.052	0.009	0.55	0.70	0.11
	Average	19	26	17	80	76	84	0.073	0.121	0.012	0.76	1.41	0.13
	Weak	7	8	8	38	40	38	0.027	0.069	0.003	0.33	0.95	0.04

TABLE 2. SUMMARY OF ESP SCORES

Participant—J. Parsons.

Agent—S. C. Wallwork.

(a) Previous significant series

Date	Total Calls	-1 hits	0 hits	+1 hits	Target Cards
26-1-51	175	36	51	28	ESP
3-2-51	125	26	24	27	ESP
1-3-51	275	52	70	50	Letters
8-3-51	200	31	44	48	Letters
	775	145	189	153	
	Chance expectation	148.8	155	148.8	
	Deviation	-3.8	+34	+4.2	
	Critical ratio	0.35	3.05	0.39	

(b) Preliminary experiments with the EEG

Date	Total Calls	-1 hits	0 hits	+1 hits	Target Cards
1-6-51	200	31	33	41	Letters
17-7-51	100	25	24	21	Letters
	300	56	57	62	
	Chance expectation	57.6	60	57.6	
	Deviation	+1.6	-3	+4.4	
	Critical ratio	0.24	0.43	0.65	

(c) EEG experiments with alpha-rhythm rating

Date	Total Calls	-1 hits	0 hits	+1 hits	Target Cards
28-8-51	186	39	37	39	Letters
4-9-51	200	33	43	33	Letters
	386	72	80	72	
	Chance expectation	74	77.2	74	
	Deviation	-2	+2.8	-2	
	Critical ratio	0.26	0.36	0.26	

## EXTRASENSORY PERCEPTION AND PSYCHOKINESIS

AN EXPLANATION IN TERMS OF INTUITIVIST EPISTEMO-  
LOGY AND PERSONALIST METAPHYSICS

BY N. LOSSKY

PROFESSOR C. D. BROAD, in his article 'The Relevance of Psychological Research to Philosophy', writes as follows: 'To sum up about the implications of the various kinds of paranormal cognition. I have the impression that we should do well to consider much more seriously than we have hitherto been inclined to do the type of theory which Bergson put forward in connection with *normal* memory and sense perception. The suggestion is that the function of the brain and nervous system and sense organs is in the main *eliminative* and not productive.'<sup>1</sup>

While fully agreeing with Professor Broad, I should like to put forward some suggestions as to the kind of philosophical system which would render explicable the facts of extrasensory perception established beyond all doubt by modern research. It seems to me that these facts can be best explained on the basis of metaphysical personalism combined with an intuitive theory of knowledge, i.e. with the doctrine that we directly perceive not only our own mental states, but objects of the external world—as well.<sup>2</sup>

Personalism is the theory that the world consists of actual and potential individual centres of consciousness. A well-known instance of personalism is the philosophy of Leibniz, according to whom even atoms—or, we should now say, electrons, protons, etc.—are monads or potential personalities. These elementary entities not only create material processes of attraction and repulsion, but have inner experiences, unconscious psychoid processes which differ from psychic processes solely by their extreme simplicity. Leibniz calls such entities 'sleeping monads'. Under the influence of experience they develop and combine with one another, forming atoms, molecules, unicellular and multicellular organisms. Their life thus becomes more and more complex, and at last they become actual personalities.

Every self is a supertemporal and superspatial entity, but it imparts to its feelings, strivings, and actions the form of time

<sup>1</sup> *Philosophy*, October 1949, p. 306.

<sup>2</sup> A short exposition of such a philosophical system is given in my *History of Russian Philosophy* (New York, International Universities Press, 1951), an English edition of which is to be published by Allen & Unwin early in 1953.



(psychical or psychoïd processes) or of both space and time (e.g. material processes of attraction, repulsion, movement, etc.).<sup>1</sup>

A supertemporal entity manifesting itself in time is called in philosophy a substance. It is preferable to use the term 'substantival agent', in order to emphasize its active character.

Leibniz maintained that monads 'have neither doors nor windows', and that the being of each is entirely separate from that of others. It is a mistake, however, to conceive of the world as broken up into disconnected units. Every agent's manifestations are in accordance with the principles and mathematical ideas that determine the structure of time and space and are identical for all. As bearers of identical formal principles of the world's structure, substantival agents or monads are *consubstantial* with one another. The founder of neo-Platonism, Plotinus, used an excellent metaphor to illustrate this consubstantiality: he said that the human race consists of a number of men who look in different directions but are joined together at the backs of their heads. The consubstantiality is of course only partial, but it constitutes such an intimate bond between substantival agents that each one's experiences exist not for it alone, but, if only unconsciously, for all the others, i.e. from the personalist standpoint, for the world as a whole. This is of essential importance to epistemology. If I direct intentional acts of awareness, attention, and discrimination upon an external object, it is cognized by me as it is in itself, and not by means of subjective images, symbols, etc. Such direct contemplation of objects as they actually are may be called intuition.

It will be said that if direct perception of external objects be a fact, there can be no need for us to have eyes, ears, and other sense organs. According to the theory worked out by Galileo, Hobbes, and Descartes, stimulation of the sense organs is the cause which gives rise in our minds to the subjective image of an external object. Bergson rejected this *causal* theory of perception: he maintained that physiological processes in the sense organs and the cortex do not create the perception, but are merely a stimulus inciting us to direct attention to the actual external object that has impinged upon our body and may be harmful or useful to us. Unfortunately Bergson's intuitivism did not go far enough: he did not regard all cognitive acts as intuitive. Scientific knowledge expressible in rational concepts was for him, as for Kant, a subjective construct of our reason and not contemplation of reality. He thought that in addition to the activity of

<sup>1</sup> See my article 'The Absolute Criterion of Truth' in the *Review of Metaphysics*, June 1949.

reason we also have a faculty of intuition as the contemplation of creative living reality not expressible in rational notions.

For fifty years I have been developing an integral theory of intuitivism, i.e. the theory that all cognitive acts are different kinds of intuition.<sup>1</sup> My book *Sensuous, Intellectual and Mystical Intuition* is a detailed exposition of the view that sensory qualities of objects are not our subjective sensations, but properties of material processes in the external world; rational ideas—e.g. mathematical truths—are objects of intellectual intuition by means of which we contemplate the ideal aspect of the world—the aspect owing to which the world is a system; mystical intuition is the source of religious experience. A theory of intuition based upon the conception of the intimate bond between every self and the world as a whole may be called a *co-ordinational theory of perception*. On that theory the most ordinary sense perception, e.g. seeing a tree within ten yards of me, is a kind of clairvoyance: the excitation of the eye and of the visual centres in the cortex is merely a stimulus for my self to perform an act of clairvoyance in space. Bergson says that in the course of evolution sense organs have been developed for purposes of self-preservation, signalling the appearance of an object which may be useful or harmful to us and inciting the self to direct its attention upon it.

According to this view, every agent's characteristics and experiences exist not only for it but, unconsciously, for all other entities in the world. Hence it follows that stimulation of sense organs is not a necessary condition of perception. It is useful for the satisfaction of our ordinary daily needs, but in certain important cases the perception of objects remote from us in space may take place without the stimulation of the sense organs. Death, illness, or danger threatening those dear to us affect us unconsciously even though we be far apart, and, without any excitation of our sense organs, may serve as a stimulus for us to direct upon those events acts of awareness, attention, and discrimination.

Many people possess the faculty of bringing into their field of consciousness, without the help of sense organs, events of lesser moment that are of practical use to them. For instance, when they go to bed they decide to wake up at a certain hour, and actually do so; probably this is due, in many cases, to an extra-sensory perception of the hands of the clock. Professor Rhine's experiments have proved that even objects of no practical import-

<sup>1</sup> See my *Intuitive Basis of Knowledge* (English translation published by Macmillan in 1919), *Sensuous, Intellectual and Mystical Intuition* (English translation published by the Russian University of Prague, 1934-8), and other books and articles.

ance may be extrasensorily perceived if attention be concentrated upon them. Both extrasensory perception and precognition are explicable on the assumption of the unconscious bond between the human self and the world as a whole. They become intelligible if we give up the causal theory of perception and adopt the co-ordinational, according to which even normal sense perception is a kind of clairvoyance.

Precognition does not imply that time is unreal and that there is no clear distinction between past, present, and future. We contemplate past, present, and future as such because our self is a *supertemporal* entity and therefore can direct its acts of intuition upon any section of time.

The co-ordinational theory of knowledge is incompatible with the positivistic view of causality, according to which the cause of an event is the totality of events upon which it follows with necessity. The champions of this view have lost the true conception of causality, for they do away with the *dynamic* aspect of it and retain only the temporal sequence, putting main emphasis upon its regularity. A 'dynamic' theory of causality is more in keeping with the facts of experience; it is concerned with the *creation* of an event, and regards temporal sequence as a derivative aspect of the creative act. Events do not arise in time of their own accord, but are produced by someone. Having a temporal form, they fall away every instant into the domain of the past and are therefore incapable of creating the future; only a supertemporal substantival agent can be the bearer of creative power manifesting itself in time. Of course, a substantival agent creates a new event on the basis of his former and his present experiences. Hence it follows that we must distinguish between *cause* in the strict sense of the term, and *occasion*: the cause, i.e. the creator of an event, is always some substantival agent, and the 'occasion' are the circumstances in connexion with which an agent manifests its creative power. Uniformity and necessity do not form part of the idea of causality: no one has ever proved that an agent is compelled with absolute necessity to repeat the same actions in similar circumstances. The possibility of science does not in the least require uniformity of causation: it is sufficient that events should arise with a certain amount of regularity. In the lower kingdoms of nature, studied by physics and inorganic chemistry, this regularity reaches a considerable degree of uniformity and can be expressed by *statistical laws*.

On this view of causality there is no need for us to account for precognition by the strange assumption that the future causally affects the present: the future simply provides the occasion for us to direct an act of perception upon it. The co-ordinational

theory of knowledge leads to the conclusion that extrasensory perception in all its varieties does not essentially differ from normal sense perception, because in normal perception the activity of the sense organs is only of secondary importance.

Let us now consider the problem of psychokinesis, and, to begin with, ask the general question as to whether psychical processes can influence material processes. Take the simplest form of material process, e.g. repulsion between two entities. Repulsion is only possible as push and counter-push made by two agents, say, electrons A and B, in such a way that these two actions arise absolutely *simultaneously*, have the *same* force, and are carried out along the *same* line in opposite directions. Such simultaneous mutual determination of two objects is placed by Kant in his table of categories next to causality as a special category of reciprocity (*Wechselwirkung*). How are we to understand the marvellous correspondence of the mutually opposed actions of the two electrons? In order to explain it, we must distinguish between the psychoid or psychical *striving* to make a particular act of repulsion in a definite direction, and the actual *realization* of this striving as a material process in space. In the case of mutual repulsion of two agents A and B the initiative may belong to A, in the form of a striving to repulse B; in virtue of the agents' consubstantiality this striving is unconsciously experienced by B. If B reacts to it by a corresponding counter-striving, the two agents may realize in space a material process of mutual repulsion with equal force along the same line in opposite directions. Thus every mechanical process is psychoidly-mechanical or, at a higher stage of development, psychically-mechanical.

The dependence of material processes upon psychical in no way conflicts with the law of the conservation of energy. The only admission to be made is that the increase, say, of repulsion in one direction, required for a particular purpose, is accompanied by a decrease of repulsion in other directions so that the quantity of energy remains the same. To use Hartmann's terminology—without having recourse to his conception of non-central forces—it can be said that the influence of the psychical factor results in the transference of energy from one co-ordinate of space to another, where it is needed for our purpose. Besides, if the manifestations of an agent's energies depend upon its own psychic or psychoid states, it can be easily imagined that they may temporarily cease altogether, and the corresponding energy may pass from actual to a potential state, or *vice versa*. This explains the slowing down and the re-starting of material processes discussed by Driesch.

Nor does the doctrine here expounded conflict with the law of inertia. According to this law a body can only change its state of rest or motion under the influence of some force external to it. If an animate body could begin or cease to move, or change the direction of its movement solely through wishing to do so, the law of inertia would be violated. Suppose that on hearing some noise in the street I wanted to discover the cause of it, got up from my chair, and walked to the window. If this change in the position of my body were wholly determined from within, by my wish alone, this would certainly be contrary to the law of inertia. In truth, however, we must distinguish between an inner striving to make a movement—a psychical or a psychoid process—and the external realization of this striving, i.e. the material process of movement. When I want to rise from my chair and go to the window, my striving to push my foot away from the floor is intensified, but if the floor did not offer a corresponding resistance, my movement would not take place.

The law of entropy, on this view, must be limited, at any rate with reference to vital processes in plant and animal organisms. To see the reason for this, consider the action of the human self upon its body. By 'body' I mean here the totality of comparatively undeveloped agents allied with a highly developed agent, the human self, and serving its purposes as its organs. The human self embraces the life of its organism in one integral act of experience, and is therefore able to co-ordinate the activity of the different organs so that they work for one complex purpose and for the good of the organism as a whole. Each cell of the organism, sympathetically participating in the life of the human self, carries out the task assigned to it by the co-ordinative striving of that self. Consequently the organism behaves purposively as a whole, although many organs participate in its activities. Without using any energy the human self transforms chaotic movements into orderly ones, giving them, for instance, the same direction; hence, vital processes often are of ectropic character. In the words of Driesch, our self really is 'Maxwell's demon'.

Since a man's will is capable of influencing substantial agents which form part of his body, it is possible that in certain cases it also acts upon agents external to the body. They, too, are consubstantial with the human self and therefore unconsciously experience our strivings directed upon them; and sometimes they may comply with these strivings, or may react to them negatively. In order to prove experimentally the influence of human will upon the external world, we must direct our efforts of will upon potential or actual personalities. Such experiments may be carried out

with unicellular organisms; this was done, for instance, by Mr Nigel Richmond in his experiments with paramecia.<sup>1</sup>

The above considerations show that the metaphysics of personalism combined with an intuitive theory of knowledge enable us to regard paranormal phenomena as essentially akin to normal: both extrasensory and sensory perception are species of direct contemplation of objects in the external world; the action of our will upon objects external to our body is explained in the same way as its normal action upon our own body.

A totally different character must be ascribed to attempts of influencing by will not personalities but material things, such as dice. From the point of view of personalism a die consists of potential personalities—atoms and molecules. But their combination in the die is only an aggregate and not an organic whole headed by a potential personality which could be influenced by our will. Accordingly, in this case we should have to exercise command over millions of agents constituting the die; and it is extremely improbable that these millions should all together obey our will. If Professor Rhine's experiments prove that human will can affect the fall of the dice, this fact would have to be explained in a different way than the influence of human will upon paramecia. The following hypothesis might provide an explanation: the effort of will directed upon the falling die may be accompanied by some sort of radiation proceeding from the body and acting upon the die just as the human hand would act in pushing it. In that case a paranormal event would be analogous to the normal.

## J. G. PIDDINGTON AND HIS WORK ON THE 'CROSS-CORRESPONDENCE' SCRIPTS

BY W. H. SALTER

By the death in April of J. G. Piddington one of the last links with the founders of the Society has been broken. J. G. Smith, as he then was, joined the Society in 1890, and became a member of Council in 1899. About this time he adopted his mother's surname of Piddington, to avoid confusion with several other Smiths

<sup>1</sup> 'Two Series of PK Tests on Paramecia', *Journal of the Society for Psychological Research*, March-April 1952, pp. 577-88. I had thought that experiments with potential personalities like electrons were not possible, but I am informed by the Editor of the Journal of the S.P.R. that these could be done with an electronic computer or possibly a geiger-counter.

then prominent in the Society. In the same year on the proposal of Frederic Myers he became Hon. Secretary, acting jointly with Myers until the latter's death in 1901 and resigning in 1907. He was during his term of office closely concerned in two events of importance to the Society; first the creation in 1901 of the Research Endowment Fund, of which he was for many years the active Trustee; and secondly the separation, after Richard Hodgson's death in 1905, of the American Branch of the S.P.R. and its transformation into the present American Society. Piddington visited the United States and negotiated on behalf of the S.P.R. the financial and other arrangements consequent on the new state of affairs.

From 1917 to 1921 he was Hon. Treasurer. A successful business man himself, he looked after the Society's finances with especial care, nursing the Research Endowment Fund from very small beginnings, so that in due time it might produce an income sufficient to pay a salary for a full-time research officer with qualifications and status equivalent to those of a university research worker in any of the recognized branches of Science. The income of the Fund grew indeed, but not, since the First World War, as fast as the value of money declined, so that fifty years after the Fund was started the Society still lacks an endowment sufficient to enable it to make the most of present possibilities of research.

For some years before the First War Piddington had been living at Woking as a near neighbour of G. W. Balfour, and by 1920 he had come to reside at Balfour's house, Fisher's Hill, where both of them lived until after the outbreak of the Second War. Mrs Sidgwick also made her home there about the same time, remaining there until her death in 1936. So it came about that for the period between the end of the First War and the Society's Jubilee in 1932, three leading members of the Council, Mrs Sidgwick, G. W. Balfour, and Piddington were in close contact with each other, living for most of that period in the same house, and were thus well placed to give the Society the strong central direction which in those critical times it needed.

Critical they were because the three principal founders of the Society, Sidgwick, Myers, and Gurney, were all long since dead and had become mere names to a large portion of the membership. Hodgson and Podmore, who had from early days been closely associated with them, had died before the War, and during the War the breakdown in Alice Johnson's health had compelled her to resign the post of Research Officer. The War had brought a large influx of new members, many of whom knew nothing of the past history of the Society or its standards of investigation, ap-

proached some of our problems emotionally and uncritically, and were easily beglamoured by reports of astounding successes obtained in mushroom institutions with grandiloquent titles.

How easily in such conditions spreads the complaint,

‘All old, nothing new?

Only the usual talking through the mouth,  
Or writing by the hand? I own, I thought  
This would develop, grow demonstrable,  
Make doubt absurd, give figures we might see,  
Flowers we might touch . . .

‘The Pennsylvanians gained such. . . .’

How seductive, even to persons with less excuse than Mr Sludge, the example of ‘Pennsylvania’, or its twentieth-century counterparts! Attempts were made, and not without influential backing, to force the S.P.R. on to a ‘positive’ doctrinal basis, to substitute showmanship for research, and, failing all else, to split the Society, but they all broke on the rock of Fisher’s Hill.

There might be a risk in any society that so close a combination of Elder Statesmen would confine the society’s activities to those in which the Elder Statesmen had a special interest. Nothing of that sort happened with the Fisher’s Hill group. Their own interest was at this time centred on the ‘mental’ phenomena—trance-mediumship of Mrs Leonard’s type, automatic writing, ‘phantasms’, experimental telepathy—and much research on those lines was being carried out during the period. But a glance at our *Proceedings* or *Journal* will show that ‘physical’ mediumship was at the same time being investigated with as much vigour as at any period of the Society’s history.

Piddington was President for the years 1924–5, and retired from the Council in 1932 after more than thirty years’ membership of it. He continued to be an active member of the Committee of Reference until 1940.

Valuable as were his administrative services to the Society, it is his part in interpreting the scripts of the ‘S.P.R. group of automatists’ that calls for special notice. This was the task of a team that included Mrs Sidgwick, Oliver Lodge, Alice Johnson, and G. W. Balfour, but the main work fell on Piddington, and increasingly so as ill-health or advancing years compelled the withdrawal of the others. This is not the time or the place to attempt a final appraisal of the significance of the scripts, but it may be a convenient occasion to remind members of the general character of a piece of work on a larger scale perhaps than any other undertaken by our Society, and of a kind that has not been and could not have been undertaken by any other body.



The problem that faced the interpreting group may be shortly stated thus. For about thirty years from 1901 onwards a round dozen of automatists, many but not all of them members of the Society, produced well over three thousand 'scripts', a word given for convenience an extended meaning to include not only pieces of automatic or inspirational writing, but also records of trance utterance, inspirational speech, and impressions received in sleep or waking or various states between. At an early stage it was noted that there were connexions between the scripts of different automatists, and also between them and the records of sittings with Mrs Piper, that appeared to be neither fortuitous nor due to normal association between them. These were the simpler 'cross-correspondences'. The connexions were usually made through the recurrence of the same phrase, quotation from the same literary source, or insistence on the same topic.

As the result of wider and closer study it was noted that the cross-correspondences interlocked with each other in such a way as to make a pattern covering a very large portion of the script material. As integral parts of the pattern were found (1) references to verifiable facts which were certainly not within the conscious knowledge of the automatist at the time they were made and had, so far as could be ascertained, never been within his or her normal knowledge, and (2) predictions relating both to public and private matters. Was it possible, by any rational and consistent method of interpretation, to establish what the pattern was, how it came into existence, and whether it conveyed any particular meaning?

This would have been a stiff enough job if the interpreters had been set to work on a final, complete set of scripts, in neatly typed fair copies, with all the quotations traced to their sources in half-a-dozen literatures, all the personal allusions annotated, and everything indexed. But in fact all the searching out of literary sources and all the annotating and indexing had to be done while the scripts were pouring out, and the interpreters had at the same time to frame, with such assistance as they could derive from directions in the scripts, canons of interpretation for a mass of material, disconnected, allusive, and symbolic. Further study made it almost certain that the obscurity was often deliberate and designed to prevent either automatist or interpreter from guessing the drift before what the script-intelligence, to use a non-committal term, considered the appropriate moment. In the Introduction to his paper in Volume XXXIII of *Proceedings* (pp. 439-60) Piddington gave a brief and lucid account of the principles on which he and his fellow-interpreters had worked. The work had been mainly done by him and Balfour in consultation, but no progress could

have been made in it without the index, the compilation of which fell almost entirely upon him.

It was a task calling for infinite patience, tireless industry, and scrupulous accuracy in detail, in all of which Piddington was highly gifted. If there was the smallest variation in the form in which a quotation occurred in different scripts, any slight misspelling of a name or error as to a date, Piddington was down on it at once. The slip might, so far as the automatist's conscious mind was concerned, be unintentional, but possibly it might be a device of the script-intelligence, as I have called it, to attract special attention to the passage where it occurred. Above all, having fixed his rules of interpretation he was prepared to follow them to their logical conclusions, even if it meant attributing to the script-intelligence intentions of a surprising kind and, as regards details of the pattern, meanings repugnant to his own robust common-sense. For it must be emphasized that he was not at all a cranky or eccentric person. Apart from psychical research, he conformed very closely in manner, opinions, and interests to the typical Englishman of his age and education. Fortunately, perhaps, although he was well-informed and well-read on many subjects, he had no special leaning towards poetry, so that in tracking down the sources of the many poetic quotations with which the scripts overflow, he had to move from step to step by careful study and had no temptation to jump to conclusions.

What, it may be asked, was the net result of all this labour and ingenuity? To put it at the lowest, the interpreters produced order out of chaos, so that when they had done their work nearly the whole of the enormous mass of scripts fitted into a coherent pattern of which they had no conception when they started their labours—*nearly* the whole, that is to say, apart from exhortations to the automatists and interpreters and discussions as to experimental methods, which of course lie outside the pattern; and even when these are excluded there remain passages where the interpreters confessed themselves unable to discover a coherent meaning.

It would of course be easy enough for an unscrupulous interpreter to select tit-bits here and there from so large a mass, arrange them arbitrarily, interpret them according to the caprice of the moment, and thereby obtain any pattern he pleased. Those, needless to say, were not the methods of Balfour or Piddington or any of their fellow-workers. That so many pieces of the puzzle fitted neatly into place to produce an elaborate design incorporating many details that, considered by themselves, are extremely odd, seems to me strong evidence that the design really is

there, and is not the product of the interpreters' fantastic ingenuity.

Nor can it be assigned to the normal knowledge of each others' scripts acquired by the different automatists, e.g. through reading *Proceedings*, where several scripts were from time to time published, nor yet to rational inference based on such knowledge. Of the principal members of the group 'Mrs Holland' (Mrs Fleming) died without knowing even the main outlines of the pattern; Mrs Stuart Wilson does not know them yet; Mrs Verrall only learnt them from Piddington after most of her scripts had been written, and was with difficulty persuaded to accept his account; and my wife was equally hard to persuade when informed after her own scripts had ceased.

The choice seems to lie between three hypotheses, all paranormal, separately, or perhaps in some combination of them. To begin with the hypothesis involving the smallest departure from views acceptable to most psychical researchers, it might be argued that some person consciously or subconsciously designed the pattern and contrived to distribute it by telepathy among the members of the group, each of whom reproduced in his scripts the portion allotted to him. Those who incline to this view generally cast Mrs Verrall for the principal part, as she was the earliest of the group, and had the literary knowledge sufficient for producing the pattern as we have it. On the other hand she had no normal knowledge of some of the facts woven into the pattern, and the pattern went on unfolding itself for many years after her death in 1916. It should be noted that even this hypothesis, which may be taken as the minimum worth discussion, postulates telepathy of a kind very different from anything established or suggested by quantitative or qualitative experiment.

This same difficulty attaches to the second hypothesis, which is that the pattern was created by the subconscious minds of the automatists, acting as a group with a collective character that persisted notwithstanding the death or retirement of some of its members and the accession of others. This meets the difficulty as to the development of the pattern after 1916, but raises problems of its own, particularly as regards the implication of collective constructive activity on a very large scale carried on entirely at the subconscious level.

The third hypothesis is that the pattern was devised by some intelligence or group of intelligences external to the group of automatists, and, more specifically, by a group of communicators including Sidgwick, Myers, and Gurney, with each of whom some of the scripts claim to originate. This was the view that after prolonged study all the principal interpreters accepted, although

several of them were not lightly persuaded to accept it. Piddington's natural scepticism was reinforced by a personal dislike, which he expressed to me, for the idea of surviving; but this did not prevent his accepting the survivalist view which he maintained in his paper in Volume XXXIII of *Proceedings*.

It may be doubted, however, whether Balfour, Piddington, and the other interpreters would have been willing to devote the immense amount of time and labour that the elucidation of the scripts demanded, if the sole object of the scripts had been to establish the survival and identity of the ostensible communicators. That was only the first stage in the declared purpose of the script-intelligence. The declared ultimate purpose is the creation of a universal and durable order of peace between nations and between classes, in the promotion of which the communicating group is, with many other persons, represented as engaged. This, one need hardly say, is an ideal that has appealed to many people at many times, and occasional references to it might have been expected in the scripts of automatists whose outlook on life was that of the members of 'the S.P.R. group'. When found there, they would have called for no particular notice. What struck the interpreters as significant was the prominence given to this topic in the whole mass of scripts, the persistence with which it is dwelt on from their beginning in 1901, and the peculiar way in which it is woven into a pattern embodying various paranormal features, cross-correspondences, verifiable statements of facts not normally known to the automatist making them, and predictions of public and private events, of which some had in the interpreters' opinion been fulfilled.

All this seemed to warrant their belief that they were in touch with discarnate intelligences having power to influence the actions of living men and women and foresee future events, and using that power to promote an enterprise of immense importance for the welfare of mankind. Holding that belief they were willing to spare neither time nor labour year after year in clearing up every doubtful point connected with the scripts, and to put the result of their labours in an orderly permanent form, so that, if the predictions were fulfilled, it should be beyond dispute exactly what the predictions were, when they were made, and from whom they claimed to come.

'These all died . . . not having received the promises.' The interpreters never supposed that they would receive them during their lives. But Piddington, the last survivor of them by several years, told me in the autumn of 1950 that though he was sure that the predictions had been correctly interpreted, the continued deterioration in world affairs had for some time made him wonder

whether the communicators and their associates had not undertaken more than they could perform. No precise date for fulfilment is given in the scripts, but the suggestion is certainly made that a beginning at least would take place in the lifetime of persons living at the date of the First War. It seems to me desirable that this should be stated now, without waiting for the predictions to be fulfilled by the course of events, or falsified by the lapse of time.

As I have already said, this is not the occasion to attempt a final appraisal of the scripts, and when the occasion does arise the attempt should be made by someone with a fuller knowledge of the whole of the scripts than I can claim. But I have read carefully more than half of them and, so far as concerns the material I have read, I am prepared to accept the interpreters' construction of it as substantially correct, even as regards points where the eccentricity to be expected in any work of complex symbolism might appear to have been given very free scope. But just as in ordinary life two people telling the same story with every desire to be accurate will each tell it from a different angle, so with the automatists of 'the S.P.R. group'. For example, all the scripts claim to be inspired by persons who have survived bodily death, and there is general agreement among the automatists as to who these persons are, but with some of the automatists they are presented in a highly individualised dramatic form, with others the emphasis is on what 'they' say and do, on their collective and, perhaps, inter-personal activity. It has sometimes seemed to me that in this and in other connexions the interpreters have overstressed the *personal* aspect, and that the cause of this may have been that with both Balfour and Piddington their subtle powers of reasoning were not tempered by psychic faculties, so that, quick as they were to discern what was or was not significant in substance, they were prone to accept too literally the dramatic form with which the substance was clothed. I hesitate, however, to bring even this minor criticism against a piece of work on so great a scale as the interpretation of the scripts, and one carried out with so much toil and skill.

On the other hand, Piddington had in full measure two indispensables for psychical research, human sympathy and a sense of humour. Because of his sympathy and his obvious trustworthiness and discretion, whenever, as often happened, it was necessary to verify some personal allusion in the scripts, he had no difficulty in getting people to answer questions as to their private affairs or to allow him to inspect their confidential diaries. His keen sense of humour made him a delightful companion and correspondent as soon as one penetrated his natural shyness and the reserve that went with it.

The poor health from which he suffered for many years did not prevent his deriving much quiet enjoyment from country life, golf and music, all of which he found at Fisher's Hill. From the windows of his book-lined study there he could enjoy delightful views of garden, woodland, and down, and there, whenever scripts became a weariness to the flesh, he could console himself at the piano, which he played very well. But above all Fisher's Hill gave him the company of like-minded friends. The last few years of his long life were spent in his daughter's charming house overlooking the Bristol Channel.

### ICHTHYOSIS TREATED BY HYPNOSIS

STUDENTS of psychical research are familiar with claims that conditions resistant to normal medical treatment have been cured or relieved by 'psychic' means, by religious faith, or by a combination of the two. Thorough investigation of these claims is much more difficult than would appear at first sight, but most experienced psychical researchers—and certainly the medical profession—would attribute the results to suggestion applied to functional disorders of psychological origin. This view is supported by the absence of evidence that a congenital condition of an organic nature has been cured or relieved by suggestion alone. It is for this reason that a recent report in the *British Medical Journal* has aroused such interest.<sup>1</sup> The following condensation is printed by permission of the Editor of the *British Medical Journal* and of the author, Dr A. A. Mason, Senior Registrar, Queen Victoria Hospital, East Grinstead.

The patient, a boy of sixteen, suffered from congenital ichthyosis ('Crocodile Skin'), a condition whose aetiology is unknown and which is regarded as resistant to all forms of treatment. 'The lesion', writes Dr Mason,

consisted of a black horny layer covering his entire body except his chest, neck, and face. The skin was papilliferous, each papilla projecting 2-6 mm. above the surface, and the papillae were separated from each other by only a very small distance, perhaps 1 mm. The papillae themselves varied in size from small thread-like projections

<sup>1</sup> A. A. Mason, M.B., B.S., 'A Case of Congenital Ichthyosiform Erythrodermia of Brocq Treated by Hypnosis', *British Medical Journal*, No. 4781, 23 August 1952, pp. 422-3.

on the abdomen, back, and flexor surfaces of the arms to large warty excrescences 5 mm. across on the feet, thighs, and palms. The small amount of skin which was visible between the papillae was also black, horny, and fissured. To the touch the skin felt as hard as a normal finger-nail, and was so inelastic that any attempt at bending resulted in a crack in the surface, which would then ooze blood-stained serum. In the skin flexures there were fissures which were constantly being reopened by movement and were chronically infected and painful. The ichthyosiform layer, when cut, was of the consistence of cartilage and was anaesthetic for a depth of several millimetres.

The condition varied in severity in different areas of the body, being worst on the hands, feet, thighs, and calves, and least on the upper arms, abdomen, and back. The skin on the face, neck, and chest appeared normal, although, as is shown later, it became papilliferous when transplanted to the palms. . . . The patient's parents are alive and well, and there was no family history of ichthyosis. His birth was normal, after an uneventful pregnancy.

After treatment at various hospitals without avail, on 25 May 1950 skin from the chest was grafted to the palms of both hands by Mr F. T. Moore, consulting plastic surgeon to the Queen Victoria Hospital, East Grinstead, but within a month it had become indistinguishable from the rest of the affected skin. A second attempt at grafting the palms two months later produced the same result, with the added complication of severe contractures of the fingers. Sir Archibald McIndoe and other plastic surgeons who saw the patient agreed that further grafting operations were unlikely to be successful, and that no alternative plastic procedure was possible.

Under hypnosis on 10 February 1951 the patient was given the suggestion that the left arm would clear.<sup>1</sup> (The suggestion was limited to the left arm so as to exclude the possibility of spontaneous resolution.) 'About five days later', says Dr Mason,

the horny layer softened, became friable, and fell off. The skin underneath was slightly erythematous, but normal in texture and colour. From a black and armour-like casing, the skin became pink and soft within a few days. Improvement occurred first in the flexures and areas of friction, and later on the rest of the arm. The erythema faded in a few days. At the end of 10 days the arm was completely clear from shoulder to wrist. . . . The right arm was treated in the same way (see Figs. 1*a* and 1*b*),<sup>2</sup> and ten days later the legs (see Figs. 2*a* and 2*b*; 3*a* and 3*b*) and trunk were treated.

<sup>1</sup> Dr Mason informs us that the treatment was carried out by him.

<sup>2</sup> The photographs are not reproduced here.

The result of the treatment is shown in the report as follows :

<i>Region</i>	<i>Before Treatment</i>	<i>After Treatment</i>
Hands	Completely covered	Palms clear. Fingers not greatly improved
Arms	80% covered	95% cleared
Back	Covered, but only lightly	90% cleared
Buttocks	Heavily covered	60% cleared
Thighs	Completely and heavily covered	70% cleared
Legs and feet	Completely and heavily covered	50% cleared

During the first few weeks of the treatment, 'clearance of the affected areas was rapid and dramatic', but 'during the last few months there has been no appreciable change. There has, however, been no relapse of the improved areas over a period of one year.'

## REVIEWS

FATHER THURSTON. A Memoir with a bibliography of his writings. By Joseph Crehan, S.J. London, Sheed & Ward, 1952. vii, 235 pp. 12s. 6d.

THE PHYSICAL PHENOMENA OF MYSTICISM. By Herbert Thurston, S.J. Edited by J. H. Crehan, S.J. London, Burns Oates, 1952. viii, 419 pp. 35s.

Padre Pio da Pietrelcina. By C. C. Martindale, S.J. (*The Month*, June 1952. pp. 348-57.)

When Fr Thurston died in 1939 the English Jesuits lost one of the most distinguished members of the Society and Roman Catholics in England their greatest expert in psychical research. He was a tireless worker and his first ticket of admission to the British Museum Reading Room was dated 1880. During the latter years of his life I used to meet him constantly as we both laboured under the dome, and we were often in consultation over controversial points, and sometimes combined in an innocent plot to persuade the authorities to buy some rare book which we both needed and thought should be added to the shelves.

Fr Crehan's memoir is an admirable sketch of Thurston's life and activities and throws a vivid light on the course of his mental development and outlook. His interest in psychical research was



early aroused, and in 1899 George Tyrrell wrote to him saying that Everard Feilding wanted him to go to hear the ghostly choir at Slindon since the S.P.R. was not welcome 'because they deal in devils'. Nevertheless, Tyrrell tried to persuade Thurston to join the Society at the same time as himself, but it was not till much later that he did so, and in 1921 read a paper on the phenomena of stigmatization.

In his discussion of Fr Thurston's many controversies Fr Crehan has, in the main, succeeded in being as objective as could be expected, although his lapse in printing the utterly unfair and untrue attack on the late Dr G. G. Coulton (p. 153) is to be regretted. It is true that the latter's clash with Fr Thurston was one of the bitterest moments of his controversial career, but his defence of Lea was doubtless influenced by his belief in Lord Acton's judgment and his opinion (which I did my best to shake) that Fr Thurston was far from being the doughty adversary that I knew him to be.

In his treatment of Thurston's psychical interests Fr Crehan is wary, although he reveals the fact, hitherto quite unknown to me, that Thurston visited Naples in 1925 to see the liquefaction of the blood of St Januarius, a phenomenon to which he and I had devoted a good deal of attention. For reasons which have never been explained neither he nor any other authority has, to my knowledge, attempted to deal in detail with the so-called exposure which was published in the *Hibbert Journal* in 1921, and all my recent attempts to obtain information from Catholic sources have met with no success.

Apart from a few minor errors, such as 'Douglas' Home, Mrs 'Woodhill' and that Fr Thurston joined the S.P.R. in '1919', Fr Crehan's biography is an excellent introduction to the work of a man whose book, *The Physical Phenomena of Mysticism*, to which I will now turn, is the most brilliant and fascinating account of the phenomena of the Saints ever written. In editing the work, however, it is a pity that Fr Crehan has not given the precise history and date of each of the papers of which this book is a collection, as since he is not sufficiently acquainted with the subject, it is often impossible to determine whether Fr Thurston omitted to discuss relevant material or whether such material was published after the paper itself had been printed.

The book itself is a mine of information on the alleged physical phenomena associated with various holy persons. In dealing with these extraordinary manifestations Fr Thurston displays a wide acquaintance with the different sources, and usually estimates the degree of credence to be placed upon them with considerable acumen. Some of his criticisms and exposures will come as a

shock to those unaccustomed to weighing evidence; and the objectivity with which he treats the material cannot fail to commend itself to the psychical researcher, especially since he states that some of the evidence as printed in the *Processes* is 'notably better attested' than any to be found in the *Proceedings* of the S.P.R.

Among the phenomena discussed and documented in this volume will be found levitations, the stigmata, telekinesis, fire-tests, bodily elongation, the alleged absence of *rigor mortis* and the usual signs of corruption, and some remarkable cases of living without eating. For levitation a number of instances are adduced, some of which are very difficult to explain away, although I find it hard to understand why Fr Thurston, in trying to show that these phenomena were still noted as occurring in the nineteenth century, should quote the quite unacceptable cases of Marie Baourdie (d. 1878) and of Maria della Passione (d. 1912), neither of which have, I think, the slightest evidential value.

Among all the remarkable phenomena recorded by Fr Thurston it is, to say the least, somewhat suggestive that many of the mediumistic phenomena regarded by psychical researchers as almost certainly fraudulent are scarcely to be found among those recorded by hagiographers, materializations being confined, so it would seem, to the very curious cases of the alleged multiplication of food, which phenomena Thurston thinks cannot be lightly dismissed, although he agrees that the evidence is often inadequate to sustain them.

To anyone acquainted with the works of such writers as Schram, Görres, Ribet, Poulain, or Saudreau, this book must come as a surprise. For it illustrates the changes in the viewpoint of intelligent and informed Catholics in these matters better than any work I know. For not only was Thurston both *chercheur et critique* in theology, as the head of the Bollandists called him, but a man who knew it to be his duty to make himself informed on what psychical research was doing undeterred by the ill-informed gossip about the devils and the S.P.R. It was this comparative survey which enabled him to treat many mystical phenomena as examples of conditions common in psychopathology and having nothing supernatural or miraculous about them.

It must be realized that, to the Catholic, the supernatural is of two main types: (a) the essential supernatural which is not open to scientific scrutiny and is the object of faith, and (b) the modal supernatural or more simply the marvellous. Thus the stigmata and various alleged diabolic manifestations belong to the modal supernatural in certain cases, the *fact* of which can be established by science, but of which the explanation is not to be sought on

material grounds. Thus, in order to determine the truly marvellous, all natural explanations *must* be eliminated. It is precisely here that we can see how grave differences of opinion are likely to arise in, for example, cases of diabolic possession where, to use the words of a recent writer in the *Études Carmélitaines*, 'le monde ecclésiastique n'est spontanément que trop porté, en cette matière, à une crédulité naïve', and where the imprudent and ill-considered action of certain priests is liable to do harm rather than good to the psychotic subject. Thus in the case of the stigmata, which psychical researchers have for long regarded as closely connected with hysterical and neurotic states (which conditions, in their relation to mysticism, had already been discussed by Fr G. Hahn in 1883), Thurston was insistent on the fact that hardly any cases are known where nervous disorders had not appeared before the development of the wounds, and indeed, he went so far as to say that he would like to hear of one case of a stigmatica who had no bad family history. In view of the prevalence of hysteria in the conditions under which women were formerly brought up, he pointed out how, during the last seven centuries, female stigmatics vastly outnumber the male, of which only two clear cases seem to have been recorded in which all the five wounds were externally observable. Although this disproportion is more than once mentioned by Thurston, it does not seem to have occurred to him that this is possibly due to the fact that the object of concentration and worship is male, and that it is this sexual element which is mainly responsible, a supposition supported by the cases of the very curious espousal rings which he describes in Ch. III, and by the fact that, at least in one case, a stigmatica, having happily married, was no longer stigmatized.

Although Thurston admits a number of instances of imposture among those with whom he deals, he has been clearly very careful to avoid any account of the more flagrant cases (such as the Holy Bambino of Bari) which were supported by high ecclesiastical authorities presumably for the benefit of those unable to detect or to expose the fraud.

In dealing with cases of stigmata prior to St Francis, Fr Thurston comments very unsympathetically on the English case of the early thirteenth century where Merkt claimed a genuine example of stigmatization, but which, as seems fairly clear, was a case of crucifixion and self-inflicted wounds, which is actually stated to have been so in the *Annals of Dunstable* and by Ralph of Coggeshall. That this is not the explanation in the modern case of Padre Pio seems to be certain, judging from the admirably concise account by Fr C. C. Martindale in the June 1952 issue of *The Month*. The extreme caution displayed by the Holy Office

in this case is clearly due to the influence of more modern views of the nature of the stigmata and their close connexion with psychopathological factors operating in the subject. What must strike the non-Catholic as so odd is that it does not seem to have occurred to some of the ecclesiastical authorities that, just as it is affirmed that the devil can profit by functional disequilibrium to gain influence over a sufferer and, indeed, cannot exert his power without such mental and nervous trouble being present, so God might exert His power in the formation of the stigmata only in persons whose psychological make-up lends itself to such Divine interference. From this point of view the stigmata of Padre Pio can be clearly classed as supernatural. This caution on the part of the Holy Office can, perhaps, be linked with the clarification of the views of Fr Thurston concerning the stigmata which were due to his study of the alleged production of such marks on the Austrian peasant woman by the Lutheran physician, Alfred Lechler, in 1932. Apparently Thurston accepted this story as conclusive proof that 'stigmata' could be produced solely through suggestion, and his acceptance is yet another indication of his increasingly credulous attitude when examining medical testimony concerning unusual physiological and psychological phenomena. This tendency, now becoming increasingly common among well-informed Catholics, is due to the fact that they deliberately cut themselves off from any *practical* experience of the phenomena with which they are dealing. Relying on printed sources they have little or no acquaintance with the true nature of the facts described, and thus are apt to credit tales of events which, had they actually been present, they would at once have seen had but little resemblance to the description given later by enthusiastic and untrained witnesses.

Towards the end of his life Fr Thurston became more and more prone to believe the stories of spiritualistic phenomena printed in presumably reputable journals, and my oft-repeated words of caution merely seemed to him to be the product of an unreasoning scepticism. With his acceptance of much of the Margery phenomena, even *after* the thumb-print controversy, he showed that even his critical mind had failed to resist the barrage of suggestion and display of pseudo-science which was becoming one of the most marked features of psychical research and which, once understood, was to lead many orthodox scientific men to doubt the validity of almost everything that came out of the parapsychological laboratories. That Fr Thurston failed to see through what lay beyond was another proof, if any were needed, of the goodness, simplicity, and faith which inspired him. His book is not only the achievement of a scholar and critic which, for the first time,

offers the evidence for the phenomena of the saints in an acceptable form, but the work of a very human and lovable personality to whom I am grateful for having been allowed to share, in however slight a degree, in the controversies and problems to which so many years of both our lives have been devoted.

E. J. DINGWALL

TELEPATHY AND SPIRITUALISM: personal experiments, experiences and views. By J. Hettinger. London, Rider, 1952. 150 pp. Illus. 16s.

This book covers a number of substantially unrelated topics and includes much that has already been published. Ch. VII, for example, gives Dr Hettinger's views on 'telepathy *v* clairvoyance', which were printed in S.P.R. *Proceedings* for June 1946. Ch. XI is a record of mediums' impressions collected by Dr Hettinger relating to the as yet unopened envelope left by the late Sir Oliver Lodge. Ch. X gives Dr Hettinger's views on Spiritualism, together with his account of the personal experiences with mediums that have made him a believer in communication with spirits. In Ch. I there is a discussion of the 'handicaps in the search for truth' deriving from 'the limitations of the field of mental vision' and 'our personal mental worlds'. A prominent feature of this chapter is the 'integrated diagram' (circular) of 'our personal mental worlds' in which there are assigned positions (segmental) to the realms of 'nature', 'mind and spirit', 'human relations', and so forth. It is interesting to juxtapose this with the comment in the following chapter that Whately Carington had a weak spot for philosophy.

Several chapters are a summary of Dr Hettinger's previously published investigations in object-reading. It is unnecessary to say much about this, since his two books on the subject, *The Ultra-Perceptive Faculty* (1940) and *Exploring the Ultra-Perceptive Faculty* (1941), have both been reviewed already in this *Journal*, and in addition the whole research has been analysed by Christopher Scott in S.P.R. *Proceedings* for November 1949. Briefly, the method is to present mediums with personal objects and collect impressions from them which are supposed to relate to the owners of the articles. Dr Hettinger adopted various control devices. For instance, he would pair all the items given by the medium in connexion with one article with items given in connexion with other articles in order to find out if the owner would be able to identify that member of each pair which was actually intended to relate to him.

Experiments of this type are not as easy to carry out objectively

as they appear to be at first sight, and the methods of Dr Hettinger made error likely. In some cases it seemed that the control items were not of the same quality as the items with which they were paired. Another difficulty was the possibility of bias in selected recording, since it was Dr Hettinger himself who sat with the mediums and wrote down their statements, and in most cases he knew the owners of the objects. There were also many elementary errors in the statistical evaluation tending to exaggerate the significance of the results, which were already doubtful on grounds of faulty experimental method.<sup>1</sup>

In Dr Hettinger's later work, the owners of objects looked at magazine illustrations at the time of the tests, and the mediums' impressions were supposed to relate to the content of these target pictures. Dr Hettinger considered that the correspondences obtained were self-evidently more than chance expectation, so that no controls were needed. The coincidences illustrated in *Exploring the Ultra-Perceptive Faculty* were certainly striking and likely to convince an uncritical reader, but then they had been selected from a large mass of material. The whole matter depended upon Dr Hettinger's subjective judgment, and that this was not altogether to be trusted is obvious because he seemed unable to appreciate that some at least of the correspondences would have occurred in the absence of telepathy. When he was prevailed upon by the American S.P.R. to have some of his data, together with some control material, scored by an independent judge, the scores on the control material were no different from the scores on the rest of the data. Christopher Scott also carried out an experiment with Dr Hettinger and, unknown to the latter, he substituted control pictures for some of the illustrations that had been used as targets in the usual tests. Dr Hettinger himself did the scoring, and again there were as many correspondences on the control pictures as on the others. A reasonable conclusion is that Dr Hettinger judged wrongly, and that the results were no different from chance expectation.

In Ch. VIII Dr Hettinger describes a new development, the attempt to transmit a code message by the use of his picture tests. A prepared set of target pictures is made to stand for the code words, and these are concentrated upon by an agent in the order representative of the words of the message. As before, the sensitive, at a distance, handles an object belonging to the agent. Her impressions are recorded by the experimenter, who notes any correspondences with the known target pictures and, from these

<sup>1</sup>The best available methods for the conduct and evaluation of object-reading tests are described by Pratt and Birge, *J. Parapsychol.*, December 1948.

correspondences, attempts to deduce the order in which the targets were looked at and thence the words of the message. This, of course, is an example of the use of pre-arranged targets in place of free material, a method which in another chapter Dr Hettinger condemns. It is a method which gives results that can readily be assessed statistically, but the figures are not quoted. Dr Hettinger tells us only that this method, and various modifications of it, all proved inadequate for the transmission of messages. As the evidence suggests that the previous results from picture tests were on a level with chance expectation, this conclusion is not surprising. Dr Hettinger gives a number of plausible rationalisations to explain his frustration.

It is a tragedy that years of effort were wasted on these faulty experiments. Chapter II reveals what is mainly responsible for their failure: an attitude of puerile animosity towards other investigators has prevented Dr Hettinger from seeking friendly advice at an early stage and so forestalling criticism. Instead he maintains his lone though vulnerable eminence by simply ignoring other people. It is typical of him that beyond the comment (p. 27) that other investigators have criticised his work as he criticises theirs, he gives the reader not the slightest hint that much of his research has been demonstrated to be ill-founded. His opinions of the research of others make droll reading. He has a bee in his bonnet about statistics, which he obviously does not grasp, so that he finds himself unable to accept the results of card-calling tests without an empirical control of randomly picked cards to demonstrate the level of chance expectation (p. 24). If only he had been as particular about his own picture tests! He condemns both Rhine and Soal for using a fixed number of symbols instead of 'free' material like drawings, but he is dissatisfied with Carington's work with drawings because the results were statistical and the subjects were not specially selected sensitives. As for Dr Rhine's PK hypothesis, he is 'very much surprised that the idea was mooted at all . . .' (p. 26). I have a similar sentiment about Dr Hettinger's hypothesis (Ch. VII) that cases of apparent precognition are due to the intervention of spirits. According to this theory a spirit anticipates or brings about an action in individual A. and at the same time gives individual B. an apparently premonitory impression of A.'s future action.

I have deliberately left to the last consideration of Ch. IX, because I believe it is the most important. In it there is a description of an ingenious new experiment designed to demonstrate the long-distance telepathic transmission of emotions. An agent is subjected at irregular intervals to painful or startling stimuli. The sensitive is connected to an apparatus for recording the psycho-

galvanic reflex—that is, changes in electrical resistance of the skin which coincide with emotional reactions. Deflections of the galvanometer corresponding in time with stimulation of the agent are held to indicate that the sensitive, subconsciously at least, is reacting to the agent's emotion.

The experimental design is amenable to a simple statistical control. The agent's stimuli are always given at exactly full minute intervals from the moment of commencing. On the average there is a stimulus at one in three of the minute intervals throughout the experiment. Whenever there is a galvanometer deflection within a fifteen-second range around the minute intervals this is counted as a reaction. It is stated that reactions *always* occur more frequently at the minute intervals when there is an agent stimulation than at the minute intervals when there is no stimulation.

No detailed account of the experiment is given, and it may be that randomisation of the times of agent-stimulation and other obvious precautions may not have been taken. If so, the project would be just one more example of a typical Hettinger experiment, excellent in conception yet faulty in execution. Personally I am optimistic. It would be an outstanding advance if this should prove to be the long-awaited method of obtaining accurate ESP responses by circumventing conscious inhibitions. I am sure we all hope to find in the full report the fruitful and valid consummation of his labours which Dr Hettinger's persistence richly deserves.

D. J. WEST

WHERE TWO WORLDS MEET : the verbatim record of a series of nineteen séances with John Campbell Sloan, the famous Glasgow direct voice medium. By Arthur Findlay. London, Psychic Press, 1951. 624 pp. 1 plate. 12s. 6d.

Do spiritualists realize how much harm is done to their cause by a claim, such as that made by the author on the cover of this book, that 'No more conclusive evidence of survival is available' than the reports of sittings he has printed? If such a claim could be substantiated, the case for survival would be very much weaker than I believe it to be.

John Campbell Sloan, who died in May 1951 while this book was being printed, had practised as a medium for fifty years, and Mr Findlay's acquaintance with him goes back more than thirty years. It would naturally be distasteful to any living medium to be forced into competition with another medium who has recently died, but as Mr Findlay has made this claim it must be pointed



out that the communications recorded in this book were produced under much less rigorous conditions than those recorded in the case of several other mediums both living and dead. The sittings with Sloan were held in a circle of about half a dozen, most of whom on every occasion appear to have been regular sitters living in the Glasgow area. He could hardly have failed in course of time to have acquired normally much information about the regular sitters and their friends and relations, living and dead. The same was true of the Piper mediumship in its early stages, and it was to avoid prejudicial inferences that might be drawn from this fact that Mrs Piper was brought over to England, to sit with persons about whom she had no previous knowledge. After her return to the United States she was confronted with a very large number of sitters quite unknown to her and often introduced anonymously. Much more evidential value is to be attached to communications received through a medium giving sittings to single sitters who are frequently changing than to communications given to a more or less constant circle.

The sittings recorded in this book took place at various times between April 1942 and July 1945, when Sloan was already an old man. Several times during the sittings he complained of feeling tired, and it is probable that, whatever his psychic powers may have been, they were already in decline. It is accordingly the sitters, who seem to have been very easily satisfied, rather than the medium, against whom criticism should be directed. Take, for example, the incident reported on pp. 148-9 when the medium said he had been looking at a photograph of the husband of Mrs Lang, and that it was not very clear. A voice here interrupted, 'Yes, you lost the negative and it had to be taken off an old photograph,' and persisted in this assertion notwithstanding Sloan's denial of all recollection of the incident. Mrs Lang, one of the regular sitters, then exclaimed, 'Yes, that is right, Mr Sloan. I remember you lost the negative and hunted all over for it. Who is it who knows all about photos? It must be someone who knows me very well.' So of course it was, as the incident had at one time been perfectly well known to Sloan's conscious mind, and the interruption is at best a dramatization of a lapsed memory.

It is stated that Sloan frequently spoke in foreign languages 'which sometimes could not be understood', but we are given very few examples of foreign speech, whether understood or not. Twice we are given two words of Gaelic, a language of which Sloan probably knew something as his mother came from the Highlands. There are four words of Latin, 'Ars longa, Vita brevis,' a sufficiently familiar tag, and a Danish communicator, together with some broken English, spoke two words which are

apparently claimed as Danish. Another communicator 'started to speak fluently in French', but we are not told whether any of the sitters took part in the conversation, and if so whether the communicator responded intelligently to their remarks, or whether any of the circle knew French well enough to judge if the communicator's language and pronunciation were correct. Sloan had at two periods of his life followed the sea and may very likely in that way have acquired a smattering of the languages of two countries with which we have as close commercial relations as France and Denmark. What a pity that the Circle did not use a recording machine so that the Danish and French could be played over to persons familiar with those languages, and the Red Indian, East Indian, and African Guides have been invited to make records of their own languages which could have been submitted to experts!

Two among the communicators gave the names of W. E. Gladstone and J. M. Barrie, who in this life were notable for intellectual gifts and powers of expressing themselves which they appear to have lost when shuffling off this mortal coil (see pp. 119 and 176). In justice to Sloan it may be said that some of Mrs Piper's Controls represented even less worthily the great names they claimed.

Generally speaking, while the records are satisfactorily full as to what was said at the sittings, the reader looks in vain for the wealth of annotation by the sitter which gives value to the records of Mrs Piper, Mrs Leonard, Miss Cummins, and other mediums of their type. The absence of details of this kind puts this book definitely on a lower level than the published records of these mediums.

A characteristic of the Sloan mediumship, which distinguishes him from these other mediums and appears to Mr Findlay to be of crucial importance, is the association of physical phenomena, such as the movement of trumpets, with the communications. The sittings were held in complete darkness and Sloan was not subjected to either manual or mechanical control. Mr Findlay complains that the scientists of Glasgow University paid little attention to Sloan's phenomena and did not employ infra-red photography. One may fairly ask why Mr Findlay and his friends did not employ this technique themselves, or if they felt they had not the expert knowledge, why they did not pay somebody to attend the sittings and do the necessary things on their behalf. But is there any reason to suppose that Sloan would have agreed to this? The infra-red telescope was not available for general use at the time these sittings were being held, but it is not irrelevant to note that since it has become available physical mediums are very

shy of it, although it is well known that the use of it makes unnecessary some of the other techniques of control which mediums profess to find irksome. In the points, therefore, in which Sloan's mediumship during the years 1942-5 can be compared with that of such mediums as have already been mentioned, it falls far short, while its distinctive features are, owing to the absence of control, of no evidential value.

On pp. 342-3 Mr Findlay makes an attack on the Society which calls for a brief comment. He says that in 1925 he suggested to Sir William Barrett to put before the Society a proposal to investigate Sloan in London, Mr Findlay paying the expenses. He continues, 'Sir William went to the next Council meeting and put forward my offer. Much to his disappointment the Council turned it down with neither an explanation nor an expression of thanks, and, when he told me of its decision not to investigate Sloan's mediumship, he was not only disappointed but angry. That ended the matter and nothing was ever done. My reaction was to resign my membership of the Society.' This is a charge against the Society of failure to perform its duty as an investigating body, and also of discourtesy to Mr Findlay. As regards the second and minor charge Barrett never laid the proposal formally before the Council, although he consulted some of the Officers. I remember very well hearing of the proposal, doubtless as being then Hon. Treasurer. What report Barrett gave Mr Findlay of his discussion with the Society's Officers I do not know, but if there was any failure of courtesy it was certainly not on the part of the Officers or Council.

The other charge is a more serious one. In bringing it Mr Findlay should in candour have mentioned a very material condition that he attached to his proposal, namely that the Society should engage Sloan as caretaker at Tavistock Square. While the Officers of the Society had no reason to regard Sloan as personally untrustworthy, they did not consider it good policy to allow a professional or semi-professional medium the free run at all hours of the day and night of premises where a large amount of highly confidential information was (and is) kept. Much of this information has been entrusted to the Society precisely because it could only come into the hands of officials specially charged with the duty of secrecy. Any suspicion that a medium has had access to material subsequently referred to in communications received through him or through any other medium goes far to discredit any evidential value that might otherwise attach to them. The best mediums realize that it is to their interest that no such cause for suspicion should occur. In this respect they show a better appreciation of evidential standards than Mr Findlay

who, when informed of the reason why his proposal had been declined, light-heartedly brushed it aside.

Incidentally it may be mentioned that this incident occurred in 1923, not 1925, when the Society had no vacancy for a caretaker, and that Mr Findlay's reaction to resign did not take effect until 1932.

W. H. S.

THE DEVIL IN MASSACHUSETTS : a modern enquiry into the Salem witch trials. By Marion L. Starkey. London, Hale, 1952. 269 pp. Illus. 18s.

This 'modern enquiry into the Salem witch trials' fully deserves the claims made for it by the publishers. It certainly 'combines a narrative that has the pace and excitement of a novel' with 'an authentic history of the Salem witch trials', and if the application of 'modern psychiatric knowledge to the witchcraft hysteria' is not very elaborate or profound, it is quite sufficient for the purpose in hand. The primary purpose of the book is to present a clear picture, as accurate as careful documentation can make it, of the intense but short-lived outbreak of witch-hunting which swept through Salem village and some of the surrounding districts in 1692. The phenomena, wholly unintelligible in that age, reveal upon a tiny scale, for only twenty victims in all were put to death, the astonishing effects of mass hysteria, such as held Europe for three centuries at a cost of hundreds of thousands of lives. But at Salem they took place in a community rigid alike in Calvinist theology, in Puritan morality, and in a real desire for justice. The very forces which produced the outbreak by driving a group of adolescent girls into the self-display of hysterical convulsions, in which they named the alleged witches who were supposed to be tormenting them, were the forces which speedily led to its control. The whole story is fully documented by court records as well as by the contemporary writings of Cotton Mather and others immediately concerned, and is here admirably told.

From the point of view of parapsychology the most striking fact is the obvious honesty of all concerned, except perhaps the slave Tituba who first awakened the hysterical symptoms in the children who formed the original 'young people's circle'. Once the fever of suspicion had spread every sort of story was believed, 'shapes' were reported as acting anywhere and everywhere, and the visions of hysterics completely outweighed the ordinary evidence of sober people. One of the strangest figures of all is the grim Chief Justice Stoughton, who seems to have been wholly sincere and wholly free from hysteria, devoted to the one purpose of exter-

minating witches by strict process of law, and yet quite unable to see the complete worthlessness of the evidence upon which his judgments were based. And the fundamental religious strength of a community has never been more fully demonstrated than by the public pardoning of Ann Putnam, no longer a hysteric, and her restoration to her place among them by the very relatives of those whom her testimony had sent to the gallows.

But whether the ghost of Mary Esty really appeared to Mary Herrick, and so won the day, the reader must judge.

L. W. GRENSTED

BORDERLANDS OF SCIENCE. By Alfred Still. London, Rider, 1952.  
276 pp. 20s.

This work is in the main a potted history of the wax of science and psychology and the wane of superstition, with the theme extended very naturally to include a brief survey of the various phenomena which come under the heading of psychical research. Unfortunately, one is left with the impression that most of the information could have been equally well obtained by studying jointly an encyclopaedia and a *Who Was Who*. But if Mr Still adds little of his own to the general discussion, he does at any rate point a very clear way to other sources of information.

He makes a bold, interesting, and praiseworthy effort to show the error of science in rejecting out of hand those problems at present beyond its normal specialized scope. But though repeatedly concerned over the fallibility of human judgement, the author would nonetheless seem to have made up his own mind on the strength of evidence that is left quite unexplained. Thus, when we are told (p. 102) that 'Many careless people are sceptical about a future existence . . .' we search unrewardingly for an expansion of view that Mr Still has been anything but careless to hide.

We cannot fail to appreciate his obvious sincerity, however; nor yet the unusual courage he displays in squirting Spiritualists, scientists, astrologers, believers and unbelievers from the same bottle of ink! Throughout this book the desire to be objective is certainly unmistakable.

In spite of the fact that an interesting chapter on Body, Mind, and Memory shows the author to be not entirely unfamiliar with the complex problems of subconscious activity, the practical importance of intuition is grossly overrated. However pure the basic intuition may be, it can rarely if ever emerge into reflective awareness without being modified by a very fallible subconscious. While, therefore, we can study the results of supposed intuition, the basic factor is itself a matter at present beyond the scope of

final objective pronouncement. In other words, intuition is largely a private matter, and is far too unreliable to serve as a self-sufficient aid to the establishment of any 'universal truth'.

Perhaps the author's failure to realize this explains his aversion from controlled experiments in ESP, and his complete neglect of Dr Soal! Thus, regarding quantitative experiments we read that 'Conclusions drawn from such data are not likely to impress the scientist, especially if he is also a mathematician. Yet several recent investigators have adopted similar methods in the belief that the analysis of the accumulated data provides better evidence of a telepathic faculty than the many plausible stories of spontaneous thought transmission' (p. 217). But overleaf, on the one page devoted to Dr Rhine, Mr Still has this to say about the ESP tests: 'These experiments also go a long way towards shattering the generally accepted doctrine that nothing can enter the human mind except by way of the recognized bodily senses.'

We are led to assume, therefore, that in the author's opinion, only where the experimental and statistical method produces relatively quick, favourable, and spectacular results is it admissible!

The range of subjects in this book is, of course, such that perhaps one could not reasonably expect a combined historical and critical approach in 276 pages. We are introduced to witchcraft, magic and superstition; to faith-healing, time, and the subconscious; to the divining rod, levitation, and poltergeists; to Copernicus, Galileo, and Newton. So, no doubt inevitably, we are left in the end to form our own acquaintanceships.

As I have said, this work is mainly a potted history. And judged as such it can be readily recommended to those who are just beginning to take an interest in the manifold problems of the 'paranormal'.

THOMAS GREENWELL

JOURNAL OF PARAPSYCHOLOGY. Vol. 16, No. 2, June 1952.

Durham, N.C., Duke University Press. \$1.50.

The central place in this number of the *Journal* is taken by an article by Professor Rhine on 'The Problem of Psi-missing'. The fact that some subjects score consistently below mean chance expectation while others drop below mean chance expectation at a late stage of experimenting is a challenging observation, both because of its practical inconvenience and its possible theoretical implications. Professor Rhine has made a scholarly study of the experimental evidence on the subject and focusses attention on the problems it raises.

Dr Gertrude Schmeidler has made a study of 'ESP Scores of Patients suffering from General Concussion'. She finds that eighteen concussion patients score better on ESP tests than the general average. She suggests that this may not be a direct result of the concussion, but of the relaxed and uncritical attitude which results from concussion. Every study that suggests how scores in ESP tests may be improved is to be welcomed, although a less drastic way of inducing a relaxed attitude than knocking our subjects on the head must still be used in our experimental work.

There are reviews of Ducasse's *Nature, Mind, and Death* by Professor H. H. Price and of Rawcliffe's *The Psychology of the Occult* by Dr D. J. West.

R. H. THOULESS

JOURNAL OF THE AMERICAN SOCIETY FOR PSYCHICAL RESEARCH.

Vol. 46, No. 3, July 1952. New York, A.S.P.R. \$1.50.

Communications on psychical research from South Africa are a rarity and so welcome. May Bell of Rhodes University, Grahams-town, in 'The Normal Function and Manifestation of Psi', postulates a trisection of the self into 'Con', the conscious self, 'Uncon', the Freudian Unconscious, and Psi (Thouless-Wiesner). By definition Uncon and Psi are mutually exclusive.

Some of the arguments adduced by the author to support her postulated framework seem to me to beg the question. Thus, of dreams: 'Uncon's repressed desires are, apparently, the motive of the dream, but what tidies it up into a nice unshocking story that Con will cheerfully accept? Hardly Con, whose feelings are being saved, scarcely Uncon, which we are led to think of as wholly concerned with getting its desires across to Con. There seems to be a censor, with some dramatic ability; if this is neither Con nor Uncon, unless we are to invent further entities, must it not be psi?'

The interesting paper on precognition by C. T. K. Chari which appeared in the *Journal* of the S.P.R. for November-December 1951, is reprinted in slightly expanded form, and Mrs Allison contributes a memoir of J. G. Piddington.

An exchange of letters between Dr Rhine and two Yale University psychologists is reprinted. Sheffield and Kaufman, the Yale men, were reported in the *New York Times* (30 March 1952) as having carried out a PK experiment by a Duke technique using both human and photographic recording. According to Sheffield and Kaufman, the camera showed that believers in PK tended

to make recording errors which inflated the scores while disbelievers understated the scores. Rhine takes them to task, claiming that their criticisms are irresponsible, inviting them to an open forum to discuss the matter, and offering to publish their paper in the *Journal of Parapsychology*. Sheffield and Kaufman decline the invitation to a public debate, but promise their paper for publication.

D. P.

## CORRESPONDENCE

### THE INTERPRETATION OF ESP EXPERIMENTS

SIR,—I wish to draw attention to a point regarding the logical interpretation of ESP experiments, which appears to be generally overlooked. The procedure used when we infer a causal relation between two phenomena from a statistical experiment is as follows. A set of experimental objects (say, plots sown with oats) is divided at random into two or more subsets, each member of one subset being treated in one way (say, with a certain fertiliser) and each member of another subset in another way (say, with no fertiliser). Then we measure every member of both groups in respect of some chosen character (say, yield) and, treating the two sets of measures as statistical 'populations', we can calculate the probability on the given data that the differences between them are due merely to the random segregation of the original set of objects into two groups. If this probability is small enough, we feel confident that the differences are not due to this cause *but to the only other difference we have made*, that is, to the different treatments. Provided the randomization has been properly carried out (and the process involves various technical complications varying from one experiment to another) we can attach a definite probability to the validity of our conclusions.

Parapsychological experiments do not in general conform exactly to this type. Their material is not divided into two groups, of which one serves as a control; the place of the control is taken by an imaginary set of results obtained by purely random selection, and this selection is not really carried out but represented in the calculations by the known statistical parameters of such series. We have in effect two series of guesses, one made by a conscious subject supposedly trying to obtain correct hits, the other by an imaginary machine working with mathematical perfection as a



randomizer. The positive results which have been obtained prove that these two methods of guessing produce different results, and in particular that a subject consciously attending to his guesses will very often not be guessing at random even if he have no sensory clue to guide him. That is obviously an interesting and important conclusion. But it involves no more than an association of phenomena, and gives no hint as to the causal relations. For this, a proper control is needed.

The mathematical control in these experiments lacks at least three factors present in the 'treated' runs: (a) the experimental situation, (b) the act of conscious guessing, and (c) any special mental faculty which may be involved. It is illogical to dismiss (a) and (b) lightly; the anecdotal evidence rather suggests that the experimental situation may influence the results, and it needs proof (however plausible it may appear) that a conscious subject *can* make a series of guesses statistically uncorrelated with the 'targets'. A more radical possibility which really ought to be ruled out by formal experiment is that the non-random runs are primary and produced by chance, but that they in some manner attract ESP experimenters; put thus, it sounds rather fantastic, but it is really no more contrary to our accustomed habits of thought than is precognition, widely accepted as an alternative but narrower hypothesis.

I am unable to suggest any means of deciding these questions experimentally in the case of psychognosis; but the case is better in psychokinesis, for here one can make a genuine control in which the attention of the subject is the only missing factor. If one makes a hundred runs of dice-throwing provided with random targets, and if ten of these are selected beforehand at random and a psychokinetic subject called in to influence these only, one could tell from the results if the efforts of the subject were an objective 'cause' of departures from randomness, for if it were not so the results of the 'treated' runs would not differ significantly from those of the 'controls'.

It is already proven that phenomena occur which do not fit in with our customary habits of scientific thought. That being so, we must give up those habits, to the extent of abandoning common sense in the framing of our hypotheses. Until we have reformulated guiding principles of thought, nothing is too absurd to be worthy of formal disproof; for we may be sure that there are still many absurdities which will turn out to be true.

A. F. PARKER-RHODES

Magdalene College, Cambridge

QUALITATIVE MATERIAL AND THEORIES OF PSI  
PHENOMENA

SIR,—In the May–June 1952 issue of the *Journal* of the S.P.R., W. H. Salter has a note under the above title. In it he refers to my last article in the A.S.P.R. *Journal*, January 1952, entitled ‘The Psychodynamics of Spontaneous Psi Experiences’.

Mr Salter very wisely warns against what he calls ‘a source of possible misconception’. He is fearful that my emphasis upon *experiencers* rather than *experiences* may lead the reader to draw the conclusion that the *experiences* need not be investigated. This would be equally unscientific, in my opinion, as the current trend to emphasize the *experiences* to the exclusion of the *experiencers*.

A scientific approach to a problem demands that all known parameters be investigated as thoroughly as possible. What happens, unfortunately, is that the availability of materials and hypotheses, or the personal interest or prejudice of the investigator, permits one or another aspect to be emphasized or excluded. The trend in psychical research has been steadily away from human beings. It was against this tendency to ignore the human personality and to see only the ‘phenomena’ (and quantitative data) that I wished to take a strong stand. I never suggested or intended that research work along other lines be stopped. We are grateful, therefore, for Mr Salter’s expression of concern.

I am, however, not in agreement with the very tendency against which I warned even when it appears subtly in Mr Salter’s statement. The preference for the *experience* rather than the *experiencer* is insinuated by Mr Salter in his value judgment that the psychological analysis must be held up until ‘the laborious first step be taken of sorting out the material under examination into veridical and non-veridical’. Efforts at such a sorting process are time-consuming and, at present, result in findings that are of dubious reliability, at best. Moreover, the supply of research funds and personnel is very limited. Mr Salter’s recommendation, therefore, would in effect perpetuate the barren direction of contemporary psychical research.

EMANUEL K. SCHWARTZ

New York.

## CONDITIONS FAVOURABLE TO ESP

SIR,—It is, perhaps, worth reminding ourselves from time to time that the limitations so far imposed by laboratory technique on research into the process of ESP are not unlike those suffered by observers of wild animals confined in a cage, and also that

spontaneous cases—even though some are chance coincidences or spurious—may give us clues to Whipsnade conditions for experimental percipients.

The following incidents, for example, three of which occurred during the last few weeks, demonstrate conditions which have synchronized with apparent ESP in my own family for many years, and which are well known elsewhere. The difficulty of incorporating them into planned experiments does not alter the fact that they seem to be soil in which ESP appears to flourish.

In the first four cases there was sudden emotion on the part of the apparent agent. They might almost be called trivial crisis cases. Last month I went, very exhausted, to catch a bus to our country cottage. It was, for the first time, full up, and only after much waiting and three changes did I arrive four miles, instead of half a mile, from home. I felt far from well and telephoned anxiously to my husband to fetch me, but, to my great disappointment, he was playing golf. Fortunately a stranger gave me a lift to the turning where I usually got out of the bus, and, as we arrived, my husband drove up. 'What an extraordinary coincidence,' I said, for there was no bus due, I had in any case not said when I meant to come down, and I had never before wanted him to meet me when going to the cottage, and he had never done so. 'Not at all,' he replied. 'As we finished our game, I felt you wanted me, so I refused the drink offered by my opponent, saying that if I hurried I should just catch you, and I came at once. I knew you would be here.'

Last week, again, my husband went away to golf, inadvertently taking the key of our son's car in his pocket. The latter was greatly inconvenienced, and I, rather distressed, suggested telephoning the Club. He did so at once, and the caddie-master replied, 'I can see the Colonel coming in now.' My husband, on being called, said that he had come in on purpose, feeling that I wanted him on the telephone. (He does not do this on occasions when I do not want him.)

A wave of feeling, however trivial, seems to do the trick in both directions between my husband and myself. On one occasion I felt quite suddenly that he needed me, when I was on my way home, having left him at an office for an appointment. I turned the car, rather inconveniently in a narrow country road, and went back, to find him leaving the building, having heard that his appointment had fallen through and much annoyed at being without transport home. On another occasion, during the war, I was sitting quietly at tea with my mother in the country, when I felt suddenly convinced that my husband had no torch for his walk home from the station in the blackout, on a very dark, unpleasant

winter's night. So, in spite of my mother's indignant protests that he had never before forgotten his torch, and why should I imagine he had done so now, I walked with a torch to the station. 'Thank goodness you've come,' said my husband. 'My torch went wrong as I left the Ministry.' Chance coincidence apart, this seems more likely to be a case of response to my husband's annoyance at the prospect of an unpleasant walk in the dark than of precognition, since a moment later a friend offered us a lift home and the torch was not needed.

The following case reminds us of the value, both of a really carefree attitude and of an encouraging atmosphere. An experienced member of the SPR handed me an envelope, saying casually, 'Can you tell anything from handwriting?' I said, no, but he insisted on my trying, *as a joke*. 'Is it a man or a woman?' he asked. The writing looked very feminine, but I had a sudden conviction that it belonged to a man, an extreme homosexual, and I gave a fairly detailed description of a type I thought most unlikely to be corresponding with my questioner.<sup>1</sup> He laughed, but said no more than, 'Go on. You haven't put a foot wrong yet.' Extremely pleased with myself, I made a few more statements, and I seemed to hear an interior voice make a special point that I should mention that the man liked budgerigars. I was then told that all I had said had been correct and that the man was very fond of exotic birds and constantly painted them. Next day my questioner telephoned to say that he had had another letter from the man I had described, saying that he had just bought a pair of budgerigars. I feel pretty sure that I should not have got the second wave of facts had I been met with detached silence instead of amused encouragement after the first.

The connexion of an entirely casual attitude with success is, of course, well known. I remember once asking Mr G. N. M. Tyrrell, who was convinced he was a hopeless percipient, to take part in an experiment which I was carrying out, with drawings at a distance, for the late Mr Whately Carington. He consented, as a joke, and, to his astonishment, got seven drawings right out of twenty all different. But his later efforts never deviated from chance.

To save correspondence, may I add that I do not quote the above cases as *evidence* of ESP. I naturally do not consider them fraudulent, though others may, and they may of course all be chance coincidences. My point is that, *if genuine*, they are examples, among many, of cases containing features in common

<sup>1</sup> I have seen the envelope said to have been used in this experiment. For what my impression is worth, I should have said the writing was that of an elderly woman.—ED.

which might with advantage be remembered when planning experiments.

ROSALIND HEYWOOD

London, S.W. 1.

#### RESISTANCE IN ESP EXPERIMENTS

SIR,—One of the psychological factors in ESP experimental work which does not appear to have received much, if any, attention is what is termed by psychoanalysts Resistance. This occurs when unconscious material attempts to break through into consciousness, and manifests itself in various ways. The patient being analysed may remain silent, since he is completely unable to think of anything to say; he may be evasive, untruthful, forgetful, may make mistakes and slips of the tongue; he may take a dislike to the doctor and even in extreme cases break off treatment.

These and other similar mental mechanisms betray to the trained analyst the anxiety—in ordinary parlance fear—itsself largely unconscious, of the irruption into consciousness of emotionally charged material painful to the patient. Similar mechanisms may be detected in normal people approaching the subject of ESP, since there is a general fear of the 'Uncanny', that is, anything which does not appear to obey the ordinary rules of everyday, commonsense life, including such material emanating from deep levels of the personality as cannot be easily assimilated in consciousness. As a lady said to me not long ago when relating a recurrent dream of a house which she eventually recognized and entered in Central Africa, much to her surprise, 'Talking about things like this makes me feel cold down my spine.' In my opinion, such frank confession of shivers actually reveals less fear than the hostility to psychical research displayed by many matter-of-fact people. This fear of the Uncanny, which is biological, has been fully dealt with by Mr G. N. M. Tyrrell in his writings. At the same time it must not be overlooked that there is attraction to as well as repulsion from the Uncanny, and it would be an interesting point to reflect on; what mingling of these factors results in serious, dispassionate psychical research?

My attention was first drawn to this subject of 'resistance' some time ago, when I noticed how frequently reports to the S.P.R. of spontaneous phenomena were delayed, and how often letters in reply to questions asking for further details and confirmation began with apologies for not writing sooner. I began to think that this was more than a coincidence. Later, when I became a member of the S.P.R., I observed in myself an irrational reluct-

ance to report a certain dream which appeared to be both telepathic and precognitive, the circumstances of which were evidential. I rationalized this reluctance by telling myself that the S.P.R. had plenty of material more interesting than mine. It was only some time later that I was able to overcome this inhibition or 'resistance' and report the dream. Freud says: 'It is by no means impossible for the products of unconscious activity to pierce into consciousness, but a certain amount of exertion is needed for this task. When we try to do it in ourselves we become aware of a distinct feeling of repulsion which must be overcome, and when we produce it in a patient we get the most unquestionable signs of resistance to it' (*Collected Papers*, 1925, pp. 26-7).

I noticed when I began experiments in card guessing that I felt an unusual and baffling sense of confusion at first, and a tendency to forget the instructions, simple as they were. This passed off as soon as I became aware that I was resisting. I observed the same air of slight confusion and tendency to make mistakes in my friends taking part in the experiments, although they were all persons of high intelligence and some intellectual attainment. A favourite gambit was to seize a pack of Zener cards as soon as a run of guesses had been made and shuffle it so that the target could not be checked. There was also a tendency to lose interest in the experiments if at the first session no superficially interesting scores could be seen at a glance, and to have difficulty in finding time for further sessions. It may be argued that on my hypothesis the less successful subjects should show less resistance; but it is after all the conscious ego who consents to take part in the experiments, and a little encouragement keeps him at the task, while lack of success discourages him. Moreover, I believe that everyone has psi faculties, and it is probably the strength of his fear which prevents him from scoring significantly; he is like the patient who, finding that nothing spectacular happens at the beginning of his analysis, gives up his treatment, saying, 'There is nothing in psychoanalysis', and is all the time secretly glad of the excuse not to meddle any more with it. The real resistance of the more successful subject sets in later, when he ceases to score significantly and gives chance or negative results. It is as though the psi part of the personality announces that, having given evidence of his existence, he declines to perform any more. Probably the same mechanism works in mediums under investigation, since psi does not appear to wish to give away any secrets.

The analyst has a technique for helping to overcome resistance, which may be summed up in one word, reassurance. This is not so easy as it sounds, however. It depends upon the transference

—the formation of an affective bond between doctor and patient—a situation which needs skilled handling. Psychological research has no comparable technique yet, although a pleasant and friendly atmosphere seems to be so encouraging in helping the emergence of psi faculties in experiments that in my opinion it is essential.

It may be that psychological investigation of 'sensitives', such as mediums, automatic writers, and others subject to marked spontaneous experiences, will throw light on the problem of resistance, and suggest why it should be weaker in some subjects than in others. If, as one suspects, the appearance of psi faculties is connected with a tendency to dissociation, one would hesitate to apply regularly a technique for splitting a normally well-integrated personality for experimental purposes. The work already done with drugs and hypnosis is interesting, but has obvious dangers. As we are all, no doubt, only too well aware, the task of the investigator into ESP problems is not an easy one.

JESSIE BLUNDUN

Kingskerswell, Devon.

#### PSI FUNCTION AND PSYCHODYNAMICS

SIR,—An attempt was made in 1947 to carry forward the study of psychodynamics, recognising 'psi' as an active entity. The nucleus of a research unit was formed. Methods of working were based on techniques and observations made during research in psychotherapeutic fields. Basically this consisted of group and individual work, symbiotically conducted, the dream being used throughout as a projection screen to observe psychic processes. This has applied to work concerned with either spontaneous or statistically assessable phenomena. To date, upwards of 1,200 clinical and research group meetings have been held and over 50,000 dreams carefully examined and many recorded.

Long-distance work has now become a very active branch of the unit. The object of this work is still, primarily, the study of psychodynamics and the basic techniques are the same. Some of the experiments are with Zener cards, and some are of a more spontaneous nature.

Home or overseas members of the Society who would be interested in such work and its approach, and who would care to take part in one or other aspects of the work, are asked if they will kindly get into touch with me.

ALICE E. BUCK, M.D.(PSYCH. MED.), CH.B.

46 Queen Anne Street,  
Wimpole Street, London, W. 1

## G. N. M. TYRRELL

As this issue of the *Journal* goes to press, we learn with deep regret of the death, at Reigate on 29 October, of Mr G. N. M. Tyrrell. Mr Tyrrell became a member of the Society in 1908, and was elected to the Council in 1940. He was President for the years 1945-6. He was the author of *Grades of Significance* (1930), *Science and Psychological Phenomena* (1938), *Apparitions* (the Frederic W. H. Myers Memorial Lecture for 1942), *The Personality of Man* (1946), and *Homo Faber* (1951). An obituary notice will be published later.

## INDEX TO VOLUME XXXVI

VOLUME XXXVI of the *Journal*, which is completed with the present number, consists of Nos. 662 to 672 (January-February 1951 to November-December 1952). The Index will be circulated with the issue for January-February 1953.

The cost of the binding case for Volume XXXVI will be announced in due course. In the meantime, readers who require any numbers to complete their sets are advised to order them from the Secretary of the Society, 31 Tavistock Square, London, S.W. 1.



# Society for Psychical Research

31 Tavistock Square · London · WC1

SUPPLEMENT

TO

JOURNAL

Volume XXXVI No. 672 NOVEMBER-DECEMBER 1952

FOR MEMBERS AND ASSOCIATES

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## RETURN OF LIBRARY BOOKS

A NUMBER of Library books are several months overdue for return. Members can relieve the Secretary of clerical work if they will kindly return them in response to this notice.

## TESTS FOR EXTRASENSORY PERCEPTION

### AN INTRODUCTORY GUIDE

THIS is the title of a pamphlet prepared for the guidance of all who wish to embark on ESP experiments. Written by Dr D. J. West, so far as possible in non-technical language, it consists of the following sections: The Use of Standard ESP Cards; Telepathy Tests; Preliminary Assessment of ESP Card Results; Other Varieties of Card Tests—'Clock' Cards; the Use of 'Free Material' as Targets; Conditions Favouring Success; Telepathy, Clairvoyance, and Precognition; Secondary Effects; a Note on Psycho-kinesis. There are four statistical appendixes, and a glossary of terms used in ESP research.

The pamphlet will be ready early in the New Year. One copy will be sent free to any member who applies for it.

## INDEXES TO 'PROCEEDINGS' AND 'JOURNAL'

PART 181 completes Volume 49 of the *Proceedings*, which will consist of Parts 177 to 181. The Index will be circulated with Part 182.

Volume 36 of the *Journal*, which is completed with the current number, consists of Nos. 662 to 672 (January-February 1951 to November-December 1952). The Index will be circulated with the issue for January-February 1953.

Details of the cost of binding cases for these two Volumes will be given in the Supplement to the *Journal* for March-April 1953. In the meantime, members who require any back numbers to complete their

sets are advised to order them from the Secretary. They are available to members at half of the price to the public.

### HONORARY OFFICERS : AN OMISSION

OWING to an oversight, the name of Dr D. J. West, Hon. Investigation Officer (ESP), was not included in the list of Honorary Officers of the Society printed on page ii of the Supplement to the *Journal* for July-October 1952. The Editor wishes to express his apologies for this omission.

### ADDITIONS TO THE LIBRARY

THE following list consists largely of works which, while not directly concerned with psychical research, have an important application for the investigator and experimenter. It does not include books which have been reviewed in the *Journal*.

- ALLPORT, GORDON W. *Personality: a psychological interpretation*. London, Constable, 1949. 588 pp.
- BROWN, WILLIAM, and THOMSON, GODFREY H. *The Essentials of Mental Measurement*. 4th ed. Cambridge University Press, 1940. ix, 256 pp.
- Chambers's Mathematical Tables*. Consisting of logarithms of numbers 1 to 108000, trigonometrical, nautical and other tables, edited by James Pryde. New ed. London, Chambers, n.d. xlii, 454 pp.
- <sup>1</sup> D'AGAPEYEFF, ALEXANDER. *Codes and Ciphers*. London, Oxford University Press, 1949. 149 pp.
- <sup>2</sup> DAVID-NEEL, ALEXANDRA. *Mystiques et Magiciens du Thibet*. Paris, Plon, 1929. vi, 306 pp. Illus.
- DE RIENCOURT, AMAURY. *Lost World: Tibet, Key to Asia*. London, Gollancz, 1950. 317 pp.
- DUNBAR, FLANDERS. *Emotions and Bodily Changes : a survey of literature on psychosomatic interrelationships, 1910-1945*. 3rd ed. New York, Columbia University Press, 1946. lix, 604 pp.
- EISLER, ROBERT. *Man into Wolf: an anthropological interpretation of sadism, masochism and lycanthropy*. London, Routledge & Kegan Paul, 1951. 286 pp.
- EZEKIEL, MORDECAI. *Methods of Correlation Analysis*. 2nd ed. New York, Wiley, 1941. xix, 531 pp.
- FISHER, RONALD A. *The Design of Experiments*. 5th ed. Edinburgh, Oliver & Boyd, 1949. ix, 242 pp.
- FISHER, RONALD A., and YATES, F. *Statistical Tables for Biological, Agricultural, and Medical Research*. 3rd ed., revised and enlarged. Edinburgh, Oliver & Boyd, 1948. viii, 112 pp.
- FITZKEE, D. *Magic by Misdirection*. San Francisco, Lakeside Press, 1945. 227 pp.

<sup>1</sup> Presented by Mr T. E. Wood.

<sup>2</sup> Presented by Baron Dayet.

- FREEMAN, G. L. *The Energetics of Human Behavior*. New York, Cornell University Press, 1948. vi, 344 pp.
- FREUD, SIGMUND. *On Dreams*. An entirely new translation by James Strachey. London, Hogarth Press, 1952. viii, 80 pp.
- GUTHEIL, EMIL A. *The Language of the Dream*. New York, Macmillan, 1939. 286 pp.
- HORNEY, KAREN. *New Ways in Psychoanalysis*. New York, Norton, 1939. 313 pp.
- HUGHES, PENNETHORNE. *Witchcraft*. London, Longmans, Green, 1952. viii, 220 pp. Illus.
- HULL, B. *Thirty-three Rope Ties and Chain Releases*. New York, Burling Hull, 1947. 48 pp. Illus.
- HULL, CLARK L. *Hypnosis and Suggestibility*. New York, Appleton-Century-Crofts, 1933. 416 pp. Illus.
- HUYSMANS, J. K. *Là-bas*. (Eng. trans.) London, Fortune Press, n.d. 216 pp.
- KENDALL, M. G., and SMITH, B. BABINGTON. *Tables of Random Sampling Numbers*. Cambridge University Press, 1939. 60 pp.
- KEYNES, JOHN MAYNARD. *A Treatise on Probability*. London, Macmillan, 1921. xi, 466 pp.
- KRUSE, JOHANN. *Hexen unter uns? Magie und Zauberglauben in unserer Zeit*. Hamburg, Verlag Hamburgische Bücherei, 1951. 210 pp.
- LASLETT, PETER (ed.). *The Physical Basis of Mind*. Oxford, Blackwell, 1950. viii, 79 pp.
- LÉVI, ELIPHAS (ALPHONSE LOUIS CONSTANT). *The History of Magic*. Trans. Arthur Edward Waite. 4th ed. London, Rider, 1948. 384 pp. Illus.
- LEVY, HYMAN, and PREIDEL, E. E. *Elementary Statistics*. London, Nelson, 1944. vii, 184 pp.
- LEVY, H., and ROTH, L. *Elements of Probability*. Oxford, Clarendon Press, 1936. Reprinted from corrected sheets of the 1st edition, 1947. x, 200 pp.
- MESMER, DR. *Mesmerism*. London, Macdonald, 1948. 63 pp. (The first English translation of Mesmer's *Mémoire sur la découverte du Magnétisme animal*.)
- MULDOON, SYLVAN J., and CARRINGTON, H. *The Projection of the Astral Body*. 4th ed. London, Rider, 1950. 242 pp.
- MURPHY, GARDNER. *Personality, a biosocial approach to origins and structures*. New York, Harper, 1947. xii, 999 pp.
- MURPHY, GARDNER. *Historical Introduction to Modern Psychology*. Rev. ed. London, Routledge, 1949. 466 pp.
- <sup>1</sup> OSBORN, ARTHUR W. *The Superphysical: a review of the evidence for continued existence, reincarnation, and mystical states of consciousness*. London, Nicholson & Watson, 1937. 350 pp.

- OUSPENSKY, P. D. *The Strange Life of Ivan Osokin*. London, Faber, 1948. 204 pp.
- <sup>1</sup> ROHLFS, DORA. *Irrationales und Rationales Erkennen: Mediale Erlebnisse in philosophisch-physikalischer Sicht*. Munich, published by the author, 1950. 108 pp.
- ROSENZWEIG, SAUL, and KOGAN, KATE L. *Psychodiagnosis: an introduction to tests in the clinical practice of psychodynamics*. New York, Grune & Stratton, 1949. xii, 380 pp. Illus.
- RUNES, DAGOBERT D. (ed.). *The Dictionary of Philosophy*. New York, Philosophical Library, n.d. 343 pp.
- RYLE, GILBERT. *The Concept of Mind*. London, Hutchinson's University Library, 1949. 334 pp.
- SACKVILLE-WEST, V. *The Eagle and the Dove, a study in contrasts: St Teresa of Avila, St Thérèse of Lisieux*. London, Joseph, 1943. 182 pp. Illus.
- SALTER, ANDREW. *What is Hypnosis. Studies in Auto and Hetero Conditioning*. London, Athenaeum Press, 1950. 95 pp.
- Satan*. London, Sheed & Ward, 1951. xxv, 506 pp. Illus.
- SCHILDER, PAUL. *The Image and Appearance of the Human Body: studies in the constructive energies of the psyche*. London, Kegan Paul, 1935. 353 pp.
- SHELDON, W. H., with the collaboration of STEVENS, S. S. *The Varieties of Temperament: a psychology of constitutional differences*. New York, Harper, 1942. x, 520 pp.
- <sup>2</sup> TENHAEFF, W. H. C. *Oorlogsvoorspellingen*. The Hague, H. P. Leopolds Uitgeversmaatschappij, 1948. 271 pp.
- THOMPSON, ERNEST. *The History of Modern Spiritualism*. Manchester, Two Worlds Publishing Co., 1948. 172 pp.
- TIPPETT, L. H. C. *Statistics*. London, Oxford University Press, 1943. 184 pp.
- TURNER, JAMES. *My Life with Borley Rectory*. London, Bodley Head, 1950. 272 pp.
- <sup>2</sup> WINTERSTEIN, ALFRED. *Telepathie und Hellsehen im Lichte der modernen Forschung und wissenschaftlichen Kritik*. 2nd rev. ed. Vienna, Phönix Verlag, 1948. 256 pp.
- WOLBERG, LEWIS R. *Hypnoanalysis*. New York, Grune & Stratton, 1945. xviii, 342 pp.
- WOODWORTH, ROBERT S. *Experimental Psychology*. London, Methuen, 1950. xi, 889 pp.
- WOODWORTH, ROBERT S., and MARQUIS, DONALD G. *Psychology*. 20th ed. London, Methuen, 1949. xiii, 674 pp.
- YOGANANDA, PARAMHANS. *Autobiography of a Yogi*. London, Rider [1950]. xii, 403 pp. Illus.

<sup>1</sup> Presented by Dr G. Walther.

<sup>2</sup> Presented by the Author.

## MEETINGS OF THE COUNCIL

Meetings of the Council were held as follows :

477th 11 June 1952 Chairman : Admiral the Hon. A. C. Strutt.

478th 19 Sept. 1952 Chairman : Dr S. G. Soal.

At the meeting of the Council held on 11 June 1952 Mr K. E. Shelley, Q.C., was unanimously co-opted a member of Council for the current year.

## MEETINGS OF THE SOCIETY

### 1. *Private Meetings at 31 Tavistock Square*

251st Thursday, 11 September 1952, at 6.30 p.m. G. W. Fisk and A. M. J. Mitchell : The new 'Clock Face' Method for ESP and PK.

252nd Thursday, 25 September 1952, at 6.30 p.m. Discussion Meeting. How to investigate a Case.

253rd Thursday, 16 October 1952, at 6.30 p.m. Royston H. Low : Modern Psychiatry and the Psi Function.

### 2. *Public Lecture at Caxton Hall*

Wednesday, 16 July 1952, at 8 p.m. Professor H. H. Price : Survival and the Idea of 'Another World'.

## NEW MEMBERS

### MEMBERS

(*Elected 11 June 1952*)

BREDIN, MRS H.M., Barnoon, Parkgate, Wirral, Cheshire.

CARLETON-JONES, MRS. F. V. M., P.O. Box 236, George, C.P., South Africa.

CORNELL, A. D., B.A., Brookside, Histon, Cambs.

DAVIES, MISS E., B.A., 52 Carpenter Road, Edgbaston, Birmingham 15.

HALL, TREVOR H., The Balk, Walton, nr Wakefield, Yorks.

HULL, MRS N. K., 55 Milson Road, Cremorne, Sydney, New South Wales, Australia.

LATHAM, L. J., 49 Scarsdale Villas, Kensington, London, W. 8.

NEAVE, MRS A. M. S., 11 Carlyle Square, Chelsea, London, S. W. 3.

OLDHAM, O. W., 52 Monmouth Road, Bristol 7.

### *Student-Associate*

Fairbrother-Jacobs, K. J., 38 Graham Street, Pascoe Vale, Melbourne W. 7, Australia.

## MEMBERS

*(Elected 19 September 1952)*

- CLAYTON, MRS H., c/o Mrs Whaley, Fairhaven, Nailsea, Bristol.  
 COLEING, H., 184 Johnson Street, Maffra, Victoria, Australia.  
 DUVEEN, MRS P. S., 6 Strathearn Place, London, W. 2.  
 FINDLAY, S. H., M.A. (Oxon.), 45 Kynaston Road, Orpington, Kent.  
 GRANT, L. C., M.Com., 16 Dexter Avenue, Mt Eden, Auckland S. 2,  
 New Zealand.  
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 Hants.  
 GRIMSON, MISS M., 119 King Street, Cambridge.  
 JENNINGS, MISS M. M., 5 Campden Hill Gardens, London, W. 8.  
 KINGSTON, E. M., 6 Chichester Close, Chichester Place, Brighton.  
 MARDALL, B., 42 Naunton Way, Cheltenham, Glos.  
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 N. 3.  
 RICHARDSON, MRS E. J. L., 14 Sheridan Road, Merton, London, S.W.  
 19.  
 RIDGE, C. H., Parc Clies, Gulyal, Penzance, Cornwall.  
 ROBERTS, W. A., Grittleton House, nr Chippenham, Wilts.  
 SAHIB, A. G. RAO, B.A., 13 Hall's Road, Madras 8, India.  
 SCOTT, K. F., Newland, Kirkby Road, Ripon, Yorks.  
 SPICKETT, MISS D. C., 41 Lowndes Street, London, S.W. 1.  
 WHALEY, MRS P., Fairhaven, Nailsea, Bristol.

*Student-Associates*

- de Ward, T. C. M., Lynton House, East Street, Blandford, Dorset.  
 Lowe, G. B., Corpus Christi College, Oxford.

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# INDEX TO VOLUME XXXVI

1951-2

For the sake of brevity such qualifications as 'supposed', 'alleged', etc., are omitted from this index. It must, however, be understood that this omission is made solely for brevity, and does not imply any assertion that the subject-matter of any entry is in fact real or genuine.

Roman numerals and dates in brackets refer to the Supplements to the *Journal* which are issued to Members only. For example 'iii (May 51)' means page iii of the Supplement to the May-June issue.

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