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PROCEEDINGS OF THE SOCIETY FOR PSYCHICAL RESEARCH

PART 177

PSYCHICAL RESEARCH AND PERSONALITY

By Professor Gardner Murphy

Presidential Address delivered at a General Meeting of the Society on June 8th 1949.

I CANNOT tell you how deeply grateful I am for the privilege of being here, and of this more intimate association with the work of the Society for Psychical Research, which has always meant so much to me. I should like to use the opportunity which this occasion offers me to share with you some thoughts about the relation between the study of personality and the inquiries with which we in psychical research are concerned, in the hope that each may illuminate the other.

The term " personality " is used in two senses. In Mr Tyrrell's stimulating volume, *The Personality of Man*,[1] our chief concern is with personality considered generically; that is, with those attributes which belong to personality as such, and not simply to certain individual persons here and there. On the other hand, the term personality is also used to mean individuality: to denote not the property of being a person as such, but the distinctive properties by which one person is differentiated from another. In the feeling that both uses of the term are warranted, we shall try to relate psychical research to personality in general, and also to individuality, as expressed in specific paranormal gifts which belong to some individuals and not to others.

I

We must still begin, I believe, with Frederic Myers's[2] conception that every personality is an integration of which only a limited portion appears at the conscious level. Personality is a system of energies which may throw up to its surface certain visible forms—specific cravings, or images or thoughts—but which is not in essence contained by the boundaries of explicit consciousness.

It is doubtful, of course, whether Myers's original conception of a rather sharp line of demarcation between supraliminal and subliminal can today be maintained. It has appeared more and more that personality is a matter of shadings or gradations, not only with respect to consciousness or degree of organization, but with respect to almost every aspect of its being. From

[1] Pelican Books, 1947.
[2] *Human Personality and its Survival of Bodily Death*, 1903.

this point of view we should have to say that supraliminal and subliminal processes appear to be essentially alike in most respects. There is, nevertheless, one basic sense in which Myers's conception has been vindicated by recent research, namely through the evidence that the conscious portion of our make-up may forcibly inhibit the operation of subliminal activities, including the operation of those paranormal powers with which psychical research is concerned. Of course it is not necessarily true that the subliminal has powers which the conscious can never realize. Yet the fact remains that for most people living in a civilization like ours, conscious intelligence is pretty well saturated with fears or resistances relating to the paranormal in general or to the paranormal in specific forms, and that this resistance may operate to make the subliminal less effective in the realization of its paranormal powers than it can become when such conscious control is removed. It would appear that the facilitating effect which dreams, hypnosis, sensory and motor automatisms, as well as states of "trance, possession, and ecstasy", seem to have in liberating the paranormal, may lie largely in the freedom from inhibiting conscious factors. Though the lower degree of effectiveness of the supraliminal in paranormal processes may lie in its preoccupation with the immediate physical environment, rather than through any intrinsic incompatibility between consciousness as such and the paranormal as such, the modern view would be similar to that of Myers in regarding the subliminal of all human beings as endowed with paranormal powers with which one ordinarily has scant commerce at the conscious level.

But we need a sharper clarification of the way in which subliminal processes are set free. A convenient example is the Groningen experiment in telepathy.[1] A young student of dentistry, blindfolded, in a black cage in a lower room of the university psychology laboratory, received telepathic impressions from experimenters in a darkened room just above him, tapping out with his finger the specific points on a board which had been chosen by lot by the experimenters above. The feature of this experiment that I would stress is that the man had fallen into a semi-trance condition, a dissociated or abstracted state, and that this state of withdrawal from active preoccupation with the outer world seemed to afford the basis for his telepathic powers. It was, so to speak, the dissociability of this man's personality, the openness of his subliminal to impressions from the experimenters, that made him so good a subject.

One is tempted here to use a hypothesis which has passed already through many schematizations and which I will offer in a form suggested by Warcollier.[2] In this view we are concerned with subliminal operations not only on the part of the percipient, but also on the part of the agent. The hypothesis is that the agent's conscious desire to transmit impressions activates a subliminal operation within him which causes a subliminal response in the percipient, and is then able to relay the content of the message to the conscious level of the percipient's mind. In point of fact, Mrs Sidgwick's suggestion in 1923 about the reciprocity, the two-way

[1] "Some Experiments in Telepathy Performed in the Psychological Institute of the University of Groningen," *Compte-Rendu du Premier Congrès International des Recherches Psychiques*, 1922, pp. 396–408.

[2] *Experimental Telepathy*, 1938.

action involved in telepathy, is compatible with this view; she writes: " . . . I think the kind of union of minds, the thinking and feeling together, here shown may be regarded as the type or norm of telepathic communication to which all other cases conform in varying degrees."[1]

We would then have the hypothesis that all human personalities are capable of paranormal processes in so far as there is freedom from conscious preoccupation with the immediate sensory world, and in so far as there is some sort of reciprocity between the deep-level operation of two individuals.

II

Now let us face the question: is it true that *all* human beings have paranormal powers? When we speak of hunting for a "good subject", the suggestion is offered that paranormal power is a special gift, like absolute pitch. Is this the case, or is it in some degree the gift of all human beings? I confess that over the years I have wavered back and forth between these alternatives: and have been very unsure how to answer the practical research question: Is it worth while to set up experiments for Tom, Dick, and Harry, or should we confine our experiments to the gifted Tom, and leave Dick and Harry out? But it seems to me that after these years of uncertainty the evidence has finally driven us directly into the view that we are concerned with generic, and not simply with individual gifts. Much depends upon the subtlety of the method, and the devices that we use for reinforcing and bringing to maximal expression whatever primitive and half-choked functions may be waiting for our detection and cultivation. But many mass experiments have given positive results. In the Pratt-Woodruff[2] experiment of 1939, a large number of subjects took part in a well-controlled experiment involving "screened touch matching", with no part of the observation dependent upon what any one experimenter did or reported. Each subject had to match cards against targets placed on the other side of the screen from himself. Material was locked away after each session in boxes to which only the experimenters had keys, and the data were doubly checked. In this investigation, as in others before and since, the effect is clearly a collective effect, and not dependent upon the performance of a few individuals.

It may, of course, be urged that a number of other mass experiments have given negative results, but this I think misconceives the statistical issue. The critical ratio of 5 which was obtained by Pratt and Woodruff should not be expected to occur even a single time among all the large-scale ESP experiments ever performed. It is possible either to get or not to get a particular group phenomenon depending on the method used. For example, in public health research one may find evidence of vitamin D deficiency in a given North American urban group, or not find it, depending upon the adequacy of one's technique: but if a competent investigation finds the deficiency appearing generically in a New York

[1] "Phantasms of the Living. An Examination and Analysis of Cases of Telepathy between Living Persons Printed in the 'Journal' of the Society for Psychical Research since the Publication of the Book 'Phantasms of the Living', by Gurney, Myers, and Podmore, in 1886," *Proc.* S.P.R., Vol. 33, 1922–3, pp. 23–429 (quotation is from p. 419).

[2] "Size of Stimulus Symbols in Extra-Sensory Perception," *Journal of Parapsychology*, Vol. 3, 1939, pp. 121–58.

population, it is not a sufficient answer to show that others working with other essentially similar populations *but* with a different method, have not found it. It may be true that something happened in a group of human beings at Duke University which could not happen in other groups of human beings elsewhere, but it seems more natural to believe that there was something important about the *method*. The Pratt-Woodruff experiment does not stand alone. Whately Carington[1] repeatedly found mass effects. In the studies conducted at Stanford by Charles Stuart[2] and analyzed by Betty Humphrey,[3] mass effects were found, and there are many other examples. But I should be willing if necessary to rest my case for mass effects on the Pratt-Woodruff investigation.

III

We turn to the individualized aspects of such data, that is, to the problem of individual predispositions to the paranormal which may differentiate one personality from another. In Charles Stuart's[4] method of testing for clairvoyance a picture was placed in a large opaque envelope ; to the outside of the envelope was clipped a drawing sheet upon which the experimental subject drew his guess as to the picture within. The positive results came *en masse* from subjects who were shown on the basis of the Elkisch drawing test[5] to be people prone to make expansive drawings. And people who were prone to compressive drawings tended to miss the targets to a significant degree. Humphrey[6] has suggested that those who are expansive in the drawing test are people who reach boldly and vigorously for a challenging hidden target ; they are capable of overcoming the obstacles and asserting themselves successfully in this task. The compressives not only fail, but overshoot the mark in their failure. Six different cycles of clairvoyance tests yielded these same trends. We have, then, a meaningful relation between personality attributes and paranormal performance.

The Schmeidler[7] experiments appear to warrant the same general conclusions. Her investigations at Harvard between 1942 and 1945 and

[1] " Experiments on the Paranormal Cognition of Drawings, IV." *Proc.* S.P.R., Vol. 47, 1944, pp. 155–228.

[2] " A Classroom ESP Experiment with the Free Response Method," *Journal of Parapsychology*, Vol. 9, 1945, pp. 92–105 ; " An ESP Experiment with Enclosed Drawings," *ibid.*, Vol. 9, 1945, pp. 278–95 ; " GESP Experiments with the Free Response Method," *ibid.*, Vol. 10, 1946, pp. 21–35, and " A Second Classroom ESP Experiment with the Free Response Method," *ibid.*, Vol. 11, 1947, pp. 14–25.

[3] " Success in ESP as Related to Form of Response Drawings : I. Clairvoyance Experiments," *Journal of Parapsychology*, Vol. 10, 1946, pp. 78–106, and " Success in ESP as Related to Form of Response Drawings : II. GESP Experiments," *ibid.*, Vol. 10, 1946, pp. 181–96.

[4] See footnote 2, above.

[5] " Children's Drawings in a Projective Technique," *Psychol. Monogr.*, Vol. 58, 1945, pp. 1–31.

[6] See footnote 3, above. Refer also to " Some Personality Characteristics Related to ESP Performance," *Journal of Parapsychology*, Vol. 10, 1946, pp. 269–89, and " The Relation of ESP to Mode of Drawing," *ibid.*, Vol. 13, 1949, pp. 31–47.

[7] " Predicting Good and Bad Scores in a Clairvoyance Experiment : A Preliminary Report," *Journal*, A.S.P.R., Vol. 37, pp. 103–10, and " Predicting Good and Bad Scores in a Clairvoyance Experiment : A Final Report," *ibid.*, Vol. 37, 1943, pp. 210–21.

continued in New York indicate that subjects calling ESP cards, prepared by random numbers and placed in concealment, can make paranormal contacts with the material in a manner related to their attitude to the task. Those subjects who believed it possible to succeed in such a task, gave a significant positive deviation, while subjects who excluded this possibility yielded a significantly below-chance score. The former group, the " sheep ", in some sense know where the stars, circles, and so on actually are ; but the second group, the " goats ", must also know where they are, because they cannot consistently miss them *unless* they know where they are. This latter process, sometimes called " negative perception ", has been well demonstrated by Bruner and Postman[1] in the ordinary, normal process of sense-perception. Such studies suggest that a complex subliminal process of feeling one's way toward the target is going on, and that other subliminal processes operate to prevent the contact with the target from appearing at the conscious level. The distribution of scores makes it clear that this is a mass effect, not an effect due to a few individuals.

So far, the Humphrey and Schmeidler approaches are identical ; in so far as attitude reflects personality, personality counts in paranormal performance. Yet this did not seem to Schmeidler to be a sufficient clarification of the problem. It was certainly not true that all believers could be counted upon to score above chance nor was it possible from the data, as so far described, to make clear how individual personalities are operating. As an experienced clinical worker with the Rorschach ink-blot method, Schmeidler determined to do systematic Rorschach analyses of those taking part in her current group experiments. Administering the Rorschach test in group form, and scoring it by Ruth Munroe's method[2] to indicate good or poor social adjustment, she was able to show that well-adjusted sheep can be differentiated in their paranormal performances ; likewise, well-adjusted and poorly-adjusted goats. The data thus yield four groups, which score in the following manner : 1. Well-adjusted sheep score significantly high. 2. Poorly-adjusted sheep score about at the chance level. 3. Poorly-adjusted goats likewise score at about the chance level. 4. Well-adjusted goats score significantly below chance. It is, she remarks, just as if each of the well-adjusted groups succeeded in doing what it wanted to do—the sheep to score above, the goats to score below chance.[3] The two poorly-adjusted groups, however, manage only to stumble and fall, being bogged down apparently by their own intrapsychic conflicts, so that the sheep cannot score high, and the goats cannot score low.[4]

This result was altogether " too good " ; and naturally she felt that the

[1] Bruner, J. S., and Postman, L. " Emotional Selectivity in Perception and Reaction," *J. Personality*, 1947, Vol. 16, pp. 69–77.

[2] " The Inspection Technique : A method of rapid evaluation of the Rorschach protocol," *Rorschach Research Exchange*, Vol. 8, 1944, pp. 46–70, and " Prediction of the Adjustment and Academic Performance of College Students by a Modification of the Rorschach Method," *Applied Psychology Monographs*, No. 7, Sept. 1945, pp. 104 ff.

[3] The goats, of course, if well-informed and rational, would aim at the chance level, not *below* it. In trying to avoid positive scores, they overdo it and miss too many targets.

[4] " Rorschach Variables in Relation to ESP Scores," *Journal* A.S.P.R., Vol. 41, 1947, pp. 35–64.

experiment should be repeated. Two large-scale repetitions have been made by Schmeidler herself, with results in the same general direction; and now Mrs Adeline Roberts, another Rorschach worker, has independently obtained corroborative results with a fresh set of Rorschach data. This, of course, is not the same as to say that similar results can be obtained by everyone with every group. It is enough, however, to indicate that the data are not entirely dependent on the Schmeidler procedure alone.

We might summarize the results so far by saying that individual needs, or purposes, bear a direct relation to paranormal cognition; and at the same time evidence that individual subliminal activities operate to set free or to inhibit such processes. Perhaps we should say, as Hugh Woodworth[1] did, that there is continuous "blocking and unblocking"; a process by which the extension of ourselves in the direction of the target is throttled and constrained, and likewise a process by which the constraint is sometimes removed.

From this standpoint there arises the question: Assuming that we are all motivated to reach a given target, are some of us more free than are others to unblock, *i.e.* to remove these local blockages which seem to blunt our paranormal capacities? For example, are some of us more free from censorship, more ready to make contact with anything and everything which is out there waiting to make its mark upon us? And are some of us by inheritance or by training more loosely put together, more easily induced to fall into states of dissociation than others? The more easily dissociated individuals might be freer of blockages, simply dropping off the offending baggage. This carries us back to emphasis upon devices which make it possible for sensory processes, as in crystal vision, to externalize images which have been subliminally received, or to carry into overt motor expression, such as automatic writing, the words or other symbols which have failed of an outlet. Assuming that there exists in the subliminal a paranormally perceived reality, we may say that an automatism is effective in accordance with its degree of removal from contact with the conscious system of ideas. There are large individual differences in capacity for such automatisms. There is, of course, no special virtue in automatisms as such, and many of them are devoid of all discernible traces of the paranormal; yet if once we have evidence that the paranormal is struggling to express itself, we may perhaps help it on its way through the cultivation of automatisms.

Sensory automatisms are rather easy to cultivate. And if the present approach is sound, it is possible that normal everyday perception is in some degree—now more, now less—affected by paranormal processes operating through sensory automatisms, and that we might learn to detect their effect. Thus a number of spontaneous cases of telepathy suggest that the vehicle of their expression is the restructuring of present external stimuli. Rorschach plates and other indistinct material as used in the "projective tests" of personality, by permitting large individual differences in the form of perceptual organization, allow personality trends to influence cognitive structuring. Just so, perhaps certain spontaneous cases function essentially as projective tests. In a recent case the end

[1] "Further Consideration of Multiple-Blocking and Unblocking in Normal Subjects," *Journal* A.S.P.R., Vol. 37, 1943, pp. 117–33.

result of a death compact between two men took the form of the survivor's noting, in a restaurant, a face which startlingly resembled that of his friend. His friend, with whom he had long been out of touch, had in fact just died. The stranger's face encountered in the restaurant had for the moment been transformed ; had been built up to resemble, one might say, a death mask of the distant dying individual.[1]

You will recall in *Phantasms of the Living*[2] and in other collections, a number of cases of this type. Our hypothesis would take the following form : Other things being equal, those who are prone to sensory automatisms are thereby prone to the distortion of their ordinary sense perceptions through contamination by paranormal impressions. Likewise, since automatic writing and other motor automatisms have in general the same releasing functions, those most prone to such motor automatisms would, other things being equal, be most likely to show an admixture of the paranormal with their other motor activities.

If this makes sense, it may be worth while, in the study of extra-sensory phenomena, to do some preliminary tests upon the proneness of each subject to automatisms, both sensory and motor. As a reason for believing that this is worth while, I would emphasize that in the cross-correspondence group, and in other sensitives studied by the S.P.R., there is abundant evidence that automatisms yield data which the conscious individual cannot achieve unaided. Take the "one horse dawn experiment",[3] the effort to convey a Greek phrase to Mrs Verrall. Despite the fact that the thought was at various times in the experimenter's mind (both supraliminally and subliminally) and available as the target for a period of months, it was only through automatism that success was finally achieved. In the classical cross-correspondence, "Hope, Star and Browning",[4] the successful transmission to automatists in Britain of a message formulated quite independently was accomplished through automatic writing in which reference to Browning's "Abt Vogler" expresses a theme given in Mrs Piper's trance.

But in setting up an experiment to test the relation of degree of dissociation to degree of success in paranormal processes, one notes the distinction made by Margaret Reeves[5] between the conditions operating in spontaneous cases and in experimental cases. In developing the implications of Kurt Lewin's topological psychology, Reeves makes it clear that in spontaneous cases the type of dissociation which is operative is the temporary removal of an outer shell or hull consisting of the daily preoccupations of the conscious, waking individual. When this outer shell is removed, he withdraws from the world into sleep, or trance, or a state of abstraction. There may then be a profound release of the deeper capacities. In experimental cases, on the other hand, the experimenter must employ relatively superficial motivation such as curiosity, or the desire to gratify the experimenter, or win a prize ; consequently, dissociation will

[1] "Cases," *Journal* A.S.P.R., Vol. 38, 1944, pp. 48–52.
[2] Trübner & Company, 1886.
[3] *Proc.* S.P.R., Vol. 20, 1906, pp. 156–67.
[4] Piddington, J. G., "A Series of Concordant Automatisms," *Proc.* S.P.R., Vol. 22, 1908, pp. 31–416, especially 59–77.
[5] "A Topological Approach to Parapsychology," *Journal* A.S.P.R., Vol. 38, 1944, pp. 72–82.

have a very much less marked effect because nothing much is happening in the deeper strata. But when the motives which are near the surface are themselves activated, as in a furious and successful effort towards high scores, nothing is to be gained by dissociation.

Indeed, if this is the case, some questions emerge regarding the logic of attempting to test by experimental methods those hypotheses which are most reasonable in relation to spontaneous phenomena. In the spontaneous cases, Nature often hurls at us profoundly moving dynamic forces which we can only occasionally control in the laboratory; and the attempt to find in the general population individuals who will behave as if they were successful recipients of spontaneous cases may be based upon a misconception of the problem. In this matter of testing for ESP, I am afraid that my colleagues and I have often resembled the bees described by Samuel Butler, which wandered into the house through the open windows on a summer day, attacked the flower designs on the wallpaper, and followed them slowly to the ceiling. Then they began at the foot of the wall nearby and worked their way hopefully to the ceiling again, and so on across the room; learning, it would appear, rather little by the experimental method of hypothesis testing. It seems likely that our attempts to obtain positive results in telepathy and clairvoyance with the mass of people is going to be successful only when we have fully analyzed the problem of motivation and of working atmosphere. I suspect that in many of the successful mass experiments some favourable psychological factor in the atmosphere was achieved, and that it is not worth while to perform such experiments unless one tries to learn more about such atmospheres. We know as yet very little about them. In Rhine's[1] and Tyrrell's[2] experiments the subject's enjoyment of the task seems to be an asset, and in Rhine's early work the likelihood of a positive result was made so real and compelling to the subjects that they felt they must "stand and deliver". But our present formulations are naive, and we have years of work to do before we can define the favourable states for a given individual in a given task.

For if and when it is finally established that all human beings by virtue of their needs and their capacity to free themselves from intrapsychic barriers are capable of paranormal processes, it will only be because we have in the meantime learned much more both about needs and about barriers. What we know today is hardly more than the clue to a clue. There is no direct evidence that the successful subjects reported by Soal and Goldney,[3] Tyrrell, Rhine, Martin and Stribic,[4] for example, differ essentially from other people either in their needs or in the barriers to the cognitive activities which express these needs.

If we ask, then, what more we must find out to do better research, we might first stress the great complexities through which needs and barriers evolve in childhood before they take the form revealed in adult personality.

[1] *Extra-Sensory Perception*, 1934.

[2] "Further Research in Extra-Sensory Perception," *Proc.* S.P.R., Vol. 44, 1936–37, pp. 99–166.

[3] "Experiments in Precognitive Telepathy," *Proc.* S.P.R., Vol. 47, 1943, pp. 21–150.

[4] "Studies in Extra-Sensory Perception: I. An Analysis of 25,000 Trials," *Journal of Parapsychology*, Vol. 2, 1938, pp. 23–30, and "Studies in Extra-Sensory Perception: II. An Analysis of a Second Series of 25,000 Trials," *ibid.*, Vol. 2, 1938, pp. 287–95.

One finds, for example, that childish needs undergo what Freud calls cathexis, or what McDougall calls a process of sentiment formation. It is not the needs in their raw infantile form but a complex and elaborate pattern of needs that constitutes the going concern of the adult individual. In order to work effectively with the question of his needs, we should have to know, so to speak, what the paranormal means to him ; what he sees in the process, how he feels towards it, as it relates to the possibility of making contact with the world outside his immediate orbit of experience. We should have to know in what way he protests against the restrictions of time and space ; the nature of his adventuresome challenge of an unknown world. We must also know the specific meanings, direct and symbolic, which are served by the particular content, the particular drawings or card-symbols towards which he reaches out. In the same way, we need to know very much more than we know about the nature of barriers and their removal. It may be that in one person the mind is like a city built on islands interconnected with strong and solid bridges. Dissociation would be like the breakdown of one or more of the bridges, and could be overcome only by arduous reconstruction. Another mind might have the easy dissociability of a system of drawbridges, with an easy-break, easy-make, every few minutes or hours. It is almost certain that most barriers are of a still more complex sort, to which psychoanalysis and other deep probing methods have pointed. The paths of association or interconnection are criss-crossing lines almost like the lines of communication in a military terrain : devious, complex, irregular, and subject to bombardment as well as natural erosion, so that it would take a combination geologist-map-maker-tactician to figure out the possible lines of communication and of rupture of communication which are most important in any given terrain at any given time.

This mode of thinking would suggest that great progress is to be expected from psychoanalytic studies. This does not mean that anyone must accept any theory which does not intellectually appeal to him ; but it means that deep-level exploration of unconscious psychic structures, in all their infinitely complex dynamics, is a major tool for psychical research. In this belief, the group of medical men and women, mostly psychoanalysts, who have recently constituted themselves the Medical Section of the American Society for Psychical Research, have embarked upon studies which may throw light upon telepathic dreams and other paranormal processes which appear in their practice. This line of inquiry, initiated by Freud[1] himself over twenty years ago, has been carried forward by Servadio,[2] Eisenbud,[3] Ehrenwald,[4] Pederson-Krag,[5] and others.

[1] *New Introductory Lectures on Psycho-analysis*, 1933.

[2] " Psychoanalyse und Telepathie," *Imago*, Vol. 21, 1935, pp. 489–97.

[3] " Telepathy and Problems of Psychoanalysis," *The Psychoanalytic Quarterly*, Vol. 15, 1946, pp. 74–9 : " The Dreams of Two Patients in Analysis interpreted as a Telepathic *Rêve à Deux*," *ibid.*, Vol. 16, 1947, pp. 39–60, and " Analysis of a Presumptively Telepathic Dream," *The Psychiatric Quarterly*, Vol. 22, 1948, pp. 103–35.

[4] " Telepathy in Dreams," *British Journal for Medical Psychology*, Vol. 19, 1942, pp. 313–23, and " Telepathy in the Psychoanalytic Situation," *ibid.*, Vol. 20, 1944, pp. 51–62.

[5] " Telepathy and Repression," *The Psychoanalytic Quarterly*, Vol. 16, 1947, pp. 61–8.

More light on the unconscious may also be expected from the use of the projective methods of personality diagnosis, not only by the group method mentioned earlier, but by intensive analysis of individual predispositions. Not only the Rorschach but many other methods such as free drawing and painting, and graphological techniques, promise a good deal for the next few years, in relation to the tangled skein of unconscious intercommunication between the various aspects of psychic structure. All of this is ultimately directed by the belief that if once the complex blockages at an unconscious level may be removed, one may move toward understanding and control of the paranormal.

IV

This is, of course, a long-range goal, a matter of many years. But even when all of this has been accomplished and stands in full stature before us, I must confess that I believe that beyond both needs and barriers there is a *tertium quid*. There is, I suspect, some supplementary principle, or indeed, some over-arching all-encompassing principle. To introduce my *tertium quid*, I will tell you the odd story of Lillian Levine.

Lillian Levine was one of a group of Hunter College women who came to our laboratory in a group experiment under the direction of Mrs Dale.[1] Miss Levine sat in an experimental room operating a signal set which required her only to depress one or another of five keys, to indicate which of five cards she guessed to be the target in a randomly prepared series. In another room sat Dr Ernest Taves, who witnessed the experiment, and Mrs Dale, the experimenter, with a deck of ESP cards, from which one card at a time was removed and exposed as a target. Well, as Miss Levine began a run, she got 15 consecutive calls correct. Since these cards were set up by random numbers, and the odds of one in five remain constant throughout the operation, it is about a one in thirty-thousand million shot to succeed in 15 consecutive calls.

Hot on the trail of this bizarre phenomenon, we attempted to get some sort of clue as to what Miss Levine had done. The most that we could find out was that she had looked at the radiator in the room in which she sat, and had seemed to see the various symbols, like crosses and waves, in the rhythmic protuberances and recesses on the side of the radiator. So far she was like the man who saw his friend's " death mask ". She had not, however, been in any marked trance or abstracted state. In fact, when she saw these images in the radiator, it did not really mean the kind of seeing that one has with a crystal vision, but rather the kind of half-seeing, half-imagining which occurs in responding to a cloud or a Rorschach test. We proceeded, of course, to give Miss Levine a Rorschach test, and we wearied her a good deal, I think, with attempts to probe into what happened. But we got nowhere. We did not find out anything so very unique about Miss Levine's needs or intrapsychic barriers. Even if we had done so, we should still be unable to explain how she fell into the successful groove and how she fell out again. We are not in a mood to say that such an amazing performance is "just one of those things". Rather, we are inclined to say that psychical research is full of cases of our

[1] " A Further Report on the Midas Touch," *Journal* A.S.P.R., Vol. 37, 1943, pp. 111–18.

tertium quid, cases in which the maximum you can do with the theory of needs and with the theory of barriers still leaves you with something big upon which you still cannot get your fingers. For the point is that something new and different happened suddenly to her—perhaps a deep-level contact with Mrs Dale, perhaps a basically different way of orienting herself to her task. But what happened was not a gradual drifting away and back ; it was a clean break with her usual procedure.

I have wondered whether the Shakespeare plays have not attempted to tell us the same thing. Notice, for example, the playwright's handling of Banquo's ghost. The phantasm appears suddenly, sharply—cleanly, one might say. Macbeth does not toy with the question whether he is suffering from a hallucination of the " heat-oppressed brain " ; he screams : " Thou canst not say I did it." When the apparition disappears, Macbeth instantly recovers, exclaiming, " Why, now, being gone, I am a man again! " The playwright, as if to reinforce his intention, has actually given us stage directions : twice the Ghost enters, and twice " exit Ghost ". One is not dealing in such instances with the normal waxing and waning of human needs or of human barriers relative to such needs ; one is not simply reaching out and making some sort of contact with the vast world outside ; rather, something is *invading* the individual, invading almost in the sense which Myers used in *Phantasms of the Living*. The process of psi-gamma, as Professor Thouless and Dr Wiesner[1] name it, is action not only *by* the individual, but *upon* the individual.

The exploration of the *tertium quid* seems to lead to a result largely foreseen by Myers and Mrs Sidgwick. This result, I believe, has the regular characteristics of a new scientific idea in the sense that such ideas are likely at first flush to be quite shocking ; then after a moment's catching of the breath, they are likely to appear utterly banal, obvious, not worth the point of making, and then third, as one thinks over the two earlier phases of one's thought, one begins to say, " Well, this is after all a different way of looking at things ; let us set up experiments to see whether one can predict the outcome more accurately from the new formulation than one can from the old."

So, for whatever my suggestion may be worth, I will suggest that the third clue to the paranormal lies beyond the realm of needs and barriers, indeed that it does not lie inside of human personality at all, whether in its generic or in its individualized aspects. I believe, on the contrary, that it is strictly interpersonal ; that it lies in the relations between persons and not *in* the persons as such. If it be objected immediately that it must be personal if it is to be interpersonal, then let me plead that there is all the difference in the world between our stretching the conception of the personal to the breaking-point and on the other hand, our burning all our individualistic bridges behind us, and saying that the world of interpersonal phenomena is a world which must be faced on its own terms ; pursued in its own right ; its laws made clear and recognized to be essentially different from those laws which apply to individuals. I would plead for the direct empirical study of the laws of the interpersonal ; the functions of an interpersonal field. I suggest that it is not within the individual

[1] " The Psi Processes in Normal and ' Paranormal ' Psychology," *Proc.* S.P.R., Vol. 48, 1947, pp. 177–96.

psychic structures, but within certain specific relations between the psychic structure of one individual and the psychic structure of another that our clue lies; or if you like, that the phenomena are, so to speak, trans-personal, just as they are, indeed, trans-spatial and trans-temporal.

In this audience are investigators who have done much to confirm this view, however little they may think of the theoretical interpretations I would put upon their work. For did not Soal and Goldney[1] tell us that the telepathic gifts of B.S. were not liberated by all situations, nor by all agents, but only under certain conditions, with certain people serving successfully as agents and others utterly unsuccessful in the attempt? Did they not clearly demonstrate that the powers were not the powers of B.S., but the powers, so to speak, of certain couples—or, indeed, powers expressed by certain field situations in which experimenters, agents and percipients were all essential dynamic constituents?

Has not Dr Soal told us in his Myers Memorial Lecture[2] about the extraordinary phenomenon of divided agency? Mrs Stewart can receive telepathically from two agents, neither of whom actually knows the picture to be transmitted. One of them knows the spot where the target picture lies, but not what picture it is, and the other knows what pictures lie at 5 given spots, but not which spot will be selected as the target location. Here is a field function with a vengeance! This is indeed reminiscent of the hypothesis offered by Mrs Sidgwick,[3] according to which a sitter's mind acts in such a way as to establish a relation between the medium and a distant living person, so that the interaction of at least three personalities in involved.

This would mean that systematic, sensitive, resourceful investigations of the personalities of experimenters as well as of subjects, need to be taken, and of the interrelations of personality. I would like to quote here an observation of Schmeidler's[4] made on the basis of one of her studies of group atmospheres as they relate to clairvoyance tests:

> " I should like to generalize from the results in some such way as this: in a group which considered the atmosphere of the experiment to be unpleasantly cold and intellectual, only the subjects who were themselves rather cold and intellectual responded positively and made good scores. In the other experiments a different atmosphere was established, and a different personality pattern in the subjects led to successful responses.
>
> " If this generalization is correct, what are its implications? One conclusion would be that my research does not show the personality correlates of ESP ability as such, but only of ESP ability under the particular conditions of the experiment. Whenever the situation varies widely from these conditions, we can expect the optimum personality pattern to vary also."

[1] See footnote 3, p. 8.

[2] " The Experimental Situation in Psychical Research," being the Ninth *Frederic W. H. Myers Memorial Lecture*, Society for Psychical Research, 1947.

[3] " Discussion of the Trance Phenomena of Mrs Piper," *Proc.* S.P.R., Vol. 15, 1900–1901, pp. 16–38.

[4] " Personality Correlates of ESP as Shown by Rorschach Studies," *Journal of Parapsychology*, Vol. 13, 1949, p. 30.

If for no other reason than to stimulate discussion, I would go on to urge that if some one other than Dr Soal, let us say Dr Q., had been systematically scouring this country for gifted ESP subjects, using an equally objective and severe method, he might have found that B.S. was a poor subject, and that someone else, let us say, X.Y.Z., gave consistent, positive results. Indeed, what *did* happen when B.S. was tested by a prearranged telepathy method to see if he could get an agent's thought at the time? He failed; and it was only later discovered that he had his own way of functioning in this situation, namely, with reference to the future and the past. What about the people whose way of functioning we have not yet happened to discover? Are they gifted or non-gifted, or is the answer relative to the method? Again, forgive me when I say I am confused when I hear people tell us that we should spend all our time looking for good subjects. Can we really be sure that there are any good subjects in an absolute sense? Individual endowment, like that of B.S. and Mrs Stewart, is of the utmost importance; but the endowment appears in relation to a particular task, method, and personal setting. It is true, and very important, that B.S. and Mrs Stewart scored with several agents. It is true that Mrs Piper and Mrs Leonard have exhibited brilliant powers with many sitters. If what I am urging is sound, there should be found in certain gifted individuals a great many "open lines" of interpsychic communication, so to speak; but these are still dependent on a larger context.

Interpersonal factors released by the experimenters are certainly major factors in such contexts.

One of the outstanding things about the Duke University research, I think, has been the inculcation of certain attributes in certain experimenters which make it possible for them to set free something with certain individual subjects. This does not mean that they can always set it free, nor that what they obtain from one subject is the same as what they obtain from another. But my mind goes back to the year 1934, in which I first visited Rhine at Duke University, and saw the rugged force of the demands which he made upon his co-workers and subjects. In the light of his glowing intensity, it became possible to begin to understand the accounts given in his book of the way in which he had driven some of his subjects in the demand to get extra-sensory phenomena. It may well have been this intensity which produced the results—including some of the best-authenticated long distance results which we have in all this field. In the case of Schmeidler's studies in clairvoyance I believe the results may well have arisen from a very different kind of intensity, namely her sheer unwillingness to let people fail. And it was, I am convinced, the intensity of Mrs Dale's[1] devotion to her first independent PK experiment, of which she was so proud, and in which so much ego was invested, from which her brilliant positive results emerged. Whately Carington's methods were successful time and again with groups that he organized, and which caught his spirit; but no such comparable results have been easily obtainable away from the white heat of his own brilliant personality. There must, of course, be the fullest possible control whether the intensity level is high or low.

I doubt whether we can go on with the tradition that an experimenter—

[1] "The Psychokinetic Effect: The First A.S.P.R. Experiment," *Journal A.S.P.R.*, Vol. 40, 1946, pp. 123–51.

any experimenter—undertakes to test a subject—any subject—with a standard method—any standard method—for ESP or PK. If an experimenter in the abstract tests a subject in the abstract with a method in the abstract, experience shows that we can be pretty certain that we shall have nothing to show for our pains. I am much gratified to note in the most recent number of the S.P.R. *Journal* that Dr West has ably stated the case for individualizing the method of testing.

But I am really asking you to consider a rather simple, naive, and disturbing hypothesis, a conception which points not to the solitary grandeur and rugged independence of personality, as we like to conceive it, but to personality as a node or region of relative concentration in a field of vast and complex interpenetrating forces, in which none of us is completely individualized any more than he is completely washed out in a cosmic sink of impersonality. Our roots lie between the personal and the impersonal, between the I and the It, between the local and the universal, between the present and the timeless. Here, one comes close to some classical conceptions both of India and of our Western tradition, which suggest the relativity of our independence and separateness from one another, and indicate that the anchorage of our personal natures in the circumstances of the moment and of the place may perhaps be considerably less absolute than is supposed. Just as the field theory of Clerk-Maxwell has taught us to think of the distribution of energy in a time-space rather than in terms of little chunks of matter, so in psychology one may find it feasible to think in terms of the field relations that develop to encompass and express a group of persons.

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Along these lines, we find a rich opportunity for closer cooperation with psychology, especially social and clinical psychology, so deeply concerned as they are with interpersonal relations. Much more can be done as clinical methods and methods of research on social groups progress. This is why I am in such full agreement with Professor Thouless as to the need for an organic unity of psychology and psychical research, in which each will throw light upon the other.

The moral effect of psychical research in breaking down classical dogmatism regarding the limitations of the human personality to the world of its senses, is beginning to be glimpsed here and there. And the methods by which unconscious motivation, blockages to communication, interpersonal dynamic effects can be explored in relation to the paranormal will help us to understand psychological and interpersonal dynamics as they appear in daily life. At the same time, we in psychical research owe a great debt to experimental and clinical psychology. It has over and over again given us new techniques for the study of motivation, of dissociation, of unconscious blocking and unblocking. It has given us projective tests, devices for studying atmospheres and interpersonal effects. Just as psychology cannot get along without psychical research, so psychical research cannot get along without psychology. It is even possible that, as Schmeidler and Pratt and Humphrey have suggested, the same general laws which hold in all psychology, laws relating to the structuring of the world of perception, relating to the influence of motivation upon such structuring, relating to the Gestalt principles of membership character, closure, salience, relating to the satiation of motives and the role of substitutes during such satiation, and indeed all the general psychological laws may be found to apply perfectly to paranormal perception. At the same time, certain laws emerging first in paranormal perception, such as the ability of subjects consistently to miss targets to a significant degree, later emerge in normal perception.

It is possible, in short, that the two worlds are one except for some single principle which, so to speak, throws on a particular switch. If this should prove to be the case, our attention might ultimately be directed to the nature of this switch. It is also possible that the three clues suggested, namely, unconscious motivation, dissociation, and interpersonal organization or field relationships may prove to be all that is needed. It is quite possible that if we can state the interpersonal structure of a situation so fully that its motivational dynamics and its intrapsychic and interpsychic barriers can be fully defined, we shall be able to state when and where a particular paranormal process will appear. At any rate, I would suggest the experiment of looking upon personality as the same subject matter whether it happens to be studied by psychologists or by psychical researchers ; that we regard the paranormal as emerging from lawful and ultimately intelligible factors operative within normal personalities ; that we regard psychical research and general psychology as interpenetrating and at times fusing, and always sharing outlooks and methods ; and finally, since all psychological phenomena are to some degree individualized, that we make the most of all of those methods by which individuality may be studied with a view of trying to understand individual paranormal gifts ; remembering that the individual with his marked gifts is never utterly sundered from the less gifted about him, and that his special gift is in some degree a function of that interpersonal existence which all human personality expresses.

If this is sound, there is equal need in the coming years for two types of research : first, a need to continue the exceedingly important studies of those individuals who are highly gifted in specific ways, such as clairvoyance or precognition, finding *why* it is that they fluctuate in the presence of different persons and under different conditions, and setting up testable hypotheses regarding interpersonal dynamics. Secondly, there is a need for mass researches along lines in which the group atmosphere or social climates can be fully specified and empirically tested. When one gets a group effect, one would at once attempt to define what is operating ; one would develop such clinical methods as have already been used by Humphrey, Schmeidler, and others, and apply them mercilessly to all participants, including oneself.

So, as my time draws to a close, you find me pleading for more study of those deep resources of human personality of which Frederic Myers first made us fully aware, working in close contact with psychology, psychiatry, and the social sciences ; more explicit recognition that psychical research has a huge contribution to make to an understanding of human nature ; and indeed a willingness to consider the possibility, even in times as troubled as our own, that we may do our own part to help find a sound basis upon which to predicate the oneness of the human family ; its fulfilment, through deeper interpersonal ties, of its place in its cosmos.

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EXPERIMENTAL OBJECT-READING*: A CRITICAL REVIEW OF THE WORK OF DR J. HETTINGER

By Christopher Scott

ABSTRACT

In this paper an exhaustive analysis is attempted of the work of Dr J. Hettinger on the experimental investigation of object-reading.

The first section deals with the work reported in Hettinger's book *The Ultra-Perceptive Faculty*, in which a painstaking attempt was made to develop objective control methods for demonstrating the existence of a paranormal factor in object-reading material. These methods are closely examined and a large number of faults both in Hettinger's use of statistics and in his experimental design are found. It is shown that when the statistical errors are corrected the results are still highly significant. There remain, however, more than twenty methodological faults any of which might have contributed to the positive results without recourse to the paranormal. The data provided by Hettinger are inadequate to show which of these sources of error, if any, contributed to the significant figure obtained. The section concludes with the verdict that there is quite insufficient evidence to justify the conclusion that there is any paranormal factor at work in the results reported in this book, and that Hettinger's technique for investigating object-reading is very far from foolproof.

The second section deals with Hettinger's book *Exploring The Ultra-Perceptive Faculty*, in which a novel method of investigating object-reading —the " pictorial method "—is developed. This book scarcely attempts to be of a scientific nature, since it relies on the technique of selecting a number of the most striking coincidences from a total collection of unspecified size. An attempt is made in this section to estimate the size of this selection factor and to show how highly misleading is this method of presenting the evidence. The conclusion is reached that only an objective control technique can put the " pictorial method " on a scientific footing.

The third section deals with an article by Hettinger in the *Journal of the American S.P.R.*, in which an attempt is made to demonstrate trans-atlantic telepathy by means of a control technique applied to the " pictorial method ". Various control methods are described by Hettinger in the article. These are examined in this section and it is shown that on the only occasion on which a sound method was used the results were completely negative.

In view of Hettinger's suggestion that these negative results may have been due to the inexperience with mediumistic material of the judge

* The term *object-reading* is used in this paper to denote the (alleged) faculty of using an object to obtain paranormal knowledge about its owner (or, occasionally, about the history of the object). This faculty has often been called "psychometry", but the word has an alternative and well-established meaning in psychology, and is so inappropriate tnat it has been decided to use the less objectionable term 'object-reading' in this paper.

who carried out the assessment in this particular case, the writer of this paper carried out an experiment along the same lines in which a completely objective control method was used and Hettinger himself acted as judge. The results of this experiment were also entirely negative. The fourth section of this paper deals with this experiment and attempts to draw a moral. In the concluding paragraphs the possibility that the control methods themselves might obscure or inhibit positive results is considered, and it is shown that the nature of the control methods used render this theory untenable.

Although this paper may seem almost entirely destructive in its content it is written with the object of paving the way for the design of a totally foolproof technique for the experimental investigation of object-reading.

INTRODUCTION

Dr J. Hettinger is an electrical engineer who, since 1934, has devoted himself to a remarkably industrious series of experimental investigations of object-reading. His reports of these investigations are to be found in two books,* *The Ultra-Perceptive Faculty* (London, Rider, undated but apparently 1940), and *Exploring the Ultra-Perceptive Faculty* (London, Rider, 1941), and an article in the *Journal of the American S.P.R.* for July, 1947, vol. XLI, No. 3, pp. 94–122. Hettinger's work has attracted considerable attention. In September 1947 he read a paper on some of his recent experiments before the British Association for the Advancement of Science, and in September–October 1947 carried out a short experiment in cooperation with the *Daily Express*. His work has also received frequent mention in recent psychical literature.

The importance of Hettinger's work is not only that it forms one of the most thorough experimental investigations that have yet been made into mental mediumship, but also that it brings psychical research through the pioneer stage in this extraordinarily difficult study. Like many pioneers, Hettinger has had the unfortunate role of falling into traps in order that those who follow him may know how to avoid them. If my criticisms of Hettinger's methods seem harsh, it must be remembered that it is only by drawing attention to his errors that we can get the full benefit from his pioneer work and learn by his mistakes how to design an experimental technique that is beyond criticism.

Apart from reviews of Hettinger's books in the S.P.R. *Journal*, I know of no attempt at a criticism or analysis of his work. This is a serious deficiency, which the present article is designed to correct. I shall analyse, as thoroughly as seems necessary, Hettinger's two books, his report in the A.S.P.R. *Journal*, and two control tests he carried out with myself as subject. Finally I shall attempt to sum up the conclusions that can be drawn from all this work.

The reader who prefers to skip passages demanding mathematical knowledge should not miss anything of very great importance, provided he is prepared to take for granted the conclusions which emerge from them.

* Both now out of print.

The reader may be surprised to find, on one or two occasions in the section on Hettinger's first book, speculations about Hettinger's exact procedure followed by a footnote answering these speculations with a definite statement given to me verbally by Hettinger. I have adopted this arrangement for two reasons: first, the main text of my paper (except for the last section, on some hitherto unpublished experiments) is intended to stand as a criticism of Hettinger's *published* work; and secondly, the information obtained directly from Hettinger was given me verbally—in answer to questions which I put to him quite without notice—more than ten years after the events it referred to. For this reason I am uncertain whether the same reliance should be placed on it as on the statements made in the more official published account, and I have relegated the very small amount of such information to footnotes (pp. 29, 33).

Finally, I would like to thank all my friends who have helped me with this paper, especially Mr A. M. Western for many helpful discussions, Dr West for his continued help and encouragement, and Mr Fraser Nicol, who very kindly undertook some of the most laborious statistical work for me. I am very grateful, too, to Dr Hettinger himself for arranging for me, at his own expense, the two sittings referred to in Part IV.

PART I

"THE ULTRA-PERCEPTIVE FACULTY"

I. Summary of the work.

The Ultra-Perceptive Faculty, for which Hettinger was awarded a Ph.D. by London University, consists principally of an account of 12 series of experiments in object-reading carried out by Hettinger at King's College, London, between May 1934 and September 1937. The essential basis of the experiments remains constant throughout. Articles such as wallets, pencils, rings, cuff-links, combs, keys, letters, and sometimes blank sheets of paper, are submitted to Hettinger by his friends and acquaintances, or by *their* friends and acquaintances, and enclosed in sealed envelopes. They are then given by Hettinger to a medium or "sensitive" who gives readings on them (*i.e.* attempted descriptions of their owners) in Hettinger's presence and without removing them from their envelopes. These readings are recorded by Hettinger and returned to the owners for annotation. The 12 series of experiments described in *The Ultra-Perceptive Faculty* constitute successive attempts to find improved methods of experimenting on this basic method in order to arrive at a technique producing results which are at the same time favourable and valid.

Before the experiments are described in detail a note on terminology would perhaps be useful.

The *article* is the object submitted for object-reading.

The *subject* ("absent sitter") is the owner of the article.

An *item* is a statement made by the medium relating to the subject.

A *reading* is the collection of items given on a single object at one sitting. (A sitting in Hettinger's work usually elicits from 4 to 6 readings and from 40 to 100 items.)

A *test* consists of the object-reading of one article, and hence provides one reading.

A *record* consists of a list of items (usually including control or dummy items) sent out to a subject for annotation (*i.e.* for marking which apply and which do not).

The 12 series described in the book use a variety of methods, and the whole forms a rather heterogeneous mass of material, so that it is impossible to describe it more briefly than by reproducing Hettinger's own summary (Chap. X, Section IV).*

" The methods used in the preliminary tests (1st Series) were of *tentative* character. They were as follows :

" 1. *A ' multiple control ' method*, according to which copies of all records (generally four) obtained at one sitting were given to the four subjects and each one had to *select* the one he or she thought would be the most applicable one to him or her. Since a good many applicable items appeared on all the records, the subjects found it difficult to make a proper selection.

" 2. *A ' made-up control ' method*, according to which the record given by the sensitive was coupled with a ' made-up control ' record. The experimenter did not find it convenient, nor easy, to make-up such a record, and he also thought the method would be open to serious criticism, even if such record were made-up by another person than himself.

" 3. *A combined method of ' made-up control ' and ' evaluated ' items*, which differed from 2, in that the items were given different values. The drawback of this method is the arbitrary nature of the evaluation.

" 4. *A ' checking ' method*, according to which the owner of the object on which the record was obtained filled in the record, and a number of other subjects were then asked to state which of the items on that record were applicable to them.

" The latter method, which seemed promising, was adopted for statistical purposes in the 2nd Series of tests. However, the series was soon discontinued in view of the results of a *reliability test*. This test showed that the high scoring obtained in that series had an easy psychological explanation, namely, the tendency of some of the subjects to accept more items on the record they *knew* was theirs than on the records which they *knew* did not belong to them.

" There followed a short 3rd Series, wherein the checking method was retained with the modification that each subject was asked to *fill in* all the records obtained at the same sitting, without being told which one was his or hers. This method, which is very similar to the tentative ' multiple control ' method above referred to, had the drawback that, since the subjects knew that only one record was theirs, yet all of them contained applicable items, there was a great tendency to guessing in the case of some of them, so that, by deciding that one of the records was theirs, a definite bias was introduced in their annotations of all the records as a whole.

* My thanks are due to Dr Hettinger and to Messrs Rider & Co. for permission to reproduce this extract. I have made a few minor alterations to bring Hettinger's terminology into line with my own.

" The statistical proof of the probable existence of the ultra-perceptive faculty was solely based on the *control methods* which followed. These were:

" 1. *The method of admixed control items* (4th Series), according to which twelve items given by the sensitive were mixed with twelve ' control ' items, and the subject had to state which of the items were applicable to him and which were not. The control items were drawn from a ' guess-box ' which contained the items given by the sensitive in connection with previous records.

" This method, like all the variations that followed, has the advantage that the character of the results obtained is independent of the tendency of the subjects to accept a small or a great number of items, being determined by the ratio of the items R : W [Right : Wrong] accepted from among the items given by the sensitive and the control items respectively.

" The method proved satisfactory and reliable, but the experimenter thought an improvement thereon could be secured by dispensing with the ' guess-box ' and introducing the following modification.

" 2. *The method of selection by mutual consultation* (5th Series). According to this method the items given to two subjects were intermixed on one record, and both were asked to cooperate and decide between them which items were more applicable to the one and which to the other. This method, which the experimenter still considers a good one, was abandoned for the following practical reason : he did not find sufficient suitable subjects who could conveniently cooperate in that way.

" 3. *The method of selection by graded significance* (6th Series) was a further modification of the method of admixed control items ; it mainly consisted in that the items given to two subjects in connection with their respective articles were intermixed, and the subjects were asked to grade their selected items according to their significance. The method was not found satisfactory.

" 4. *The method of selection of one of two paired records* (7th and 8th Series). Each subject was given *two* records—one being his own and the other one a ' control ' record belonging to another subject, without his knowing which was which—and he was asked to decide by *general impression* which one, *considered as a whole*, was his. The method did not prove satisfactory, but it was decided to try it again in a modified form.

" 5. In a *modification* of the method just referred to, all items were given in advance different values, 1, 2, and 3 (9th Series). As the results were not satisfactory, the idea of selecting one of two paired records was ultimately abandoned, but it was thought worth while to test again the method of evaluation [*i.e.* the above method of scoring an item 1, 2, or 3 according to how specific it is] in a longer series, in spite of the objection to its being arbitrary. This was done in connection with the next method.

" 6. *The method of paired items* (10th Series). According to this method each item given by the sensitive was paired with a control item, and the subject was asked to select the one which was substantially more applicable than the other, unless both were equally well or not at all applicable, in which cases they were both accepted or both rejected. The control items were taken from ' guess-lists ' obtained by tabulating the items previously given by the sensitive, in three separate groups, valued 1, 2, and 3 respectively.

" It was, however, found that evaluation did not change the character of the results. In view of this finding, the idea of evaluation was abandoned in the subsequent series.

" 7. *A modification of the method of paired items* was introduced in the 11th and 12th Series, according to which the items were no longer paired with control items obtained from ' guess-lists ', but with the items given to another subject at the same sitting. The method proved to be very convenient and satisfactory.

" Summarizing the above comments on the latter *control* methods, (1 to 7) employed for establishing a statistical proof of the probable existence of an ultra-perceptive faculty in connection with ' psychometry ', the *method of admixed control items*, such as used in the 4th Series, and the method of *paired items without evaluation*, such as used in the 11th and 12th Series, seem to be the best ones for the purpose in view."

In every one of the experiments in which controls were used the subjects showed a tendency to pick out their own items rather than the controls. The 622* readings obtained in the main series (Nos. 4–12) contained 6,631 items. The number correctly selected by the subjects was 2,570, while the number of control items selected was 1,913. The deviation, according to Hettinger's calculations, is 28·6 × its probable error, or 19·1 × its standard deviation, which is very highly significant.

At first sight these results might appear to provide overwhelming evidence for the existence of what Hettinger calls " the ultra-perceptive faculty ". A closer examination, however, reveals that the work is vitiated by a very large number of errors.

The majority of these are errors of experimental method, whereby Hettinger has failed to rule out various normal factors which may have operated either to produce a positive result themselves or to exaggerate the significance of any positive result obtained. Unfortunately we are unable to deduce from the information provided by Hettinger which, if any, of these factors were operating : we can only say that, with the given data, any of them *may* have been present, and that the evidence is insufficient to demonstrate that the results have a paranormal explanation.

A few of Hettinger's errors, however, are more definite than this : in his use of statistics we can point to some clear mistakes and make a correction to the results to allow for them.

II. Errors in Hettinger's use of statistics.

Let us start with two surprisingly elementary statistical errors.

First, the use of empirical probabilities in calculating the standard deviation.

Let S be the number of originals† and C the number of controls accepted by the subject in a given batch of data.

Let $N = S + C$ be the total number of items (or readings) accepted.

* The table on p. 115 gives this figure as 623. This appears to be an error (see below, p. 27, footnote).

† I propose to avoid much circumlocution by using the noun " original " as the opposite of " control ". An " original " item or reading is one actually given by the medium as applying to the subject who is annotating the record.

In nearly every case the originals and controls are equal in number. Furthermore, we may assume, for the moment, that the originals and controls have exactly equal antecedent probabilities of acceptance.

Hettinger departs (quite legitimately) from custom by working with the deviation of the fraction $\frac{S}{N}$ from its expected value of $\frac{1}{2}$, instead of with the deviation of S from its expected value of $\frac{1}{2}N$. The standard deviation, assuming a binomial distribution (see p. 25) is therefore given by

$$\sigma = \sqrt{\frac{p \times q}{N}},$$

where p, q are the probabilities of choosing originals and controls.

Now the correct procedure (as nearly always in psychical research) is to assume the null hypothesis that only chance is operating, and to determine on this hypothesis how large a coincidence would be necessary to give the results which we have in fact obtained. And clearly in this case the assumption that chance alone is operating in the subject's choice means that there is an equal probability of the subject's selecting originals and controls, *i.e.* that $p = q = \frac{1}{2}$, so that we have

$$\sigma = \sqrt{\frac{\frac{1}{2} \times \frac{1}{2}}{N}} = \frac{1}{2\sqrt{N}}.$$

Hettinger, however, for a reason that he does not adequately explain, uses not the theoretical values of p and q but the values determined empirically from the results obtained, *i.e.* $p = \frac{S}{N}$, $q = \frac{C}{N}$. This is inconsistent. Either we work from the null hypothesis and proceed as above, or we base our calculations entirely on empirical results, in which case the expected value of $\frac{S}{N}$ is not $\frac{1}{2}$ but $\frac{S}{N}$, and there is no deviation at all. Hettinger calculates the *mean* theoretically and the *standard deviation* empirically. There is clearly no justification for this.

Hettinger's method gives $\sigma = \sqrt{\frac{S \times C}{N^3}}$, which is always less than or equal to the correct value, so that the significance is overestimated.

The error increases as the significance increases, but in practice it is seldom large. (Incidentally, on Hettinger's method, if the subject gets 100 per cent success the significance of the result becomes infinite.)

By a rather similar confusion of the use of theoretical and empirical probabilities Hettinger is led to put forward a very remarkable argument which appears to imply that in every experiment where there is an observed deviation from a mean, one is justified in doubling that deviation.

The argument seems to be as follows:

Using the same notation as above, $\frac{C}{N}$ is the "empirical probability for the controls" or the "empirical probability of coincidence".

The relevant deviation, therefore, is the deviation of $\frac{S}{N}$ not from $\frac{1}{2}$, the *theoretical* probability of coincidence, but from $\frac{C}{N}$, the *observed* probability of coincidence. Since $\frac{C}{N}$ is less than $\frac{1}{2}$ by the same amount that $\frac{S}{N}$ is greater than $\frac{1}{2}$, this means in effect that we can double any deviation we find.

It is hardly necessary to explain the absurdity of this argument. Perhaps the simplest reply would be as follows :

If $\frac{C}{N}$ is the "probability of coincidence", what is $\frac{S}{N}$? Presumably the probability of success; *i.e.* the probable proportion of successes is $\frac{S}{N}$. But the observed proportion of successes is $\frac{S}{N}$. Hence the observed proportion equals the probable proportion, so that the deviation from chance is precisely zero.

The argument is manifestly absurd, but it is a direct extension of the argument used by Hettinger to enable him to double his deviation.

Of course, despite the absurdity of Hettinger's argument, it would be legitimate to use the deviation of $\frac{S}{N}$ from $\frac{C}{N}$ instead of from $\frac{1}{2}$, provided one used the standard deviation of this new variate and not that of the old one. Since the new standard deviation, like the new observed deviation, would be just twice the old one, this procedure would have no effect at all on the final anti-chance probability.

A further statistical error arises in Hettinger's use of "the method of evaluation". In series 9, 10, and 11 an attempt is made to "evaluate" the various items so as to take account, in the statistical results, of the varying degrees of significance of the items and to give more weight to those items whose acceptance is clearly antecedently less probable. Obviously such a procedure, if a valid statistical method can be worked out, is very desirable. Hettinger's method is to score every item (originals and controls) either 1, 2, or 3, before the annotator marks it, according to how specific it is. Items of a general character are marked 1, specific items 2, and exceptional items 3. When the subject has annotated the reading, the total score on the accepted items is added up ($=N'$, say) and the chance probability is calculated on the basis of an expectation of $\frac{1}{2}N'$ and a standard deviation of $\sqrt{N'pq}$, where p and q are the probabilities of correct and incorrect choices by the subject.

But this is erroneous. The expectation is correct, but the standard deviation is too small and leads to an exaggeration of the significance of the results. The correct way of applying the method of evaluation is a little more complicated and has to take into account the number of items put into each class.* However, as long as we use the correct formulae we

* If the number of items given the score, or "weight", w_i is n_i, $i=1, 2, \ldots s$, and if $\sum_{i=1}^{s} n_i=N$, the correct variance for a binomial distribution is $pq \sum_{i=1}^{s} n_i w_i^2$,

may proceed as Hettinger does (or more arbitrarily if we wish—in fact we may be as arbitrary as we like) in assigning scores to the various items before annotation.

It must be admitted that Hettinger says more than once that the method of evaluation he uses is arbitrary and that he does not consider it satisfactory: he uses it, apparently, " only tentatively, by way of experiment ". Although it is difficult to see what results of any value this " experiment " could have—the trouble with the method, after all, is not that it has no effect on the results, but that any effect it might have would be illegitimate—it would scarcely be reasonable to criticize Hettinger on this score. I would, therefore, not have mentioned the matter of Hettinger's use of evaluation if it were not that, despite his repeated expressions of doubt as to the method's validity in 1938, he uses the same method (or one rather like it and just as invalid) as the *only* method of scoring in the transatlantic experiments of 1945–6 (series 1). It is therefore pertinent to Hettinger's work as a whole to point out that the method of evaluation which he uses is statistically quite inadmissible, and to provide the formula which he should have used.

Another serious objection to Hettinger's application of the method of evaluation is that the items are classed by Hettinger himself, who knows which are originals and which controls. Since the standard by which an item is judged to be " specific " is necessarily very arbitrary, it is clear that this method is dangerously open to the influence of any bias Hettinger may have. It would have been safer to leave the evaluation to the subject, who did not know the originals from the controls, and to use a statistical method which would make it impossible for any arbitrariness in evaluation to give a spurious significance.

A rather more complicated problem arises over the question of whether we should deal with the number of *items* which the subjects choose correctly or the number of *records* in which they score higher on the originals than the controls. Hettinger gives the results for both methods. It is clear that the second is very wasteful, and therefore insensitive; but, unfortunately, there are a number of objections to using the first. Consider for example series 7, 8, and 9 where the subject is presented with two separate readings, one original and one control, and asked to mark every item as applicable or not applicable. If we use, as Hettinger does, the straightforward method of adding up the number of original items accepted and the number of control items accepted in all the records of a series and dealing with the totals by the ordinary statistical procedure, we are making the assumption that the items constitute a *random* population of 50 per cent originals and 50 per cent controls. In fact, of course, nothing of the sort is true. The subjects know that the items of a given reading are either all originals or all controls. A subject, therefore, who took full advantage of this knowledge would not worry about choosing between items, he would merely choose the *reading* which he thought to apply and mark all the items on that reading as applicable and all those on the

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and for a hypergeometric distribution (see p. 26) is

$$\frac{pq}{N-1}\left[N\sum_{i=1}^{s} n_i w_i^2 - \left(\sum_{i=1}^{s} n_i w_i\right)^2\right]$$

other inapplicable. In this way he would have a fifty-fifty chance of getting all the items right—a chance which the straightforward statistical procedure applied to the items-scores would represent as more than 1,000 to 1. Both Hettinger* and West† have shown in their studies of bias that this is, in fact, to a considerable extent what actually happens.‡ Of course the effect cuts both ways, and the objection is not that it leads to a spurious positive result but that it causes the significance of any result that may be obtained to be overestimated. If the effect were working at its maximum and if there are 12 items in a reading, evaluation by means of the items-scores assuming a binomial distribution would cause the critical ratio to be multiplied by $\sqrt{12}$, *i.e.* about 3·5. This would mean, for example, that a result which is at about the 1 in 20 level of significance could be represented as having a chance probability of 1 in a million million. Potentially, therefore, the error is very serious indeed, so that in using the method of presenting pairs of readings to the subject it is important to avoid applying to the items-scores any statistical method which assumes, as does Hettinger's in series 7, 8, and 9, that the items are not grouped together. Much the simplest and safest procedure is to work with the number of *readings* correctly chosen.§

From the statistical point of view the substance of this objection is that Hettinger has assumed a binomial distribution for the items-scores when the grouping of the items into readings makes this assumption false. The objection therefore only applies to series 7, 8, and 9 where such grouping takes place.

There is, however, another reason why the assumption of a binomial distribution for the items-scores is certainly false for all series except 10, 11, and 12. The variate with which Hettinger works is the number R of originals chosen by the subject from a record containing n items, $\frac{1}{2}n$ of which are originals and $\frac{1}{2}n$ controls. Now, except in series 10, 11, and 12, where original and control items are paired together, the distribution of such a variate is very far from binomial—quite apart from the point raised above about series 7, 8, and 9. It is not very difficult to see why: the binomial distribution assumes that the chance that the next item chosen from a record will be an original is quite independent of how many originals have already been chosen from that record. But this is clearly false. Suppose a certain record contains 10 original and 10 control items, and suppose that the annotator is in the process of considering the record and has already accepted 9 items as applicable, and that every one of these 9 is in fact an original. What is the chance of the next item to be accepted being an original? The binomial distribution assumes that it is an even chance. But in fact it is a chance of 10 to 1 against, since among the 11 items remaining to be considered there is only 1 original. Thus it is in fact much more difficult to get a very high score—or equally, a very low one —than a binomial distribution supposes. This means that the assumption

* *The Ultra-Perceptive Faculty*, p. 46.

† Unpublished study.

‡ A statistical analysis of the figures for series 7, 8, and 9 indicates that on the average this bias alone determines the subject's decision about the item in three items out of every five.

§ Methods of dealing legitimately with items-scores exist, but as they are cumbersome and rather complicated they will not be discussed here.

of a binomial distribution leads to an *under*-estimation of the significance of the results.

The correct distribution is called *hypergeometric*, and its variance depends on the total number of items accepted as applicable from the record being considered. If we call this number A, then the hypergeometric variance is $\frac{1}{4}A(n-A)/(n-1)$. It will be seen that this is never greater than the binomial variance $\frac{1}{4}A$.

In Hettinger's data the number A is variable but averages about $n/3$, which means that the variance should be reduced by about 30 per cent, and the critical ratio increased by about 15 per cent. The effect on the chance probability of this error of Hettinger is therefore appreciable —and could be quite serious if the average number of items accepted per record were rather larger. Thus it seems worth mentioning as an important point to be noticed if any attempt is to be made to repeat an investigation of this sort. However, the error is on the safe side and does not vitiate Hettinger's method but merely detracts somewhat from its sensitivity.

The correct (hypergeometric) variance can be worked out in every case from the data supplied by Hettinger ($A = R + W$), and this particular error corrected. In series 10, 11, and 12, however, where original and control items are paired together, the distribution of the variate R, defined as the number of pairs in which the right member is accepted, is truly binomial (that is, neglecting other objections to be raised below).*

Since for the moment we are only concerned with purely statistical difficulties I shall leave till later (see below, p. 34) the discussion of further objection to dealing with items rather than readings. It would perhaps be wise to repeat that in the above criticisms the objections to dealing with items is not that one might obtain a spurious positive effect, but that any effect one does obtain, whether positive or negative, may have its significancc exaggerated.

Another statistical error which operates *against* a significant result should be mentioned. If one is classifying each reading as either a " success " or a " failure " according to whether more originals or more controls have been accepted from it by the subject, the question arises: what should one do with readings in which the subject accepts equal numbers of originals and controls? Hettinger's practice is variable. In series 4, 10, 11 and 12, he puts these borderline cases with the " failures ", but in series 7 and 8, he rejects them from the experiment altogether for the purpose of evaluation (the remaining series are not evaluated statistically). I am unable to understand Hettinger's arguments for these changes in method, but it seems to me quite clear that the latter method is always valid.

The same question arises over items, instead of readings, in series 10, 11, and 12. Here the items are paired togethcr, one original and one control, on the record sent to the subject for annotation, and the subject is asked to mark each item Yes or No. Now in practice this does not always mean that the subjcct marks one from each pair Yes and one No.

* The hypergeometric distribution is the special of what is known to some psychical researchers as the " Stevens formulae " arising when there are only two different kinds of target—in this case, originals and controls.

Often he marks both Yes or both No. Hettinger, continuing his practice in the earlier series of giving scores only for Yeses, counts one right and one wrong when the subject says Yes, Yes, to a pair, and gives no score when the subject says No, No. Obviously a more sensitive scoring procedure would have been to reject all cases where the subject says Yes to both or No to both and to deal only with the cases where he said Yes to one and No to the other.

The procedure of not taking into account the borderline cases is equivalent to what is known as " free matching " in experiments with drawings and it is difficult to see why one should not always take advantage of it. By putting the borderline cases with the " failures " Hettinger unnecessarily squanders the significance he has amassed.

III. Summary of results with statistical errors corrected.

We have now come to the end of Hettinger's statistical lapses,* and it would perhaps be helpful to give an interim report of the results with all the necessary statistical corrections made. In the table on p. 28,

1. The standard deviations are calculated on the basis of the theoretical probabilities, $p=\frac{1}{2}$, $q=\frac{1}{2}$.
2. Hettinger's " statistical correction of the results " which leads to a doubling of the deviation is omitted.
3. In view of difficulties already discussed and another to be discussed below (p. 34), the results for each series are evaluated on the basis of the number of successful *readings*, and not the number of *items* correctly chosen.
4. The " method of evaluation " for giving more weight to the more specific items is not used.
5. The method of rejecting borderline cases is used wherever possible.

It will be seen from this table that, serious as Hettinger's statistical errors are, they are not themselves sufficient to account for the results. Correct statistical analysis of the material provided gives a figure which, though not so high as Hettinger believes, is still overwhelmingly significant.

IV. Errors of method.

While Hettinger's statistical errors are, so far as I know, peculiar to himself, his errors of experimental design are of more general importance, for at least one of them is to be found in almost every attempt that has been made at objective evaluation of the mental phenomena of mediumship.† A critical discussion of these errors will therefore have the

* Two numerical mistakes should also be mentioned. The figure 948 which appears twice on p. 53 should almost certainly be 912. This increases by 1·4 and 1·3 respectively the two percentages which follow, but does not affect any of the calculations. On p. 76, Table " F ", for the right selection (*Rs*) the number of records for which $R/W<1$ is 6 and not 5, and the number for which $R/W=1$ is 3 and not 5. This alters the figure for the number of right selections from 71 to 70. The effect on the results is negligible.

† I can exclude only some of West's experiments (unpublished) in object-reading and Hettinger's scrap-book method modified as described below (p. 47), though the former seem nevertheless to be open to other considerably less serious criticisms. However, both of these investigations produced negative results.

Series	"*Sensitive*"*	*Successes (readings)*	*Failures (readings)*	*Critical Ratio, X*	*Probability, P*†
4‡	K	50	19	3·7	·0002
5	K	9	2	—	—
6	K	8	5	—	—
7	K	28	16	1·7	·09
8	K	26	27	—	—
	F	70	41	2·7	·007
9	K	5	4	—	—
	F	6	2	—	—
10	K	20	18	—	—
	F	32	14	2·6	·009
11	K	11	9	—	—
	F	40	19	2·7	·007
12	K	10	7	—	—
	F	28	16	1·7	·09
Totals	K	167	107	3·6	·0003
	F	176	92	5·0	6×10^{-7}§
Total F and K		343	199	6·1‖	10^{-9}§

additional constructive function of pointing the way to the design of a fool-proof technique in experimental object-reading.

We come now therefore to a consideration of some errors which are not purely mathematical but depend, to varying degrees, on the nature of the items and on the details of Hettinger's experimental procedure.

The central difficulty in the design of an objective method is to provide controls which are exactly like the originals in every respect except that they are not in fact originals. This presents considerable difficulties. The following are the sources of error that have to be avoided :

1. The controls must not be systematically more specific, or in any other way antecedently less likely to be accepted, than the originals.

2. The originals must not have some quality in common (such as subject matter, style, etc.) which the controls lack.

* Hettinger worked with two sensitives, Mrs F. Kingstone (K) and Miss F. Fallows (F).

† Probabilities less than ·05 are usually considered " significant ".

‡ Series 1 and 2 are purely exploratory and do not make use of serious control methods. Series 3 is a short one with only slightly positive results which do not conveniently lend themselves to statistical evaluation.

§ These figures are not accurate.

‖ c.p. Hettinger's figure of 19·1. About two-thirds of this discrepancy is due to Hettinger's " statistical correction " leading to a doubling of the deviation. The remainder is chiefly caused by the insensitivity of working with whole readings rather than with items. A fairer estimate of the significance, based on items-scores and not assuming independence of items, but not completely reliable since, in theory at least, it requires the unjustifiable assumption of a normal distribution and one which is not " faked " by the subject, is provided by Student's t test applied to the differences between the number of originals and the number of controls accepted from each reading. This gives a t (equivalent to a critical ratio) of 8·8.

3. The originals must have no observable connection with the article that the controls lack.

4. There must be no possibility of the subject recognising controls as items given to him to annotate on earlier records.

5. The controls must not be selected by anyone who knows who the subject is unless they are selected by a truly random method.

There is no evidence that Hettinger has made any conscious attempt to fulfil any of these conditions except the fifth. Let us consider them in turn.

1. The possibility that the controls may be antecedently less likely of acceptance than the originals arises in series 4 and 10, where the control items are taken from the items given at earlier sittings. There are a number of ways in which this might happen. (*a*) As the series of experiments proceeds the mediums may become less and less specific in their statements. Such a change is psychologically very plausible when we consider that these no doubt rather tedious tests continued for nearly 3½ years. (*b*) Some of the items given by the medium may be related to topical events or conditions. In this way the originals would have an advantage over the controls. Thus if the item, "Been somewhere recently where there was a heavy fall of snow" had appeared in a reading given during the summer or autumn it would certainly have been rejected as being a control. (*c*) The sensitive "F" was introduced in series 8. The possibility arises that her success in series 10 may have been partially caused by a tendency on her part to give items which are less specific than those of "K". Perhaps this would account for the greater success of the medium "F". (*d*) A further danger arises in series 4, 6, 10, 11, and 12. In these series the number of items per record is fixed by Hettinger. There are equal numbers of originals and controls on the record to be annotated and the numbers of originals are:

In series 4:	12
,, ,, 6:	8, 10, or 12
,, ,, 10, 11 and 12:	10*

Now Hettinger does not tell us *how* he managed to fix the number of items per record. Anyone who has experience of sittings for object-reading will know that a medium may give anything from four to twenty items on different objects. Did Hettinger urge his sensitives to give more until the correct number was reached? If so, might they not, once they had given all they wished, resort to vaguer and more general items? In series such as the 4th and 10th where the controls came from earlier series in which this process of "squeezing" the medium for more items had not been applied, the originals might well in this way get a higher antecedent probability of acceptance than the controls. And what happened when the medium had given the required number of items and began to give more? Did Hettinger stop her? Those who have experience of mediums will be reluctant to believe that Hettinger could have done so while continuing to hold her cooperation.† Did he cease to record the items as soon as the

* It is not entirely clear from the text whether or not the number of items per record is fixed in these three series. However, from the specimen record on p. 89 and the figures given on pp. 88, 94, 103, 104, for the aggregate number of items it seems almost certain that it was fixed at 10.

† Dr Hettinger has since informed me that this is, in fact, what he did.

required number had been given, or record them but discard them afterwards? This would have been satisfactory provided they really were entirely discarded; if they had been eliminated from the record sent out for annotation but put into the "guess-box" or "guess-list" for later use as controls, the possibility arises that the medium might give more specific items as she "gets into her stride" with each object, so that the controls would contain a higher proportion of more specific items than the originals and hence have a greater antecedent probability of being rejected. Or again, did Hettinger cut down the number of items per record by arbitrarily rejecting some of them? Clearly this would not be legitimate unless the method of selecting the ones to be rejected were truly random. Finally there is the possibility that Hettinger reduced the number of items by condensing two or more items into one without actually reducing the amount of material. Such a procedure would be dangerously arbitrary, and would lead to considerable possibilities for manipulating the results—whether consciously or unconsciously.

Hettinger's reticence in his book about all this is very unfortunate. The procedure of fixing the number of items per record is riddled with possibilities for error. From the account given by Hettinger there is no evidence to show that these possibilities were even seen, much less guarded against.

(*e*) A further query arises over series 10. Here we are told that the control items were taken from a "guess-list" made up of *some* of the items previously given. It is natural to ask, *which*? Unfortunately Hettinger is not explicit on the point. Whatever Hettinger's method of choosing which items to put in the list it is important to know whether his procedure might have resulted in items on the "guess-list" being more specific than the average.

Again, this guess-list was made by tabulating items previously given the values 1, 2, or 3 in the "method of evaluation". Hettinger does not say so explicitly, but presumably the purpose of this tabulation was to pair a control from one of these three classes with an original falling into the same class. But whether or not this was the procedure it is certain that Hettinger (or whoever was selecting the controls from the list) could *see* which controls he was selecting, so that we cannot be certain that his choice of the controls was not affected by bias. This difficulty was avoided in series 4, where a "guess-box" was used, containing *all* the items previously given, from which the typing assistant drew the controls to be used.

2. Similarly Hettinger seems to have overlooked the possibility that the original items may have had some quality in common (such as subject matter, style, etc.) which the controls lacked. This difficulty might arise in series 4 or 10 where the controls, unlike the originals, did not form a single reading. The possibility of style providing the clue to the originals *might* arise with only one medium if her style were variable, but is more likely to cause serious trouble where two or more mediums are being used. In series 4, there was only one medium: in series 10 there were two, though apparently (the text is not very clear on the point) the controls were taken from series 9, in which there were also two mediums, so that, assuming styles remain constant, the style of the "original" medium

would also appear in the controls, though only in about half of them. The effect, therefore, is not very likely to be a serious one, though it must be considered as a possibility. Subject matter is a much more probable clue.

3. Hettinger attempted to rule out the possibility of normal deductions about the owners from the objects by always (" with few exceptions ") enclosing the objects in sealed envelopes. We are also told that " in the case of one of the sensitives, most of the tests were carried out without her touching the envelopes ". In view of the striking superiority of the medium " F " over " K " when they are competing it is a pity that Hettinger does not say which of the mediums this statement refers to. This appears to be a case where Hettinger quite needlessly provokes the suspicious reader, for in his second book, *Exploring the Ultra-Perceptive Faculty*, he makes it quite clear that it was " F ", the more successful medium, who did *not* handle the envelopes and " K " who did. In series 11 and 12, we are told, neither medium handled the envelopes: they " were placed on a table or chair and the sensitives concentrated on them at a distance of about two feet ". It should be realised that though a sealed envelope usually conceals the detailed characteristics of an object it does not always—and hardly ever if handled—conceal the *nature* of the object; and a certain amount (particularly sex, age, etc.) can be deduced from the nature of an object, especially if the subjects voluntarily submit objects for experiment. Of course there is no need to suppose that the sensitive is playing this game alone.* The subject, on his side, can do much to help the results by picking out items or readings which seem to him likely to have been given on the object he has submitted. (Even if he has submitted several objects he would probably know on which of them any particular reading was given, as the nature of the object is written at the top of the specimen records reproduced in the book. Unfortunately Hettinger does not make it clear whether this information was provided before or after the subject made his annotation.) It might perhaps be objected that this hypothesis makes rather unreasonably harsh assumptions about the ethical standards of Hettinger's subjects. For my own part I do not think it does. From our point of view as researchers trying to work towards the truth, deliberate sabotage of this sort does perhaps seem unethical. But it must be remembered that, as Hettinger says, the subjects were " of most varied occupations and temperaments, including a number of King's College students ".† It is surely not very difficult to imagine that many such subjects, having no interest in psychical research, would regard the whole affair as something of a farce, but a farce which presents a rather interesting puzzle to be solved. In any case even if we assume that all the subjects were wholly well-intentioned, we have ample evidence from Hettinger's own study of bias that a well-intentioned person may behave in a way that would appear to the naive as deliberately dishonest. Intentions count for little where the will to believe is strong.

* It is difficult when discussing questions of this sort not to seem to imply that the medium is necessarily a conscious fraud. I must emphasise that (at any rate for the present part of the discussion) I consider such a hypothesis both superfluous and unlikely.

† Series 1 and 4 each involved 63 subjects. We are not told how many subjects took part altogether in the whole work.

4. The possibility that the subject may recognize controls as such because he has seen them before—either on his own records or on someone else's—arises in series 4 and 10 where the controls are taken from items given on earlier sittings. Dr Hettinger has told me personally that he did not consider it necessary to take precautions against this possibility. While I agree with Hettinger that it will certainly affect only a small proportion of the items, it seems to me that in two long series such as these, involving over 3,000 items altogether, it would have been wiser to have taken precautions to ensure that nothing of this sort could occur. As things stand it is almost impossible to estimate the probable magnitude of this error.

5. The condition that the controls must be selected by a random method or by someone who does not know who the subject is seems to have been fulfilled in series 4, where the controls are taken from a guess-box, containing all items previously given, by the typing assistant. Provided we can assume the *bona fides* of this anonymous assistant this method appears—at any rate in this respect—to be unexceptionable. In series 5, 6, 7, 8, and 9, a "compensating" method is used; the items from a reading applying to the subject S_1 are the controls for the record sent to S_2, and the items on this record applying to S_2 are the controls for the record sent to S_1, *i.e.* S_1 and S_2 are sent the same pair of readings, and the control for one is the original for the other. This procedure avoids the possibility of any general difference in quality between controls and originals, but it still leaves an opening for bias if the person deciding which readings are to be paired together knows who the subjects are. For example, if one of the subjects has red hair and one of the items given on a reading *not* applying to him was "this person has red hair", the person making up the records for annotation could reduce the chance of the red-haired subject selecting the wrong reading by ensuring that this item does not appear as one of his controls but is allocated to someone else's record. There is here, then, ample opportunity for bias to vitiate the results if the originals and controls are paired by someone who knows the subjects. Unfortunately we are not told who did this pairing. In series 10 a "guess-list" of controls is used. Again, we are not told who selected the controls from this list. In series 11 and 12, the genuines are paired "with the items given to another subject at the same sitting". It is not clear whether this means that a true compensating method was being used, that is, that each reading was used as a control once and once only, but it is evident that where an odd number of readings was given at a sitting this could not have been done by simply putting the readings together in pairs. Unless Hettinger was taking great care over this point —and he gives no suggestion that he was—it seems likely that some of the readings were used twice as controls. If this is so, the method does not completely succeed in ruling out, by compensation, the possibility of there being a general difference in quality between controls and originals, and is therefore open to the influence of bias. And even if Hettinger did take care to see that no reading was used twice as a control, the method is open to the same criticisms as were raised above against series 5 to 9.

In all the series except the 4th, therefore, we need to know: (*a*) Who chose the controls? (*b*) Did this person know the subjects? To the first question we find no answer in the book, though it seems reasonably likely that it was Hettinger.* If it was not, we have no answer to the second question. If it was, we must inquire about Hettinger's knowledge of the subjects. The evidence on this point does not seem entirely consistent, but its general import is clear. On p. 75 Hettinger says, " Quite a number of the subjects were well known to (me)." On p. 172 we read, " The experimenter, too, was completely ignorant with respect to the bulk of items given, *viz.* as to whether they were applicable or not and in many cases he did not even know the subjects." While on p. 149 we find, " A subject who is very quick or very slow will *almost invariably* produce the response (from the sensitive) : ' I sense a quick vibration,' or ' I am in touch with a slow vibration '. Many of the personal characteristics, such as a peculiar way of laughing, of moving the hands, of talking, of sitting, etc., are often imitated, almost to perfection." (My italics). It seems reasonable to take this last statement as a mild exaggeration and to conclude that Hettinger knew the majority of the subjects to some extent but only a few of them very well.

In any case our conclusion is clear : if Hettinger chose the controls in series 5–12, there was ample opportunity for bias to affect his choice.

So much for Hettinger's methods of providing controls. In the course of a necessarily rather tedious discussion I have proposed a large number of possible hypotheses which might explain some of Hettinger's results without resort to the paranormal. Unfortunately these all remain hypotheses. Perhaps Hettinger's most serious fault is that he does not give us enough information to enable us to judge whether or not these hypothetical explanations have any basis in fact. All that we can say on the available data is that any of these things *might* have occurred.

The relevant evidence on the criticisms I have made is all quoted in the above discussion except for five specimen records which are provided in the book, one each from series 2, 4, and 10, and a pair from series 7. Unfortunately none of these comes from the more successful medium F and only one (that from series 4) can really be said to be selected from a group which gives significant positive results. This scanty evidence throws further light on only one of the criticisms that have been made : the possibility of a normal connection between the object and the items given on it. On pp. 67, 68, we are shown a pair of readings, one given on a wallet and one on a ring. The latter is a specifically feminine reading, while the former might apply to a man or woman. This suggests the possibility that the medium gave a feminine reading for a feminine object, and that the female subject selected it and the male rejected it chiefly on these grounds. But only one example cannot tell us much ; this hypothesis remains, like the others, unproved and un-disproved.

I shall now pass on to a few further criticisms before I attempt to sum up the results of this investigation and give a verdict.

(*a*) We have already seen that mathematical considerations alone, regardless of the nature of the items, make it inconvenient to base the statistical calculations on the number of *items* accepted rather than on the number of *readings*. We shall now consider a further objection which makes this procedure unreliable. The danger is that the items of one

* Dr Hettinger has now informed me that it was.

reading may not be independent of one another, so that if the annotator sees, in a record containing originals and randomly selected controls, two items which seem to be connected he can infer that they are both originals. When the controls (as well as the originals) come from a single reading this difficulty does not arise, but another takes its place: if the annotator now sees two connected items he will know that (probably) they are either both originals or both controls, while two contradictory items must be one original and one control. Now the statistical analysis which we would normally apply to the number of original and control items selected makes the assumption that the acceptance of each item is entirely independent of the acceptance of any other. If the items are connected in the way suggested above, this assumption is false. In theory it might be possible for an annotator to group all the items into two groups. He would know that all the members of one group are controls and all those of the other originals, but he would not know which group was which. Suppose he makes a guess. He has a 50–50 chance of doing so correctly. Suppose he succeeds. Clearly this is not significant. Yet if we were working with items only (and supposing there are 12 originals and 12 controls in the record) we would observe only that he had got 24 items out of 24 right, which is statistically highly significant. In practice, of course, the annotator is not likely to be able to make such a successful division of the items into two groups. But as long as we are unable to estimate *how* successfully the grouping might have been made the safest procedure is to work as though it could be done with complete success—*i.e.* to deal not with items but with whole readings.*

Hettinger appears to have overlooked this difficulty entirely. There is no evidence that he attempted to group together as one item items which were clearly interdependent.

(*b*) In series 7, 8, and 9, the method was to send a pair of readings given on two different objects to the two owners of the objects, to see if each could pick out his own. A number of questions arise. In the first place is the specimen pair of records shown on pp. 67, 68, an actual copy of the original records sent out for annotation? The specimen records contain: first a column of items; on the right of this a column for annotations by the owner of one of the objects; and on the right of this a similar column for the owner of the other object. The specimen records are reproduced with the subjects' annotations filled in. The first observation to be made is that in the first specimen record shown, containing items given on a wallet, the two columns for annotations are: for the owner of the wallet, on the left; for the owner of the ring, on the right; while in the second record, containing items given on the ring, the column for the owner of the *ring* comes on the left, and the column for the owner of the *wallet* on the right. In other words, in both records the first (*i.e.* left-hand) of the two columns for annotation belongs to the *true* owner. I do not wish to over-emphasise the significance of this fact. It may well be merely a curious coincidence. But we must recognize the possibility that this procedure

* Of course the effect works both ways. As I have already said, the objections to dealing with items are not that we might obtain a spurious positive effect but that any effect we do obtain, whether positive or negative, may have its significance exaggerated.

may have been regular throughout these three series. If it was, the results must, of course, be rejected as quite invalid. It is almost a certainty that *some* of the subjects, presented, probably several times, with such a pair of records, would make the deduction (or discover by trial and error) that their record was the one in which their annotation column came first. And even if they did not it is probable that there would be a bias (i) for making such a choice, or (ii) for annotating first the reading in which one's own column comes first. In the latter case the reader has only to consider the probable behaviour of an optimistic subject in marking to see that we might expect a bias in favour of accepting the reading first marked.

A second important question is, did the annotators work independently or could the second annotator see what the first had done? If the specimen records shown are copies of the originals, we must presume he could, for otherwise there would be no point in having *two* annotation columns on each record. Now if the second annotator can see how the first has marked, there will be a very strong bias for the second to accept the items, and the reading, rejected by the first. (That this bias was in fact acting is suggested by the otherwise rather remarkable amount of agreement between the two subjects in the specimen records shown.) If therefore the two annotators are cooperating in this way, we must not treat their choice as *two* choices, for almost always it will in fact be *one*. Since Hettinger does not give us the necessary information to enable us to discover when both of two subjects chose the same one of the two readings, the only safe procedure is to assume that they always chose different ones, *i.e.* that the bias effect on the second annotator was complete. On this assumption we must halve the figures given by Hettinger for series 7, 8, and 9. It also looks as though the same correction must be applied to series 6—though it is difficult to be certain since the procedure is not described very clearly. Certainly the correction must be applied to series 5, where the method used was to invite the two subjects to cooperate with one another and pick out their own items only after mutual consultation. The correction does not very seriously reduce the total significance of the whole investigation. The critical ratio for series 4–11 is reduced from 6·1 to 5·6.*

(*c*) Perhaps the most serious source of error arises from the presence at the sittings of someone who knew who the subjects were. It appears that Hettinger was present at every sitting except a very few, and when he was not, the articles were sent by post to the medium. Apparently Hettinger was the only sitter and was his own note-taker. Such conditions are, of course, hopelessly lax. We cannot begin to judge how many times the medium may have changed her mind about a statement in response to a change in Hettinger's facial expression, how many statements may have escaped record, how many may have suffered modifications in being recorded or in the transcription after the sitting when the rough notes of what the medium has said have to be converted into separate, short,

* In applying the correction, figures for any one series which are not integers must be rounded upwards. The presence of odd numbers in some of the figures for series 5–9 is due to the disqualification of some of the readings for being returned *after* the annotator was told which reading was his. The correction as applied makes the conservative assumption that one disqualified reading would have been always paired with another.

independent statements. The possibilities are almost unlimited. It is only when the experimenter and note-taker do not know the subjects that we can safely assume these sources of error either to be absent or to cancel out.

Can we assume, too, that the mediums never knew any of the subjects? We have only Hettinger's bare assertion as evidence—but was he in a position to know? Considering the length of the investigation such an assumption seems excessively risky; it would be extremely difficult to maintain effective secrecy over such a long period. We must remember, too, that the mediums were professionals: they must have known that (the reader will excuse my bluntness) they had secured an almost permanent source of employment. It would be most unwise to overlook the possibility of their gaining normal knowledge of the subjects.*

The complete absence of witnesses in Hettinger's experiments and his failure (so far as we can judge) to take any precautions to prevent normal leakage are both highly unsatisfactory and make it impossible for students of Psychical Research unreservedly to accept his results.

(*d*) Another point on which Hettinger is most unfortunately reticent is his procedure when subjects gave non-commital replies to items. Other investigators have found that, even with the most explicit instructions, there are always a few people who cannot be prevented from putting question-marks, qualifying their replies, or accepting one part of an item and not the other. The reader who has seen a mediumistic reading will find this understandable: it is extraordinarily difficult to say "yes" or "no" to every item. Hettinger admits (p. 35) that he obtained such vague answers, but his only mention of how he deals with them is a statement (p. 35), referring to series 1, that in this series all such answers were disregarded. The wording of this statement, however, seems rather to suggest that this was not the practice in later series. Since it was presumably Hettinger who decided what to do with a doubtful answer and since Hettinger knew which were the original items and which the controls we have here yet another serious opportunity for bias in the experimenter to invalidate the results.

V. Summary of the above criticisms.

I find I have mentioned 33 separate possible sources of error in Hettinger's work. Three of these are mutually incompatible, reducing the number of errors that may have been present to 31. We can tell from the data provided that 6 of these have definitely occurred, and when we modify the statistical methods to allow for them and for 3 others whose occurrence is very probable we are left with a critical ratio for the 9 main series of 5·3 ($P \sim 10^{-7}$), which is still very highly significant. Of the remaining 22, 9 would apply to every series and 13 to only some of the series.

With so many possibilities there is little hope of making a very good guess at which error or errors are likely to have most seriously influenced

* In this connection it is important to note that some of the subjects were unknown to Hettinger, and that articles could apparently be submitted voluntarily, without any request from Hettinger, by anyone who knew of the investigation. The marked difference in success between the subjects, which is noted on p. 126, is perhaps relevant to this and to some of the other criticisms that have been raised.

the results. My own opinion (though little importance should be attached to it), based on the consistency of the positive results throughout the 9 main series, is that the most important sources of error were probably those described above under (*c*) and (*d*), pp. 35, 36, that is, those arising from the fact that Hettinger, who knew who the subjects were, was present at the sittings and also was the final judge of whether a subject should be considered to have accepted an item in a doubtful case.*

It is very unfortunate that Hettinger would not allow me more than a brief glance at his original material. If a thorough examination had been permitted, I have very little doubt that many of the possible errors I have mentioned, which Hettinger does not rule out by the description of the conditions he provides in his book, could have been shown not to have been in fact present, or at any rate, not to have seriously affected the results.

As it is we can only conclude : that any of these errors might have been present (though it is unlikely that they all were), that there is quite insufficient evidence to justify the conclusion that any paranormal factor was at work, and that Hettinger has failed, by a long way, to devise a fool-proof experimental technique for investigating object-reading

Part II

Exploring the Ultra-Perceptive Faculty

Exploring the Ultra-Perceptive Faculty (1941) is a much more readable book than *The Ultra-Perceptive Faculty*. It makes no serious attempt at a scientific proof of the existence of the " ultra-perceptive faculty ", but confines itself to a presentation of a number of striking coincidences selected from an enormous mass of material. These coincidences are both interesting and entertaining, but the possible role of chance is so hard to evaluate that only the most daring psychical researcher could accept them as serious evidence.

I propose to explain the method, to discuss some of the most striking coincidences and their possible relation to chance, to consider some of Hettinger's comments, and then to pass on to some of Hettinger's later experiments, where substantially the same technique is adapted to control experiments in which the role of chance can be estimated exactly.

The basic idea of the method which Hettinger uses in *Exploring the Ultra-Perceptive Faculty* has a great many merits and ingeniously avoids many of the difficulties encountered in the more usual type of experimental investigation of object-reading. The possibility of normal deductions from the psychometrised object is completely ruled out, the danger of the sitter giving away information is very easy to eliminate, and the method adapts itself very conveniently to control experiments in which bias has no chance of vitiating the results. In addition, unlike many experiments in psychical research, the procedure for the agent (for this method is a sort of compromise between object-reading and telepathy) is anything but

* It is perhaps worth mentioning that none of the subsidiary conclusions at which Hettinger arrives are incompatible with the hypothesis that the results have a simple normal explanation. (See, for example, those on pp. 38–9, and pp. 183–5. Note that the former do not arise from control experiments.)

tedious, and very effectively ensures his normal attention to the material instead of—as we so often get—either his bored inattention or his exaggerated concentration. In my opinion a proper control experiment based on this method is much the most satisfactory way of investigating object-reading mediums—provided (and this, of course, is the real difficulty) a medium who finds after one sitting that she has not had any great success does not object that she cannot, and cannot be expected to, exercise her faculty in this way.

Hettinger describes his new method as follows.

"The Subject is asked to obtain any illustrated paper or magazine he fancies, but not to look at its contents until the prearranged time of the test, when he shall start perusing it quite normally, without any effort of concentration, marking on each page the exact time that page was read or the pictures thereon were contemplated. Simultaneously therewith, the Sensitive, miles away, and not informed of the actual nature of the experiment, psychometrizes, *viz.* mentally concentrates on, an object belonging to the Subject and submitted to her in a sealed envelope by the experimenter; the latter writes down the items given by the Sensitive together with the time when they were actually uttered by her."

In comparing the sensitive's impressions with the pictures contemplated, a time-discrepancy of 1 minute in either direction is allowed. In addition an item given by the sensitive may be compared with any picture or article on the page being perused at the time, or on the page facing it. It is a serious fault in Hettinger's presentation of the experiments that he makes no attempt to estimate how many pictures on the average each item may be compared with. It is very hard to arrive at a fair estimate of this figure. Four or five pictures per item would probably be a reasonable guess.*

At any rate, using this procedure Hettinger finds a number of resemblances between the items given by the sensitive and the material being perused at roughly the same time. A large number of these resemblances, including 151 reproductions of drawings, photographs, etc., appear in the book.

After presenting nine of the best of these resemblances (under the heading: *Cases of indisputably correct correspondence between the illustrations and the sensitive's perception*) Hettinger "submits that any one of the examples above given is *per se* more convincing than a significant figure in any statistical calculation". This is an ambitious claim. Let us consider what rational basis it has, and hence how reliable "convincingness" is as a quality of evidence.

The total number of items given in the whole series of experiments was 6,576. We have seen that, on the average, each item may be compared with about 4 or 5 different pictures. The total number of comparisons that were made was therefore in the region of 30,000. Let us consider the best (No. 9). The picture is one of a very large horse with the caption "Biggest horse in the world . . . still growing". The only other things in the picture are the halter, the tassels made by plaiting the horse's mane

* In the brief glance which Hettinger permitted me at his originals I saw one case of an item which could be compared with 10 pictures and one case which could be compared with only 1.

and seen in silhouette, a man holding a rope from the halter and dressed in a dark suit and a trilby, and a farmhouse faintly visible in the background. (This picture is remarkable, among those reproduced, for having almost no detail or other material than its main subject. Very often it is detail or one of the less prominent points on which the resemblances depend.) The medium's comment was: " Some admiration for large cart horses." The question we have to ask is, would one such coincidence be expected to arise by chance (*i.e.* would its occurrence be at least as likely as not) in 30,000 comparisons? Even if the reader feels he can answer this with a definite " No " we still know only that the result is positive. For it to be on the borderline of *significance* we must be able to say that such a coincidence would not be expected to arise once in 600,000 comparisons, and for it to be fairly strongly significant (say a chance probability figure of 1 in 300) we should have to consider 10 million comparisons. I find it difficult to believe that anyone, however much he may dislike, disapprove of, or be puzzled by, statistics can, in the face of these figures, agree with Hettinger that this single resemblance is more convincing than a significant figure in any statistical calculation. It is surely absurd to imagine that our intuition can be reliably used to inform us that such a coincidence would not be expected to occur once in 600,000 comparisons.

The point, of course, about Hettinger's appeal to "convincingness"—or, what comes to the same thing, intuition—is that many people's intuition can be safely relied upon to overlook the number of comparisons from which the resemblances shown have been selected. While no reasonable psychical researcher is likely to be taken in so easily, material presented in this way must completely mislead those who do not possess the habit of critical consideration of evidence. Hettinger, to have been fair, should have emphasised repeatedly that the resemblances he produces as examples are selected from a total of some 30,000 comparisons.

In my view there are about three other resemblances which are almost as striking as that quoted (Nos. 31, 62, 137). In these the actual connection is quite as good, if not better, but the pictures contain a good deal of other detail and one could imagine a number of other possible items which would have applied equally strikingly. The remaining resemblances range from the good, such as the appearance of the word " dairy " once in the item and twice in the corresponding picture (No. 1)*, to the very far-fetched, such as the item " peculiar noise going on here " given when the subject was reading " a short paragraph concerning 500,000 cats " (Hettinger's comment is, " A rather intriguing case of ' ultra-acoustic ' perception.").

It does not seem worth-while to discuss the various supplementary experiments that Hettinger carried out using the same general method, such as " association tests ", tests without articles, a " relay " experiment, etc., since there is no serious evidence anywhere for the presence of paranormality.

* In considering the extent of this coincidence, the question one must ask is not, of course, " How many items can be expected to contain *the word ' dairy '*? " but, " How many resemblances *as good as this* can be expected in so many comparisons? " There would clearly be far more of the latter than the former, so that to base an estimate on the former question—as one is tempted to do at one's first approach—would lead to a gross overestimate of the significance of the coincidence.

Before we leave the discussion of this work we ought perhaps to consider Hettinger's own attitude to it. Discussing the question of control experiments, Hettinger writes :

" The question arises, however, whether an extensive statistical investigation of the magnitude of the writer's earlier experiments would add anything further to that which has already been attained, *viz.* proof of the probable existence of an ultra-perceptive faculty. Judging from a cursory ' control ' comparison between a few paired illustrated papers, as referred to above, in the writer's opinion the only further attainment that could be expected from the application of the control method to ' illustrated paper ' experiments would be a higher significant figure.*

" On the other hand, he feels that the very frequent occurrence of exact correspondence in substance and time during a one-hour test—the accuracy of which correspondence is sometimes quite staggering, because of the very specific character of the items perceived—is *per se*, from a psychological point of view, far more convincing than statistics, which at the most can only prove a probability. Indeed, the present experiments seem to have a tendency that way, and, because thereof, the pictorial series method which has been conceived may, more especially in view of its constructive potentiality in further research work, be destined to supersede the statistical method, now that the latter has fulfilled its main function, *viz.* in establishing the probability of the existence of an ultra-perceptive faculty. . . .

" The writer was prompted to make these comments because the method was conceived, *inter alia*, also with the object of providing a means of investigation which would carry conviction directly, instead of having to rely on lengthy statistics undertaken by others and on what the investigator and his critics may have to say about them respectively. Nevertheless, seeing that chance coincidence is also an indisputable possibility, for those who may desire to investigate the matter afresh from a statistical point of view the writer highly recommends the ' psychometric pictorial series method ' used in the present research. As already hinted, he believes that the significant figure of probability will be found substantially higher than those which he himself obtained in his statistical experiments, in which he had to rely on the memory of, and recognition by, the Subjects, instead of on some definite means of identification."

Hettinger then goes on to offer a short control experiment of his own. A number of objections could be raised to this but need not, for Hettinger himself comments :

" Since the comparison [of the pictures with the medium's statements] was made by the writer himself, who knew which was the ' right ' and which the ' control ' paper, he does not wish the above results to be looked upon as a statistical proof, in the sense of his earlier experiments. Anyone wishing to repeat the present tests with a view to providing further statistical proof, rather than exploring the subject for the purpose of ascertaining further facts, will have to use a separate adjudicator, as above explained."

* Hettinger seems to have overlooked another possibility : that there might be *no* further attainment, and no confirmation of the earlier figure. To argue the presence of paranormality in these experiments purely on the basis of the significant figure obtained in the totally different earlier work is unreasonable.

The above quotations make a fair sample of Hettinger's opinions of the purpose of the work. " Statistical proof " is not offered, partly because it has already been provided in the earlier book and partly because " the very frequent occurrence of exact correspondence in substance and time during a one-hour test* . . . is *per se*, from a psychological point of view, far more convincing than statistics, which at the most can only prove a probability ". The implication seems to be that, whereas statistics can only prove a probability, intuitive estimation can provide certain proof. This is an absurd claim. There are only two basic differences between intuitive and statistical methods of estimation : one, that intuitive methods are easier to apply ; two, that their results are much less objective and much less reliable. Intuitive methods are valuable for providing pointers to what is going on and for suggesting possible new lines of experiment, but they only have a place in a *proof* when the evidence is so clearly and indisputably significant (and by " indisputably " I mean, literally, that no intelligent person can or does dispute it) that their inherent inaccuracy may safely be neglected. It is difficult to believe that anyone, however personally convincing he may find Hettinger's results to be, could claim that their significance is indisputable in this sense.

But we need not labour the point. Their significance *is* disputed—if only by the present writer ; and where there is any disagreement we cannot afford to trust our intuition when perfectly sound objective methods are available. As scientists, therefore, we are forced to set aside Hettinger's qualitative work with an agnostic shrug and pass to the study of objective control experiments.†

Before we leave *Exploring the Ultra-Perceptive Faculty*, however, it would perhaps give the reader who is not familiar with the work a better idea of the quality and type of material that appears in this and later investigations by Hettinger if we considered two or three examples taken at random from the resemblances reproduced in the book.

We have already seen one of the " cases of indisputably correct correspondence ". The next chapter after these is headed, *Cases of obvious correspondence with some addition or slight distortion.* The first example is a picture of a boat (perhaps a small liner) over the side of which hangs a rope ladder. Standing on the bottom rung is a man in a diving suit (connected by a pipe to the deck) tentatively putting one foot in the water. Two men, one in a diving suit, are looking down at him from the deck. The background contains only clouds and sea. The caption is, " How's the water? " The medium's statement is, " Something to do with water. I cannot move for water ; something overflowed." Hettinger comments : " The correspondence is exact, but the addition 'overflowed' does not apply. Maybe, the Sensitive vaguely perceived a man (the diver) moving in the water‡ and the pipe, which she inferred was an overflow."

* This seems to be a serious exaggeration. Hettinger makes no claim elsewhere that *exact* correspondences are *very frequent* in *one* test.

† I have confined myself to consideration of chance as an explanation of the results reported in this book. If the results had been really striking, other possible normal explanations—in particular, perhaps, the possibility of normal knowledge by the medium of the material to be perused by the subject—might have had to be examined.

‡ The picture shows him with only the toes of one foot in the water.—C. S.

The first picture in the next chapter, *Cases of correspondence as regards sensations, feelings and emotions*, is a drawing of a small dog with long ears sitting down holding a handkerchief to his nose and crying. The caption is, "Hector says: 'I don't think I ought to try to be funny today.'" The medium's impression is, "Something I want to test in my hand; using pressure too." Hettinger comments: "Notice the one paw of 'Hector'." (Hector is using one paw to hold the handkerchief to his nose.)

Perhaps one further example will suffice from those headed, *Cases which may seem "far-fetched"*. Hettinger's view of these cases is that, although none of them is "*per se* sufficiently convincing", they are, compared with the more striking resemblances, "perhaps more important as regards the exploration of this baffling form of cognition, with a view to tracing its fundamental factors." The first example from this group is a picture of a naval officer wearing a large macintosh, standing with a pair of field glasses in his left hand and his right hand apparently in his pocket. There are quite a number of other details. The medium's impression is, "Someone has something the matter with the right hip; puts hand on to be able to walk."

It will be seen that Hettinger's interpretation is always exceedingly optimistic. As in his earlier book, the possibility that any particular success may be due to chance is not for a moment considered.

I believe, nevertheless, that the average reader of *Exploring the Ultra-Perceptive Faculty* will get the impression that the examples reproduced could not easily be accounted for by chance alone. There is no doubt that the cumulative effect of so many examples is extremely convincing. But whether this subjective experience of conviction is due to the actual presence of an extra-chance factor, or whether to the difficulty of bearing in mind the very great size of the selection factor, the large number of different things contained in one picture, and the variety of meanings that might be attached to most of the medium's impressions, is a question which can only be answered when we have before us the results of a properly designed objective control experiment. It is a thousand pities that Hettinger allowed himself to spend such prodigious amounts of time and effort on purely qualitative experiments before such objective evidence became available.

PART III

THE TRANSATLANTIC EXPERIMENTS OF 1945–6

Hettinger's first serious, published attempt at applying a control method to his pictorial technique was carried out in conjunction with the American S.P.R. between February 1945 and July 1946, and is reported under the title, *Psychometric Telepathy Across the Atlantic*, in the *Journal* of the A.S.P.R. for July 1947 (Vol. XLI, No. 3).

The control methods are by no means satisfactory, and the work is described by the Editor as no more than a pilot study, so that it should not be necessary to make a thorough analysis.

The experiments consist of 16 one-hour tests using the "pictorial method" and the same sensitives as in all Hettinger's earlier work. Tests 1 to 9 use one sensitive at a time, tests 10 to 16 use both simultaneously.

A complicated method of " evaluation " (*i.e.* of assigning different scores to different degrees of resemblance) is used which attempts to be objective, but is clearly bound to contain some arbitrariness.

No justification is given for the use of the formula $\sqrt{\frac{pq}{N}}$ for the standard deviation of the fraction $\frac{S}{N}$ from its expectation $\frac{1}{2}$, where N is the total score on the originals and controls and S the score on the originals only. Since the score on an item, instead of being fixed at unity, is mathematically capable of taking almost any value it is difficult to see any reason other than habit for Hettinger's use of this formula.* Hettinger continues the unjustifiable procedure (see above, p. 22) of taking for p and q their empirical values $\frac{S}{N}, \frac{C}{N}$ instead of their theoretical value $\frac{1}{2}$. (Note also that the formula for the score of an item or group of items $Sc = \frac{I_f \times M_f}{V_t}$ given on p. 101 is upside-down. It should be, and is in fact used as, $Sc = \frac{V_t}{I_f \times M_f}$.)

The experiments were divided into two series. In series 1 (tests 1 to 10) the procedure was the same as that used in the experiments reported in *Exploring the Ultra-Perceptive Faculty*. A copy of a magazine was perused by the subject or agent (Mrs L. A. Dale, Research Associate of the A.S.P.R.) in New York during the hour of the test while a sensitive in London gave object-reading impressions on specimens of the subject's handwriting. The control method used was to compare the sensitive's impressions not only with the magazine that was being perused when they were given but also with another issue of the same magazine.

Hettinger's own assessment of the results of this series is highly significant. We cannot take this as very serious evidence, however, since Hettinger knew which were the originals and which the controls. As he says himself (p. 97): " The main difficulty is assessment, in which the personal element plays an important part. If, when a control method is used, the assessor knows which is the actual material and which is the control material, the criticism may reasonably be advanced that he was influenced by a bias one way or the other. If an inexperienced assessor works ' blind ', and if the impressions of the sensitives are not absolutely concrete hits, he begins to ponder and waver, finally deciding one way or the other, and laying himself open to the criticism that his assessment was of an arbitrary character. If, as has been found to be the case, a large number of the impressions are not concrete hits, but rather distortions, associations, inferences, allusions, relevant interpretations, etc., an assessor not fully conversant with this specific kind of investigation simply ignores them. . . . I must confess that I myself, although I have spent hundreds of hours working with this kind of material, often have difficulty in deciding whether or not to accept a given impression as an indisputable hit. . . ."

Apparently dealing with this difficulty about a judge who knows the

* For the correct formula for the standard deviation when " evaluation " is used, see p. 23, footnote.

originals from the controls Hettinger quotes some figures obtained on an assessment by Mr E. P. Gibson, of tests 1, 2, 3, 4, 5, 6, and 10. He does not say explicitly that Gibson did not know the originals from the controls, but this is at any rate implied from the context. Other important details, however, are omitted. The subject in all these experiments notes on the magazine the times when each page or picture is being perused. In Gibson's control assessment we are not told whether dummy notes of similar times were put in the control magazines or, if so, exactly what steps were taken to ensure that these were distributed with the same frequency as in the originals. Other criticisms could be brought against this assessment, but need not, for Hettinger admits that it is unsatisfactory and inconclusive. Moreover, the figure obtained by Hettinger's method of calculation is only twice the standard deviation (corresponding to a chance of 1 in 20) which is only just significant.*

A further assessment was made " by a young student who was entirely unfamiliar with this sort of work ". She obtained nearly equal scores on originals and controls.

It seems reasonable to conclude from these results (*a*) that there is no substantial evidence for the presence of a paranormal factor, and (*b*) the process of assessment is very far from objective.

In view of these inconclusive results a new control method was devised for tests 11–16. This method was, if anything, even more unreliable than that used in the first series, but it is of considerable importance because a slight modification of it provides much the most satisfactory method of experimenting with Hettinger's pictorial technique. The method was to prepare two scrapbooks of 60 pictures each, randomly drawn from magazines, one to be used as an original and one as a control. The pictures in the original were contemplated for one minute each during the hour of the test. (The same scrapbooks were used for all six tests.) At the end of the series of tests the two books were sent to Hettinger who without knowing which was which, compared the pictures in each book with the items given by the sensitive at the corresponding times. He found 362 items applicable to Book II and 264 to Book I. These figures he treats in the usual way, to obtain a significant difference in favour of Book II. This book was in fact the original.

It was not until after the experiments had been completed that it occurred to anyone that this experiment provides a chance of no more than evens for the working of the " Ultra-Perceptive Faculty ". Mrs Dale wrote to Hettinger as follows : " The hypothesis is (or could be raised) that *you* performed, so to speak, *a single extrasensory act*, and by virtue of this learned which was the correct scrapbook.† From then on, scoring of

* This figure is unreliable for the reasons given above. Applying Student's *t*-test to the figures provided we get a quite insignificant probability, but it is clear that, with so few figures and such a large variance, this is a case in which the *t*-test gives very conservative results.

† The Editor, in a footnote to Hettinger's article, stresses that " great pains were taken to subject the two books to the same amount of wear and tear, etc., so that there would be nor normal way of telling which one had actually been used in the experiment ". Even if we could feel sure that this very difficult aim was achieved it seems hardly necessary to postulate an " extrasensory act " to account for an event which has a chance probability of just 1 in 2.

individual items could be coloured by this bedrock paranormal knowledge. In other words, the sixty pictures scored are not actually independent items and are therefore not properly evaluated by the CR (critical ratio) method. It is easy to be wise after the event, but I realise now that I should have sent you the sixty pictures actually used, each paired with a control picture—rather than the series of pictures prepared in scrapbooks. Then there would be no question but what you would have had to make sixty independent judgments. We now suggest that you return the picture material, and that we ask Mr Gibson to re-evaluate the data when the pictures are removed from the scrapbooks and presented to him in a series of sixty paired items. Whether or not the actual stimulus picture is to be the first or the second of the pair can be determined by a suitable random method. The temporal order of the pictures from one to sixty would of course be retained."

Hettinger, in his reply, wrote as follows : " I have received your letter of the 13th inst., and I must confess that I was rather taken aback. I can see your point of view, however, although I am afraid that whatever one may do or say on the statistical side in connection with this kind of material, one cannot escape both the difficult problem of assessment and some form or other of criticism, since evaluation concerns not only items of a concrete character, but also subtle associations.

" The assessment of the last series was done picture by picture, and without any knowledge, perception, or guessing as to which of the two books had been used in the experiment. The high scoring was not due to any predilection on my part for one book rather than the other, but to the accumulation of identical, similar, or obviously distorted associated items (sensitive's impressions) in connection with one and the same picture. . . ."

However, the proposed assessment was carried out by Gibson and a score of 98 was obtained on the *originals* and 126 on the *controls*.

It is quite clear from these results that we cannot safely draw any conclusions as to whether or not a paranormal factor is at work. Hettinger's conclusion, " that these tests as a whole provide substantial *prima-facie* evidence of transatlantic telepathy," is certainly far too optimistic.

The complete lack of agreement between Hettinger's and Gibson's assessments of series 2, strongly suggests that Hettinger was, in fact, influenced by bias in the way suggested by Mrs Dale.* But, whether or not this was so, one thing stands out as self-evident: the process of assessment is so unreliable and subjective that it is quite unsafe to put the slightest trust in it unless it is applied to an objective control method.

The greater part of Hettinger's article is taken up by reproductions of some of the better resemblances. Sometimes the original pictures are both reproduced and described, sometimes they are only described. It is clear from the former cases that Hettinger's descriptions are sometimes biased in the direction of suggesting a greater similarity between the pictures and the medium's impressions (see, for example, figures 1 and 2). Nevertheless, there is no doubt that some of the resemblances reproduced are qualitatively very striking. Unfortunately, however, we are given no idea of the total number of comparisons from which these are drawn (a

* With or without the " single extrasensory act ".

very rough calculation puts the figure at about 10,000; but we are not given enough information to enable us to make any but the vaguest estimate). Nor are any of the resemblances obtained on control pictures reproduced. In view of the results of Gibson's assessment it seems very probable that many such control resemblances would appear quite as striking as the originals we are shown, and that, had they been reproduced, their cumulative effect would have completely destroyed any intuitive conviction the reader may have had that the original pictures bore a significant similarity to the medium's impressions.

In this case we can only conclude once more that Hettinger's method of presenting his material is utterly misleading. It is astonishing that, when the control experiments show so clearly how narrow and elusive is the margin between the successes on the originals and those on the controls, Hettinger can continue to consider it a fair method of presentation to reproduce a number of resemblances, selected from a total of unspecified size, and to overlook entirely the resemblances obtained on the controls.

PART IV

HETTINGER'S EXPERIMENTS WITH THE PRESENT WRITER

In his report on the transatlantic experiments Hettinger stated that in view of the unsatisfactory nature of the statistical results he was contemplating a further series of transatlantic experiments in which he hoped the statistical " difficulties " might be overcome. The Editor also mentioned that a third series was planned and pointed out that the first two series could not be considered as more than a pilot study. Feeling that it would be unreasonable to judge (and perhaps condemn) the first two series as a self-contained piece of work without waiting for the promised third series, I enquired of Hettinger in January 1948 when this series was to take place. He replied that no further control tests were now contemplated.

The position at this stage was most unsatisfactory. Hettinger seemed to imply in his article that the work of assessment demands a skilled and experienced judge. This is a fairly reasonable claim, but it leaves us completely in the air and quite unable to draw a definite conclusion from the experimental evidence. The only objective control experiment we had was assessed by an inexperienced judge (Mr Gibson): the only experienced judge we had (Dr Hettinger) had never assessed, and did not contemplate assessing, an objective control experiment. Apparently we were at an impasse.

Clearly, with the increasing attention that was being paid to Hettinger's work (in Sept., 1947 he addressed the British Association for the Advancement of Science, and in Sept.–Oct., 1947 the *Daily Express* ran a feature on some experiments conducted by him), this situation was most unsatisfactory. It was essential to be able to say definitely whether or not there was any evidence for the paranormal in his results.

When, therefore, Hettinger offered to give me two sittings, using the scrapbook method with me as subject, I resolved to take this opportunity of carrying out a control experiment with Hettinger as judge.

It was arranged that for each test I should make up a scrapbook of 60 pictures from magazines and look at the pictures, in order, for a minute each during the time of the test. After each test, Hettinger would send me a copy of the medium's* statements and I would make my own annotations and assessment. These I would return to Hettinger with the scrapbook, and he would make his assessment, finally returning everything to me.

The control procedure I decided upon was as follows :

Before each test a collection of 90 drawings was made from copies of *Lilliput*.† These were thoroughly shuffled, and 60 were randomly drawn and placed in the scrapbook, one between each leaf, without being stuck in position. These pictures were contemplated during the test. Because of the faint possibility of the medium's faculty working precognitively, the book was left with these pictures in it for several hours after the test. A random selection‡ of 30 of these 60 " originals " was then removed and the 30 pictures left over from my original collection of 90 were substituted as controls. The 30 originals which had not been removed and the 30 controls were then stuck in the book. In this way there could be no normal means of deducing from the pictures or the book which were the originals and which the controls. (No difference in " wear and tear " could arise : each page of the book was turned over the same number of times, and the pictures were not handled during the test.§)

Since I knew the originals from the controls there seemed little point in my making annotations. Mr A. M. Western, who did not have this information, very kindly undertook to make the annotations and assessment instead, and it was his results that I forwarded to Hettinger. The possibility also occurred to me that if Hettinger's assessment gave negative results, the criticism might be made that it was not the truly free, unbiased judgment of an experienced assessor, but was influenced by the results of Western's assessment, which Hettinger would have before him while he made his own. I therefore asked Hettinger if he would make his assessment quite independently of Western's. This he agreed to do, and I sent Western's assessment in a separate envelope, which Hettinger did not open until he had completed his own.

In making the assessment it was decided to divide the resemblances into three classes : A, very striking ; B, good ; C, far-fetched but possibly significant. It was found that Western classed about seven-eighths of his resemblances C, one eighth B, and none A. Hettinger classed about one half C, one third B, and one sixth A, with the reservation that " practically all those I marked C are in my opinion *applicable* and *not* far-fetched, but . . . not as good as those marked B ".

* The medium for both these tests was Miss F. Fallows (" F ").

† Bearing in mind the limitations set by social convention on the medium's statements, I thought it best to omit all pictures containing any appreciable sex-interest.

‡ In test 1 the determination of this selection was made some days before the test ; in test 2, the day after. In both cases it was truly random and quite independent of the actual pictures.

§ As this short experiment would have been, in any case, inadequate as a completely conclusive and formal proof of paranormality, I did not consider it necessary to have my procedure witnessed. The real purpose of the experiment was to test objectively the reliability of Hettinger's judgment, so that no further precaution seemed necessary beyond those usually taken in experiments in psychology.

Before considering the results of the control experiment it is important to note that Hettinger, speaking of *all* the results *before* he knew about the controls, wrote : " I consider them quite good and up to the standard of the other results." (This was in reply to a comment of mine on the vagueness of the resemblances.) And again : " Apparently you are a very good subject so far as psychometry + telepathy is concerned. I am judging this by comparison with many other subjects who have participated in my experiments ; also by the character of the impressions received, which seem to cover a wide range."

We have, then, Hettinger's unequivocal statement that the results of these tests, on the controls as well as the originals, are qualitatively quite as convincing as those obtained in others of Hettinger's experiments—experiments which Hettinger considers to provide " substantial *prima-facie* evidence " and to be " *per se* more convincing than a statistical figure in any calculation ".

Now let us consider the objective results.

The following tables show the number of pictures eliciting at least one hit.*

1. *Annotation by Hettinger*

	Test 1				Test 2			
	A	B	C	*Totals*	A	B	C	*Totals*
Originals	3	4	11	18	4	4	5	13
Controls	3	7	8	18	1	8	5	14
Totals	6	11	19	36	5	12	10	27

2. *Annotation by Western*

	Test 1				Test 2			
	A	B	C	*Totals*	A	B	C	*Totals*
Originals	0	0	11	11	0	1	9	10
Controls	0	2	9	11	0	3	14	17
Totals	0	2	20	22	0	4	23	27

There is clearly no appreciable indication at all that the originals tend to do better than the controls. The only favourable figure is four A's on the " original " pictures of Test No. 2 of Hettinger's annotation against one on the controls. But this is almost balanced by a larger deviation the other way with the B's, and is in any case quite insignificant.

I communicated these results to Hettinger and proposed a further assessment of the 30 originals in each test which had not yet been used in the assessments, but he replied that he did not believe that any useful purpose could be served by any further assessment or scoring.

* The reason for working with the number of successful *pictures* rather than *items* is that the former are known to be independent (or rather, strictly speaking, only dependent in a random way) the latter may be interdependent, in the sense explained above, p. 34. If we are to use the ordinary statistical formulae we must be certain that the units from which the scores are built up arise from mutually independent events.

In a subsequent talk I had with Hettinger he gave an impression of complete confidence in the presence of paranormality in these tests. But one reason, he suggested, why such experiments gave negative results might be that the controls were not sufficiently different from the originals. With such a wide range of resemblances as he permits, any two pictures drawn at random will nearly always have some element in common; indeed it is a very difficult task to find two pictures which have nothing at all in common. Instead of selecting the controls randomly, therefore, I should have carefully chosen each one so as to be as different as possible from the original it replaced.

While I fully appreciate the soundness of this criticism I must point out that it leads directly to the conclusion that any given picture has so many elements which are potentially capable of giving rise to mediumistic impressions that chance resemblances between an impression and a picture must inevitably be very frequent, and, in general, quite indistinguishable from genuine paranormal resemblances; so that this criticism merely gives added weight to the argument that a mere enumeration of resemblances obtained without a control is a totally unreliable way of evaluating the results. That chance resemblances between pictures are so common means that chance resemblances between impressions and pictures must be common. It follows that control methods are absolutely essential, and that even when controls are used and the greatest care is taken to choose controls each as different as possible from the original it replaces, any paranormal effect must be greatly diluted with chance resemblances.

The conclusion that must be drawn from these two tests is not that there is no paranormal factor at work in them or in any of Hettinger's work, but that Hettinger has not yet devised a technique for adequately demonstrating the "ultra-perceptive faculty", and that his subjective judgment as to whether coincidences are paranormal or not does not tally with the results of objective tests. Since the conclusions to be drawn from the work described in Hettinger's second book and his A.S.P.R. *Journal* report depend almost exclusively on the reliability of his judgments in this respect, we must conclude that very little weight can be attached to any of the evidence put forward in these reports.

Hettinger's ingenious and original "pictorial method" has many advantages. In particular it seems that it might be peculiarly well suited for what Hettinger calls "exploring the ultra-perceptive faculty", that is, discovering what *sort* of resemblances are most likely to arise paranormally. But one thing is certain: nothing of value can come from it unless it is combined with a technique for estimating the role that can be played by chance in bringing about resemblances. There can be no possible reason for relying on subjective judgments where the simplest of checks is available. Nor is it possible in this case to put forward the defence—which may, perhaps, be valid on other occasions—that control experiments are misleading or unnecessary or insensitive, or that they mask important facts or disturb the psychic faculty or lead only to arguments about statistics. All such arguments miss their mark because we are not proposing to modify the free uncontrolled experiments or to use a new method. All that is necessary in effect is to do a few additional experiments in which chance is known to be the only operative factor. Control experiments in Hettinger's work would not involve a change of method, but merely an addition to the existing method. What objection can there be to this addition? Nothing could be simpler to carry out; the statistical assessment is the most straightforward there can be; and there is no conceivable possibility of information being *lost* by getting the judge to assess a number of extra pictures. In no other science would an experimenter insist on relying on his subjective judgment when a perfect objective check is available.

It is a great misfortune to psychical research that the work of such an energetic investigator should have been deflected from the path of scientific method into a field where guessing is the only form of judgment, and measurement is replaced by the bare presentation of unrepresentative samples of the data.

Editorial Note

[*Complete candour between psychical researchers engaged in the same investigation is of the utmost importance and has been a cherished tradition of the Society. There are some kinds of experiments, intended to check previous work, which are in other respects desirable, but are difficult to carry out with strict adherence to this principle. In designing the interesting experiment described in the foregoing paper Mr Scott appears to have found this difficulty unsurmountable.*

All psychical researchers should bear the principle in mind, and take all possible care, when planning their experiments, to avoid any infringement of it.

After completion of the experiment Mr Scott offered to show his paper to Dr Hettinger, who raised no objection to its publication.]

REVIEW

Matter, Mind and Meaning. By W. Whately Carington. With a preface by Professor H. H. Price. Methuen, 1949. Pp. xx, 257. 12s. 6d.

Mr Whately Carington died on 2nd March, 1947, a great loss to psychical research and to our Society, of which he had been an active member for some thirty years. Up to the last few months of his life, when he became incapacitated through illness, he was engaged in writing a book on philosophy, entitled *Matter, Mind and Meaning*, to which he attached great importance. At the time when he ceased to be able to do further work on it he had completed about two-thirds of it. After his death the manuscript passed into the hands of Professor H. H. Price, who has prepared it for the press and contributed an appreciative and most interesting preface and occasional footnotes.

Professor Price says that the first five chapters were almost complete in their final form. The main body of the book now ends with the sixth chapter, entitled *Mind and Matter*. Of this there existed only two very brief alternative versions, which Professor Price has tried to conflate. The editor has added, from papers left by the author, three appendices. One is a more popular form of the argument in Chapter II on the failure of metaphysics. The other two contain what are probably Carington's latest thoughts on the notion of life after death and on the nature of precognition.

Whately Carington's final philosophical position is a form of neutral monism. Minds and matter are complexes composed of constituents of the same kind, viz., sensa (extra-somatic and intra-somatic) and images, arranged in characteristically different constellations. He calls these constituents " cognita " or " cognizables ". His explicit reason for holding this view is that he thinks that it is necessitated by the theory of meaning which seems to him to be the only one acceptable. This is the doctrine that every intelligible sentence which is not tautological must be in principle verifiable or refutable by sensation or introspection.

Neither of these views is original, and Whately Carington fully acknowledges his debt to Earl Russell for the theory of neutral monism, and to Messrs Ogden and Richards and Professor Ayer for the theory of meaning and the contention, based upon it, that metaphysical sentences are meaningless. " Metaphysics " is described as an attempt to infer true and intelligible propositions about the actual world from *a priori* premises. Actually I think that few distinguished metaphysicians have attempted to argue from premises which are *all* of them *a priori*. The description might perhaps fit Spinoza ; it certainly does not fit Leibniz, or Kant, or McTaggart, or Alexander, or Whitehead.

I do not consider that the book is of any great interest or importance as a contribution to philosophy. The main value which can be ascribed to it from that point of view is as a lively popular introduction to the weightier and more technical works of Ogden and Richards, of Professor Ayer, and of Earl Russell on the topics which it treats. I found Professor Price's introduction, which contains a number of bold and fruitful suggestions, considerably more interesting than the book itself.

What gives to Whately Carington's posthumous work such interest and importance as it possesses is the fact that he had an expert firsthand acquaintance with the established results of psychical research, that he fully realised that they constitute a challenge to philosophers, and that he had himself already made a bold and original speculation as to the *modus operandi* of telepathy. Now Earl Russell and Professor Ayer (in common with the vast majority of western philosophers) have never shown the least interest in, or acquaintance with, the results of psychical research; and they have never shown any sign of realising that philosophy ought to take account of them and try to fit them into its picture of the world along with the facts of normal sense-perception and normal interaction of mind and matter.

Whately Carington tries to show that the occurrence of telepathy and clairvoyance is by no means paradoxical on the neutral monist theory of mind and matter; and he maintains that their existence presents extreme difficulties to other theories. It is to be presumed that he intended to try to show that well attested paranormal *physical* phenomena, both sporadic and experimental, fit easily into the neutral monist world-picture. This, as Professor Price points out, he did not live to accomplish. Price himself makes two very interesting alternative suggestions on this topic in his prefatory essay. One is an extension of the notion of ideo-motor action. The other is an extension of the notion of telepathically produced hallucination.

The book contains a large number of misprints, though none of them is serious enough to obscure the obvious meaning. I have noted no less than eight, and there may well be others which I have overlooked.

C. D. BROAD

PROCEEDINGS OF THE SOCIETY FOR PSYCHICAL RESEARCH

PART 178

OBITUARY

Miss Isabel Newton

Miss Newton's services to the Society and to psychical research were exceptionally long and in many ways remarkable. They began in 1903 when, being on the lookout for a secretarial post, and having no previous interest in psychical research, she was moved to apply for a vacancy on the staff of the S.P.R. A fortunate day, that, for the Society!

Alice Johnson had a few months previously been appointed Organizing Secretary, and Miss Newton was at first her assistant, becoming Secretary in 1908. This post she held until her retirement in 1938, when the Council signalized their appreciation of her services by placing her name on the very short list of Honorary Members.

From 1916, when Alice Johnson retired from the position of Research Officer, until 1938 Miss Newton was the mainspring of the Society's office organization and gained the confidence and affection of all the members with whom she came into contact. She also gained a practical knowledge of psychical research that few of her contemporaries could rival.

After a year of well-deserved leisure, on the outbreak of War in 1939, Miss Newton at once offered to assist the Council in carrying on the Society's work. This offer was gratefully accepted. Very wide emergency powers were delegated to her and to myself as Hon. Secretary. She was unremitting in her attention to her new duties, and it was largely through her devotion to the Society's work at this time and the confidence of our members in her, that the heavy decline in numbers which took place in the early years of the War was arrested, and that by 1942 a slight, at first a very slight, increase was observable. When she retired from the Council in 1944 she consented, notwithstanding indifferent health, to continue to serve on the House Committee.

Her career was an outstanding example of the triumph of mind and character over the serious initial disadvantage of an education which did not include any time at college or any specialized training whatever. But sympathy, personal loyalty, freedom from all trace of egotism and ability to learn from others, were part of her innate endowment.

How much she learnt from close association with Alice Johnson may be seen from her tribute to the latter in *Proceedings*, vol. xlvi, pp. 19–22. Superficially the two were very different. Alice Johnson was small in build, austere in appearance and reserved in manner : Miss Newton in all respects the opposite. But there were many underlying points of similarity, and many of the things Miss Newton writes of Alice Johnson might well be applied to herself, as when she mentions " the consideration she showed for everyone with whom she was in contact, directly or indirectly ". Again, " I remember her impressing upon me that, in replying to correspondents, it was our duty to *help* them to understand, and telling me that she found it helpful to assume, for that purpose, that they were ignorant and rather stupid. . . . Her devotion to the interests of the Society impressed me greatly. She had a great capacity for sympathy, and a gift for expressing it in letters simply and sincerely."

I have spoken of Miss Newton learning from Alice Johnson, but in these matters she needed little teaching. All she needed, and that she got, was an example to encourage the development of her own natural tendencies. She did not possess that strong grasp of complicated detail, or that ability for marshalling it into ordered hypotheses, that was characteristic of Alice Johnson. During her term of office various specialized forms of psychical research successively engaged a large part of the Society's activities. First came the investigation of "the S.P.R. group of automatists ". Then, after the First War, came the heyday of physical phenomena, and of recent years the great extension of quantitative research. All these required technical knowledge which, with her other duties, she could not have been expected to acquire. She contented herself with a good general knowledge of the work being done.

But it would be a mistake to allow her long and admirable record in administration to obscure the amount of research which fell to her lot in the course of her work as Secretary, or the ability with which she handled it, or, again, the extent of the knowledge she gained thereby. If a practicable arrangement could be made whereby some person, chosen by lot from among our members, Council members included, undertook for a week to answer all the letters received in our office, and to take all the interviews, the fundamental problems of psychical research would be much more widely and thoroughly understood than they are. Whoever does work of that kind has under his observation a garden plot, as it were, in which the sprouting of every kind of psychic phenomenon, real and spurious, may be watched. Miss Newton cultivated this plot for a lifetime, and in so doing developed an uncanny flair for distinguishing weeds from flowers as soon as the first green shoots appeared.

To do the work as well as she did it required reserves of sympathy, patience, tolerance and tact that were inexhaustible. I have seen her emerge from an interview that would have prostrated most persons, and turn quietly to the next piece of work, simply remarking that there were some very queer people about. Calmness did not, I think, come easily to her. During her last illness she told me that in early days she often found herself getting vexed, and had to remind herself that Isabel Newton was one person, and the Secretary of the S.P.R. another. Doubtless the example of imperturbability set by Mrs Sidgwick helped her here.

And her keen sense of humour must also often have come to her assistance. She was not particularly witty, but had a very sharp appreciation of the many oddities of mind and temperament with which her daily duties made her familiar. This was apparent on even a short acquaintance, but it was only after I had known her well for several years that I was favoured with an exhibition of her remarkable talent for mimicry. This, it should be added, was never unkindly in intention.

As the result of her long and varied experience, she came to form definite opinions, but she never obtruded these in her letters or interviews. Any enquirer could be sure of getting from her a well-informed, impersonal, objective statement as to the points at issue in any debateable matter, with good guidance as to the literature most likely to be helpful to him in forming his own conclusions. It was typical of her independence of mind that, although her admiration for Mrs Sidgwick and Alice Johnson was almost unbounded, she continued to hold her own much more sceptical views on the question of survival. She was candid but unobtrusive in stating her own opinions, and it was not until I had worked closely with her for many years that I discovered how deep-seated her scepticism was.

Nor was it merely in opinion that she was independent. No one could have accused her of the autocratic temper sometimes attributed to her predecessor, but she could act forcefully on her own initiative when she thought the Society's interests were at stake. On one occasion, when there happened to be already a good deal of friction in the Society, she had reason to believe that a senior and much respected member was intending to include in a public address a passage that would have been taken as a personal attack on other members, several of whom would probably have resigned. She asked to see the manuscript of the speech, sought an interview with the speaker, and after some argument procured the cancellation of the passage. Later she thought it proper that as Hon. Secretary I should know of her action, but very few of our members ever knew how her discretion and tact had averted a crisis.

It was through her initiative that the Society acquired its premises in Tavistock Square. When the lease of its former rooms in Hanover Square expired, the Council were for some time at a loss where to go. Various quarters were offered them, each more inconvenient than the last. But Miss Newton was not discouraged. She gave herself no rest till she had found what seemed needed, and on the basis of the careful estimate she prepared, she persuaded the Council that it would be more economical as well as in every way more convenient for the Society to have a house of its own rather than to be a tenant of rooms in someone else's house. Again, in the matter of exemption from income tax she refused to accept as final the adverse opinions that the Council had more than once received. She was sure that these were due to an inadequate presentation of the facts, set about to get the matter put on a more satisfactory basis, worked up the evidence that she found would be necessary, and eventually had the satisfaction of seeing the Society granted an exemption without which its financial position would have been desperate in recent years. It was characteristic of her that she more than once after her retirement protested that the very modest pension she was receiving after a life-time of service was more than the Society could afford.

During the last year of her life she suffered a great deal of pain, and her serious operation last summer gave her only temporary relief. To within a day or two of her death there was nothing she enjoyed more than a talk about the S.P.R., whether it turned on the great figures of the past with whom she had worked, or on more recent developments, which she followed with equal keenness. The pleasure of such a talk was certainly not one-sided. The staff of the Nursing Home adored her, and said they had never had so good a patient.

It is forty-seven years since Miss Newton was appointed Assistant Secretary. Those years saw great changes in the personnel actively engaged in S.P.R. work, in the subject matters and methods of research, and in the Society's fortunes. To some of us who were closely associated with her in the later period of her work, her loyalty and friendliness through all our difficulties seemed among the most trustworthy guarantees of future progress.

I am glad to be able to print below personal impressions from several of her old friends and former colleagues.

W. H. S.

American members of the S.P.R. visiting London had reason to be grateful to Miss Newton. With her manifold duties she was never too busy to examine records that were referred to her and to weigh the evidence with strict adherence to the methods and standards of Mr Piddington's High-and-Dry School. This increased the inquirer's confidence and respect for psychical research, regardless of disappointment when things were sometimes not what they had seemed to be.

Miss Newton's generous nature went much farther than this. She saw to it that American visitors met members of the S.P.R. who had taken an active and valuable part in the Society's work. Thus it was my good fortune to become acquainted with leaders in the field who still lived in the late 1920's and 30's.

Miss Newton's loyalty and devotion to the S.P.R. need no comment from me. But I should like to mention one instance of her innate modesty. We happened to have a dinner engagement on the day when the Special Commissioners upheld the Society's claim for exemption from income tax. When Miss Newton arrived she was radiantly happy, and she told me in detail about the hearing of the case. Not a word was said of her own part in what had led to the momentous decision, only how much the others had done. It was not until some months later when I read Mr Salter's report of the outcome in the S.P.R. *Journal* that I learned this important success was very largely due to Miss Newton's enterprise and tact.

Over the years we became great friends. Psychical research was not our only common interest. And it was then that I frequently teased Miss Newton about having missed her real vocation. She was a delightful mimic and was able to impersonate mutual friends and acquaintances to the life, throw their foibles into sharp relief, and always with such warm good humor that my invariable reaction was an increased liking for or interest in the " victim ".

Miss Newton faced her personal difficulties in the last years with the fine courage that was implicit in her character. A message last Christmas said, " About the same," when I knew that she was on a much lower level. It was cheering to be informed that loyal S.P.R. members were providing Miss Newton with personal attention and physical comfort she might not otherwise have had in her long illness. It was a token of the affection and high regard in which she was held by her many friends in psychical research.

LYDIA W. ALLISON
(*Secretary*, American S.P.R.)

It was very many years ago that I first met Miss Newton, in the long-ago days when the Society's rooms were in Hanover Square. I had gone there to recount some telepathic experience, and to enquire into the work of the Society. The immediate impression which she then made on me, of a happy mixture of intelligence and kindness, was confirmed and deepened as the years went on. Her open but critical mind, her sympathetic insight, and her great respect for truth made association with her a most happy and valuable experience. I only now realize how much I learnt from her.

She was always good company, for she saw life both dramatically and humorously, and brought zest both to leisure and work. Books, the theatre, travel, talk, all experiences were savoured to the full. Highly intuitive herself, she could give sympathetic understanding to those who experienced paranormal phenomena, she had instinctive insight into the workings of the unconscious mind, and early prophesied the help which would come from its scientific study.

She was a tirelessly interested and sympathetic observer of life in its endless aspects. Few things made her impatient, except perhaps, a defeatist attitude. For her, a problem or a difficulty was always a challenge, and this is how I see her, I think, most clearly, and shall most clearly remember her, responding with generous energy and with colours flying, not conscious of how much she spent herself, till the difficulty was resolved, or the friend rescued. Her help was so wholehearted that often the difficulty seemed miraculously solved, to her own great astonishment.

Her own generosity enabled her to express gratitude generously; during her last illness her faith in life even grew in strength, and her unselfish courage remained undimmed to the end, endearing her to all who knew her.

INA JEPHSON

I did not have many opportunities for meeting Miss Newton personally, nor did we have much correspondence. None the less she made a considerable impression, in various ways. Her appearance, for one thing: she had such beautiful eyes, hair, and skin, and good features too; I often thought she must have been lovely as a girl, and was astonished when, latterly, she told me her age. There was also her ability to remain calm and unruffled in controversy, and never to seem hurried at work. And I found her full of that rare gift, common sense. Critical of people, though never maliciously so, while she assessed character accurately she was always loyal, and I never heard her say anything better left unsaid. She was also a wonderful mimic, and I gathered had a suppressed desire to write stories. Our chief bond was our mutual admiration and affection for Mrs Sidgwick: I think we talked more of her than of anyone else in our brief encounters.

NEA WALKER

I first got to know Isabel Newton when I began to work for the Society in 1908, and she had recently become its Secretary. My personal contact in these early days was mainly with Alice Johnson, whose Assistant I was, but I soon became aware of Miss Newton as some one upon whom one could always depend for help and information, and after Miss Johnson's resignation we worked much together, and came to know each other well. Two words which come immediately to my mind when I think of her are sympathy and loyalty. Her sympathy was the result of a genuine and quite unforced interest in other people's problems, and a practical desire to help them in any way she could.

I was her colleague all through the years of the first War, when we received almost daily visits from people (by no means all of them Members of the Society) who had suffered recent bereavements. To some of these visitors their bereavement had come as a shattering blow, and they were finding it difficult to look forward into the future and face life. Miss Newton's skill and patience in setting them on the road again were quite exceptional. She was scrupulously honest, she offered them no easy anodynes, but she instilled into them something of her own courage and buoyancy, persuading them to "try again".

Her loyalty to the Society in general and to her colleagues in particular stood like a rock; I never at any time felt that there was the remotest possibility that she would let me down. One incident I remember which was characteristic of her: the Office staff at that time consisted of Miss Newton, Mrs Thatcher (Assistant Secretary), and myself. Miss Newton had been out of health for some time and had at last gone away for a much needed holiday, when I arrived at the Office one morning to get news that Mrs Thatcher's husband had been killed at the front and she would not be coming to work. I wrote at once to inform Miss Newton (I knew she would not wish to be left in ignorance of what had happened), but I assured her I could carry on single-handed for a time, and she was on no account to curtail her holiday. She came back next day.

I shall always remember with pleasure the friendly talk I had with her in the Nursing Home only four days before she died. It was evident to me that the end could not be far off, and Miss Newton told me frankly that she would be glad when it came. But her mind was perfectly clear, and she was serene, and contented, as she said, to look back upon a long, full and happy life. She told me it was a pleasure to her to realize from the many letters she had had how much help she had been able to give to others by merely "being herself".

Miss Nea Walker has referred to her physical appearance, and as I looked at her face on that last visit,—the flesh fallen away, so that one saw the shape of the bone,—I thought how beautiful she was.

H. DE G. S.

When in 1935 I took office as Research Student of the Society (becoming Research Officer the following year), I knew very little about psychical research. I had assisted, mainly on the scientific side, with some experiments with a physical medium ; I had done a certain amount of somewhat unsystematic reading of psychology—mostly of what was then called " the New Psychology " ; and I had examined some of the more recent literature of psychical research. I was thus deeply dependent upon Miss Newton, who was then Secretary, both as a general psychical counsellor, and as a director of studies. She was able to tell me exactly what I should read, and—more important—which of it I should read first. She was also ready to set me right on the many and varied day-to-day points which my work threw up ; and she gave me useful hints about the various members of the Society, and others, with whom my work brought me into contact. Her advice was invariably sound. She was, of course, a very clever woman, not so much brilliant as soundly clever, and her intellect, I thought, was more like that of a man—and a gifted man—than a woman's. For example, she never made up her mind without careful consideration of all the available evidence ; and, once her mind was made up, she rarely changed it.

She was of immense assistance to me, both on account of her wisdom and experience, and from her kindness and enthusiastic encouragement. Her knowledge of the literature made her invaluable to a beginner in the subject. Without her advice, I should not have found until months or years afterwards many papers which were of the greatest value to my work, and I should have wasted time in ploughing through much that, though highly praised by some, was really of smaller importance. She knew all the tricks of the trade, and could point an unerring finger at the one weak spot in what appeared to be a cast iron " case ". At first, I disagreed with some of her judgments of individuals : people whom I thought were sound she gravely discounted, and *vice versa*. But I soon discovered that she was almost invariably right. " Tell me your longitude," said Sir Lulworth Quayne in " Saki's " story *The Blind Spot*, " and I'll know what latitude to allow you." Miss Newton could tell everyone's longitude to within a second of arc, and she knew exactly what latitude to allow them. While full of kind encouragement, she made no attempt to disguise the extreme difficulty of the subject, and the immense value of the work which had already been done, much of it, of course, by persons of the very highest intelligence. Soon after my arrival she propounded a principle which (though of course it was most tactfully and charmingly disguised) really amounted to this : " If ever you have a brilliant new idea, you are almost certain to find out that it has already all been said, much better, by Mrs Sidgwick." I used this principle as a sort of psychical William of Occam's Razor—and I found it most salutary.

She appeared to be an untidy worker. Her desk was always stuffed and littered with countless papers, arranged apparently in a truly random sequence—but she was always able to lay her hands instantly on the document that she wanted. She was also fond of making notes on odd little scraps of paper. But in reality she worked quickly and systematically.

We never discussed general subjects. With so much to talk about in relation to our work, we had no time for that. I never discovered whether she was artistic or fond of music ; whether she went to the theatre ; or what sort of books she read. On the one occasion that I visited her charming little flat, we talked mostly of the business on hand (I had been asked to come for the purpose of photographing the relative positions of two objects which were thought to have been referred to in a sitting of the " book-test " type). But after the beginning of the war (by which time she had retired from the post of Secretary) she spoke to me and made a suggestion, which seemed to me a very sound one, in connexion with the broadcast messages which were then being given to the people of England with the object of maintaining morale. I was able to pass on her suggestion to the appropriate quarter, and it was acted upon—and with splendid effect. Whether this was due solely to Miss Newton's idea, or whether the suggestion had been made previously by others, I do not know.

C. V. C. HERBERT

THE EXPERIMENTAL EVIDENCE FOR PK AND PRECOGNITION

By C. W. K. Mundle

The purpose of this paper is to ask whether, and if so how, we can distinguish experimentally between cases of PK and cases of precognition. Before tackling the problems which face the experimenter, it seems proper, however, to decide how we shall interpret the words " PK " and " precognition ". There may turn out to be substantial disagreement regarding the definition of these words : for one thing, it is difficult to define these words at all, without implying or suggesting some particular metaphysical theory. I shall, however, for the purpose of this discussion, provisionally accept the theory of psycho-physical dualism, in that I shall assume that mental events and physical events are of irreducibly different kinds ; but I wish to leave it an open question whether, or in what sense, a mind or a piece of matter may be called " a substance ". If some readers disagree with my definitions of " PK " and " precognition ", I hope, at least, that my definitions will make it clear in what respects I am departing from their usage.

When I speak of " precognition " in this paper, I shall be referring only to what Professor Broad has more accurately described as " supernormal non-inferential precognition ".[1] I suggest that we should define " a precognition " as a human event, which stands in certain relations to a later event (which may be of any kind whatsoever), the relations in question being (*a*) that the later event exerts direct causal influence on this human event or on one of the antecedent conditions of this human event : (*b*) that the later event corresponds epistemologically to this human event, that is to say, the later event resembles the human event in ways which make us say that it *verifies* the human event.

This definition calls for some explanations :

(i) I speak of a *human event* because I wish to include both mental events, like having an image, seeing an apparition, or entertaining a proposition, and also pieces of bodily behaviour like writing or uttering a word or sentence, or drawing a picture. In some cases of ostensible precognition, we are not entitled to describe the earlier event as mental. For example, in automatic writing, all that can be observed in many cases is the writing movement and what is written, for automatists are often unable, later, to remember writing a given sentence or entertaining the proposition thus recorded. If we say that the automatist must, in some sense, have been simultaneously conscious of what she wrote, this is a conclusion reached by problematic inference, and not an observed datum.

(ii) I speak of the later event exercising direct causal influence on the human event *or on one of the antecedent conditions of this human event.* I introduce this alternative because many psychical researchers consider that, in cases of supernormal cognition, the object of the percipient's

[1] " The Philosophical Implications of Foreknowledge ", The Aristotelian Society, Supplementary Volume XVI.

conscious experience is usually a more or less distorted product of processes occurring in the unconscious mind of the percipient. If this is so, the later verifying event would presumably exert direct causal influence on unconscious mental events which are causal ancestors of the observed event which we call " a precognition ". The immediate causal ancestors of the latter event may, however, include physical events in the percipient's brain, in which case it might be these cerebral events which were directly influenced by the verifying event.

(iii) My above definition may appear to some people to leave out part of what they mean by " precognition " (or by " foreknowledge " if they use that term), namely the element of *knowing* (or truly *believing*) a fact about the future. I suggest, however, that, in so far as the words " precognition " and " foreknowledge " suggest this element as part of their meaning, they are misleading for describing the facts in question. For, in most of the spontaneous cases, the earlier human event is interpreted as precognitive, only after the observation of a later event which verifies it.[1] And in the experimental situation, the percipient appears incapable of distinguishing which of his guesses are " hits ".[2] Perhaps I should mention here that when I speak, later in this paper, of a person being *informed* by precognition of a later event, I do this to avoid circumlocution, and intend to say not that this person has ever thought of the occurrence of the future event, but merely to say that the future event has directly influenced prior events in his mind or brain, in such a way as to increase the probability of his behaving in ways which are appropriate to the occurrence of this future event.

Our definition of " PK " will, I imagine, be less controversial. It seems that Professor Rhine, and other psychical researchers, use " PK " primarily to refer to the following property—direct causal influence of a mental event on a physical event which occurs outside the physical organism associated with that mental event. It is true that Rhine has gone on to suggest that direct causal transaction between a mental event and an extra-somatic physical event may be a process of the same kind as that which occurs normally between a mind and the body it animates. Nevertheless, " PK " is used primarily to refer to causal transactions which appear supernormal, and it seems best to limit its meaning to this sense. We cannot, however, take the above property as a complete definition of " PK ", for it would never occur to us to ask whether a mental event exerts direct causal influence on an extra-somatic physical event, unless we already knew that these events were related by epistemological correspondence. All the cases of ostensible PK which have been, and can be, investigated are cases where the extra-somatic event corresponds epistemologically to an idea or volition, and it is this correspondence which makes us suspect a causal connection. (Of course, the epistemological correspondence may be negative as well as positive. In a dice-throwing experiment in which the agent is aiming at high-dice, a significant deficit,

[1] H. F. Saltmarsh, *Foreknowledge*, p. 25, " most precognitions are not immediately known to be such, they are not predictions in the sense that they are, at the time of their occurrence, consciously recognised as having a reference to the future ".

[2] *Vide* Soal and Goldney, " Experiments in Precognitive Telepathy ", *Proc. S.P.R.*, Vol. XLVII, pp. 77-8.

as well as a significant surplus, of high-dice would be taken to indicate PK.) I shall define " an instance of PK " as the occurrence of certain relations between a mental event and a physical event which occurs outside the body associated with that mental event, the relations in question being (*a*) that the mental event exerts direct causal influence on the physical event ; (*b*) that the mental event and the physical event are related by epistemological correspondence.

If these definitions are correct, they reveal that our concepts of PK and precognition are very similar in form. Each involves reference to a pair of events related (*a*) causally and (*b*) epistemologically. The differences between these concepts are :

(i) *In respect of the nature of the events which stand in these relations.* We may notice here that the kinds of events involved in PK are subclasses of the kinds of events involved in precognition (*mental events* being a sub-class of *human events* : *extra-somatic physical events* being a sub-class of *events*).

(ii) *In respect of the direction of the causal influence.* In PK, the causal influence is *from* a mental event *to* a (simultaneous or later) extra-somatic physical event. In precognition, the causal influence is *from* a later event *to* an earlier human event.

The latter difference requires some comment. For philosophers, the question of what we mean by " causation " raises many controversial issues which cannot be discussed here, but there are two points which we should not evade :

(i) Some philosophers have held that it is meaningless to speak of causation as " operating in a certain direction " : have held that to assert causal connection between *A*-like events and *B*-like events is merely to say (*a*) that *A*-like events are always accompanied by *B*-like events, *or* are always followed or preceded at some characteristic interval by *B*-like events, and is not to say (*b*) that *A*-like events *influence* *B*-like events in any sense in which *B*-like events do not influence *A*-like events. Now, *if* this view were accepted, PK and precognition would not be independent hypotheses (or concepts), but instead, PK would be a species of precognition. For consider a case where we had a mental event followed by an extra-somatic physical event, these events being related by epistemological correspondence. Whatever evidence we might obtain that such events are causally connected, there would be, on the above view of causation, *no difference in meaning* between describing this connection as an instance of PK and as an instance of precognition. For in such a case the meaning of " PK " and " precognition " could only be distinguished by reference to the direction of the causal influence.

(ii) We might, however, maintain that there is *one* sense in which we can (and do) intelligibly speak of causation as operating in a certain direction ; that the common sense criterion of the direction of causal influence is that it is always *from* earlier *to* later events. However, if this were our only criterion of the direction of causal influence, we could not render intelligible our definition of " precognition ", for what could we then mean by speaking of the causal influence of a later event on an earlier event?

Let us regard these considerations as a challenge, admitting that if PK and precognition are to be intelligible as *independent* hypotheses, this must be by virtue of some criterion of the direction of causal influence other than the common sense criterion based on the temporal order of events.

Let us turn now to the practical problems. Firstly, can we obtain experimental evidence for PK, which could not be otherwise explained? This seems possible. It may have already been done. However, it seems clear that many PK experiments which were, *prima facie*, successful, have failed in a fairly obvious way to eliminate the possibility that the significant scoring was due to precognitive ESP. In this section of my paper, I could not, and shall not, attempt a complete and detailed examination of all the PK experiments which have been performed. My purpose is, first, to specify certain conditions which must be fulfilled by an ideal experiment, and which have not apparently been fulfilled in many (perhaps all?) earlier experiments: and then, to suggest an experimental design which would close the loopholes which I am considering. I shall refer to some of the Duke experiments in order to illustrate the nature of the loopholes in question, but I do not wish thereby to detract from the value of the pioneer work which has been done at Duke University.

The first loophole I want to consider has probably been noticed by many psychical researchers, but I shall now elaborate it, to make my argument clear, and shall later refer to it as *Hypothesis* 1.

In the dice-throwing experiments, the person who selects the target to be " willed " by the " agent " may (unconsciously) precognise what face (or group of faces, in high-dice or low-dice calling) is going to preponderate above chance-average in the next throw or run, set or series: and his choice of targets (including decisions when to change targets) may be influenced by this supernormally acquired information. In many of the Duke experiments, it seems to have been left to the " agent " to select the target: but even if this is done by an experimenter, we may assume that this experimenter hopes that the test will succeed, so that a motive would exist to account for the direction of his " precognitive faculty " in this way, and for the use to which, we are supposing, he unconsciously puts it.

An objectionable feature of many experiments is, then, that it has been left to the spontaneous choices of agent or experimenter to determine the targets at which the agent should aim. Rhine may be able to show us that the declines within sets and runs are independent of changes of target to a degree which renders hypothesis 1 most unplausible. I shall, however, argue that the ideal method of eliminating hypothesis 1 is to have the targets selected in a random way by a machine, selected in such a way that *if* precognition of the faces which will preponderate occurs in any mind, this mind *could* not use this information in order to effect extra-chance scoring *without exercising control by PK over the target selecting machine*. However, before we can state what sort of mechanical selection of targets would achieve this object, we must consider a further loophole which was left open in many of the earlier experiments. This may now be stated briefly, and I shall refer to it in future as *Hypothesis* 2.

In the dice-throwing experiments the extra-chance scoring may be due to control, normal *or abnormal*, by the agent of the processes in his own body which " trigger " the movements of the dice.

Hypothesis 2 may appear, at first, to be merely the repetition of an obvious criticism. It has often been suggested, for example, that when dice are hand-thrown, some people may be able to get a preponderance of the results they wish by means of a normally acquired manual dexterity. Hypothesis 2 is intended, however, to put us on our guard against possibilities which are more unexpected. Psychical researchers have treated seriously the possibility of hyperaesthesia in connection with ESP. Experimenters have recognised that they ought to consider whether, and if so to what extent, ostensible ESP may be mediated by processes occurring in the afferent nervous system, assuming abnormal sensitiveness and discrimination on the part of the senses : they ought equally to consider whether, and if so to what extent, ostensible PK may be mediated by processes occurring in the efferent nervous system, assuming abnormal control of such processes. The precautions to eliminate hypothesis 2 in PK experiments should be as rigorous as the precautions to eliminate hyperaesthesia in ESP experiments.

There is no doubt that hypothesis 2 can be applied to many of the Duke experiments : for example, to all of those in which the agent performed a manual movement which constituted a differential condition of the subsequent movements of the dice, and hence was a cause factor in determining the final resting positions of the dice on the table. Take, for example, the experiments in which the dice were manually replaced in " the same " positions on a starting line, and, after a release-movement which was manually initiated, ran down a corrugated surface on to the table. The physicist could argue here that you would get the same result from every trial, if each of the following conditions were fulfilled :

(*a*) the same dice are always in exactly the same starting positions,
(*b*) the release movement is always identical in speed and direction,
(*c*) the positions of all the surfaces, which the dice strike in rolling, remain unchanged.

But no one would expect conditions (*a*) and (*b*) to have been fulfilled, since they depended on manual movements. If we grant that the precise positions of the dice at the moment of release and the speed and/or direction of the release movement were almost certainly variables as between different throws, then we might explain the significant scoring by reference to abnormal control of the manual movements in question.

Differently designed experiments would provide different kinds of opportunity for abnormal manual control as a factor which might affect scoring. Consider the experiment in which dice were released from a rotating cage when the agent pulls a string. In this case, even if the speed and direction of the agent's pull were causally irrelevant, the timing of his pull would have been relevant. In the rotating cage the dice are presumably in motion relative to each other, to the cage and to the table. At one instant the spatial distribution and motion of the dice would be such that release *then* would result in an excess of high dice, at another instant . . . in an excess of low dice. Significant scoring with this set-up might be due to the agent's abnormal control of the *timing* of his motor behaviour.

This hypothesis is of theoretical importance, for it would enable us to accept Rhine's facts, without regarding any mental events as *proximate* differential conditions of any physical events other than those occurring in the corresponding brain. Assuming that volitions are *remote* causal ancestors of some extra-chance hits, they would be performing this role " by normal channels ", *i.e.* via brain, efferent nerves and muscles. The old mystery of how mental events control cerebral events would remain, would indeed be heightened by virtue of the extraordinary discrimination which, upon this hypothesis, we should be attributing to the causal efficacy of volitions. However, the physicist who ignores *this* problem (how cerebral events are determined) could, on our present hypothesis, still maintain (*a*) that we need invoke no causal connections between events which are not linked by a spatio-temporally continuous series of events, and (*b*) that all the necessary conditions of any physical event are contained in prior physical events and states. (I don't know whether the physicists have decided whether they want to maintain these propositions. They seem, when discussing Quantum Mechanics, to be abandoning both the Principle of Continuity and Determinism in the " microscopic sphere ", yet they seem to wish to retain both in the " macroscopic sphere ".)

If hypothesis 2 is true, some human beings have an unexpected faculty, but one which is not at all like PK as Rhine conceives it, and as it was defined above: for we defined PK in terms of *direct* causal influence of a mental event on an extra-somatic physical event; but if hypothesis 2 is true, the causal connection between these events is not direct, but is mediated by a continuous chain of physical events which start in the agent's body.

Can we eliminate hypothesis 2? Can a person's volition influence the course of events in external bodies except by way of motor processes (or other physical processes of known kinds) in his own body? It seems easy to eliminate the agent's bodily movements *qua* differential conditions of the events which he seeks to influence. We simply debar the agent from replacing dice in the release mechanism and from initiating the release movement. But this is not enough. On the hypotheses in question, agents have unexpected powers of controlling their movements—then presumably experimenters may have like powers—so that *any* person who manually replaced the dice or initiates the release-movement might be responsible for the excess-hits. (In the Duke experiments, the experimenters knew the current target, and, we may assume, were hoping—whether consciously or not—that the experiment would succeed: and even if the experimenters did not know the target by normal means, they might have ascertained it by ESP.) We require, therefore, an experiment in which all the events which the physicist regards as differential conditions of the events which the agent seeks to influence are determined by machine. The use of a machine will not serve our purpose, if this machine is operated by a manual movement, *unless* this manual movement is rendered causally irrelevant to the physical processes which determine the events which the agent seeks to influence. Now this point applies to the elimination of hypothesis 1 as well as that of hypothesis 2. For, if as previously suggested, we use a machine to select targets, hypothesis 1 will not be conclusively eliminated unless any manual movements, by which the target-selecting-machine is operated, are causally irrelevant to the physical processes which determine the target selection. If this were not so, the significant scoring might be attributed to precognition, plus abnormal manual control of the target-selecting-machine.

Let us consider now how these requirements can be met. It is probably possible to eliminate hypotheses 1 and 2 in dice-throwing experiments, and this may already have been done. I wish, however, to suggest one method by which our aim could, in principle, be fulfilled, namely by an experiment employing the " random selector " designed by Mr R. Wilson.[1] This is an electronic device which will give a random series of selections from four alternative events, *e.g.* the lighting of one or other of four lamps of different colours. Wilson designed this machine for ESP tests, and was, for this purpose, content to have the operation of the machine initiated by the manual pressing of a button. One of our purposes being to eliminate hypothesis 2, we cannot allow this, since the *timing* of the push would (if I understand the machine) constitute a differential condition of the selection. We must, on hypothesis 2, take seriously even the possibility of abnormal control of the timing of finger movements to within 1/3200 of a second, which is the " critical period "[2] for selection by this machine. Let us therefore have the operation of the random selector triggered by the closing of a delay switch, whose " spread "[3] is substantially larger than 1/3200 second. (This should give us a wide choice of types of delay switch!) An essential specification for the triggering mechanism will be that it is designed so as to satisfy any physicist that no variations in the timing or force (or other physical characteristics) of the manual push on the button can, by any known physical process, affect the operation of the delay switch and hence of the selector.

In my proposed experiment we shall employ two of Wilson's random selectors: let us label them A and B. A single manual movement will initiate the operation of *both* machines, but machine A will be operated (and one of its four lamps be lit) a few seconds before machine B is operated. To effect this, the same manual movement will simultaneously activate two delay switches: the closing of the first switch will trigger machine A, the closing of the second will trigger machine B. The role of the agent will be to " will " that the lamp which is going to be lit by machine B shall correspond in colour to the lamp which has just been lit by machine A. For each trial, the sequence of events will be as follows: An experimenter presses a button at time T_1 and thereby (*a*) operates delay switch 1 which closes at time T_2 and thereby operates machine A by which the agent is informed at time T_3 of the alternative which he has to " will " : (*b*) operates delay switch 2 which closes at time T_4, thereby operating machine B which produces one of the four alternative events at time T_5. The timelags T_2 to T_3 and T_4 to T_5 will be negligible. Timelag T_1 to T_2 could be in the order of a second or less. Timelag T_1 to T_4 (*i.e.* the period delay switch 2 takes to close) could be in the order of 5 to 10 seconds. The agent will be willing the correspondence of the B event to the A event during the period T_3 to T_5. It would

[1] For technical details of the apparatus, see *Proc. S.P.R.*, Vol. XLVIII, Part 174.

[2] By " critical period " I refer to the period, at any instant during which operation of the machine would cause the same lamp to light.

[3] By the " spread " of a delay switch, I refer to the average deviation from its average period of operation.

be an advantage if this period can be adjusted to meet agents' preferences by adjusting the average timelag of delay switch 2. Other "frills" like the automatic recording of *A* and *B* events could easily be incorporated.

If both these machines select alternatives randomly, then, in the absence of any PK interference, the correspondence between *A* events and *B* events will approximate to chance average (one hit in four). If the results obtained, in (and only in) the presence of an agent willing correspondence, yield a significant deviation from chance-average, it would surely be impossible to explain this merely by reference to supernormal cognition. Even if one (or both) of the machines fails to give a perfectly random series and reveals some bias, this need not matter, for we can determine the chance-level of correspondence empirically by "control runs" of a number equal to the "real runs", and regularly interspersed with the latter: the *only* difference between a "control run" and a "real run" being the presence of a suitable agent who is willing correspondence.

Suppose that this experiment succeeds in the sense that the correspondence between *A* and *B* events obtained in the presence of an agent who is willing correspondence differs significantly from the correspondence obtained in circumstances which differ only in the absence of the agent willing correspondence. I cannot conceive how we could attempt to explain such facts without invoking PK. Even omniscience on anyone's part—never mind a little precognition—would not explain the facts. Precognition which was 100 per cent efficient, and which informed someone of the next *A* event and the next *B* event every time (or informed him instantaneously of all future members of the *A* series and the *B* series) would explain nothing—it would merely furnish advance information of the phenomena which demand explanation.

There are surely only two ways in which results of the kind we are considering would be explicable in terms of supernormal cognition without invoking PK.

(*a*) If precognition by any mind of the next *B* event could influence, by any means other than PK, the selection of the corresponding *A* event. (My hypothesis 1 illustrated a way in which this sort of thing might happen in the Duke experiments.)

(*b*) If information about an *A* event (whenever or however acquired) could be used to influence, by any means other than PK, the determination of the next *B* event. (My hypothesis 2 illustrated a way in which this sort of thing might happen in the Duke experiments.)

The proposed experiment would eliminate both of these possibilities. If anyone precognises the next *B* event, he cannot use this information to influence selection of the corresponding *A* event except by PK influence on the *A* machine. And equally, precognition occurring before time T_1 of the next *A* event would not enable the experimenter who presses the button to influence the selection of the next *B* event by any known physical process.

If the proposed experiment yielded the results we are envisaging, I for one would be compelled to acknowledge PK as a *vera causa*; compelled to acknowledge, that is to say, that a mental event is sometimes a *proximate* differential condition of physical events occurring in regions other than that occupied by the associated organism.

The next stage in our enquiry is to ask whether we can obtain experimental evidence for precognition, which cannot be otherwise explained. Let us, for ease of reference, describe as *Hypothesis* 3, the theory that all ostensible precognition can be explained away if we invoke PK and/or non-precognitive ESP. Hypothesis 3 appears much more difficult to eliminate than hypotheses 1 and 2. If hypotheses 1 and 2 are eliminated in the PK experiment suggested above (or by any other experiment), the case for postulating precognition will be substantially weakened. For consider in general terms the difficulty which has to be overcome in finding unambiguous evidence for precognition.

Let us assume that an idea or a volition is something which can persist in a person's mind when it is not introspectible, and can thus bridge the time-gap between a so-called precognition and the event which verifies it. Then a so-called precognition (*X*), and the event (*Y*) which verifies *X*, may both be caused by the same persistent idea or volition (*Z*)—this would explain the epistemological correspondence between *X* and *Y*. Assume, for simplicity, that *Z* belongs to the person (*A*) in whom *X* occurs. (Our explanation can easily be adapted to meet different cases.) Then if *Y* is a mental event occurring in a person other than *A*, *Z* may cause *Y* by way of telepathy ; and if *Y* is a physical event occurring outside the body of *A*, *Z* may cause *Y* by way of PK. (If *X*, *Y* and *Z* all belong to *A*, the explanation will fall within the scope of normal psychology.) Unless, or until, we can empirically determine the limits of both telepathic and PK influence, how can we possibly eliminate hypothesis 3? Bearing in mind this difficulty, let us examine the evidence for precognition offered by the experiments of Rhine, Soal and Carington.

Professor Rhine devised a very elaborate experiment to confirm the occurrence of precognition.[1] He considered hypothesis 3 and attempted to eliminate it. The first stage was to ask a subject to guess the order of a pack of cards after it had been shuffled manually at a later date. It was then recognised that the subject's " guesses " might influence the final order of the pack by influencing the mind, and hence the fingers, of the person who shuffled the pack. To eliminate this possibility, it was decided to shuffle the cards by a machine. Then it was recognised that the subject's " guesses " might perhaps exert a PK influence on the shuffling machine. It was to eliminate this possibility that Rhine devised his culminating experiment. This involved having the cards cut after they had been mechanically shuffled ; and having both the cut and the duration of the shuffling determined, in accordance with rules fixed in advance, by the figures for the daily temperature extremes obtained from a specified newspaper several days after the guesses had been recorded. Rhine claims that this experimental design eliminates PK, because " the temperature itself would have to be modified by the hypothetical effect in question [*i.e.* PK] and this seems considerably more incredible than that the [shuffling] machine might be influenced directly ".[2]

[1] See *The Reach of the Mind*, pp. 60-4, and *Journal of Parapsychology*, Vol. 6, No. 2, pp. 111-43.

[2] *Journal of Parapsychology*, *op. cit.*, p. 134.

When Rhine, in *The Reach of the Mind*, claims without qualification that this culminating experiment yielded significant results, his statement might be considered misleading. For we find on turning to the report in the *Journal of Parapsychology* that the results were not significant in any obvious sense. After 57,550 trials the deviation of hits was only +11. And, taking the results as a whole, the distribution of hits within runs (of 25 trials) was not significant, nor was the distribution of hits within segments (of 5 trials). The feature which led Rhine to judge the results significant is what he describes as covariation between the salience ratio for runs and the salience ratio for segments. (" Salience ratio " refers to a measure of the tendency for hits to be concentrated at the ends, rather than the middle, of a run or a segment.) This recherché feature is not present to a very striking degree. (Rhine gives $P = \cdot 0016$.) Dr Soal has suggested to me that this feature of the results may have been a " statistical artefact ".

However, even if this experiment had yielded results which were obviously significant, *e.g.* a significant deviation of hits, I suggest that we would not have to look far for a flaw in Rhine's argument that the method eliminated PK. For the temperature readings were presumably determined by someone looking at a thermometer, and a thermometer is a mechanical device no more complicated than a card-shuffling machine. If we admit, as Rhine does, that shuffling machines may be influenced by PK, why should we assume that thermometers are immune from such influence? In Rhine's experiment, a difference of one degree in a temperature reading would make a vast difference to the final order of the pack.

Let us turn now to the classic experiments with Mr Basil Shackelton, conducted by Dr Soal and Mrs Goldney. An important feature of these experiments was that the " telepathic targets " (*i.e.* the events which Shackleton was trying to guess) were multiply determined—by one method in the Prepared Random Numbers (PRN) series, by a different method in the Counters series. (I am not going to examine the PRN experiments in detail, since it seems to me that we could explain the results without invoking *either* precognition *or* PK: we merely have to suppose that Shackleton ascertained the current list of random numbers by telepathy from EA or from Soal, and ascertained the current code by telepathy from A. The only puzzle would be to explain why he tended to (so to speak) " read off the answers " one or two steps ahead of the number to which EA was attending.)

The method of selecting the target by counters may be briefly described here. EA (experimenter controlling agent) sits at one side of a table, A (agent) at the other side, a vertical screen being interposed between them with a small aperture in its centre. Two hundred counters, forty of each of five different colours, are placed in a cloth bag or a bowl on EA's side of the screen; the five colours correspond to the five *positions* in which the animal cards are placed on A's side of the screen. The animal cards are shuffled and placed face down in these positions after each run of fifty calls. Thus the code between colours and cards is changed each run. The procedure was that EA selected, manually and solely by touch, a counter from the bag or bowl and immediately showed it at the aperture in the screen. A, on seeing this, lifted, and looked at the face of the corresponding card. To assist A in remembering the fixed code relating counter colours to card positions, a counter of appropriate colour is placed above each card position.

In this case, it seems possible to explain the significant deviation of Shackleton's (+ 1) and (+ 2) scores in terms of " delayed-action " PK or " delayed-action " telepathy. The events which the " percipient " (P) was trying to guess, were partially determined by EA's manual movements which occurred two to three seconds after P recorded his guess. We might eliminate precognition by supposing that P's " guess " at time T_1 (*i.e.* his thought of a certain card-face) sometimes influenced the finger-movements which EA made two or three seconds later in such a way as to result in EA's selection of a counter of corresponding colour. This causal influence might be of the PK variety if P's idea directly influenced EA's fingers or brain, or it might be telepathic if P's idea directly influenced the mind of EA and thus indirectly guided processes in EA's brain and fingers.

I suggest that we need not worry about the timelag between P's guess and EA's finger movements. Assuming that an " idea " persists for some time after it has been the object of conscious attention, it may be that with some agents an idea acquires maximum PK-efficacy (and/or telepathic-efficacy) when it is fading, or has just faded, below the threshold of attention. We might on these lines explain Shackleton's tendency to get (+ 1) hits at normal speed and (+ 2) hits at rapid speed. (Ehrenwald[1] has produced a little evidence suggesting that an idea has maximum telepathic efficacy when it is in the preconscious or unconscious mind.)

Before we try to assess the plausibility of applying hypothesis 3 to Soal's results, there are two considerations which should be taken into account.

(i) Dr Soal and Mrs Goldney explicitly considered the telepathic variation of hypothesis 3, and carried out two series of trials with the purpose of testing this possibility. These are described in the section headed " Special Experiment with Counters. (Influence of (P) on (EA)) ",[2] which requires careful study. From these experiments they drew the modest conclusion—" we obtain no evidence that B.S. can influence K.M.G.'s selection of counters from the bowl ". (K. M. Goldney acted as EA in these experiments.) It might appear, however, that one would be justified in drawing the more positive conclusion—that these experiments provided evidence that B.S. did not influence K.M.G.'s finger-movements in either of the ways we are envisaging in connection with hypothesis 3, and that hypothesis 3 is thus rendered very unplausible as an explanation of B.S.'s (+ 1) and (+ 2) scoring. I suggest, however, that this conclusion would go too far.

In each of these special experiments, the conditions differed from those in which B.S. got significant results, and differed in some respect which may well have been relevant to his failure on these occasions. In the first of these experiments, B.S.'s choices of cards were *determined for him* by a list of random numbers, whereas in all of the other experiments his choices were *spontaneous*. This difference did not, however, occur in the

[1] *Telepathy and Medical Psychology*. [2] *Op. cit.*, pp. 73-5.

second of these experiments when B.S.'s choices were spontaneous. We must notice, however, that in this experiment B.S. was being asked to do something completely different from what he was asked to do in all of the successful experiments. He was now being asked to *will* that K.M.G. should select a counter of the colour which he had chosen,[1] whereas in all of the other experiments he was asked to *guess* something. I suggest that B.S. may have been able to influence K.M.G.'s fingers, but only when certain conditions were fulfilled ; and one of the necessary conditions may have been that B.S.'s state of mind should have been one of " trying to guess something ". If anyone asks why this should have been so, I should suggest that B.S. had reason to be specially confident in his guessing capacities, in view of his past successes, which, whatever their real nature, had been interpreted to him as supernormal *cognition*.

It may seem paradoxical to suggest that B.S. could exert PK influence, but only when he was trying to do something else. But is this really paradoxical? Suppose that this special experiment had yielded significant results—could we then have been sure that B.S.'s choices were influencing K.M.G.'s choices, rather than the reverse, merely because B.S. was told to will something and not to guess something? Can we, in any of the experiments, take B.S.'s instructions (or his own description of his state of mind) as a reliable criterion of the nature of the causal transactions involved? Surely we are not entitled to do this, any more than we are entitled, in dice-throwing experiments where the " agent " chooses the target, to assume that extra-chance scoring is due to PK rather than precognition *merely* because the subject was told to will, and not to guess.

(ii) There is another argument which might be advanced to show that hypothesis 3 cannot plausibly be applied to Shackleton's results. It might be argued that we could not explain these results merely by reference to a single series of " acts " of PK (or to a single series of " acts " of telepathy) from P to EA : that hypothesis 3 would only become plausible if we postulated further series of supernormal " acts ". Would we not have to elaborate some such story as the following :

(*a*) that the current code between colours and card-faces was ascertained by P by telepathy from A.

(*b*) that the relative positions of EA's fingers and the counters at the crucial moments was ascertained by P by clairvoyance,

(*c*) that P used the above information to guide the PK influence on EA's fingers.

A different story would be required on the alternative version of hypothesis 3 (in which P influences EA by telepathy), but this story, too, would involve three distinct series of supernormal " acts ".

If hypothesis 3 had to be interpreted in this way, then an argument against it might be elaborated as follows. The average efficiency of each of the three series of supernormal " acts " must have been considerably higher than their net resultant efficiency. But Shackleton's efficiency in (+1) and (+2) scoring was in fact high (compared with the usual level of scoring in ESP experiments), *e.g.* in several long runs Shackleton's efficiency was in the order of 12·5 per cent true cognitions. Hypothesis 3

[1] The experimental record is not explicit on this point, but I have obtained confirmation from Dr Soal that this is what B.S. was asked to do in this experiment.

would thus require us to postulate an *average* efficiency of about 50 per cent for each of the series of supernormal acts which we are invoking. This looks very unplausible. One might think that hypothesis 3 could be rendered progressively less plausible by devising experiments which would require more and more series of supernormal " acts ".

I do not think the above argument is conclusive, or even strong, but I think it deserves careful analysis, as it raises some fundamental issues. It seems plausible to say that B.S. could only have influenced EA's fingers *appropriately* if B.S. was guided by supernormally acquired information. The main issue here is whether PK implies ESP—a conclusion which Rhine took to be certain when he wrote *The Reach of the Mind*. We rarely find Rhine using an *a priori* argument, but he does so in order to justify this conclusion.[1] This argument has already been challenged by Professors Broad[2] and Price.[3] Rhine's conclusion is reached by an argument from analogy, and the analogy is not a strong one. He is assuming that PK must be analogous to normal voluntary action in which we interfere with environmental objects, in so far as both must be guided by perception of some kind : and since normal sense-perception cannot supply the data by which PK could be intelligently guided, he concludes that the data must be supplied by ESP. But since PK is so strikingly different from normal voluntary action in some respects (notably in not using the efferent nervous system), can we argue that PK *must* resemble it in another respect—in being a perceptually guided process, in being an intelligent response to information?

Professor Price has suggested that we may be wrong in thinking of PK as a perceptually guided process, and that perhaps the appropriate conceptual model is that of *ideo-motor action*. The sort of occurrences which Price presumably had in mind were cases where the thought of a bodily movement, *e.g.* yawning or stretching a limb, issues automatically into the corresponding behaviour, provided that it is not inhibited by some other idea. Does anyone believe that the idea of yawning can only issue into a yawn if it is guided by information regarding the physical changes in the brain which precede or accompany yawning? Surely we do not want to attribute to the unconscious mind of every child a knowledge of physiology far exceeding that attained by physiologists merely because it usually yawns when it thinks of yawning! Now the point of assimilating PK and ideo-motor action is that it suggests that, in PK, an idea may be realising itself in an external physical event without being guided by information about the physical mechanisms which precede or accompany the event in question.

I wonder if it might be possible to test experimentally whether PK is guided by ESP. This may seem a tall order, but I shall suggest a way in which something might be subtracted from the probability of this proposition. Rhine's acceptance of this proposition may have been partially conditioned by his exclusive use of dice-throwing in PK

[1] *Ibid.*, p. 106, " If PK had been discovered without any previous knowledge of ESP, the latter would immediately have had to be assumed to make the former intelligible," *et seq.*

[2] *Proc.*, Vol. XLVIII, Part 172, p. 22.

[3] *Mind*, Vol. LVIII, No. 231, p. 392, *et seq.*

experiments. (We all know, or think we know, what *sort* of interference is required to alter the course of a moving body.) I wonder if Rhine would have concluded that PK implies ESP if some of his PK experiments had been designed around electronic devices? Suppose we use Wilson's random selector and try some comparative PK tests between :

(A) agents who are scientifically educated and to whom the *modus operandi* of the selector is fully explained, and

(B) agents who are scientifically illiterate and not " mechanically-minded ", and who are told a simple false story about the *modus operandi*.

If the scoring of Group B were as high as that of Group A, it would be very difficult to entertain seriously the view that PK *must* be guided by ESP. Let anyone who thinks it possible, try to formulate a detailed account of how PK could be so guided in the case of Group B. He would, I think, find it necessary to postulate supernormally acquired data, and powers of instantaneous calculation, so extensive as to be well-nigh incredible.

How can we apply this line of thought to the experiments with Shackleton? We must admit that B.S.'s thoughts could only have influenced EA's fingers *appropriately* if his thoughts influenced them *in conformity with* the current code relating colours and card-positions. But this code was known by A from the beginning of each run. A possible explanation of the results is that EA's fingers were influenced *jointly* by B.S.'s guesses and A's knowledge of the code : or—to put it differently—that the function of PK agency (or telepathic agency as it may have been) was split between B.S. and A. This explanation might have appeared far-fetched before Dr Soal had made the " telepathy " experiment with Mrs Stewart,[1] in which she obtained highly significant results from two agents, neither of whom knew the targets. This experiment indicates that thoughts belonging to two different people can jointly influence the finger movements of a third person. (Notice that we have no means of telling whether Mrs Stewart's finger movements in recording her guesses were influenced by PK or telepathy.)

The argument we are dealing with is that my explanation of B.S.'s results implies the occurrence of more than one series of supernormal acts. Let us see where the same argument would lead us if we applied it to explaining Mrs Stewart's results. These, it would be said, must have been due to at least two series of supernormal acts (*e.g.* either Mrs Stewart must have performed two acts of telepathy, or else she must have " tapped the mind " of one agent, who had " tapped the mind " of the other agent.) However, if this were so, we should expect Mrs Stewart's scoring to be substantially lower in *this* experiment than in the experiments where only one agent was used. The results belie this interpretation, for Dr Soal tells us that the scoring in this experiment was not appreciably lower than in the others. We must conclude then, that processes which occur in the minds of different people can be as efficient in the joint production of supernormal effects, as are processes which occur in a single mind. This conclusion surely invalidates the criticism, which we have been considering, of my alternative explanation of B.S.'s results.

[1] *The Experimental Situation in Psychical Research*, pp. 42-3.

I wish now to consider Carington's classic experiments in the paranormal cognition of drawings, which furnished our first experimental evidence for " temporal displacement ".[1] Can we formulate a version of hypothesis 3 by which the ostensible precognition can be explained away? I may well be muddled, but I cannot think of any way of doing this which seems at all plausible.

Prima facie, we might think that there is as much scope here for invoking PK interference as a factor determining the selection of " telepathic targets ", as there was in the case of Soal's experiments. Carington tells us that " to select the subject matter of each drawing, I opened a copy of Chamber's *Mathematical Tables* at random, noted the last digit of the first three or four entries encountered, turned to the corresponding page of *Webster's Dictionary* and took as the subject for the drawing the first reasonably drawable word found on or after that page ". This was done and the drawing made within half an hour of zero hour for its display. Might we not then suppose that a PK influence was exerted on Carington's finger- (or eye-) movements (or a telepathic influence exerted on Carington's mind) to determine the selection of the " original " drawing? (The fact that Carington opened the Tables *at random* seems to give more scope for such interference than if he had followed a set principle in opening the Tables.) But *by whom* can we suppose such influence to have been exerted?

The novel feature of Carington's results was the *clustering* of extra-chance hits, more or less symmetrically around the occasions on which the drawings were displayed, yielding a curve which approximates to an inverted U-curve with twin peaks corresponding to the occasions, two before and two after the display of the originals. Being a fool about mathematics, I could not try to apply hypothesis 3 to Carington's results as they stand : for the latter do not reveal any regular curve with respect to this or that individual original drawing. Carington's curves emerge as a result of statistical aggregation and analysis of his data. I cannot see how to try to apply hypothesis 3 at all, unless I think in terms of a simplified model, that is, unless I assume that the inverted U-curve would apply to hits made on a single original on different occasions before and after its display. I shall assume that if Carington's curve is a " lawful phenomenon " and not a freak, it would be exhibited by the hits on any single original if we ran a series of experiments with a much larger number of percipients than Carington employed. Strictly, then, I am using, as the premise of my argument, not Carington's data as they stand, but an interpolation. My argument will have no probative value unless we can confirm by further experiments that this interpolation is warranted.

Suppose, then, that we find the inverted U-curve in the hits made on different occasions by many percipients on one original drawing. In that case, to whose agency could we attribute PK or telepathic influence on the selection of the original? The number of *different percipients* getting extra-chance hits would increase during the days (weeks) before the display of the original, and would decline in a similar way during the days (weeks) after the display. Could we explain the facts by supposing that one of the " percipients ", call him P, has an idea or complex which has

[1] *Proc.*, Vol. XLVI, Part 162.

PK efficacy and/or telepathic efficacy?—by supposing that P's complex influences supernormally the selection by the experimenter of the original, *and* influences supernormally the bodies or minds of other " percipients " so as to make them draw likenesses of this original? To say this may at first sound plausible, but in fact this theory would contribute nothing towards explaining the peculiarities of the results, namely the particular shape of the curve, and its position on the time-base (or " occasion-base "). *Why* should the hypothetical complex of P produce an increasing (and decreasing) number of hits by other percipients before (and after) the occasion of display? *If* the display of the original performs no independent causal role, why should not the assumed PK and/or telepathic-influence of the hypothetical complex of P be randomly distributed throughout the experiment? I can see no way of answering these questions unless we make *ad hoc* assumptions about the properties of the hypothetical complex of P ; unless we say that the PK or telepathic efficacy of this complex waxes and wanes in phase with the " hits curve ". (Even if we say this, we have not explained the awkward fact that the peaks of the hits curve occur before and after the occasion of display, *i.e.* before and after the only time at which P's complex is supposed to influence the experimenter who selects the original.)

But what sort of a theory is this? Surely a preposterous one. The data are that a single *known* event (the display of the original) occurs at the (temporal) centre of a cluster of similar events. Does it make sense to offer as a causal explanation of these facts a story according to which *all* of the *known* events are effects of some unverifiable entity : according to which the peculiar pattern of the known events are " explained " by assuming that the " powers " of the hypothetical entity vary in a similar pattern?

Surely it is methodologically unsound to explain the characteristics of known events by attributing them to a duplicate set of characteristics belonging to unverifiable entities which are invoked *ad hoc* : unsound, at any rate, when it is possible to explain the facts without doing this sort of thing. And in the case in question, we can explain the temporal clustering of the hits by regarding them as the several effects of a single known event which occurred at the (temporal) centre of the cluster. The obvious advantages of this last explanation are (1) that it is much the simplest hypothesis, and (2) that all the entities involved in this explanation are empirically known occurrences. These are not perhaps decisive reasons for choosing the latter hypothesis. In other sciences, the relative simplicity of a hypothesis is counted as a reason for preference ; but perhaps we should attach less weight to this criterion in psychical research, for the facts seem very complicated and the truth cannot be very simple. And regarding the second consideration, I think (and here I disagree with Carington) that we are not likely to make great progress in understanding supernormal phenomena if we adhere to a strictly empiricist philosophy. Must we not, for example, postulate unconscious mental processes? Yet these are unverifiable entities. Thus our argument has not, strictly speaking, *proved* anything. All we can say is that the obvious explanation of the facts seems preferable to a version of hypothesis 3 on the grounds of simplicity and of the principle " empiricism as far as possible ".

I think, however, that I can see a third reason for accepting the obvious explanation : it is the sort of reason I should put to a fellow-philosopher—but he might (and others probably will) find it inconclusive. I suggest that the following complex relational property (α) is *one* of the criteria by which we recognise causal connection and by which we distinguish a cause (*i.e.* a differential condition) from its effects.

" Spatial (or spatio-temporal) clustering around a central event or process of a set of events or processes which are, structurally and/or qualitatively, similar to each other." It might be argued that this property is part of what we mean by " causal connection ".

We have described property α in abstract terms, so perhaps an illustration will be helpful. Consider, for example, the group of wireless receivers which are tuned to a given wavelength for a given period : in the vicinity of each receiver will be occurring processes which are qualitatively and structurally similar, *e.g.* the sound of a certain symphony. If an observer could ascertain the (virtually) simultaneous occurrence of these processes, he would conclude, even if he were completely ignorant of the *modus operandi* of wireless transmission, that they were causally connected. If he could also ascertain the spatial (or spatio-temporal) centre of this group of processes, he would conclude that their causal source was some process occurring in this region. Now there is a criterion which would enable an observer to locate this central region (I am assuming, for simplicity, that there are no relay stations). Even if we ignore the fact that the further you go from the centre, the later the signals arrive (and our observer could only detect this fact if he were an accomplished and well-equipped experimenter!) : nevertheless, our observer could discover a spatial clustering in respect of the " volume " or loudness of the sounds, that is to say, he could discover the fact that the further one moves in any direction from what is the location of the transmitter, the weaker, *ceteris paribus*, is the volume.

To return to Carington's results—these reveal a property which is very similar to, though not identical with, property α, namely property β (as we shall call it), *viz.* " a *purely temporal* clustering, around a central event or process, of a set of events or processes which are, structurally and/or qualitatively, similar to each other ". The clustering in question is not spatial, for Carington found that the spatial relations between the bodies of the various percipients and of the experimenter are completely irrelevant, and this irrelevance of spatial relations is confirmed by other experiments in ESP. Surely property β, just as much as property α, is a criterion of causal connection. It has not hitherto been recognised as such, and it is certainly not part of what we have hitherto meant by " causal connection ", for prior to Carington's experiments we had never come across any instance of property β.

If this is granted, we may now point out that the facts which revealed property β furnish us with a criterion of the *direction* of causal influence, other than the common-sense criterion (that this influence is always *from* earlier *to* later events). There are characteristics in Carington's data analogous to the fading of volume as we move further in any direction from the broadcasting transmitter : for we find the " hits " dwindling as the temporal distance from the occasion of display increases, and we find that this dwindling occurs in both temporal directions—before and after—in a symmetrical manner. We may notice, in passing, that Carington's method of scoring was designed to enable us to apply a single numerical measure to *numbers* of hits and *accuracy* of hits. If it were possible to assess these factors separately, we might find two distinct criteria of the direction of causal influence if the dwindling, in both temporal directions, applies both to numbers of hits, and to the degree of resemblance of hits to the original. We could find an analogy for this in our broadcasting illustration, in that as distance from the transmitter increases, not only does volume decline, but distortion tends to increase. However, we must not press our analogy too far, for it is in many respects imperfect. The conclusions I wish to emphasise are that *if* we accept property β as a criterion of causal connection

(i) we can find at least one criterion of the *direction* of causal influence which is independent of the common-sense criterion ;

(ii) we must regard Carington's results as indicating precognition, that is to say, must conclude that some of the extra-chance " hits ", which were drawn before the display of the original drawing, were causally influenced, in the manner defined earlier, by the drawing or display of the original.

My tentative conclusion is that our evidence that PK and precognition are independent phenomena will become very formidable, if not conclusive, if and when we have (*a*) obtained results similar to Carington's in similar experiments using larger numbers of percipients, and (*b*) eliminated hypotheses 1 and 2 in PK experiments.

It seems necessary to apply very strict standards when trying to isolate PK and precognition, since we must surely accept what Driesch stated[1] as our first methodological principle—" no phenomenon may be admitted as fundamental if it can be in any way reduced to another, or represented as only a variant of it ". If a hypothesis, *e.g.* that precognition occurs (or *e.g.* that PK occurs), can be established by a single unambiguous experiment, this will contribute an immense increment to the probability that other facts which can be most simply explained by the same hypothesis are correctly so explained, even if other more complicated explanations of these facts are also possible. Whether hypothesis 3 can be plausibly applied to the spontaneous cases of ostensible precognition is another story. I hope that someone more familiar with those cases will examine this question. In concluding, I wish to record my debt to Professor Price, Dr Soal and Dr Thouless for many helpful suggestions, without which this paper would have been very much more inadequate and incomplete.

[1] Hans Driesch, *Psychical Research*, Bell & Sons, 1933, p. 110.

IMMANUEL KANT AND PSYCHICAL RESEARCH

By C. D. Broad

Part I

Historical

It is plain that at a certain period of his life, viz. in the sixth decade of the eighteenth century, Kant became interested in the experiences and speculations of another Immanuel, the Swedish seer Swedenborg. At that time Kant was about 40 years old. He had begun to lecture as *privat dozent* in the university of Königsberg in 1755 or 1756, and he did not become professor until 1770. He had already thought and written much about physics, astronomy, and geography, and had devoted himself to the old-fashioned metaphysics in which he had been brought up. But the system of " Critical Philosophy ", for which he was to become world-famous, was still in the future. The first sketch of it is contained in his inaugural dissertation on becoming professor in 1770.

Swedenborg was 72 in 1760. The only work of his to which Kant refers is the *Arcana Coelestia*. This had been published anonymously in London by John Lewis of Paternoster Row in eight large quarto volumes from 1749 to 1756. According to Signe Toksvig, Swedenborg's recent biographer, the authorship was first acknowledged in 1768. Presumably it had been an open secret for some time ; for Kant, in a work published in 1766, takes it for granted that Swedenborg is the author, and makes no suggestion that the *Arcana Coelestia* was anonymous.

So far as I am aware, there are two and only two known writings of Kant which are concerned with Swedenborg and his doctrines. One is a letter to Miss Charlotte von Knobloch, the other is a book entitled *Träume eines Geistersehers erläutert durch die Träume der Metaphysik*. The " *Geisterseher* " is Swedenborg. The letter contains about 1900 words, the book about 20,000. Kant lacked the art of condensation ; he was, to put it plainly, terribly long-winded. The letter abounds in stilted compliments, and the book in elephantine badinage.

(1) *Questions of dating*. The book was published anonymously in Königsberg and in the same year in Riga by another publisher. There is no doubt that the date of publication was 1766. But there is a serious muddle about the date of the letter, which I will now briefly consider.

The letter appears to have been first published by Kant's biographer Borowski, and it was alleged to be dated " Königsberg, 10 August, 1758 ". It is printed with that date in Vol. II of Hartenstein's edition of Kant's works. But, unless Kant was an even more remarkable seer than Swedenborg himself, this date is impossibly early. Hartenstein's attention was called to the point, and in the preface to Vol. III he discusses the question of the correct date. He quotes arguments by Kuno Fischer and Ueberweg which seems to show conclusively that it must have been 1763.

We need not go into elaborate detail, but the following fact suffices to make any date earlier than 1762 impossible. Kant refers in the letter to an incident in which Swedenborg seemed to show supernormal knowledge of a matter private to a certain princess in Stockholm. There is no doubt that this lady was Lovisa Ulrika, sister to Frederic the Great and wife to king Adolf Fridrik of Sweden. Now in his book, in which he also refers to this incident, Kant says that it happened late in 1761. This statement has been confirmed by the fact, which came to light many years later, that the Swedish courtier, Count Tessin, recorded the incident in his diary on Nov. 18th 1761 as having happened three days earlier. (My authority for this is Signe Toksvig's book.) It is plain, then, that news of it cannot have reached East Prussia much, if at all, before the beginning of 1762. Moreover, in his letter to Miss von Knobloch, Kant says that he had learned the story from a friend, that they had corresponded about it, and that he had instituted various enquiries. All this would plainly take some time.

I think that we may accept the arguments of Fischer and Ueberweg to show that the letter to Miss von Knobloch cannot have been written before 1763. Hartenstein states and adduces evidence that she married in July 1764 and became Frau von Klingsporr. Now Kant addresses her in the letter as "*gnädiges Fräulein*", which would have been absurd if she had been married at the time. So we may take it that the letter was written some time in 1763, *i.e.*, about three years before the publication of the book.

We do not know precisely when the book was written, but I think that it is certain that it was written after the letter. In the letter there is no suggestion that Kant has read any of Swedenborg's writings. He says that he is eagerly awaiting the appearance of the book which Swedenborg is about to publish in London, and that he has made arrangements to get it as soon as it leaves the press. Now it is certain that Kant had carefully read Swedenborg's *Arcana Coelestia* before writing *Träume eines Geistersehers*. So it is reasonable to conclude that he wrote the latter book some time after 1763 and some time before 1766.

I have gone in some detail into the question of the relative dates of writing the letter and the book, for the following reason. Where the contents of the two overlap they seem to express a very different attitude towards Swedenborg and his alleged supernormal gifts and achievements. The letter is rather strongly favourable, whilst the book is completely agnostic in its conclusions and decidely sneering and condescending in tone. In view of the fact that the book must have been written after, and cannot have been written long after, the letter, this contrast is of some interest.

I will now give a general account of the contents of the two writings.

(2) *The Letter*. It is plain that Miss von Knobloch had written to Kant some time before, to ask his opinion about a story which she had heard concerning a certain display of ostensibly supernormal knowledge on Swedenborg's part. It is evident from the context that the story concerns queen Lovisa Ulrika's letter to one of her brothers, and it will be as well at this point to give an account of the events from independent Swedish sources which were not available to Kant or to Miss von

Knobloch. In what follows I base my statements on Signe Toksvig's book on Swedenborg.

Count Tessin, a Swedish nobleman connected with the court at Stockholm, kept a diary, which has since been published. On November 18th 1761 he made an entry to the following effect. A story had been going around Stockholm concerning a recent feat of ostensible clairvoyance performed by Swedenborg with reference to the queen. Tessin asked Swedenborg for the details on November 18th. Swedenborg thereupon gave the following account to Tessin. About three weeks earlier he had had a conversation with the king and queen, and had told them of some of his experiences in confirmation of his theories. The queen asked him jokingly to try to bring her a message from her dead brother (the late Prince of Prussia), if he should happen to meet him in the spirit-world. On the Sunday before November 18th Swedenborg again presented himself at the palace and asked for an audience with the queen. He then told her something privately, which he had been enjoined not to mention to anyone else. The queen was much moved, and exclaimed : " That is something which no-one could have told except my brother!" On his way out, Swedenborg said, he met Councillor von Dalin and asked him to inform the queen that he would try to follow up the matter further for her.

So far Swedenborg's story to Tessin. He added that the queen had had a great shock, and that he would not venture to disturb her again until at least ten or twelve days had gone by.

Tessin, on his own account, states that the queen's consternation at Swedenborg's message is testified by all who were present. He mentions in particular Councillor Baron von Scheffer. He adds that the queen's account of the incident tallies with Swedenborg's, and that she has herself put Swedenborg to a new test. It should be mentioned that there is no evidence that Swedenborg ever did approach her again on this topic. It is, however, alleged (on what evidence I do not know) that, whenever she was asked about the incident in later life, she either acknowledged the truth of the story or changed the subject in an embarrassed way.

Signe Toksvig states that, after the queen's death, Count A. J. von Höpken, a Swedish statesman and friend of Swedenborg's, wrote an account of the incident. It appears from what he says that the queen had been carrying on a secret correspondence with this brother, notwithstanding that Sweden and Prussia were at war with each other at the time. Höpken's story is that the message which Swedenborg delivered referred to the last letter written by the queen to her brother before his death. Swedenborg, who claimed to have made contact with the spirit of the deceased prince, conveyed an apology from him for not having answered the letter and an appropriate reply to it. According to von Höpken, the queen said : " No-one but God knows this secret! "

We can now return to Kant and Miss von Knobloch. Kant begins by apologizing for his delay in answering her enquiry. He says that he thought it desirable to investigate the matter further before writing. He states that he is by no means inclined to accept such stories lightly. He thinks that it is a sound rule to take a negative attitude towards even the best-attested of them. He does not indeed deny the *possibility* of such alleged facts, for we know so little about the nature of a spirit, if such there be. But he thinks that, taken as a whole, such stories are not adequately attested. Then, again, the alleged phenomena are so unintelligible, and, even if genuine, so useless, that it is hard to accept them. Lastly, there are so many instances of proved fraud and credulity. Kant sums up this part of his letter by saying that, until he became acquainted with the stories about Swedenborg's feats, his attitude towards alleged supernormal phenomena was completely negative.

He then proceeds to tell how he was brought in touch with these stories, and how he tried to investigate them.

(i) *The Queen's Letter.* This incident was first brought to Kant's notice by a Danish officer, a friend of his who had formerly attended his lectures. The account given by this Danish officer to Kant was as follows. (I shall add explanatory historical notes in *square* brackets. They are based on statements in the preface to Vol. III of Hartenstein's edition of Kant's works.)

The Austrian ambassador in Copenhagen, *Dietrichstein* [who held office from 1756 to 1763], received a letter from *Baron von Lützow*, the ambassador from Mecklenburgh to the court of Stockholm. In this letter von Lützow stated that he, together with the Dutch ambassador to Stockholm, had been present at what Kant calls "the curious history which you" (Miss von Knobloch) "have already heard concerning Swedenborg". (I take this to mean that the two ambassadors had been present at the queen's reception when Swedenborg made his communication to her.) Dietrichstein in Copenhagen had either read or shown this letter to the Danish officer and other guests at a party.

Kant says that he thought it unlikely that one ambassador would send to another a false account of an incident concerning the sovereign to whose court he was attached, and would moreover send it in a letter intended to be communicated to others. He therefore wrote to the Danish officer and made further enquiries of him.

The officer said in his answer that he had again spoken to Dietrichstein on the matter, that the facts really were as stated, and moveover that Professor Schlegel had assured him that there was no possibility of doubt. (I do not know what weight, if any, is to be attached to this confirmation by Schlegel. It must be regretfully admitted that "what the Professor said" is not, as such, evidence.) The officer added that he was about to depart to the army under General *St. Germain*, and he advised Kant to write directly to Swedenborg. [St. Germain became a Danish field-marshal in 1760. The Danish army was mobilized in 1762 to meet a threatened attack by the czar Peter III.] Kant accordingly wrote to Swedenborg, and the letter was handed in by an English merchant in Stockholm. Swedenborg had received the letter favourably and had promised to answer it, but no answer had as yet come.

So much for the Danish officer. In the meanwhile Kant had made the acquaintance of a certain Englishman who had been in Königsberg in the summer before the date of writing. Kant describes him as "a fine man", says that he had become very friendly with him, and obviously has great confidence in him. This Englishman was about to visit Sweden, and Kant asked him to go into the whole question of Swedenborg's alleged marvels while there. (Signe Toksvig seems to assume without question that this man was Kant's great friend, the English merchant Green. I do not know if there is any evidence for this. No name is given to him in the letter.)

The Englishman did as Kant had asked him and wrote to Kant several letters describing his investigations and impressions. In his first letter he said that the statements of all the most distinguished persons in Stockholm supported the story about Swedenborg and the queen. He had not yet met Swedenborg, but hoped to do so shortly. He found it hard to believe the stories which highly sensible persons in Stockholm were telling about Swedenborg's intercourse with the unseen world.

In later letters the Englishman said that he had met Swedenborg and had been in his house. He describes the seer as a pleasant open-hearted man and a scholar. Swedenborg told him that God had given him the power to communicate at will with departed spirits, and he appealed to quite notorious evidence in support of this. When reminded of Kant's letter to him, Swedenborg said that he had received it. He was going to London in the May of that year and would there publish a book in which a complete answer to Kant's questions would be found. (According to Signe Toksvig, Swedenborg visited *Amsterdam* in 1762 and again in 1763. He was in England on a short visit in 1763, during which he delivered copies of his printed books to the Royal Society. But he did not visit England in 1762 and he published no such book as he had spoken of to the Englishman.)

Kant then proceeds to relate, on the evidence of his English friend, two other stories of ostensibly supernormal cognition on Swedenborg's part. These are the incidents of the lost receipt for Mme de Marteville's silver tea-service and of the fire on Södermalm in Stockholm. They are well known and often quoted ; but, so far as I am aware, there is no extant evidence for them except this letter of Kant's, quoting statements from letters of his unnamed English correspondent. Kant says in his letter to Miss von Knobloch that " the whole living public " is witness to these events, and that his friend, who relates the stories, " has been able to investigate them on the spot ". I will now reproduce Kant's account of these two incidents as reported by his English correspondent.

(ii) *The lost Receipt.* M. *de Marteville* was ambassador from Holland at the court of Stockholm. [He died April 25th, 1760.] Some time after his death the goldsmith *Croon* demanded from the widow payment for a silver service which her late husband had bought of him and which had been duly delivered. She was convinced that the bill had been paid ; but she could not find the receipt, and was in considerable distress, as the sum was a large one. She invited Swedenborg to call, explained the circumstances to him, and asked him to try to get in touch with the spirit of her husband. Three days later Swedenborg called on Mme de Marteville at a time when she had company to coffee. He said that the receipt was in a certain bureau upstairs. She answered that this was certainly a mistake, for that bureau had been cleared out and thoroughly searched and the receipt was not among its contents. Swedenborg answered that, if she would pull out the left-hand drawer, she would notice a certain board. If this were pulled out, a secret compartment would be disclosed, containing not only the receipt but also the late ambassador's private

Dutch correspondence. The whole company adjourned to the room, the drawer was opened, and everything was found as Swedenborg had foretold.

Two points are worth mentioning. In the letter the name of the ambassador is given as *Harteville*, but in the book it is given correctly as *Marteville*. The second point is that, if the incident happened at all, it must have done so in the latter part of 1760. If the correct date of Kant's letter is 1763, his friend was presumably in Stockholm some time in 1762. So about two years would have elapsed between the incident itself and the Englishman's enquiries about it.

(iii) *The Stockholm Fire*. Kant says that this story seems to him to have the greatest probative force of them all, and that it is free from all possible doubt. He gives the date as "towards the end of September 1756". Dates seem not to have been Kant's strong point, for in *Träume eines Geistersehers* he assigns it "towards the end of 1759". Hartenstein gives the date of the fire as July 19 1759, and quotes as his authority P. 77 of Part 121 of a German periodical called *Neue genealogische-historische Nachrichten* for 1760. It would be worth while, if it has not already been done, for some Swedish investigator to enquire whether there is any contemporary account of the alleged incident in public or private records either in Stockholm or in Göteborg. It should be remarked that serious fires were very common in Swedish towns, which were largely built of wood, and that presumably Stockholm was no exception. It should also be noted that the distance between Göteborg and Stockholm is about 285 miles by the present main-line railway.

The story in Kant's letter is as follows. Swedenborg landed at Göteborg from England at 4 p.m. on a certain Saturday. He was invited by a Mr Wm. Castell to dine at the latter's house with a party of fifteen persons. At about 6 p.m. Swedenborg left the company for a short while and returned looking pale and alarmed. He said that a dangerous fire had broken out on Södermalm in Stockholm, where his own house stood. He was restless and went out several times. He said that the house of a certain friend, whom he named, was already in ashes, and that his own was in danger. At 8 p.m. he again came in after a short absence and said that the fire had been quenched at the third door from his house. These statements of Swedenborg's were reported the same evening to the Governor of Göteborg. Next morning, *i.e.*, Sunday, the Governor interviewed Swedenborg, who described the fire in some detail and said how it had begun and how long it had lasted. On the Monday evening came a messenger, who had left Stockholm on the Saturday while the fire was going on. He brought letters with him, in which the fire was described in a way which tallied with Swedenborg's statements. On the Tuesday morning the royal courier from Stockholm arrived at the Governor's house with a precise account of the damage and a statement that the fire had been put out at 8 p.m. on the Saturday.

Kant says that his English friend has investigated all this, not only in Stockholm, but also during a stay of about two months in Göteborg, where he is acquainted with the chief business houses. Kant adds that, in the short time which has elapsed since 1756, most of the eye-witnesses are still alive. (If the correct date is 1759, the time-lapse is considerably shorter, for the English friend was presumably in Sweden in 1762.)

Kant ends his letter to Miss von Knobloch by mentioning that his English friend has told him something of Swedenborg's accounts of his intercourse with the spirits of the dead and of conditions in the spirit-world. Kant says that he wishes that he could himself have interrogated Swedenborg on these matters, because his English friend is not skilled in framing and putting those questions which would throw most light on essential points.

(3) *Träume eines Geistersehers.* We can now leave the letter and consider the book. This is a very curious production in itself. Moreover, a comparison of it with the letter raises interesting, but perhaps insoluble, questions about Kant's motives for writing it at all, for publishing it anonymously, and for adopting towards the subject in general and Swedenborg in particular the bantering contemptuous attitude which he does adopt.

The book begins with a preface, and the rest of it is divided into two Parts. Part I, which is subdivided into four Sections, may be described as an able and elaborate general discussion of the philosophical problems involved in the notion of a disembodied spirit, of a world of such spirits, and of the relations of body and soul in human individuals, and in claims by certain men to be in touch with the inhabitants of the spirit-world. It is not directly concerned with Swedenborg or his experiences. Part II is subdivided into three Sections. The first of these repeats the three stories of Swedenborg's alleged feats of ostensibly supernormal cognition which were discussed in the letter. The second Section contains an elaborate account of the doctrine as to the nature and laws of the spirit-world which Swedenborg professed to have derived by personal observation and from conversations with spirits. This account is based upon the contents of the eight quarto volumes of *Arcana Coelestia*, which Kant had bought and evidently studied carefully. So far as I can judge, Kant's synopsis of Swedenborg's main doctrines is adequate, accurate, and clear. The third Section is entitled *Practical Conclusion of the Whole Treatise.* The practical conclusion is, roughly, that we should cultivate our gardens, and not waste our time with either what metaphysics or what self-styled mediums claim to tell us about the spirit-world. Speculation about that world is fruitless. It can give no support to genuine morality, whilst, on the other hand, any morally good man feels assured of human survival without recourse either to metaphysics or to alleged mediumistic evidence.

I will now consider in somewhat greater detail the part of the book which covers the same ground as the letter, viz. Part II Section I. In the letter, as we have seen, the story of queen Lovisa Ulrika's interview with Swedenborg about her brother is told on the authority of the Danish officer reporting a letter from von Lützow in Stockholm to Dietrichstein in Copenhagen. The other stories are told on the authority of the Englishman, who has interviewed Swedenborg and investigated the evidence for the tales of his exploits at Kant's special request. The impression which one gets from the letter is that Kant was satisfied with the evidence, at any rate as regards the Stockholm fire.

In Part II Section I of the book Kant introduces the topic by saying that " the whole question is neither important enough nor sufficiently well prepared to enable one to come to any decision about it ", and that he presents these stories " with complete indifference to the favourable or unfavourable judgment of the reader ".

As regards the story about queen Lovisa Ulrika, he does not mention her by name, but speaks of her as " a princess . . . whose great intelligence and insight would make it almost impossible that she should be deceived in such matters ". The authority for the story is stated to be a letter from an ambassador at her court to another ambassador in Copenhagen. Kant adds that the story agrees with what has been elicited in answer to special enquiries. There is no mention of the Danish officer, but Kant is no doubt referring to his correspondence with him.

The story about the missing receipt is now correctly referred to Mme de *Marteville* instead of *Harteville*. Kant now says that this tale " has no other testimony than common report, which is very inadequate proof ".

As regards the story of the Stockholm fire, Kant says that it is " of a kind which could very easily be completely proved or disproved ". At the end of this Section, after a great deal of palaver, he says that it would be worth while for anyone, who had money enough and nothing better to do, to go and investigate these and similar stories at first hand. He gives no hint that a friend of his, of whom he has a very high opinion, has personally investigated the evidence for the story of the Stockholm fire both in Stockholm and Göteborg ; that this friend was persuaded of its truth ; and that Kant himself, in a recent letter dealing expressly with this topic, had described the story as free from all possible doubt. In fact the Englishman, who first brought the second and third of the stories to Kant's notice, and who had investigated the whole question of Swedenborg's alleged supernormal feats at Kant's special request, is never mentioned in the book. Kant does indeed admit, in the Preface, that he himself has made some investigations into the truth of such stories. But he almost apologizes for having done so, and he asserts that he " found—as is usual where there is nothing to seek—nothing ".

Towards the end of Part I Section IV Kant says that he would not venture to deny all truth in such stories. He doubts each severally, but is inclined to give some credence to them taken collectively. He remains " serious and undecided " in view of them. Nevertheless, he ends this Section by saying that for the future he will " abandon investigations which are altogether in vain ". (This remark, in view of its place in the book, may refer rather to the metaphysical speculations about spirits in Part I than to the alleged empirical evidence in Part II.) Towards the end of his synopsis of Swedenborg's teachings about the nature and laws of the spirit-world (Part II Section II) Kant says that one might be inclined to attach some weight to his unverifiable statements about the next world, if and only if one could appeal to testable instances of alleged supernormal knowledge by him, and if one were to find that they are supported by living witnesses. " But ", Kant adds, " this one never finds ". How this last remark is to be reconciled with statements which I have quoted from Kant's letter to Miss von Knobloch, I do not profess to conjecture.

At this point we may well ask ourselves what Kant's motives could have been for writing and publishing *Träume eines Geistersehers*. In the Preface he gives two reasons. One is that he wrote the book at the instigation of friends, known and unknown. Towards the end of Part II Section II he repeats that he was put on to this thankless task through the importunities of idle and curious friends. From their point of view the enquiry has led to nothing and has been mere waste of time. The second reason which he gives in the Preface is that he had bought and read through a big book, viz. Swedenborg's *Arcana Coelestia*, and did not want all his work to be wasted. Early in Part II Section II he describes this book as consisting of " eight quarto volumes of nonsense ". Later in the same Section he remarks that he has saved the curious reader from spending £7 sterling in satisfying a little idle curiosity.

Obviously these cannot have been Kant's main motives. He was not at all a wealthy man and he was a very busy one. It is most unlikely that he would have spent £7 on the *Arcana Coelestia* and then ploughed through it and given a careful synopsis of its teachings about the spirit-world merely to satisfy the idle curiosity of some or to save the pockets of others. We must remember the statement in the letter to Miss von Knobloch that he is impatiently awaiting the book which Swedenborg is about to publish in London and that he has made arrangements to get it as soon as it leaves the press.

I would suggest very tentatively that what may have happened is this. Instead of getting the book which he was expecting, viz., an account by Swedenborg of those of his ostensibly supernormal cognitions which were open to verification in this world, together with adequate testimony for them, he was landed with the eight volumes of the *Arcana Coelestia*. This is largely occupied with an elaborate symbolic interpretation of every word and sentence in the books of Genesis and Exodus. It may fairly be described as one of the most boring and absurd productions of any human pen. After reading it Kant may well have been inclined to dismiss with contemptuous impatience the alleged supernormal feats of a person who could devote a large part of his life to writing such stuff, and to ignore the fact that he himself had very recently been fully persuaded of the veridical nature of some of them, through the testimony of the Danish officer and the English friend. It was as if one had heard on very good evidence that Mr. X had made certain bold but highly ingenious emendations to difficult passages in classical texts, and had then found that he was a British Israelite whose published works were mainly devoted to proving, by help of measurements on the Great Pyramid, that the earth is flat and that Bacon wrote all the works commonly attributed to Shakespeare. If Kant could have picked up Swedenborg's *De Coelo et Inferno*, a single volume published in London in 1758, he would not indeed have got what he was expecting, but he would have found a tolerably succinct account of Swedenborg's doctrine of the spirit-world, and would have been saved much time and money and justifiable irritation.

However this may be, Kant says explicitly that he had a different end in view from that of the friends whose idle curiosity set him upon writing the book. His subject is metaphysics. That science has two functions. One is to try to answer the questions which enquiring minds raise when they seek to investigate by reason the more deeply hidden properties of things. The other is to consider whether such questions are concerned with anything that we can possibly know, and to see what relation they bear to our empirical concepts, on which all our judgments must ultimately be founded. The essential service which it renders in its second capacity is to show that we must keep within the bounds of ordinary sense-perception and ordinary reasoning. The upshot of the book is to reinforce that conclusion in reference to claims, such as Swedenborg's, to empirical knowledge beyond those limits.

Lastly, we might raise the question: Why did Kant publish the book anonymously? If there had been a good chance of the anonymity being preserved, one could think of excellent reasons. Kant no doubt wished to keep a good reputation as a level-headed burgher, scholar, scientist, and philosopher, and not to incur the contempt of his colleagues and fellow-townsmen or to prejudice his chances of eventual election to a professorship. Even in England and the U.S.A. to-day an acknowledged addiction to even the most respectable branches of psychical research would probably be somewhat detrimental to the professional prospects of a young biologist and still more to those of a young psychologist. In Sweden, unless I am much mistaken, it would still be almost fatal to one's chances of a professorship in many subjects. It is reasonable to suspect that, in " enlightened " academic circles in East Prussia in the middle of the eighteenth century, a reputation for having carefully read Swedenborg's writings and having paid serious attention to the evidence for his alleged feats of clairvoyance, would be enough to condemn a *privat dozent* to remain in that position for the rest of his life.

But could Kant possibly have hoped to preserve his anonymity? This seems to me almost incredible. I should have thought that the style of the book as a whole and the contents of the philosophical part of it would have betrayed the authorship to colleagues in Königsberg almost at once. Moreover, the " idle and curious friends ", who had urged Kant to make a study of Swedenborg, could hardly have felt any doubt as to the authorship of an anonymous book on the subject in which they are explicitly referred to, and they could hardly be relied upon to keep their suspicions to themselves. I can only suggest that the conventions of the time and place permitted a *privat dozent* to flirt with this disreputable subject, provided that he made an honest man of himself by maintaining the *form* of anonymity and by adopting a sufficiently bantering and condescending tone towards the alleged phenomena and the persons of whom they were narrated. If these were the conditions, Kant certainly complied with them.

Part II

Theoretical

I shall now consider Kant's philosophical discussion of the problems raised by Swedenborg's theories and claims. It seems to me that it is of some interest to do this. We have at our disposal nowadays much more varied, better attested, and more carefully investigated data than Kant had, but there has been very little discussion by first-rate philosophers of their theoretical implications. Kant was certainly one of the greatest philosophers of all time; he combined to an extraordinary degree critical acumen and constructive fertility and originality. He had also a most remarkable capacity for " sitting on the fence " and stating the strong and the weak points of opposing concepts. This is very noticeable in his discussion of mechanism and vitalism in the *Critique of Judgment*, and it is almost equally prominent in *Träume eines Geistersehers*.

(1) *What is a Spirit?* Kant begins by raising the question : What do we understand by a " spirit "? We often use this word, so presumably it means something, even if it expresses only a fictitious idea.

We cannot have derived the notion of a spirit from instances of it which we have ourselves observed, for we can use the word intelligibly even if we doubt or deny that there are spirits. Kant remarks here that many of our notions, though not *derived* from specific experiences in the direct way in which, *e.g.*, the notion of " red " or of " man " is derived, yet arise *on the occasion of* certain experiences by a kind of unwitting inference. Such notions may be called " surreptitious " (*erschlichene*). They may be in part mere fictions of the imagination ; but they may be in part applicable to reality, for these unwitting inferences need not always be mistaken.

In order to understand Kant's attempted definition or description of a " spirit " it will be best to begin with a brief account of his doctrine of matter.* For his account of spirits is developed in comparison and contrast with the notion of matter. I shall state what I understand to be Kant's view in my own way. He assumes that any finite body consists of a number of *simple* material substances. Each of these is, in a certain sense, *located at* some one geometrical point at each moment, though it may be located at different points at different moments. He further assumes that no two such substances can be located at *the same* geometrical point at one moment, though one of them may at a later moment be located at the point at which another was located at an earlier moment.

Now Kant equates the proposition that two simple material substances cannot be located at the same point at the same moment with the proposition that any such substance exerts upon any other a *repulsive force*, which increases rapidly as the distance between the points at which they are located is diminished beyond a certain critical amount, and which would become infinite if this were reduced to zero. He points out that, although a simple material substance is indivisible and is in one sense located at a *point*, yet there is an important sense in which it occupies a *finite volume*. This is obvious enough. I have so far talked of a punctiform element of matter located at a point and surrounded by a certain field of repulsive force. But one could more properly *identify* the elementary material substance with this field, and say that it is present with more or less intensity at any point within the sphere in which the repulsive force is appreciable. The essential fact is that there is a field of repulsive force which is at each moment symmetrical in all directions about a certain singular point at which it is infinite. If one identifies the elementary substance with this symmetrical field of force, then one can say that it is *located* at any moment at the centre of this field, and one can say that it *dynamically occupies* at any moment, with systematically different degrees of intensity, every point within a small sphere around that centre.

Kant remarks that all this is consistent with the statement that a simple

* A theory on the same lines was worked out in considerable detail by Boscovich and published in 1763 at Venice.

element of matter is unextended. To say that a thing is extended implies that it would be significant to say that it would occupy a volume even if nothing but it had existed. But the field of repulsive force associated with a single element of matter is a mere fiction unless there is at least one other element of matter. Repulsion in accordance with a certain law of variation with distance is meaningless unless we conceive that there are *at least two* elementary substances to repel each other.

It should be remarked that Kant gives the following reason for associating a finite sphere of repulsive force with each simple element of matter. A continuous macroscopic body fills a finite volume, and Kant argues that any substance which is a genuine *part* of it must therefore occupy a volume which bears a finite proportion to that which is occupied by the body as a whole. Now a point is a *limit in*, not a *part of*, a volume. Therefore a continuous macroscopic body could not consist of elementary particles which were punctiform and nothing more. I take it, therefore, that his final theory is that a continuous macroscopic body is composed of a *finite* number of spherical fields of repulsive force, each centred around a different point within the volume which the body fills, each having appreciable intensity only within quite a small radius, and each increasing rapidly towards infinite intensity as the distance from the centre decreases towards zero.

We are now in a position to consider Kant's definition of a " spirit ". In view of the account just given of elementary material substances, it is plainly useless to define a spirit as a simple rational substance. For its simplicity will not distinguish it from an elementary material substance. And the addition of " rational " will not help. For, Kant says, we know nothing about the *internal* properties of elementary material substances. So far as we know, there is nothing to prevent such a substance being rational, though there is also nothing to suggest that any of them are so.

Accordingly Kant adds two negative characteristics to distinguish a spirit from an elementary material substance. The first is that the presence of a spirit in a region of space would not involve any resistance to the entry of an elementary material substance into that region. The second is that, if we imagine each of the elementary material substances which together make up a finite continuous body to be replaced by a spiritual substance, the resulting aggregate would not be a body occupying the volume occupied by the original body. The second feature evidently follows from the first. If the volume were occupied after the change, in the way suggested, by a collection of spirits, there would be no reason why matter from outside should not freely enter it; since a spiritual substance does not oppose the entry of an elementary material substance into the region which it occupies. But, on the other hand, to say that the region was continuously occupied by a *body* after the change, would entail that matter could not enter it from outside without either shifting the present contents or encountering ever-increasing opposition if they could not be shifted.

So, in effect, Kant's proposed definition of a " spirit " is a rational simple substance whose presence in a region of space does not offer any resistance to the simultaneous presence of an elementary material substance within that region. And an immediate consequence of this is that the presence of any number of spirits within a region, however they might be located within it, would not *eo ipso* constitute a *body* continuously filling that region.

(2) *Are there Spirits?* The next question is whether there is anything answering to this definition. Kant holds that philosophers have proved satisfactorily that anything which thinks must be simple, and that the ego of each one of us cannot be a whole composed of a plurality of interconnected substances. So each of us can be sure that his soul is a simple substance. But it does not follow that it is a *spirit* in the sense defined. For there is nothing in these arguments to show that it would not oppose the entry of any material substance into any region in which it was present. And so there is nothing to show that a suitable aggregate of human souls would not constitute a finite continuous body.

The question now arises whether " spirits ", in the sense defined, are even *possible* existents. I think that the essential points in Kant's argument might be put as follows. The question comes to this. Is there any inconsistency in supposing that there might be simple substances which, like elementary material substances, *are* or *may be* present in space in a sense which does not involve their being extended, but which, *unlike them*, do not offer any resistance to the entry of elementary material substances into the regions which they occupy?

Now Kant distinguishes between cases where one has a positive rational insight into the possibility of something, and other cases where one has no such insight and one's only ground for saying that so-and-so is possible is that experience provides us with actual instances of so-and-so. He does not give any examples of the first alternative, but I think that it is quite easy to do so. One has rational insight into the fact that five and only five forms of regular solid are possible in Euclidean space, and that one of these possibilities is the regular icosahedron. One does not just find oneself forced to admit that the icosahedron is a possible form of regular solid because actual instances of such a solid have been observed. (It is in fact very doubtful if instances existed until they were constructed by makers of mathematical models in order to illustrate the already recognized possibility.) On the other hand, one has no rational insight into the fact that blue is one of the possible colours ; one knows that it is so merely because one has seen blue objects.

Kant argues that we have no rational insight into the *possibility* of the connexion between being a simple substance and being located at a point in space and occupying a sphere around it with a field of repulsive force of a certain kind. We ascribe this punctual location and this sphere of repulsive force to the elements of matter merely because of certain facts which we observe when we perceive and operate with matter in bulk. Still less do we perceive any *necessary connexion* between the various factors in the notion of an elementary material substance. As regards the concept of a spirit, we have no rational insight into either the possibility or the impossibility of a simple substance occupying a region of space and yet *not* being located at the centre of a field of repulsive force. Thus, so far as regards rational insight into the possibility, impossibility, or necessity of the combination of certain factors in a concept, the concept of a spirit is in precisely the same position as that of a simple element of matter. We have no such insight in either case. The only advantage enjoyed by the latter over the former is that we have certain perceptual experiences which establish its possibility by providing actual instances of it. Kant's conclusion is that the possibility of " spirits ", as defined by him, can never be refuted. But there is no hope of ever getting rational insight into it, and, unlike the concept of simple material substances, its possibility can never be established through instantiation by sense-perception.

(3) *The human Soul and its Body.* As we have seen, Kant holds that, although it is certain that a human soul is a simple substance, it is by no means certain that it is a " spirit " in the sense defined. For, so far as we can tell, it might be a simple *material* substance endowed with rationality. We must now consider his further discussion of the embodied human soul.

Suppose that the human soul were a spirit in the sense defined. Then the question could be raised : What is its *place* in the material world? Kant answers that the place occupied by the body which a person calls *his* body would be the place occupied by his soul. Suppose that we then raise the question which would run in my terminology as follows : At what *point* within the place occupied by a person's body is his soul *located?* Then Kant suspects that the question is based on mistaken pre-suppositions. If I may put the matter in my own way, the mistake might be expressed as follows. In the case of a simple element of matter one can distinguish a certain geometrical point, within the region which it *dynamically occupies*, as *the point at which it is located*. One can do this because of the peculiar structure of the field of repulsive force which is characteristic of an elementary material substance. The peculiarity is that the force falls off in intensity in all directions symmetrically from a certain singular point at which it would be infinite. But a spirit has been defined as a simple substance which is *not* associated with a field of repulsive force of that kind. Yet, except on the assumption that it is so associated, the question : " At what point is a spirit located within the region which it dynamically occupies? " is meaningless.

Now, so far as empirical facts go, Kant thinks that it would be reasonable to say that a person's soul is present equally at every place at which it would be natural to locate any of his sensations. As he puts it : " I feel the painful pressure when my corn pains me, not in a nerve in my brain, but at the end of my toe." In general, Kant holds that there is nothing in our experience to support the Cartesian view that the soul is located at a certain point in the region occupied by the brain. He says that he knows of nothing which would refute the Scholastic doctrine that a person's soul is present as a whole in his body as a whole and in every part of it. This would not make the soul extended. For its immediate presence throughout a whole volume would imply only a finite region of immediate action and passion, as in the case of a simple element of matter, and not a plurality of parts logically independent of each other.

Kant then proceeds to discuss the Cartesian view that the soul is located at a point within the region occupied by the brain. He asserts that the only empirical evidence for this is that, after hard thinking, one is liable to feel characteristic sensations of stress and pressure in one's head. But a similar argument, starting from other empirical facts, would locate the soul in other parts of the body, *e.g.*, in the heart or the diaphragm. Kant suggests that the reason why hard thinking is felt to take place in the head may be the following. It always takes place by means of symbols, and these are always visual or auditory images. Now visual and auditory *sensations* are specially connected with the head, because the eyes and ears are part of it; and it is very likely that visual and auditory imagery involves the same, or nearly the same, parts of the brain as the corresponding kinds of sensation. For my own part I should have thought that the grounds, whether they be good or bad, for the Cartesian view are certain anatomical and pathological facts, which seem to show that the sensory nerves are *transmissive* conditions, without which stimuli that affect the peripheral parts of the body fail to produce sensations in the soul.

Kant holds that it is scarcely worth while to discuss direct arguments for and against the Cartesian view of the location of the soul, because we know so little of the soul's nature that any such arguments are inevitably very weak. It is more profitable to consider certain implications of the theory.

He thinks that the following would be one of them. On the Cartesian view the soul would not be distinguished from an elementary material substance by the way in which it is in space. Each could properly be said to be located at a point, though each would also be dynamically present throughout a certain small volume surrounding that point. Now *reason* is a purely internal property, which we should not be able to perceive with our senses in elementary material substances even if they possessed it. There would therefore be no empirical objection to supposing that the simple elements of matter are all endowed with reason, and that a person's soul is just one such simple material substance among the millions of others which together make up his body. Its outstanding position would be due merely to the special situation which it occupies in a certain natural machine (the body), viz., at the place where the connexions of neural paths enable its inner faculties of thinking and choosing (which it shares with all other elementary material substances) to affect and be affected by the outer world. If this were so, would not the most reasonable conclusion be that a human soul (which would be just a simple material substance that has, by an extraordinary chance, come to occupy this special position in an appropriate natural machine) would revert after death for the rest of eternity to its normal condition of a simple element of matter?

Another consequence of the Cartesian view would be this. There would be a certain one tiny bit of a person's brain, the removal of which would suffice to de-animate him. Kant then points to the fact that there are plenty of cases where a man has lost a fair proportion of his brain without losing his life or his power of thinking. He does not elaborate the argument; and, as it stands, I do not think that it proves anything against the Cartesian. Let us suppose, however, that it were true that for a certain part P_1 of the brain there is an instance of a man who survived and continued to think after *that* part, or a larger part containing it, had been removed. Suppose that a similar proposition is true for parts P_2, P_3, ...P_n. Lastly, suppose that P_1, P_2, ...P_n together cover the whole of a human brain. (They might to some extent overlap each other; the important point is that they should be collectively exhaustive, not that they should be mutually exclusive.) Then, it seems to me, the Cartesian theory would begin to look very shaky. But whether there is such a set of empirical facts, I do not know.

So much for Kant's reactions to the Cartesian doctrine. He confesses that, as a matter of personal conviction, he is much inclined (i) to assert the existence of spirits, in the sense defined, and (ii) to regard his soul as such a substance. He adds that, whatever reasons there may be for these convictions, they apply equally to all *living* beings. It seems to Kant that the essential peculiarity of a living organism is to be to a certain extent spontaneous and active from within, *i.e.*, to have some power of determining its own actions and modifying itself by something analogous to *choice* in human beings.

Now it is characteristic of inorganic matter that it occupies space by a non-voluntary force which is limited by external counteraction. So it is difficult to believe that living organisms would have the features of limited self-determination and *quasi*-choice if they were composed entirely of elementary material substances. The upshot of the discussion is that Kant thinks it likely that there is something analogous to a spiritual substance wherever there is a living organism, or at any rate an *animal* organism. We cannot possibly expect to have clear ideas of the various possible grades of such little-understood entities as non-material simple substances. But at any rate we can distinguish those which are at the basis of the manifestations of purely animal life from those which include reason as part of their spontaneous activity. Only the latter would properly be called " spirits ".

If a human soul is a spirit, then the connexion between it and the organism which it animates is a great mystery. On the one hand, it has to be conceived as forming, together with the body which it animates, a whole of a peculiar kind, viz., a certain human individual. On the other hand, if the soul be a spirit, none of the well-known kinds of combination, *e.g.*, that of the parts of an organism or of a crystal or of an artificial machine, can be characteristic of this whole. How can a bodily substance act on a spirit, which, by hypothesis, offers no resistance to its entry into the place which it occupies?

Kant says that it would seem necessary to suppose that a spirit acts on the simple elements of a body, not in respect of the external repulsive forces by which such elements interact with each other, but directly in respect of their *inner* states. It seems obvious to him that every substance must have inner states and undergo a series of inner changes which are the foundations of its external relations and their changes. Leibniz, as is well known, held that these inner states are of the nature of perceptions. Kant says that the numerous philosophers who have laughed at this theory may be invited to say (i) whether they think that there could be substances with *no* internal states but *only* variable external relations to other substances, and (ii) if not, whether they can think of any better account of the inner states, on which the external actions depend, than to say that they are analogous to perceptions. To say that *every simple element in a body* has inner states which are somewhat analogous to perceptions would not of course imply that *the body as a whole* has anything of the kind.

The upshot of Kant's discussion of this topic could perhaps be stated as follows. A change in the inner state of material element A produces a change in that of material element B only indirectly. It does so by being correlated with a change in the external field of force of A, which is correlated with a change in the external field of force of B, which is correlated with a change in the inner state of B. But a change in the inner state of a spiritual substance affects the inner state of the material elements of the body which it animates *directly* by a kind of telepathic *rapport*. *A fortiori* this must be the way in which one spiritual substance affects another spiritual substance.

Kant says that he does not pretend to understand how a certain spirit and a certain body come to form one human individual at conception, nor how this union comes eventually to be dissolved on the occasion of fatal accident or disease.

(4) *The Spirit-world.* We can now pass from Kant's discussion of the nature of a spiritual substance and the problems raised by embodied spirits to his discussion of the notion of a world of inter-related spirits.

The phenomena of inorganic matter can be explained satisfactorily in terms of extension, figure, motion, impenetrability, and various natural forces expressible in mathematical terms and subject to the laws of mechanics. But there are also living organisms in the world. As we have seen, Kant thinks that there must be substances of a special kind behind *vital* phenomena. These cannot be regarded as subject to the laws of motion in general or impact in particular. On the contrary, they seem to govern themselves and to organize non-living matter by their own inner activity.

Kant admits that the only satisfactory explanations of particular phenomena in physiology, etc., are in physico-chemical terms; though he thinks that men like Stahl (who used vitalistic conceptions and terminology) have often been led to discover important facts which were overlooked by men like Boerhaave (who carefully eschewed them). We do not know how far life extends in what we take to be inorganic matter, and in any case the most that we can know of the influence of immaterial agencies in organic nature is *that* it exists, not *how* it operates or *how far* it extends. But Kant holds that, subject to these limitations, we can conclude, with reasonable though not demonstrative certainty, from vital phenomena to immaterial organizing entities obeying peculiar laws of their own, In so far as these laws are concerned with the effects produced by these entities in living matter, they may be called *organic* laws; in so far as they refer to the mutual interactions of such entities, they may be called *pneumatic* laws.

Now it would hardly be plausible to suppose that these immaterial entities are connected only *indirectly* with each other, through the interconnections of the various bodies with which they are severally connected. For it might well be argued that at any moment only a comparatively small proportion of the immaterial substances are connected with bodies; that even these are also directly interconnected; and that their connexion with bodies is contingent and transient, whilst their direct interconnexions are intrinsic and permanent. So it is plausible to suppose that all these immaterial substances are interconnected directly to form a single system, which we could call the immaterial or spiritual world.

This world would include (i) all *finite intelligences*, some of which would be united to living organisms to constitute persons, and others not; (ii) the *sensitive souls* of all animals; and (iii) all *organizing entities* in nature, even when the vital phenomena which evince them do not include spontaneous movements. All these three kinds of immaterial substance would form a system which does not depend on the peculiar conditions which govern the relations of bodies. Here, *e.g.*, spatial and temporal separations, which make the great clefts in the material world, would be non-existent, though there might be other conditions of separation. A human soul during its earthly life would be a member of two worlds. As embodied, it would be especially associated with a certain region of space and stretch of time, and it would perceive clearly and affect voluntarily only certain limited portions of the material world. But, as a member of the spiritual world, it would not be located in physical space-time, and there is no reason to think that spatio-temporal categories of any kind would be applicable to it. In that capacity it would receive and impart influences of an immaterial kind. At death only the direct relations with other immaterial substances would remain, and the soul would become clearly aware of them.

We can now conceive the following possibilities. (i) That, even in this life, each human soul is in close connexion with the rest of the immaterial world, acts on it, and receives influences from it. But under normal conditions it is unaware of these actions and passions. (ii) That disembodied spirits have no conscious sense-perception of the material world. For such a spirit is not connected with any particular organic body to form a person, and thus has no location in the material world and no bodily organs through which to perceive and act upon it. (iii) That disembodied spirits can influence and be influenced by souls which are animating human bodies, since these are of the same nature as they and stand in direct mutual relations with them. But disembodied spirits could not receive and assimilate those ideas in embodied souls which depend upon the body and its relations to the rest of the material world. Conversely embodied souls could not receive and assimilate the intuitive cognitions which disembodied spirits have of themselves and of other immaterial entities. At best each party could receive such ideas from the other only in a *symbolic* form.

Kant then considers certain psychological and ethical facts about men, which he thinks fit in very well with the hypothesis that our souls live in these two worlds. I am bound to say that I do not find his argument at all clear or convincing at this point.

One set of facts which he adduces is this. Each of us strongly desires certain kinds of unity and co-operation with other men, and feels strong pro-emotions towards such relationships, not as a mere means to his own preservation or happiness, but for their own sake. Such desires and emotions often conflict with others which are purely self-confined, such as desire for one's own happiness, fear of death, etc. Each man, *e.g.*, quite directly desires and values the recognition and approval of himself and his actions by others. He likes to compare what he thinks good and true with what others think good and true; he is disturbed if there is a difference of opinion; and he tries to secure agreement. Kant says that all this is "perhaps a feeling of the dependence of one's own judgments upon the universal human understanding, and a means of creating a kind of unified reason for the whole thinking being". I take "the universal human understanding" to mean the supposed system of directly interconnected human spirits, embodied and disembodied; and I take "creating a kind of unified reason for the whole thinking being" to mean increasing the unity of that system in such a way and to such an extent that it constitutes a kind of rational super-individual mind. If that is Kant's meaning, it seems to me to be an hypothesis which is barely intelligible in itself and derives little support from the empirical facts adduced in its favour.

Kant then passes from these to another set of facts which he considers to be "more illuminating and easier to see" for the present purpose. These facts seem to me to be a strange mixture, and the interpretation of them which Kant proposes is far from clear to me.

I think that it will be best to translate the main passages. They run as follows. "If we consider outer things in reference to our needs, we cannot do so without at the same time feeling ourselves to be bound and limited by a certain feeling which makes us notice that a foreign will, as it were, is active in us and that a necessary condition for our own will and pleasure (*Belieben*) is the concordance (*Beistimmung*) of others. A secret power compels us to direct our intentions to the welfare of others or in accordance with the choice of others, although this often goes against the grain and strongly conflicts with selfish inclinations. . . . From this arise moral motives . . . the rigid law of obligation and the weaker one of benevolence, both of which extort many sacrifices from us. In consequence of this we perceive ourselves to be dependent in our innermost motives on the *rule of the universal will.* From this there arises in the world of all thinking beings a *moral unity* and a systematic constitution according to purely spiritual laws. If we like to give the name *moral feeling* to this felt compulsion on one's own will to adjust itself to the universal will, we are speaking of it merely as a phenomenon which does in fact occur in us, without expressing any view as to its causes".* (Kant then compares this to Newton's use of the word "gravitation" to describe the mathematical formula to which the mutual attraction of matter does in fact conform, and not to suggest or imply any particular theory as to the causes of this phenomenon. But, he says, Newton had no doubt that the phenomenon of gravitation does evince a fundamental and universal activity of matter.) The quotation now continues as follows. "Would it not be possible to think of the phenomena of moral motivation in thinking beings, in respect of their mutual relationships, as a consequence of a genuine active force by which spiritual beings influence each other? In that case moral feeling would be the felt dependence of one's private will on the universal will. It would be a consequence of the natural and universal interaction, by which the immaterial world attains its moral unity as it develops into a system of spiritual completeness in accordance with the laws of its own interconnexions."

I would make the following comments on the passage which I have quoted. (i) As regards the first sentence, I will say only that it is highly obscure in the original and that my translation has been made after

* The italics throughout are Kant's.

consulting an English colleague who is an expert in the German tongue. When this sentence is taken in its context the interpretation which I have put upon it seems to be the most plausible which the words and phrases will allow.

(ii) The phrase in the second sentence about directing our intentions " in accordance with the choice of others " is highly ambiguous. We do this, *e.g.*, when we obey an order purely through fear, when we fall in with another person's choice because we like him and desire to gratify his wishes, and when we follow the advice of a person, such as a doctor or a lawyer, whom we believe to be an expert. The second alternative might, perhaps, with a little stretching, be said to come under the " weaker law of benevolence " ; but none of them would seem to come under " the rigid law of obligation ".

(iii) I think it may well be true that many persons are inclined to interpret their sense of obligation to do something which goes against the grain as involving a kind of conflict between their own will and a foreign will. And I think they would feel that the only proper solution is, not a recalcitrant external obedience to that foreign will, but a transformation of their own will into conformity with it. But, in so far as a person puts this interpretation on his feelings of moral obligation, I should have thought that he regards the foreign will as that of some *individual*—in the last resort God—who has a *moral right* to such inward conformity of our desires to his. Now Kant cannot here mean by the " universal will " the will of God. It seems plain that he must regard it as a kind of collective will, belonging to the system of all the inter-related finite spirits, considered as a kind of super-individual mind. Now I do not believe that this is an intelligible hypothesis ; and, even if it be so, I see no reason to think that a person naturally interprets his experiences of moral obligation in terms of a conflict and a conformity between his private volitions and the volitions of a collective mind composed of all the spirits that there are.

(iv) What are we to make of the analogy with the Newtonian theory of universal gravitation? What is supposed to be analogous to the phenomenal facts which Newton explained, and what is supposed to be analogous to the underlying causes by which he explained them?

(*a*) I think that the answer to the second part of the second question is fairly obvious. Newton's explanation was in terms of a system of material particles attracting each other in accordance with a certain law, and subject in all their movements to the three laws of motion. Kant's explanation is to be in terms of a system of spirits in some kind of direct *rapport* with each other, so that changes in the inner state of each telepathically produces correlated changes in the inner states of all the rest in accordance with " pneumatic " laws.

(*b*) What Newton explained by his theory of gravitation was a number of striking terrestrial and celestial rhythms, *e.g.*, the tides, the orbital motions of the planets in accordance with Kepler's laws, and so on. He further explained what might be called " second-order " rhythms, *e.g.*, the precession of the equinoxes. He thus showed that a set of particles, each of which moves under the joint influence of its own originally impressed momentum and the gravitational attraction of all the rest, will (if the originally impressed momenta fall within certain limits) settle down into a stable system, characterized by certain large-scale rhythmic regularities and by minor variations on these themes which are themselves regular and rhythmic.

Now compare the originally impressed momentum of a particle to the " private will " of an individual spirit; and compare the gravitational field, due to the attraction of all the other particles, to the telepathic influence of the inner states of all other spirits on the inner state of this spirit. Then I take Kant's suggestion to be that the latter is felt by the individual in the peculiar form of a feeling of " moral obligation ". It is, one might perhaps say, almost as if each were subject to a kind of hypnotic suggestion, exercised telepathically and unwittingly by all the others, and received by the individual without conscious awareness of its source. Kant suggests, if I understand him aright, that a set of spirits, each of which acts under the joint influence of its private will and the telepathically exercised hypnotic influence of all the rest, will settle down into a stable system, characterised by some kind of moral and spiritual pattern analogous to the rhythmic spatio-temporal pattern of the solar system.

(5) *Eschatological Consequences.* Kant thinks that such a theory as has been sketched above would help to remove a difficulty which is very commonly felt about the lack of correlation in this life between well-doing and well-being and between ill-doing and ill-being.

There is no special connexion between the rightness or wrongness of a volition and the consequences which it has in the material world. A precisely similar series of bodily movements, and therefore precisely similar results in the material world, might be initiated carelessly or deliberately, and, if deliberately, either from a good motive or an evil one. But these different causes, with the same effects in the material world, might have very different effects in the spiritual world. For the moral character of an act concerns the *inner* state which lies behind it in the agent; and so it can have its full effect, in respect of the features which make it morally good or evil, only by its immedate telepathic influence on the inner states of other spirits. Their reaction, influencing telepathically the original agent, might make a great difference to his well-being or ill-being as a denizen of the spiritual world. In particular, the moral goodness or badness of an embodied spirit's acts in this world might determine his relationships of closer or less intimate *rapport* with other spirits, embodied and disembodied. Evil acts may lay one open to the telepathic influence of evil spirits, and put one out of telepathic *rapport* with good ones, and *vice versa*.

When the soul is separated at death from the body which it has been animating, its life in the spirit-world will be merely a continuance of those relations with other spirits in which it has already been standing. The goodness or badness of its acts done in the flesh will already have produced their consequences in the spiritual world, of which it has always been a member; and those consequences will now be manifest to it in the nature of the spiritual " environment " in which it will find itself. It will wake up on the spiritual bed which it has made for itself, and on which it has all the while been unwittingly lying during its dream-life in the world of matter. This, Kant rightly thinks, is a great improvement on the popular religious theory which regards future rewards and punishments as causally contingent to virtue and vice, and as accruing only in consequence of God's special volitions.

(6) *Our Cognitions as Members of the two Worlds.* Kant deals next with the following *prima facie* objection. If there is this community of spirits, and if each of us is at all times a member of it, is it not very odd that the fact is not perfectly well known to us all? Kant's solution is as follows. Each human soul has two quite different ideas of itself. (i) It knows itself as a *spirit* by means of a kind of *non-sensuous intuition*, through which it is aware of itself in relation to other spirits. (ii) It knows itself as an *embodied self* through an *image* which originates from impressions arising from the stimulation of the sensory organs, internal and external, of the body. By this means only material things and its relations to them can be presented to it. It is indeed one *subject*, which belongs both to the spiritual and the material world. But it is not one and the same *person* in its two capacities. Its cognitions in one capacity do not link up with its cognitions in the other. What one cognizes as a spirit is not remembered by one as an embodied self, and one's cognitions of one's own states as an embodied self do not enter into one's cognitions of one's state as a spirit. However clear and intuitive one's awareness of the spirit-world may be to one as a member of it, this does not enable one as an embodied self to perceive it. In this life, under normal conditions, one can have no more than an abstract discursive conception of the spirit-world, reached by reasoning such as Kant has offered ; one cannot have an intuitive awareness or an empirically derived concept of it.

Kant compares the situation with that of the same man awake and dreamlessly asleep. No-one, he thinks, would have any difficulty in admitting that, when a man is asleep, he may have clear conscious cognitions which he cannot recall when awake. He adduces the acts of sleep-walkers, which are often more intelligent than those of the same person when awake, in support of this opinion. Indeed, Kant is inclined to think that mental activity in dreamless sleep is likely to be clearer and more efficient than it can ever be in waking life, because disturbance from the outer and inner bodily senses is at a minimum. But, for this very reason, cognitions had by the soul in dreamless sleep cannot be recalled in waking life. In order for such recall to be possible there would have to be some sensory link between the cognition in dreamless sleep and some sensation in present waking life. This would have to take the form of some sensation in dreamless sleep, which was associated at the time with the cognition and might be revived by a sufficiently similar waking sensation. But in dreamless sleep sensation, both from within and without the body, is in abeyance. Kant remarks that dreams, which he defines for the present purpose as experiences had by a person when sleeping and remembered by him when awake, are here irrelevant. The dreamer may be said to be not fully asleep. He is wrapping up the cognitions which he has as a spirit in the impressions of his outer or inner bodily senses.

In spite of this we cannot wholly rule out the possibility that our experiences as spirits may occasionally insinuate themselves into our normal waking consciousness. A spiritual cognition may do this through evoking by association sensory ideas which are related to it, viz., images or even hallucinatory *quasi*-sense-perceptions which are *symbolical* of the spiritually cognized fact or object. After all, both modes of cognition, the spiritual and the sensory, belong to the same thinking substance. We may compare this possibility with the admitted fact that the higher rational concepts generally need to be clothed in sensory symbols if they are to be intelligible to us. Kant refers, in this connexion, to the representation of duration by a line, that of eternity by endless time, and the representation of divine moral attributes in terms of human emotions, such as pity, anger, etc.

But, even if this seeping of spiritual cognitions in symbolical form into everyday consciousness be possible, it might be expected to be rare. It would be likely to happen only in persons whose brains are specially excitable, and thus more likely than those of most men to generate imaginative or *quasi*-sensory phantasies symbolical of spiritually cognized facts or objects. Such persons are apt to be presented with numerous objects which they take to be things of a *spiritual* nature actually present to their *senses*. Really they are subject to an illusion of the imagination or an hallucination of the senses, but its ultimate origin is a genuine influence from the world of spirits upon them as members of that world.

Such symbolical but veridical phantasies would almost certainly be blended with ideas derived from education, tradition, etc., and in some cases with products of mere mental or bodily derangement. It must always be very hard to disentangle the spiritual fact or object, symbolically presented, from the wrapping of phantasy and hallucination. Again, this state of " permeability ", in which the brain and nervous system are activated in an abnormal way by the purely spiritual cognitions of the soul, would involve something which might fairly be called nervous instability. It would therefore be quite likely that a genuine " spirit-seer " would be subject to mere delusions and hallucinations, with no spiritual significance, along with his veridical symbolic experiences. In this connection Kant compares the gift of seership with Juno's gift to Tiresias. In order to confer on him the spiritual insight which enabled him to foresee the future she deprived him of his bodily eyesight

(7) *Connexion with Swedenborg's Doctrines*. Now Swedenborg claimed to occupy a unique position both among men and among disembodied spirits, in that he and he alone lived consciously and almost continuously in both worlds. He was thus in a position to investigate the spirit-world for himself, to cross-question its inhabitants and detect the errors and illusions to which they are subject, and to describe its nature and structure in ordinary human language on the basis of the information thus obtained. Now, as Kant points out, the information which Swedenborg claimed to have got " straight from the horse's mouth " (if it be permissible to use such an expression in this context) agrees remarkably with the theory which Kant had reached by the reasoning outlined above. Kant does not give Swedenborg much credit for this; he says that it is " as if a poet when he raved happened to prophesy truly ". It might strike an impartial observer that the agreement may not be wholly disconnected with the fact that Kant had carefully read and epitomized Swedenborg's doctrine at the time when he was pursuing his metaphysical speculations on this topic.

(8) *The psycho-physiological Conditions of waking sensory Hallucinations*.

Kant devotes Section III of Part I of the book to an eloborate discussion of the psycho-physiological conditions of waking sensory hallucinations. The essential points may be stated as follows.

The case to be considered is that of a man who is perceiving by ordinary sight, touch, hearing, etc., his own body and the various external objects which other waking men in his neighbourhood perceive, but who *also and simultaneously* seems to himself to be perceiving *other* objects, imperceptible to his neighbours, located at various places outside his body. This case must be distinguished from ordinary dreaming. There the subject does not perceive his own body by the external senses of sight and touch, and he does not perceive any of the objects which waking men in his neighbourhood perceive. It must also be distinguished from the case of a waking man who experiences very vivid images or even *quasi*-sensations but does not locate their objects in external physical space or regard them as existing independently of himself.

The " ghost-seer " is an instance of the case under discussion. But so too is a person in delirium or madness. Suppose that the ghost-seer's experience is delusive, in the sense that there is no physical object located at the place where the ghost is ostensibly seen to be and emitting or reflecting light to the seer's eyes. Then, even if there should be a spiritual cause at the back of the ghost-seer's experience, and if his experience should be veridical in the sense of symbolizing that spiritual cause, essentially the same problem is raised by ostensible ghost-seeing and the waking hallucinations of delirium or madness. It may be stated as follows. How can a person project imaginative or *quasi*-sensory contents, which are not evoked by physical stimuli impinging on his sense-organs from certain places in external physical space, into determinate external positions, so that they appear to him to stand in determinate spatial relations to his own body and to other objects of normal sense-perception?

Kant puts forward tentatively a physiological answer to this question in terms of the old theory of " animal spirits ", *i.e.*, the theory that the interstices in the brain and the supposed pores in the nerves are filled with a very subtle fluid whose motions are correlated with our sensations and volitions. I suppose that his theory could probably be recast in terms of present-day views of neural impulses as transmissions of electrical or chemical states rather than translatory motions of a fluid.

So far as I can understand it, the suggestion may be put as follows. The motions of the animal spirits in one's brain and nerves, which are involved in an *actual sensation* of sight or hearing, follow lines within the body which, if produced, converge to a point or a limited region *outside* the body. The motions of the animal spirits which are involved in having a visual or auditory *image* normally follow lines which converge to a point or a limited region *within* the body. But in madness or delirium the normal equilibrium of the brain and nervous system is upset in such a way that the motions of the animal spirits which are correlated with an *image* follow lines which converge to a point or a limited region *outside* the body. Thus one seems to *see* objects, located in external space, corresponding to images had under such conditions.

Now it often happens that only one kind of sensible experience, viz., the visual, is disturbed, whilst the others, and in particular the tactual and the muscular, are not. Thus one seems to *see* objects located in external space, which yet offer no resistance and are intangible. Now this is what is commonly told of ghosts. It also corresponds to the philosophical notion of a spirit, viz., a substance which can occupy a region of space without offering any resistance to the entry of matter into the place which it occupies.

Kant remarks that it is likely that traditional stories of ghosts provide some of the materials for the hallucinations of delirium or madness. If a man were in the same psycho-physiological condition, but lacked this background of tradition, his hallucinations might take a very different form.

(9) *The Conclusion of the whole Matter.* Kant was much too intelligent a man to think that a psycho-physiological theory of the *modus operandi* of waking sensory hallucination *disproves* the existence of spirits, or that it disposes of the claim that some such experiences are initiated by the telepathic action of a spirit on the soul of the experient and that they express in a symbolic form the spiritual event which initiated them. Though Kant does not explicitly say so, it is obvious that the test in each case is, not the mere fact of ostensibly seeing an apparently independent figure localized in physical space outside the body, but the question whether the particular details of the experience itself and the particular circumstances under which it happened strongly suggest that it was initiated supernormally.

But, since the waking sensory hallucinations of delirium and madness involve the same psycho-physiological mechanism, and since most persons who claim to be mediums are obviously to some extent unbalanced physically and mentally, it is tempting to treat them all alike as merely pathological phenomena requiring no supernormal explanation. Kant says that anyone who takes this line will enjoy three advantages. He will be able to make up his mind easily and quickly without needing to bother about detailed investigation of particular cases. He will be explaining the facts on the basis of materials provided by common experience instead of having recourse to the doubtful speculations of reason. And, above all, he will avoid exposing himself to ridicule. For these reasons Kant will in no way blame anyone who regards every professed ghost-seer, not as a half-citizen of another world, but as a candidate for a mental hospital. But he carefully refrains from saying that this is his own view. For him the general plausibility of the theory of a world of spirits and of our double citizenship in this world and in that is enough to keep him " serious and undecided " in view of such stories.

Yet, very characteristically, he half takes away with one hand what he has so grudgingly given with the other. Were it not for our hope of a *future life* (which, he says, he cannot and would not eliminate) no-one would attach any weight to the abstract possibilities which he has developed in the metaphysical part of the book. It would be much more reasonable, apart from that hope, to ascribe all these ostensibly supernormal phenomena to natural causes than to postulate agents and modes of action so utterly unlike anything to which our senses bear witness. The fact is that we are, and are doomed to remain, *equally* ignorant concerning the *three* problems of (i) the animation of a human body by a human soul at conception, (ii) the connexion of a soul with its body during life, and (iii) its separation at death and its subsequent existence. Were it not for our hopes and fears about the future, we should be as content to refrain from speculation concerning the third problem as most men have always been with regard to the other two.

Kant's final conclusion is completely agnostic. Beyond the bare abstract possibilities outlined in the metaphysical sections we can make no further progress either by rational speculation or by experiment and observation. Genuine scientific hypotheses are concerned with the *details* of agents and forces which are already known to be *possible* because they are already shown by sense-experience to be *actual*. But, in speculating about non-material thinking beings, standing to each other in non-spatio-temporal relations and interacting telepathically in accordance with pneumatic laws, we are postulating agents and modes of action which we cannot know to be even possible. We have neither the guarantee of rational insight (as we sometimes have in pure mathematics) nor that of instantiation by sense-perception (as we have in natural science).

Admittedly, in the case of ghost-stories we have certain alleged experiences of a *quasi*-sensory nature. But, Kant says, the lack of agreement and uniformity which is characteristic of such experiences makes them useless as a foundation for any proposed laws concerning which reason might pass judgment. They show only certain anomalies and irregularities in the functioning of the senses, and, as such, it is reasonable to discount them.

Kant seems never to have contemplated the possibility of an experimental investigation of ostensibly supernormal cognitive and active powers. He does not even envisage the careful investigation and comparative study of the sporadic cases, such as was first attempted in *Phantasms of the Living*, nor a synoptic survey of the whole field of normal, abnormal, and ostensibly supernormal mental phenomena, such as Myers made in his *Human Personality*. The fact that so great a man, speculating seriously on this topic, did not consider these possibilities, and that they have now been in so large a measure realized, should increase our gratitude and admiration for the pioneers of psychical research in England, America, and the continent of Europe. It is, perhaps, permissible to conclude with the phantasy that news of these later developments has seeped through in a symbolic form to the disembodied spirit of the sage of Königsberg, and that he has received it with interest and approval.

A FURTHER TEST FOR SURVIVAL

By T. E. Wood

The *Proceedings* for July 1948 contain an article (hereinafter called " the article ") entitled " A Test of Survival " by Dr R. H. Thouless.

Dr Thouless suggests that other members should prepare ciphered passages to which they hope to supply the key after their deaths. This suggestion I now adopt.

My ciphered passage is :

FVAMI NTKFX XWATB OIZVV X

The system, which I have used, is based on the Vigenère letter square, as described on pages 258 to 260 of the article and in the manner set out in the lower portion of page 259 and in the upper portion of page 260 of the article.

The first 21 words of the key passage are used as the bases of the 21 letters in the key series of letters, but (as Dr Thouless suggests) all second and later repetitions of words already used in the key passage have been omitted, when constructing the key series of letters.

The key passage to the decipherment of my message is in an accessible book. After my death, I shall try to communicate the key passage. I shall also try to communicate in what foreign language the key passage is. The message, if and when deciphered, will not be in any one language. I shall also try to communicate what languages are used in the message. The use of foreign languages is to meet the point mentioned in the paragraph commencing " Additional Note " on page 262 of the article as to listing the commonest words in the English language and so to increase the difficulty of decipherment.

My message is now open to anyone to decipher by any means whatsoever. Will anyone attempting to decipher it, please communicate the result (even if negative) to the Society?

My name is T. E. Wood. I have practised as a solicitor for some time in Burma but most of my life in England. I was born on the 21st June 1887 in Yorkshire. For many years, I have been a Member of the Society. My hobby is boat sailing. My executors will be asked to give to the Society at my death the circumstances of it and its date. Fuller particulars as to myself and a photograph of me and of my present home will be given to the Society. The Society can give such particulars and lend these photographs, if it thinks fit, to anyone who will endeavour to ascertain the key passage either by PSI means in my life time or by any means whatever after my death, and who thinks that such particulars would be helpful.

With the view to stimulating attempts to decipher my message in my lifetime, either by a professional cipher expert or by anyone else, I will either (1) pay the sum of £20 to such successful expert or (2) pay both (*a*) the sum of £10 to the Society for its library and also (*b*) the further sum of £10 to the person, who is not a professional cipher expert, and who first successfully deciphers the ciphered passage in my lifetime. The decision of the Society as to whether any sum is so payable and, if so, to whom, is to be binding on all concerned. The Society will be furnished with money by me to enable it to pay the fees of persons employed by the Society after my death, with a view to my key passage then being ascertained.

The value of this test will be increased, if readers of this article will try by PSI methods in my lifetime to ascertain the key passage.

My honesty or otherwise is a most material factor. Anyone, who intends to make full enquiries, can obtain my full name and address from the Society.

PROCEEDINGS OF THE SOCIETY FOR PSYCHICAL RESEARCH

PART 179

A REPORT ON AN EXPERIMENT ON PSYCHO-KINESIS WITH DICE, AND A DISCUSSION OF PSYCHOLOGICAL FACTORS FAVOURING SUCCESS

By Robert H. Thouless

I. Introduction.

In 1943 Professor Rhine first reported experiments which had been going on for several years in his laboratory in which it appeared that some experimental subjects were able to influence the fall of the dice so that they fell more frequently than could be attributed to chance alone with that face uppermost which was the target intended by the subject.[1] He called this *psycho-kinesis* (or PK). It is sometimes said that these successes have not been repeated in this country. This is not quite correct since I reported in 1945 a limited but definite success in influencing the fall of a spun coin.[2] Mr Whately Carington has also carried out experiments with dice using his wife as subject which he considered were not wholly negative (unpublished). More recently Dr E. A. G. Knowles, an English engineering mathematician, was surprised to find that she obtained significant evidence for PK in a series of experiments which were intended to show that nothing but a chance result would come from such an experiment.[3]

It remains true that the volume of successful PK experimentation in this country is still relatively small. During a visit to Professor Rhine's laboratory in the autumn of 1948, I found that I was able to succeed in PK experiments with dice. It seemed worth while, therefore, during the winter of 1948/49, to try to repeat this success at home. My object was threefold. First, to see whether I could increase the relatively scanty record of PK successes in Great Britain. Secondly, to try to devise a method of experimenting which would eliminate the possibility of success being due to an unconscious skill in throwing or to precognition of the way in which the dice would fall, and which also would meet some other objections that have sometimes been made to some of the Duke experiments. Thirdly, I hoped to find out something about the psychological factors determining success and failure in these experiments in the hope that I should make it easier for other investigators to repeat any success I might have if they felt so inclined.

All the experiments which are the subject of this report were done on myself as both subject and experimenter and were unwitnessed (although other people were sometimes present). I find it quite impracticable to provide witnesses for experiments which must be fitted into odd times when I happen to have a spare twenty minutes. This would render these experiments unconvincing as evidence for the reality of PK if that reality had not already been established by a number of witnessed experiments at Duke University parapsychological laboratory. The primary purpose of my experiments was not, however, to create conviction in other people but to satisfy myself as to whether I could obtain positive PK results by self-experimentation using the methods described below, and if so under what conditions. The best road to conviction is neither my testimony nor that of a multitude of witnesses, but the ability to achieve success in such experiments one's self. I hope that the suggestions I make in the later part of this report will be of some help in enabling those who are sceptical of the PK effect to convince themselves by their own success in carrying out such experiments.

Some of my experiments were done with dice thrown by hand from a shaker ; these are referred to in this article as " hand-thrown ". I do not regard these as crucial evidence for the operation of psycho-kinesis, since the conditions of throwing do not exclude the possibility of success being due to skill in throwing. On the other hand, I have no reason for supposing that success in hand-thrown experiments is due to this cause, and such evidence as I have strongly suggests that the factor at work in these experiments is the same as that operating in mechanically thrown dice. The remainder of the experiments were done by a method of machine-throwing which completely excludes the possibility of any sort of skill causing the results. A method of randomising the targets by means of Latin squares was also introduced partly in order to exclude the possibility that success might be due to precognition of the dice falls, and partly to exclude the possibility (suggested by Mr Parsons) that position effects might be due to progressive physical changes in the dice. The dice used were plastic inlaid dice kindly supplied by Professor Rhine. In all cases four dice were used for each throw, partly for the accidental reason that this happened to be the number I brought back from Duke University, partly for the reason that with four dice there are about even chances of at least one success by chance alone, so the experimental subject is saved from the tendency to discouragement by a large number of complete failures. These dice were marked with the letters *A*, *B*, *C* and *D*, and their falls were recorded separately in that order. I always recorded all dice falls, both success and failures, and as a further precaution against the possibility of errors in recording, I repeated the count of the faces uppermost after the recording.

For machine-throwing, the apparatus shown in Fig. 1 was used. I wished to exclude all possible ways in which the fall of the dice could be supposed to be influenced by the voluntary activity of the subject. Their initial position was, therefore, prescribed. The dice were placed on the machine with the 1 face outwards from the board while the 2 face was upwards from the gate. The order was (from left to right) *ABCD*, and the four dice were placed compactly with *A* in contact with the left hand stop above the gate. They were released by pulling a string which moved a latch from under the left-hand end of the gate so that the gate fell under the influence of gravity. The speed and way of falling could not, therefore, be influenced in any respect by the way in which the subject operated the release. When the gate fell, the dice slid forward down a hinged board at an angle of about 45° and were given a rotation by striking a projecting band of corrugated rubber and fell rolling on the floor at about 1 foot vertical distance from the initial height of the dice.

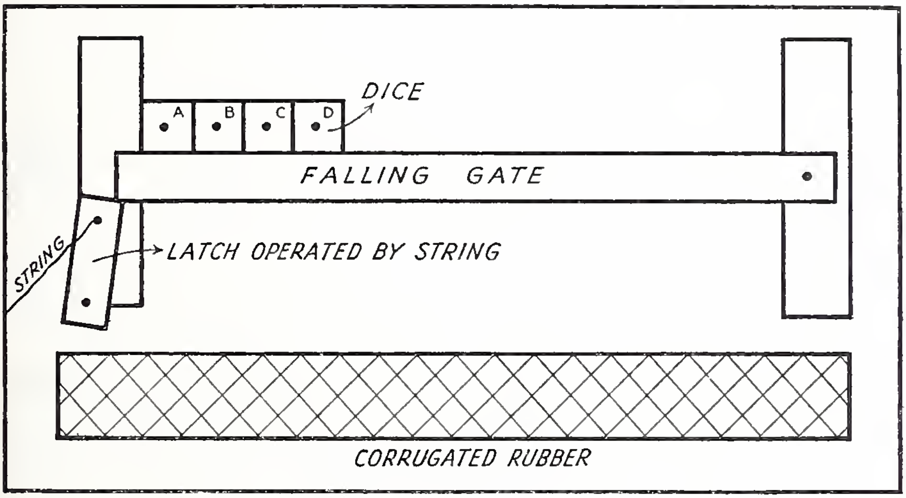

FIG. 1.—Apparatus for mechanical throwing.

It is obvious that this rigid prescription of the initial position and manner of fall of the dice might produce a tendency for the frequency of different faces to fall uppermost to be far from equal. This does not affect the validity of the method since any such departure from equiprobability would only reproduce the normal effects of bias which do not affect mean chance expectation of success in a properly designed experiment (e.g. one in which each face is target for an equal number of times). The variance will be somewhat affected by departure from equiprobability but only in a direction which will lead to an error in the safe direction, *i.e.* to an underestimation of the significance of any observed deviation. This effect may, therefore, be ignored unless the departure from equal distribution of falls is extreme.

In fact, the departure from equality proved to be very small. Since the initial position of the dice in the machine-thrown experiments was such that the 3 and 4 faces were at right angles to the surface on to which they fell, it is to be expected that these faces would fall uppermost less frequently than the others. This proved to be the case. The distribution of faces for the 5,184 machine thrown falls of Expts. IIA and IIB taken together are shewn in Table 1.

TABLE 1

Faces uppermost

	(1)	(2)	(3)	(4)	(5)	(6)	*Total*
Observed - - -	918	892	819	821	882	852	5,184
Devn. from expn. -	+54	+28	−45	−43	+18	−12	

What the table suggests at first sight is the presence of the expected deficiency of 3s and 4s, superimposed on a general bias favouring low-valued faces. The deviations in this table are, however, not large enough for significance, the odds being only about 10 to 1 against all deviations occurring by chance variation from equal probability ($chi^2 = 9{\cdot}3$, $n = 5$, $P = {\cdot}1$).

If, however, we consider separately the question of whether there is a real deficiency of 3s and 4s, we have the following table:

Table 2

	3 & 4 *faces*	*all others*	*Total*
Observed	1640	3544	5184
Expected	1728	3456	5184
Deviation	−88	+88	

This gives $chi^2 = 6{\cdot}65$, $n = 1$, $P = {\cdot}01$. The odds are thus 100 to 1 against this deficiency of 3s and 4s arising by chance. The deficiency is, however, satisfactorily small, being only about 5%.

It is not possible to give a definite answer to the question as to whether any part of the remaining inequality of the faces is due to bias. If we consider only the 3,544 faces which were not 3s or 4s we should expect 886 falls on each of the remaining four faces. The deviations from this expectation of the remaining four faces is shown in Table 3.

Table 3

	(1)	(2)	(5)	(6)	*Total*
Observed	918	892	882	852	3544
Devn. from expn.	+32	+6	−4	−34	

Chi^2 for Table 3 is $2{\cdot}52$, which with $n = 3$ gives $P = {\cdot}4$. The deviations from expectation are, therefore, only such as would be expected by chance alone. The fact that the values of the deviations fall in the order 1, 2, 5, 6 certainly seems to suggest a bias effect, but this suggestion has no real strength since the odds against this or the opposite serial order arising by chance are only 12 to 1. If bias is present, its amount is so small that analysis of a very much larger number of falls than all I have available would be necessary for a demonstration of its significance. In any case, the question is not of sufficient importance to justify so much labour. We can be confident that if bias is present, it is to a small amount. If the departure from equality shown in Table 3 were entirely due to bias, the amount of this bias would only be about 4% for extreme values. The absence of appreciable bias was confirmed by an examination of 864 hand-thrown falls in which there was no appreciable departure from equal probability of faces and no indication of the serial effect seen in Table 3. This does not mean, of course, that a method of assessment assuming freedom from bias can safely be used. A method of assessment giving a result which is independent of any bias of the dice must be used in all cases.

The purpose of the standardisation of the initial position of the dice on the machine was to eliminate the possibility that success might be due to a process of acquiring skill or to some other paranormal process than

PK, *e.g.* to the subject's knowledge by a psi process that a certain initial position of the dice or a certain method of releasing them would favour the appearance of that face which is the target. For this purpose, it is also necessary to deny him (and the experimenter) any liberty as to choice of target faces, since otherwise he might be guided in choice of target face by precognitive psi knowledge of which faces were going to fall uppermost most frequently in a series of throws. The method I have adopted for determining the sequence of targets is by use of a 6×6 Latin square, that is, a square containing the numbers 1 to 6 arranged in six rows in such a way that each number occurs once and once only in each row and in each column. A series of such Latin squares is to be found in Fisher and Yates's mathematical tables. One of their basic forms is selected by a random method, and its rows and columns are then randomly permuted by means of dice throws. The successive lines of the square were then used for the sequence of target faces. One row of a Latin square was used for the succession of targets on a single occasion, six or three throws of the four dice being made in succession for each target face. After a Latin square had been used, it was not repeated but the next experiments were done with a new square obtained by a new permutation of the rows and columns. For example, the target sequences for the first six occasions were determined by the rows of the Latin square :

2	5	6	3	4	1
5	4	2	6	1	3
4	6	3	1	5	2
1	3	5	2	6	4
6	2	1	4	3	5
3	1	4	5	2	6

This method of target determination has several advantages. First, each six sets of throws determined by a single row has all target faces an equal number of times so the total number of successes expected by chance is independent of the bias of the dice. Secondly, after six (or any multiple of six) such sets of throws, each target has occurred an equal number of times in every position. This is very desirable for the estimation of position effects ; *i.e.* the tendency to score higher in some parts of a series than in others. Unless each target has occurred equally often in every position, it is possible that an apparent position effect may be due to bias or to target preference : *i.e.* the tendency to score better on some faces than on others. Unless this precaution is taken it does not seem possible to make a separate assessment of position effect and target preference.

It also seems to be the most satisfactory way of dealing with a possibility that has been suggested by Mr Parsons that position effects might be due to progressive physical changes in the dice during the course of experimentation. It is certainly possible that such changes may take place and may tend to produce progressive changes in the frequencies of falls of different faces, and that under some conditions of experimenting such changes might produce a spurious appearance of change in rate of success. If, however, position changes in scoring rate were found consistently through a Latin square, they could not be explained in this way, since physical changes that would favour one direction of change in scoring rate in one part of the square would be neutralised by opposite changes in scoring in other parts of the square. It would indeed be necessary to postulate some kind of physical change which made them consistently change in a way determined by which face was target. This is clearly absurd except on the hypothesis that PK is taking place.

The first object of the Latin square arrangement of targets is to secure not only that the total experiment has an equal number of each target face so that without undue labour in computation one may make an assessment of the significance of the total deviation which will be independent of target bias, but also that every sub-unit of the experiment used in any comparison (such as that of different positions in the experiment) may also have an equal number of each target face. Obviously this end might be secured by other means than the Latin square. In a later experiment to be reported below (the $\Psi_{\gamma\kappa}$ experiment) a similar result was obtained by arranging the target faces by the shuffling of a pack of six cards numbered 1 to 6. This does not give equal numbers of targets in each position, but it gives a random number of targets in each position *i.e.* one that will differ from equality only by a chance amount. This would give an equally valid but less economical way of assessing position effects.

The second purpose of the use of the Latin square is to remove from experimenter and subject all choice as to target face so as to make success impossible of explanation by precognition of the dice falls. It is obvious that if either experimenter or subject has free choice of target face, a limited but considerable success might be obtained if the person choosing the target face knew by precognition which were going to be the most frequent falls in each sub-unit of the experiment for which the target face was chosen. This is so obviously a defective form of PK experiment that it is rarely employed now. There is, however, a wider possibility of the influence of precognition. Since a PK experiment necessarily consists of a system of targets chosen in some manner and a system of falls, it seems possible that any success might be explained by the assumption that the choice of targets was determined by a precognitive knowledge of the falls in whatever manner the targets were chosen. Even if the target sequences were chosen by a randomising machine, the machine must have been started at a particular time by somebody and if he had precognitive knowledge both of the targets the machine was going to select and of the falls of the dice in the subsequent experiment it might be possible that he started the machine at such a time or in such a way that it gave the targets which would correspond with the dice falls in the experiment. Such an explanation would not be a very plausible one, but if it is theoretically possible then one has not got a method of demonstrating PK in a manner which cannot be explained by precognition.

The experimental separation of PK and precognition may indeed be impossible if these are assumed to work with 100% accuracy, but it is not difficult to design a PK experiment which cannot be explained by precognition if the ability to precognise is assumed to be as fallible as experiments on precognition show it to be. This can be done by using a system of target selection such that the arrangement of targets depends on a number of independent contingent events and could only be known if all those events were correctly precognised. I think that this condition is fulfilled by the Latin square method of selection. The basic square is one of seventeen given in Fisher and Yates's tables. If this basic square is selected by a random method, the square selected would have to be correctly precognised in order that the experimenter should have any idea of what are to be the target faces. If his dice chose the seventh square and he was nearly but not quite correct in his precognition, he would be expecting a wholly different square from the one actually employed, not one that was nearly right. Nothing but an exactly right precognition would produce any effect on his results. Even if he has performed the remarkable precognitive feat of guessing the right basic square, he would also have to precognise the dice falls that determine the permutations of the columns and rows of the basic square. If he could precognise with 17% accuracy (which would be a remarkably good precognition score), that would only make right one target position out of the thirty-six of the complete square, in addition to those that would be right by chance alone. That would make very little difference to his final score, and for the second and third squares, the effect of any such rate of correct precognition would have no measurable effect since it would be necessary to precognise also the new sets of permutations of rows and columns for these squares. Plainly no level of precognitive success such as we find experimentally could produce any spurious success in a PK experiment designed in this way. 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.

I admit that there may be one somewhat serious objection to the Latin Square method of PK experimenting. It is not unlikely that the frequent and arbitrary changes in target are unfavourable to PK success so that it may be a relatively unfruitful method of working. I have no evidence that it is so, but it is not unlikely. I have myself obtained success in working with this method but it is quite possible that I should have had much better success by a method which did not involve such frequent changes of target.

II. The First Series of Experiments.

In these and all other experiments here reported I acted both as subject and experimenter. In some of the experiments I was alone, in others I was working in competition with other people, but in no case was there any formal witnessing of results. In my report I have included only my own results when I was working with other people.

During the Christmas vacation of 1948/49, I carried out a long series of experiments with a variety of methods using both machine throwing and hand throwing of the dice. It would be tedious to report these in detail since much better evidence of the reality of the PK effect is provided by a later series which I will report more fully. For some of the first of these experiments I used a modification of the Latin square method which did not involve complete equality of throws for different targets. This was afterwards abandoned since, although perfectly valid, it was uneconomical since it required a laborious method of assessment. The experiment started very encouragingly with 36 successes on the first day in 148 falls, with a probability against so large a deviation occurring in a chance series of 100 to 1. As is usual in such experimentation, however, decline set in and after about four weeks, there were strong indications of a tendency to score below mean chance expectation. As new forms of experiment were started, they generally were successful at the beginning and declined to mean chance expectation after a few days. The final total result for this series was 16,232 falls (machine thrown and hand thrown together) with 2,809 hits on the target face. The excess of hits over mean chance expectation is $103\frac{2}{3}$ which is 2·18 times its standard error. The excess is just significant (odds against occurrence in a chance series of about 33 to 1) but not sufficiently so for this result in itself to provide sufficient evidence for the reality of the PK effect.

On the other hand, there were abundant indications of a real PK effect in these experiments apart from the total score. The smallness of the over-all deviation above mean chance expectation seemed largely due to the fact that the rate of success tended to fall off with time. Thus the last batch of results by the machine throwing method was a set of 24 runs (*i.e.* 576 single die falls) with a deviation of 19 hits below mean chance expectation. This gives a critical ratio of 2·1 (P about ·03) which suggests (although it is by no means strong evidence for) a real tendency to score below mean chance expectation at this stage of the experiment. In contrast the first 24 runs of this experiment showed a deviation of 15 hits above mean chance expectation. The difference between the scores at the beginning and the end of the experiment was, therefore, 34 with a standard error of 12·64. This gives a critical ratio of 2·7 and the odds against so large a difference occurring in a chance series are rather better than a 100 to 1. This is fairly good evidence for the reality of the decline effect, particularly when we consider that a similar falling off in score was shown in other parts of the experimental series.

The evidence for decline is also evidence for PK, that is against the hypothesis that there is no influence on the fall of the dice of the intention of the subject. Nevertheless it is evidence of a kind which must be accepted with some caution since the primary purpose of the experiments was to obtain a significant deviation from mean chance expectation. One must be careful not to regard unexpected peculiarities of experimental data as substitutes for the primary evidence which it was the purpose of the experiment to obtain. On the other hand, chronological decline is a characteristic of psi results which we already have grounds for expecting from other investigations, so it is not unreasonable to regard its appearance in these results as additional grounds for considering that PK was operating here, giving further confidence in the indications of the over-all positive deviation already obtained and encouragement to carry on further investigation along the same lines.

III. The Second Series of Experiments.

I felt that I had accumulated experience in this first experiment and knew better how to avoid the extreme effects of chronological decline. The first series of experiments had been done during vacation when I could devote all my spare time and most of my waking thoughts to the experiment, and it seemed to me that as I had worked harder at it and thought more about it, the positive results fell off. I decided, therefore, to profit by the experience gained in this first series of experiments and to carry out a second set of experiments done entirely by the machine-throwing method in which I hoped to get a better positive deviation by avoiding so far as possible the causes of chronological decline and other factors productive of low scoring. Particularly I could avoid the intense concentration on the experiment which I suspected was an important factor in producing the decline observed in the first batch.

I resolved, therefore, to do this second experiment in small parts at a time, allowing intervals between occasions, sometimes of a week or more, sometimes of a few hours. On each occasion of the first of the second series of experiments (referred to as experiment IIA), I made three throws of four dice at each of the targets in one row of a Latin square. The pre-determined length of the whole experiment was 36 experimental occasions. All throws were made by a machine in the manner described earlier.

All dice falls were recorded in the die order *ABCD*. This recording of all falls seems to me to be an important safeguard against the danger of miscounting. After recording, I looked at the dice again before picking them up to make sure that my record was correct. I am satisfied that no mistakes were made in recording falls. Mistakes in counting the number of successes can, of course, be easily avoided by subsequent rechecking of the count from the record. This has always been done.

To make the experimental method more clear, I add the record for the first two experimental sittings (Table 4).

TABLE 4

30/1/49	(2)	(5)	(6)	(3)	(4)	(1)	devn.
11.15 a.m.	3446	5524	2622	2454	4113	2516	
	3224	5316	1633	4542	1665	6532	
	3652	6365	2536	2322	6361	6213	+2
31/1/49	(5)	(4)	(2)	(6)	(1)	(3)	
10. 45 a.m.	2125	6321	3122	6563	1616	4315	
	3646	6151	2362	1655	5416	4425	
	3254	3644	1222	2134	1262	4143	+8

Record of first two occasions of Expt. IIA (target figures are those in brackets).

The final deviation from expectation in this experiment was an excess of 42 hits over mean chance expectation with odds against chance occurrence of about 40 to 1. This was an encouraging result although it is not enough for strong conviction if the experiment stood alone.

Another point in which I had become interested when starting experiment IIA was the problem of the specificity of the inhibition leading to chronological decline. It appeared that one kind of experiment might be inhibited while another was succeeding. I had immediately before this time done a very large number of hand-thrown experiments for several weeks (included in the Expt. I results reported above) and I had dropped to chance scoring when working by this method. I was anxious to see, therefore, whether this failure in hand-thrown experiments would continue even though I might be succeeding in a machine-thrown series. So during the first 24 experimental occasions of IIA, I did an equal number of hand-thrown experiments (Expt. I^x) alternately before and after the machine-thrown experiments. These were not intended to be included in Expt. II whether they were successful or unsuccessful since Expt. II was to be exclusively machine-thrown. Their results are, of course, included in the over-all total results shown in Table 8.

The result of this comparison between the scoring rate at that time of machine- and hand-thrown experiments is shown on Table 5.

TABLE 5

Method	*Total falls*	*Devn.*	*Standard error*	*Critical ratio*	*P*
Machine - -	1728	+34	15·5	2·2	·03
Hand - - -	1728	− 9	15·5	·6	·55
Difference - -		43	21·9	1·96	·05

The difference in favour of the machine-thrown rate of success is large enough to suggest strongly that there was a real difference at this stage of experimenting between the scoring rates from the two methods of throwing. It cannot be concluded that machine-throwing necessarily produces better PK results than hand-throwing since at an earlier stage of my work I was succeeding by hand-throwing when machine-throwing was producing negative results. There is, however, some evidence that the inhibition of hand-thrown results is more lasting than that of the machine-thrown, although I have no positive proof that this is the case.

On the 25th occasion of Experiment IIA (Experiment I^x having been completed), I started a new machine-thrown experiment (Expt. IIB) which I continued to the same length as IIA, that is, to 108 runs or 2,592 single die falls. This experiment was done by a method which I had developed during the course of the earlier series of experiments. It is, I think, an interesting novelty and it may be found a useful variant from the more usual methods of PK experimenting. I have called it the $\Psi_{\gamma\kappa}$ technique since it involves both Ψ_{γ} (ESP) and Ψ_{κ} (PK). Essentially it is a PK experiment in which the target face is not known by normal means to the subject. Six cards numbered 1–6 are shuffled and laid face downwards in a heap before the subject. The cards are not disturbed until the check is made at the end of the attempt at all six targets. The four dice are then thrown with the intention that on the first throw the top faces of the dice shall be that number which is on the first card of the heap, on the second throw that of the second card, and so on. After six throws have been made and recorded, the results are compared with the numbers on the cards. Eight sets of six cards were used and, after the completion of one run, the set of cards just used was put on one side and a fresh set was shuffled for the next experiment. All throws in this experiment (as in IIA) were machine-thrown.

Although to succeed in the task of hitting a target without knowing what the target is may appear an impossibility, I have found this form of experiment successful both with myself and with other subjects. I also hear from Dr Betty Humphrey that she has had good success with this method at Duke University. It is difficult to determine whether it is a more fruitful method than that with a known target, both because there are individual differences between experimental subjects as to the tests in which they score best and also because scoring rate at a particular test depends on the previous history of the subject with respect to that form of test. For example, a subject who has developed a chronological decline in his results for PK tests of the ordinary type may score better when tried with this test simply because it is a novel task and not because it is intrinsically a more fruitful method. It seems, however, to give as good results as other ways of testing PK and I have some hope that it may prove to be generally more favourable to good scoring. Its advantage, I think, lies in the fact that the whole operation lies within the limits of psi-determined processes so there is less temptation to the subject to use conscious volition. A subject finds it hard to try to get the dice to fall the right way up if he does not know which is the right way, and not trying is, I believe, favourable to success. We seem to be asking the subject to perform an impossible task, and the attitude he is inclined to adopt to an impossible task is a good attitude for psi performance.

On each experimental occasion, I worked through three sets of six cards with the exception of the 12 runs from 85 to 96 inclusive when I tried the experiment of reducing the amount done to one run per occasion. To make the method of experimenting more clear, I show in Table 6, the detailed results of the first three occasions.

Table 6

										Devn.
16/3/49	[1]	6*55*3 (5)	2	[2]	2613 (4)	0	(3)	5*3*21 (3)	1	
10.15 a.m.		132*6* (6)	1		1*3*22 (3)	1		2356 (1)	0	
		*44*5*4* (4)	3		6*222* (2)	3		5513 (2)	0	
		*1*465 (1)	1		5663 (1)	0		1311 (6)	0	
		1115 (3)	0		5221 (6)	0		3635 (4)	0	
		4116 (2)	0		1232 (5)	0		2221 (5)	0	
			—			—			—	
			7			4			1	0
16/3/49	[4]	3*111* (1)	3	[5]	166*5* (5)	1	[6]	*3*212 (3)	1	
5.20 p.m.		32*6*3 (6)	1		1563 (2)	0		*2*5*2*6 (2)	2	
		52*3*1 (3)	1		2162 (4)	0		*6*341 (6)	1	
		6636 (5)	0		3363 (1)	0		6341 (5)	0	
		3221 (4)	0		5*6*44 (6)	1		3525 (4)	0	
		4664 (2)	0		5666 (3)	0		4242 (1)	0	
			—			—			—	
			5			2			4	−1
17/3/49	[7]	5146 (2)	0	[8]	3453 (6)	0	[9]	22*66* (6)	2	
		562*3* (3)	1		*11*36 (1)	2		*5*1*5*6 (5)	2	
		55*6*1 (6)	1		55*2*5 (2)	1		2616 (3)	0	
		*1*434 (1)	1		*3*4*3*2 (3)	2		11*22* (2)	2	
		*5*466 (5)	1		*4*515 (4)	1		2216 (4)	0	
		5363 (4)	0		*55*62 (5)	2		5*1*66 (1)	1	
			—			—			—	
			4			8			7	+7

The four figures on the left of each block in this table are the uppermost faces of the four dice, *e.g.* In line 1 of the first run, 6553 means that die *A* fell 6, *B* was 5, *C* was 5, and *D* was 3. The figure 5 in round brackets is the number of the top card in the pack which determined the first target so the score for that fall was 2 as shewn by the last figure in the line.

The total result of Experiment IIB was an excess of +33 hits over mean chance expectation. This excess, with a critical ratio of 1·80 is not quite enough for separate significance. The whole of Experiment II (*i.e.* IIA and IIB together) has a total deviation which, as will be shown in the next section, is strongly significant. There is also a strongly marked position effect in IIB, the first run of each occasion showing a tendency to high deviations from mean chance expectation. Such secondary characteristics of the data, which were not primarily aimed at in the design of the experiment must, of course, be admitted with great caution as evidence for the influence of PK, particularly since, although there was prior reason for expecting position effects, there was no reason for expecting it in this particular form. On the other hand, the departure from expectation is very considerable, so the position effect may perhaps safely be considered as supporting evidence for the operation of PK in this experiment.

IV. Results.

Experiment II was intended to provide a block of results obtained by the use of machine throwing to see whether positive PK results can be obtained under conditions which preclude explanation by any kind of skilled throwing. The total result of this experiment was strongly significant, an excess of hits over mean chance being obtained such that the odds against the excess occurring in a chance series were 200 to 1. The results of this experiment are shown in Table 7.

TABLE 7

Expt.	*Falls*	*Hits*	*Devn. from m.c.e.*	*S.E.*	*C.R.*	*P.*
IIA	2,592	474	+42	18·97	2·22	·025
IIB	2,592	465	+33	18·97	1·80	·07
Total	5,184	939	+75	26·83	2·80	·005

If it is preferred to consider all results together, the significance is somewhat better than that of Experiment II separately. The complete result of all the experiments that were done is shown in Table 8.

TABLE 8

Expt.	*Falls*	*Hits*	*Devn. from m.c.e.*	*S.E.*	*C.R.*	*P.*
I	16,232	2,809	$+103\frac{2}{3}$	47·48	2·18	·03
Ix	1,728	279	−9	15·5	·58	·56
IIA	2,592	474	+42	18·97	2·22	·025
IIB	2,592	465	+33	18·97	1·80	·07
Total	23,144	4,027	$+169\frac{2}{3}$	56·70	2·99	·003

The odds against the occurrence in a chance series of the total deviation of $169\frac{2}{3}$ hits over mean chance expectation is about 350 to 1, which is sufficiently strong evidence that they are not in fact due to chance. In view of the system of randomisation of targets, explanation by pre-cognition seems also to be effectively excluded. Even if skilled throwing could be the explanation of success in hand-thrown experiments, this explanation is excluded in the strongly significant success of Experiment II which was entirely machine-thrown. The results may, therefore, be considered to show strong evidence for the reality of the influence on the fall of the dice of the intention of the experimental subject, that is, for the operation of the PK effect.

V. Position effects and target preference.

The main purpose of the design of this experiment was to see whether a deviation of score could be obtained greater than that reasonably attributable to chance. One may also ask whether the results showed any of the characteristics which other investigators have found in PK data, although presence of such secondary characteristics could not be accepted as good evidence for PK unless the experiment had been designed to look for them or alternatively they attained a very high level of significance and there was sound reason based on previous investigations for expecting them.

In many experiments position effects have been reported, *i.e.* differences in scoring rate at different points of an experimental occasion. In Experiment IIA, a half run (*i.e.* twelve single die falls) was made on each occasion for each of the six target faces. Since by the use of the Latin square it was ensured that during every six occasions each target occupied each of the six possible positions, it follows that any multiple of six occasions can provide evidence as to whether there is a tendency to score differently on the first, second, etc. half-run, and that any tendency which appears cannot be attributed either to a physically determined change in the fall of the dice favouring different targets at different stages of the experimental occasion, or to a tendency of the subject to score better on some target faces than others. At the end of this experiment, there had been 432 falls in each of the six positions ; the number of hits in each position is shown in Table 9 (72 being mean chance expectation for each position).

TABLE 9

Half-runs - -	1st	2nd	3rd	4th	5th	6th	Mean
Hits - -	88	64	88	80	73	81	79
Devn. from mean	+9	−15	+9	+1	−6	+2	

If there is any position effect in this table, it is clearly not a tendency as simple as the usually reported tendency to score high at the beginning and end of an experimental occasion. What is suggested is rather a maximum scoring rate at the beginning and end and middle of the series. It must, however, be determined whether there is any reason for supposing that this is a real effect or whether it is only such a difference as might occur by chance. The evidence on this question is provided by discovering whether the variance of these quantities is significantly greater than that to be expected on the hypothesis that they are chance deviations from the observed mean of 79. The observed variance of the totals in each position is 85·6. The expected value of the variance on the hypothesis that differences between scores have arisen by chance is 64·6.[1] The ratio of 1·32 of the observed to the expected value of the variance of these scores is too small for significance. There is, therefore, no evidence for any difference in scoring rate in different positions within each occasion for this experiment.

There is, on the other hand, a very marked position effect in Experiment IIB in which it was found that the first run of each occasion was the only one which was scoring significantly above mean chance expectation. The results of the three runs for this experiment are shown in Table 10, with the omission of twelve runs towards the end of the experiment which were done with only one run per session.

TABLE 10

Run	*Falls*	*Hits*	*Devn. from m.c.e.*	*S.E.*	*C.R.*	*P.*
1st	768	166	+38	10·32	3·68	·0003
2nd	768	127	− 1	10·32		
3rd	768	127	− 1	10·32		
Total	2304	420	+36	17·88	2·01	·05

The deviation from expectation of the combined total of first runs of this experiment is thus strongly significant. Taking into account the fact that it is the selected best of three sets of observations, the odds against a deviation of this size occurring by chance is about 1,000 to 1. The significance of this position effect may also be estimated by the method used for Experiment IIA. The variance of these run totals about their mean of 140 is 507 which is 4·42 times the expected value of 114·6. This ratio is clearly significant, with odds against chance occurrence of rather less than 100 to 1.

There is, therefore, strong evidence for the reality of the position effect in Experiment IIB although there appears to be none in Experiment IIA. I am not able to account for the difference between these two experiments. It must be remembered that each run in IIB included all six target faces so that explanations alternative to that of a higher tendency to score in the first run of each occasion are excluded as effectively in this experiment as they are by the Latin square arrangement of targets in IIA.

Another oddity in scoring which has been sometimes reported is that of preferential scoring on certain target faces. Generally what has been suggested is a preference for scoring on the six face but in many cases the design of the experiment has been such that a preferential scoring tendency cannot be distinguished from the effect of bias.

The results of Experiment IIA were analysed in order to discover whether they showed any evidence for a tendency to score more heavily on some targets than on others. It is obvious that it is not sufficient to find that there are more hits on some faces than on others since this might be due simply to the fact that these faces fell uppermost most frequently. The comparison that must be made is between the number of hits on each target with the number expected on the hypothesis that the number of

[1] Calculated from the formula Npq, p and q being the *observed* proportions of hits and misses in the experiment.

hits is proportional to the number of times that that face fell uppermost. The results on different targets are shown in Table 11.

TABLE 11

Targets	1	2	3	4	5	6	Total
Falls	473	441	396	403	449	430	2592
Hits—observed	77	90	73	74	97	63	474
expected	86·5	80·6	72·4	73·7	82·15	78·65	474
Misses—observed	396	351	323	329	352	367	2118
expected	386·5	360·4	323·6	329·3	366·85	351·35	2118
Devn. of hits from expn.	−9·5	+9·4	+·6	+·3	+14·85	−15·65	

This table suggests that scoring is above average on 2s and 5s, below average on 1s and 6s and intermediate on 3s and 4s. If instead of asking how scoring on different target faces compares with average scoring, we consider how it compares with the number of hits on each target face to be expected by chance (*i.e.* one sixth of the number of falls with that face uppermost), we have the result shown in Table 12.

TABLE 12

Targets	1	2	3	4	5	6	Total
Hits	77	90	73	74	97	63	474
Chance expn.	78·8	73·5	66·0	67·2	74·8	71·7	432
Deviation from chance expn.	−1·8	+16·5	+7·0	+6·8	+22·2	−8·7	

It appears from Table 12 that the main part of the scoring was done on 2s and 5s whereas that on 1s and 6s was insignificantly below mean chance expectation and that on 3s and 4s was insignificantly above.

Whether this was a real difference in scoring rate on different target faces or a mere chance effect must be determined by finding the significance of the deviation from expectation shown in Table 11. For this table chi^2 is 9·78, which with 5 degrees of freedom gives $P = \cdot 08$. This is not small enough to be ground for regarding the deviation from expectation as significant. On the other hand, it is near enough to significance to make one hesitant to draw the conclusion that there is no sign of target preference. If we group together the hits on faces which are opposites, we find a deviation from expectation which is highly significant. Thus by combining the first and sixth columns of Table 11, the second and fifth, and the third and fourth columns, we get a contingency table with two degrees of freedom showing deviations from expectation of −25·15, +24·15 and +·9 for these three pairs of targets respectively. If the data are treated in this way, the deviation from the values expected on the hypothesis of equal likelihood of scoring on each target would appear to be clearly significant since chi$^2 = 9{\cdot}12$ and $P = \cdot 01$.

This indication of significance must, of course, be treated with some caution since the low value of P results from the particular method adopted of pairing the targets. If it were merely the result of arbitrarily pairing targets with similar scoring tendencies, it would be necessary to discard it altogether. The method of pairing is not, however, altogether arbitrary. The target faces paired together are not only those occupying opposite sides of the dice; they are also the faces that are similarly coloured. In the dice I am using the 2 and 5 faces have red spots, the 3 and 4 faces have blue spots, while the 1 and 6 faces have black spots. The scoring preference may be for a particular colour of face and not for a particular number.

While the above indications cannot in themselves be regarded as sufficient evidence for target preference, they may be accepted as giving a lead to further enquiry. I made, therefore, a similar analysis of the hits in Experiment IIb. The results are shown in Table 13.

Table 13

Targets	1 & 6	2 & 5	3 & 4	Total
Falls	867	884	841	2592
Hits—observed	153	167	145	465
expected	155·5	158·6	150·9	465
Misses—observed	714	717	696	2127
expected	711·5	725·4	690·1	2127
Devn. of hits from expn.	−2·5	+8·4	−5·9	

Although the deviations from expectation in this table are in a somewhat similar pattern to that found in Experiment IIa, they are quite insignificant (chi^2 = ·87, P > ·5). They do not, therefore, add anything to the somewhat inconclusive evidence as to target preferences given by IIa. It would, of course, have been possible to make a more extensive search for target preference in the experiments of series I. Counting target falls and hits is, however, very time consuming and I felt that I had spent enough time on a very minor problem. The situation at present is, therefore, that there is some evidence of target preference with heaviest scoring on 2s and 5s, but that the evidence is by no means conclusive.

VI. Favourable and unfavourable conditions for PK experimentation.

One of the objects of this series of experiments was to see whether I could find out anything about conditions which determined success or failure in PK experimentation. I was looking out for any hints that my experiments might give me, and the tentative conclusions I wish to report are often based on very incomplete evidence. I shall indicate this by using phrases which indicate incomplete conviction; where the evidence is reasonably good, that fact will be mentioned, but I wish to make it clear that I am not confining myself to reporting conclusions that I regard as established. Rather they are more or less tentative conclusions which must be tested out by later research. It was my impression that PK success was more affected by unfavourable conditions than is ESP success, that inhibition is more easily set up, and that success can only be expected if a good deal more attention is paid to the problem of securing optimal conditions than is necessary when experimenting with the more stable ESP processes.

The problems of conditions may be discussed under the following heads:

(i) Motivation.
(ii) Time of day.
(iii) Chronological decline.
(iv) Total length of experiment.
(v) Length of each experimental occasion.
(vi) Induction of favourable psychological conditions.

(i) Motivation. It has always been my impression that in both ESP and PK experiments on myself, effort to achieve the required result defeats its own end and tends to produce failure. Too strong motivation tends to result in effort and failure results. That some degree of motivation must be present seems likely but it must remain at a low level. Strong anxiety to succeed seems to militate against success. I have no convincing proof of this, but it is an impression gained by many indications. Effortless intention to succeed seems to me to be the ideal attitude. The way in which I have sometimes expressed this is that the attitude must be that expressed by the words: "I want to succeed but I don't really care whether I do or not."

A playful attitude towards the experiments is favourable to this relaxed motivation. At the end of December 1948, I started doing PK experiments in competition with my son and other members of my family. The results of these competitions (for myself alone) are included in Experiment I. The rate of scoring was considerably higher than I was at that time getting when working alone. It might be supposed that making PK into a competitive game would intensify motivation rather than reduce it. For a child it might do so, but for an adult the desire to win in a game is a far less strong motive than to get a positive result in a scientific experiment. It is true that the latter motive was still present but it was largely suppressed by the more trivial motive of winning the game. Under these conditions I felt that effort was relaxed and I succeeded better. But a crisis in the game inducing effort could again reduce the score. For example, in my first game with my son in which we threw in turn a set of three runs (72 falls) equally divided between the six targets, we both obtained a score of 16 above mean chance expectation at the end of 9 turns (27 runs). This is a pretty good rate of scoring for a PK experiment. Then we decided to settle the game by throwing until one was two ahead. This immediately produced a state of increased volitional tension and, for the next turn, I scored three below expectation while he scored two below chance expectation (the first time we had both scored negatively).

The same result appears to be produced by anxiety as to the total result. On one occasion I made alone a series of 27 runs with an equal number on each target face without paying any attention to my total score. Then I added up my score and found it was 17 above mean chance expectation. Then I felt that this was fine and I must keep it up; in other words volitional tension was increased. I immediately made another series of 27 runs and scored 7 below mean chance expectation.

These are mere indications of no great evidential value but they are examples of the kind of observation which makes me inclined to think that relaxation of volitional tension is necessary to success. They suggest that it may be worth while for experimenters who wish to get success to try to make their attitude towards their experiments rather playful than over-earnest. Competitive scoring may be a good way to achieve this end. There is nothing original about this suggestion ; the value of a playful attitude has been suggested both by Professor Rhine and Dr Humphrey. It also suggests that it is well to avoid being anxious about the result. The best way of experimenting may be to predetermine the total length of an experiment and then not to work out total results until the experiment is completed. It is hard to deny oneself the satisfaction of seeing how the experiment is going on, and I do not observe this principle myself, although I believe it is the best way. It is also to be considered that self-experimentation is particularly unfavourable to a care-free attitude towards experimental results, since a subject who is also the experimenter cannot fail to be anxious about the total result. On the other hand, this may not be important since it is possible that anxiety and volitional stress on the part of the experimenter may interfere with successful results however care-free the subject may be.

(ii) Time of day. On three days during the time when I was doing both experiments IIA and IIB (*i.e.* the Latin square and the $\Psi_{\gamma\kappa}$ experiments), I did the experiment both in the morning and the late afternoon of the same day. The combined results for the two experiments shows a startling superiority for the morning sessions. In every one of the six comparisons, the morning session showed a higher score and the total result was that shown in Table 14, showing a clearly significant superiority for the total morning score.

TABLE 14

	Falls	*Hits*	*Devn.*	*S.E.*	*C.R.*	*P.*
Morning - -	432	95	+23	7·74	2·98	·003
Afternoon - -	432	66	−6	7·74	·78	·44
Difference - -		29	29	10·95	2·65	·008

I was inclined at first to attribute this difference to having done the experiment twice in the same day, but it is more likely that it is due to the difference in the time of day, since it is found in a later experiment (not yet completed) in which afternoon experiments were done on different days from morning ones. In this experiment too there is a superiority of the results of morning experiments over afternoon experiments although those done in the evening are also better than afternoon ones.

The suggestion that experiments should preferably be done in the morning may seem a not very practical piece of advice for those whose occupation takes them away from home early in the day. For most people, however, it would be possible to devote half an hour to experimenting on Sunday mornings and it must be remembered that a short series of experiments under optimal conditions may give a better result than a long series under less favourable conditions with less total expenditure of time.

It will be seen, for example, from Table 15, in which I compare the total results of my Experiments IIA and IIB with those obtained when the experiment was done earlier than 12 noon, that I obtained a more significant total deviation from these morning experiments than I did from the complete series. If, therefore, I had confined myself to morning experimenting, it looks as if I should have got a more satisfactory result with the expenditure of about two thirds of the experimental time.

TABLE 15

	Falls	*Hits*	*Devn.*	*S.E.*	*C.R.*	*P.*
Expt. II (total) -	5184	939	+75	26·83	2·80	·005
do. (morning only)	3256	633	+69	22·20	3·11	·002

On the other hand, if morning is found to be too difficult a time for experimenting, evidence from later experiments than those reported here seems to indicate that evening is also a good time. The period of the day that seems most definitely unsatisfactory for positive results is the afternoon,

(iii) Chronological decline. In the course of a PK experiment (as in an ESP experiment) a process of inhibition sets in which reduces scoring rate

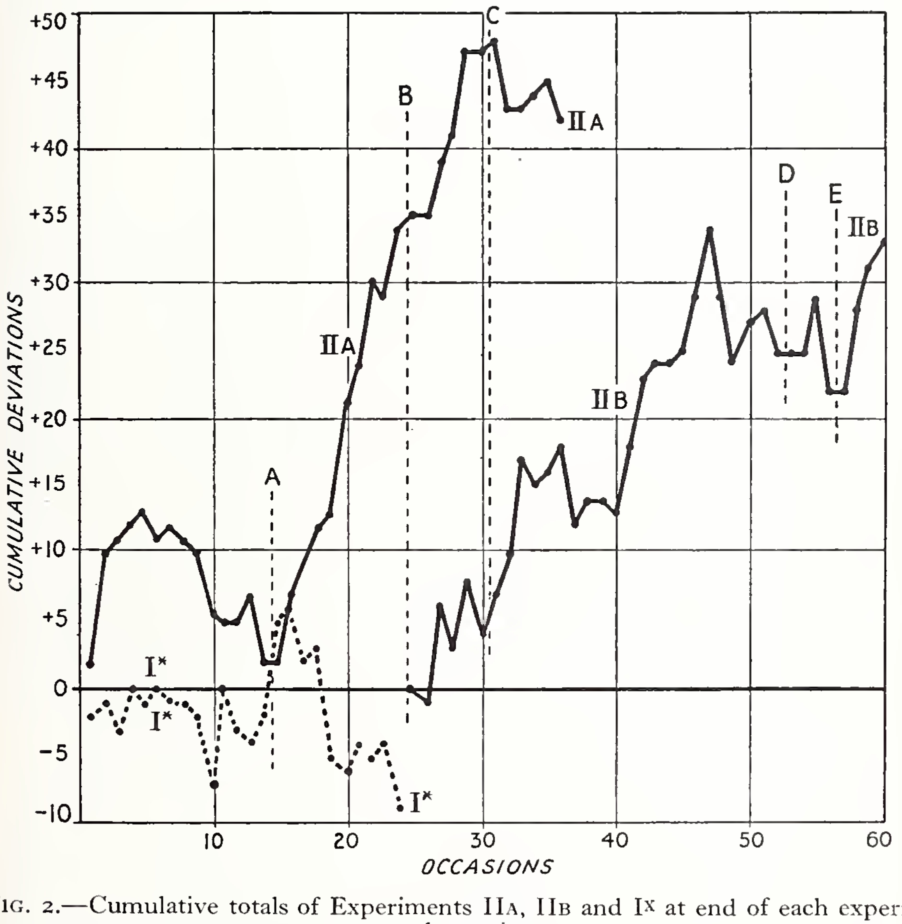

FIG. 2.—Cumulative totals of Experiments IIA, IIB and IX at end of each experimental occasion.

A. Point of beginning of practice of reciting poetry during the experiments.

B—C.—Between these points experiments were carried out alternately in the morning and the late afternoon with resulting alternate high and low scoring in both experiments.

D—E.—Between these points only one run (24 falls) was done on each occasion. In order to keep this part of the diagram uniform with the rest each entry refers to the combined total of three runs although these were done on separate occasions.

and which apparently may go on to a point at which the subject is scoring significantly below chance expectation. An experiment may, of course, be designed in which this process of decline is itself the object of study. To an investigator wishing to obtain a positive deviation, however, decline is merely a nusiance and much of the detail of experimental design (spacing of experiments, length of experimental occasions, etc.) is directed by an attempt to minimise this decline.

On each occasion, the total score obtained up to and including that occasion is plotted. The height of the graph shows, therefore, the cumulative total for that experiment, while the difference between two successive plots shows the score (deviation from chance expectation) on that occasion. I^x was an experiment with hand-thrown dice at targets determined by a Latin square. I had been doing a good deal of this experiment and it was already thoroughly inhibited; nothing but chance results were obtained. Experiment IIA, started at the same time, was in a form I had not been recently using. Target determination was the same as in I^x but the dice were machine thrown. Positive scoring started at once but chronological decline appears to set in on about the fifth occasion and the scoring continued predominantly negative until a novel feature (the recitation of poetry) was introduced at the point marked A. Thereafter, scoring was heavily positive until the thirty-first occasion when there are again indications of the onset of chronological decline, although the experiment came to a predetermined end (at the thirty-sixth occasion) too soon after this for any certainty that this decline was final. IIB was also a novel form of experiment started when I^x came to an end. The $\Psi_{\gamma\kappa}$ experiment had been done previously with hand-thrown dice but not (as here) with machine-thrown dice. Again it started off with positive scoring which was maintained until the twenty-third occasion of this experiment. After this point, nothing was added to the score although there is some indication of an end spurt. This, however, may be merely a temporary consequence of having made an unfavourable modification of the experiment between the points *D* and *E*.

These observations suggest that at least some part of the decline effect may not be general, affecting all types of PK performance, but specific to the particular task whose repetition has produced the inhibition. If this is the case, an improved score may be expected if the subject is switched to another task. I found other indications of this specificity of the decline effect. For example, on 3/1/49 during the course of Experiment I, I did six runs of a help-hinder experiment with a score of three below mean chance expectation, This was a familiar experiment. On the same day I started an entirely new experiment, the $\Psi_{\gamma\kappa}$ form of experiment, and scored a positive deviation of $+8$ for fifteen runs. On the following day, I continued at about this rate of scoring for the $\Psi_{\gamma\kappa}$ but was still below mean chance expectation for the help-hinder experiment. There seems here to be a strong indication that an inhibition of success resulting from continued application to one kind of experiment may not inhibit scoring in another form of experiment.

The difference between two forms of experiment need not be large in order to show this difference between degree of inhibition. In a series of Ψ_γ experiments reported in the *Journal of Parapsychology*,[4] I found that I got consistently negative scoring in a DT experiment which I had used many times before, chance scoring in a precognition experiment which I had done less frequently before, and consistently positive scoring in a slightly different form of precognition experiment which I had never done before.

This shows the danger of concluding that one experimental method is intrinsically more fruitful than another from the fact that it gives a better score when one starts using it. This may only mean that it is less inhibited because more novel. The practical suggestion emerging from this observation is that it may be a help towards overcoming the effect of decline to have a variety of tests rather than to use one only. There are a good many possibilities: the subject may throw for a series of pre-determined targets working alone, he may do so in competition with someone else, he may do my $\Psi_{\gamma\kappa}$ experiment, or he may do one of the variety of experiments described in Dr Humphrey's useful handbook.[5] Alternation of a variety of the available techniques is likely to give a higher score than the attempt to repeat a large number of times a single form of experiment.

(iv) Total length of experiment. The fact of the onset of chronological decline sets a limit to the total length of experiment which it is profitable to carry out with any one subject. It is impossible to make any definite rules on this matter since it is clear that different subjects differ very much in the degree to which they are affected by chronological decline. For example, Dr Soal's first subject Mr Shackleton seems to have carried on a single experiment to a very great length without serious falling off of score.[6] If my own results may be taken as a general guide, it looks as if about 30 experimental occasions for any one experiment were enough. Both my IIA and IIB experiments went on for 36 occasions and reference to Fig. 2 showing cumulative totals for these experiments suggests that both went on rather too long for maximum significance. Even this length may have only been possible because each session was a short one (3 runs of 24 falls). It is obvious that further research is necessary before one can make a general rule as to the optimal length of experiment. Provisionally one may consider about 60–90 runs as a length unlikely to produce serious chronological decline. The main point for experimenters to bear in mind is that there is an optimal length beyond which one should not go in testing any one subject with one test and that it is more economical to make a series less than this optimal length than to have it too long.

(v) Length of each experimental occasion. In the effort to make as much use as possible of each occasion on which the subject comes, the experimenter is tempted to give him as many tests as possible on each occasion. My experiments suggest that this is not an economical procedure. In the Series I, I worked for an hour or more on each occasion and succeeded in inducing a considerable decline after a few occasions on each experiment. It is true that these few occasions included a large number of falls, but I hoped to be able to work more economically by cutting down the amount of experimentation on each occasion. In Experiment II, I had only three runs of each experiment on any one occasion, although for the early part of the series two experiments of this length were performed on each occasion. When IIA was finished and I was doing IIB alone, I was doing only three runs on each occasion.

I then noticed a fact which made me wonder whether even this was not too long. When I had completed the twenty-eighth occasion of IIB I discovered that all my scoring up to date had been on the first run of each occasion. The totals for the first, second and third runs of each occasion showed deviations from mean chance expectation of +31, −5 and −1 respectively. While there was no positive deviation on the second and third runs, the score on the first run was highly significant. Taking into account the fact that this was not a single observation but the selected best of three, the odds against so large a deviation occurring by chance are about 250 to 1. For this run alone the mean scoring rate is the very satisfactory amount of rather better than +1 per run. It seemed, therefore, that it would be more economical to cut down the length of each occasion to a single run.

This was done on the next 12 occasions with the disappointing result that these 12 occasions gave a total score of 45 hits, three less than mean chance expectation. Reducing the length of occasion from three runs to one did not, therefore, result in the expected improvement in score. I do not understand why this was; it appears as if the superiority of the first run depends on the intention to carry out two more runs afterwards. In a sense, it may be considered that the logic underlying my expectation was faulty. I considered that in replacing three runs by one, I was doing the first run and leaving out the second two; it could be argued that I might as well have considered that I was doing the last one and leaving out the first two. The failure to get a satisfactory score on one run was not due to a general inhibition due to chronological decline since the remaining four occasions of this experiment showed a deviation of +7 on the first run, of +4 on the second run, and of 0 on the third run. Although the numbers are too small for statistical significance, the indication is that positive scoring was resumed and (less strongly) that the old pattern of scoring predominantly on the first run was resumed.

I feel no very strong conviction of the superiority of short experimental sessions on each occasion. Clearly chronological decline does set in even with an experimental session as short as the one I used in Experiment II and I have no clear indication that it does not set in as soon, when measured in terms of total experimenting, when the sessions are short as when they are long. I am still inclined to favour the short session and my present practice is to have not more than six runs of all experiments together for each experimental session. Further research is, however, necessary to establish whether this is really an economical procedure.

(vi) Induction of favourable psychological conditions. All Psi phenomena depend to a very large extent on the psychological condition of the subject; I am inclined to think that the performance of PK tasks is more easily upset by unfavourable psychological conditions than that of ESP tasks. In self-experimenting, it has seemed to me that I have not scored well either on ESP or PK if I am tired, cross, ill or anxious. In experimenting with other subjects I have noticed that positive results tend to disappear if there is any hint of a hostile or suspicious attitude on the part of those present or if tension is increased by over-emphasis on experimental precautions. These, of course, must be adequate but the rule should be: adequate precautions without fuss. If witnesses are present, their attitude should be that of co-operative friendliness.

It is easy to avoid experimenting when the subject is ill, cross or tired. Anxiety is more troublesome since there is always present a situation to become anxious about—the success of the experiments. Possible ways of dealing with this source of anxiety have been discussed earlier.

As well as trying to avoid these obvious sources of unfavourable conditions for positive scoring, we may consider whether there is any technique that can be used for inducing a psychological condition favourable to positive scoring. It would be ideal if we could find a method which led to consistent positive scoring without chronological decline. I can report only very limited success in the search for such a method. My only success in inducing a state of mind which seemed temporarily at least, to have a real effect of scoring rate, was when during experiment IIA I hit on the device of repeating poetry to myself while the experiment went on. The point at which this device was introduced is shown by the line A on Fig. 2. The immediate effects were striking. The deviations for the first six occasions after this method was adopted were 0, +5, +3, +2, +1, +8 with the total deviation of +19 for 18 runs (*i.e.*, 432 single die falls). The previous six occasions had given scores: −1, −4, −1, 0, +2, −5, totalling −9. The difference of +28 in favour of the deviations after the change of method is significant ($C.R. = 2{\cdot}56$, $P = {\cdot}01$).

This looked good and I thought I had discovered a method of overcoming the problem of low scoring. This hope was not fulfilled. That the method is not adequate to overcome the inhibition due to chronological decline is shown by the fact that it did not affect at all my rate of scoring in the thoroughly inhibited hand-thrown series going on at the same time. Moreover, even the series IIA showed distinct evidence of chronological decline in its later stages although the recitation of poetry went on all the time. I continued this method to the end of Experiment IIB although I was doubtful then whether it was having any useful effect. In experiments carried on later than those reported here I have used it only occasionally with no clear indication that it now makes any difference one way or the other.

It may well be that the initial success of the method was due not to any specific effect of the poetry but rather to the fact that it was a novelty; it may have been another example of an inhibition to a particular form of a test having been overcome by a change in the test. It may, on the other hand, have been due to the fact that the change in psychological attitude induced by the poetry at the beginning was no longer induced later on. I am inclined to think that both factors may have been operative. Certainly the experience of exaltation brought about by the poetry at the beginning was not maintained. It soon came to be part of the rather boring routine of the test, no more exciting then would have been the recitation of the alphabet.

It does, however, suggest a principle of experimenting worth trying out in order to help positive scoring. It may be worth while to try all kinds of emotionally stimulating stimuli such as music, discussion, problem solving, etc. before or during the test, varying these so that none of them become a merely accustomed routine. I have no doubt that different conditions may be effective for different subjects. The tense atmosphere of the hushed experimental room is likely, on the whole, to prove unfavourable for most of them.

VII. Conclusion.

It is obvious that, even at the end of my experiments, I was very far from having solved the problem of how to ensure success. That is clear from the low rate of scoring I had reached at the end. One would like to be able to describe conditions which would ensure that any experimenter could get successful results by adhering to them. That is not possible even for ESP experimentation, and it is my impression that successful PK experimenting is more difficult, since the effect seems more quickly inhibited by continued experimentation and more easily affected adversely by unfavourable psychological conditions. It is not, therefore, a matter for surprise if other experimenters do not always succeed in getting successful results.

There are, however, clear indications of certain things that they should avoid. One must avoid increasing the emotional tension of the subject by showing gratification at his success and disappointment at his failure. One must not introduce an atmosphere of tension by maintaining silence during sessions. If witnesses are present they must not be allowed to create tension by maintaining a suspicious attitude or by such behaviour as whispering. The subject should not be required to do large numbers of experiments at a time or to do a sufficient total number of sessions to make him exasperated by the experiment. A single experiment should not be repeated without variation throughout the experimental series. The satisfactoriness of an experimental design should be judged, at least in part, by the extent to which it avoids all of these unfavourable conditions.

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PROCEEDINGS OF THE

SOCIETY FOR PSYCHICAL RESEARCH

PART 180

SOME ASPECTS OF EXTRASENSORY PERCEPTION

BY DR S. G. SOAL

(*Presidential Address delivered at a General Meeting of the Society on 3 November* 1950)

I WOULD first express my sincere appreciation of the honour you have conferred upon me in making me your President. Many of my predecessors in this office have been men eminent in Science, Literature or Philosophy. I can only claim that my training has been that of a pure mathematician, and at present mathematics plays the useful though subsidiary role of informing us how likely it is that the coincidences we observe can be accounted for by the operation of chance. But it is conceivable that mathematics may one day become the language of mental relations as it is already the language of the physical sciences. Perhaps the small progress which psychology has made during the past hundred years in comparison with the gigantic strides taken by physics and chemistry may be due in no small measure to the fact that we have failed so far to discover a calculus which is appropriate for dealing with the phenomena of mental life. However that may be there is truth in Dr Rhine's paradox that today we know more about the atom than about the mind which conceived the atom. Psychology remains a rudimentary science and we are woefully ignorant of the working of those higher level functions which express themselves in creative activity of various kinds. And it would appear that the phenomena of psychical research are more intimately linked with the unconscious or subconscious strivings of the mind than with the conscious aspects studied by classical psychology.

I need hardly remind you that the term psychical research itself has very wide connotations, and has been held by many investigators to mean the examination of any unusual happening which does not appear to conform to a normal scientific explanation. When I was a member of that now defunct body the University of London Council for Psychical Investigation we were asked to examine the most diverse and fantastic claims. One man asserted that he was able to generate an abnormal amount of heat in his hands, but a physicist with a thermopile quickly disposed of this claim. Another man, a Kashmiri, alleged that when his eye-sockets were stopped with dough and bandaged he was yet able to see to read through the nasal mucosa, that is, the inside membrane of his nose. Though this man put up a good show it soon became clear that he was seeing down the outside of his nose instead of the inside.

Then there was the alleged talking mongoose Jeff who was the familiar of a sheep farmer and his wife and daughter who dwelt in a wooden frame house on a windswept upland of the Isle of Man. Unfortunately the investigators arrived at the lonely steading only to learn that Jeff had most inconsiderately left for a month's holiday.

Though such investigations may have some possible psychological value as providing material for studies in motivation, they involve a dispersal of time and energy and I cannot help feeling that psychical researchers would do well to restrict themselves to the study of those types of phenomena for which there is a respectable amount of good evidence. I mean by these the problem of extrasensory perception in its various aspects and the study of psycho-kinesis initiated by the workers at Duke University. This programme would also naturally include the observational study of spontaneous cases and the phenomena produced by those remarkable persons known as mediums. Hypnotism and the study of multiple personality come also within the proper province of psychical research.

On the other hand we must never forget that psychical research is a field of infinite possibilities. We must be prepared to gamble and take long shots. Who knows, for instance if ESP is confined to human beings and animals? An experiment worth trying would be to pit against each other two electronic selectors generating choices of, say, four symbols and observe if there were more coincidences than chance would predict. It is conceivable that the machines might "telepath" to each other. Such an experiment might be unsuccessful but it would not be silly.

But do not let us fritter away our time on the feats of stage telepathists and other illusionists. These persons are able to simulate extrasensory perception only because they work under their own carefully planned conditions. They are unmasked very quickly when asked to produce their effects under the normal experimental conditions of the parapsychological laboratory. The genuine telepathic subject is readily distinguished from the charlatan. The former submits without demur to any reasonable conditions which the experimenter may think fit to impose. The charlatan always begins by telling the experimenter in minute detail just how the test is to be conducted. The slightest deviation from his prescribed routine is liable to upset all his calculations. That is why stage telepathists generally refuse to permit examination of their claims by psychical research workers.

There are some who maintain that the best way to convince scientific men of the reality of telepathy is to go on demonstrating under increasingly rigid controls and to more and more witnesses the basic fact of extrasensory perception. This, I think, is a mistaken policy. One might, it is true, succeed by such methods in convincing a few individual scientists but the majority who had not seen the demonstrations would remain unaffected by them. And of those who witnessed them a certain proportion would prefer to believe it had been hoodwinked by some trickery than be compelled to abandon the outlook of a lifetime.

A wiser method would be to encourage young psychologists and others, by means of grants, to experiment for themselves, more especially with a view to discovering the mental and bodily factors which facilitate the emergence of telepathic or clairvoyant impressions, as well as the psychological types that are most successful as percipients, as agents and as experimenters. It is only by asking useful questions and devising careful experiments which provide the answers that we may hope at our present stage to make any real progress. To go on repeating the same experiment would be like trying to study electricity by continually stroking a piece of ebonite with flannel. Nor is it reasonable to expect a man who has convinced himself over and over again by carefully controlled experiments of the reality of telepathy to spend the bulk of his time and energy in trying to convince sceptics who have probably not given an hour's thought to the question in the whole of their lives.

When we have reached the stage (and I think it is probably only a question of time) at which we are able to say to the ordinary scientific worker: " Apply such and such psychological tests to a group of persons and by means of the results select your experimenters, agents, and subjects and you will be reasonably certain with a little perseverance of getting moderately significant results," we shall have achieved something of more importance than if we had convinced by demonstrations a dozen members of the Royal Society.

The study of the correlation between personality traits and ESP performance so ably begun by the late Dr Stuart and continued by Dr Humphrey and Dr Schmeidler will require for its successful progress the co-operation of highly-trained psychologists. On this score I do not anticipate any serious difficulties so far as this country is concerned. We have now in London at least two University colleges with psychology departments whose heads are sympathetic to our studies. In an age in which almost everyone is faced with the necessity of earning his living it would be unreasonable to expect heads of psychology departments to encourage young post-graduate students to make psychical research a main interest. But with financial assistance there seems no reason why a limited amount of work in this field should not be carried out by students in psychology who possess the right temperament.

To sum up, it is my belief that a large number of well-controlled experiments by independent investigators dealing with specific problems connected with extrasensory perception will have more influence upon orthodox scientific opinion than efforts, however rigorous, which are confined to a mere demonstration of the existence of the faculty. Indeed can it be said that one has succeeded in establishing completely any natural phenomenon until one knows something of the conditions in which it takes place? The best way then to convince ordinary men of science of the reality of telepathy is for as many workers as possible to do well-planned experiments with the object of finding out something about the way in which it works.

I would not, however, wish to give the impression that competent witnesses are unnecessary at experiments in extrasensory perception. They are indeed essential when the experimenters are young and inexperienced, and even in the case of investigators who are well acquainted with the various sources of error it is advisable for intelligent persons to be present in order to assist in the checking of records. Further, I think that whenever possible two experimenters should work together on an investigation. But there is clearly no sense in creating an atmosphere in which every person is suspected of cheating or of being in collusion with someone else. I think it is fairly certain now that the getting of results in extrasensory perception depends not only on the percipient and the agent but also on the general harmonious relations which exist among all the persons who take part in the experiment. As Professor Gardner Murphy would put it, the emergence of successful results is a function not only of individual mentalities but also of the intra-personal psychical field. When we have discovered a sensitive we must find a suitable group of persons to work with him if his faculty is to have unobstructed play. I would not, however, go so far as Professor Murphy who suggests (unless I have misunderstood him) that a considerable percentage of the population could become Basil Shackletons if only they were brought into contact with the right agents and experimenters, for I think this is too good to be true. I certainly believe that the powers of good subjects can be inhibited if the experimenters are uncongenial to them.

The reluctance of many men of science to accept the phenomena of extrasensory perception is largely the result of a misconception. It is assumed tacitly that there can be nothing in the Universe which does not obey the laws of physics as we know them today. I cannot see the least justification for this naïve assumption. As Professor Broad has pointed out more than once, had it not been for the accident that certain substances such as lodestone and amber are fairly common on the earth's surface, we might have known nothing at all about the existence of electromagnetic phenomena and it would have been supposed that the laws of Newtonian mechanics held throughout the Universe. Moreover, as Professor Hardy has asserted, it is impossible to account for biological phenomena adequately in terms of physics and chemistry. Still less is it possible to explain the complex diversity of consciousness and its creative activities in terms of present-day physics.

When a physicist asserts categorically that there can be no such thing as extrasensory perception, he is simply making an illegitimate excursion outside his own province. The question of the existence of paranormal perception is to be settled on the adequacy of the evidence and on that alone. Many eminent philosophers, biologists and psychologists have expressed the judgement that the experimental evidence for, say, telepathy is now adequate both as regards quality and quantity.

Nowadays there is very little criticism of the experimental evidence for telepathy that need be taken seriously. More often than not the would-be critic betrays the fact that he has not even taken the trouble to make himself conversant with the published reports which he is presuming to criticise. The general method adopted is to list a few of the well-known sources of error and then insinuate that all apparently successful cases of ESP can be explained away as being due to the presence of one or more of these flaws acting either singly or in combination. But there is no attempt to make the accusations specific ; the critic does not go through an actual scientific report and point out just where the errors lie in this particular piece of work. He confines himself to vague generalities. It is suggested even more vaguely that there is a " will to believe on the part of the investigators ", in spite of the fact that some of the most successful experimenters were sceptics when they began their experimentation. In a recent letter to the *New York Times Magazine* a lady medico asserted that a pure mathematician would be prone to believe in mystical things like telepathy which have no existence in fact as a relief from his professional work which consisted of hard logical abstractions! But how can the lady possibly *know* that telepathy does not exist? The issue is prejudged without the slightest attempt to weigh up the evidence. A year ago the writer of a letter to *The Times* stated that so-called telepathy was to be accounted for by supposing that the agent and percipient had the same preference for colours, numbers, etc. and common habits of mental association. He mentioned experiences of his own in support of this contention. It was as if Rip Van Winkle had suddenly awakened after a sleep of thirty years to make a pronouncement on telepathy! Yet the Editor printed this absurdity as though it were a serious contribution to the discussion.

It is certainly true to say that ninety-five per cent of present day criticism of extrasensory perception is either uninformed criticism by persons who have never made any competent study of the experimental reports of the past thirty years or blind irrational prejudice by writers who merely dislike the subject and its implications.

Effective criticism of the card-guessing techniques ended about the year 1940. By that time all sound objections had been adequately met by the experimenters, and it seems very unlikely that any further weaknesses, either experimental or statistical, will be revealed. The mathematical criticism was trivial from the very beginning but it was true that in some of the earlier experiments at Duke University sufficient care was not taken to eliminate the possibility of sensory cues from the cards themselves. But not one of the objections raised against the American experiments could in the slightest degree apply to those done in England.

Let us now turn to a recent critique of ESP entitled "Rhine or Reason" which appears in the autumn number of the *Modern Quarterly*, a Marxist Review. It is very significant that Marxism has begun to attack ESP. Already the Communists in Russia have issued propaganda against Dr Rhine, and now we have this bitter polemic against extrasensory perception appearing in an English Communist-inspired journal. It is written by John McLeish, who is Staff Tutor in Psychology at Leeds University. Now in my view the most serious defect of this article is the author's obvious lack of up-to-date knowledge of his subject. Had this critique been written in 1940 instead of in 1950 it would even then have been ineffective but in the past decade so many careful reports on ESP have been published, which the writer completely ignores, that his criticism is robbed of any value it might once have had.

He gives for instance an excellent popular discussion of the more ordinary sources of error which affect experiments in telepathy and clairvoyance and a somewhat more superficial treatment of the statistical aspects of card-guessing.

But there is no attempt whatever to show precisely how these different kinds of error could have vitiated, say, the Pearce-Pratt clairvoyance experiments in which the experimenters and guesser were in different buildings of Duke University, or the Pratt-Woodruff series, or the very carefully reported Martin and Stribic experiments at the University of Colorado. Mr MeLeish does not even mention the Martin and Stribic report, or the classic picture-guessing experiment of Whately Carington, or the Soal-Goldney report.

It is of course perfectly true that in certain of Dr Rhine's early experiments insufficient care was taken to eliminate the possibility of sensory cues from the backs of the cards. It is also true, as the writer affirms, that the reporting of many of the experiments in Rhine's first book, *Extra-sensory Perception*, does not permit the reader to form any exact picture of the experimental procedure, but these facts are now universally admitted by all competent parapsychologists. But these objections certainly do not apply to many of the later investigations carried out by Rhine and his disciples. So far as I am aware, there is not a single error of the type mentioned by Mr McLeish that could have any possible bearing on say the Whately Carington experiments, the Martin and Stribic series or the Soal-Goldney experiments.

Turning to my 1935–1939 experiments the author states that "Dr Soal . . . although recognising the possible operation of some such mechanism as 'unconscious whispering' or changes in breathing, failed to take the precautions necessary to exclude this factor except for warning the agent about it". Here the writer is clearly referring to my preliminary report, "Fresh Light on Card Guessing: Some New Effects", in which 160 subjects were tested. No precautions were taken to obviate the possibility of unconscious whispering because the result of the investigation was at the time judged to be entirely negative and had been negative for some years. It was therefore thought unnecessary to make the conditions any more stringent than they were. This was quite plainly stated in my report. The precognitive effects with two of the subjects were only discovered long after the experiments were ended.

But surely what Mr McLeish has to explain away are the results of the Soal-Goldney report, "Experiments in Precognitive Telepathy" (1943), of whose very existence he seems to be unaware since he does not even mention it. He has to explain just how Basil Shackleton, seated in a different room from the agent and guessing at the rate of 25 calls per minute could, over a period of two years, produce persistent *precognitive* effects by picking up "unconscious whispers" from the agent in the next room with the door almost completely closed. It is a tall order—especially to explain that precognitive effect which appeared week after week with three different agents. I cannot even imagine any reasonable explanation in terms of "changes in breathing" or "whispering" on the part of the agent. Referring again, I suppose, to the preliminary 1940 report, McLeish rather cryptically remarks that the tendency to displacement "is probably to be explained by the differences in experimental technique which involve timing differences, rather than from the theory of subconscious time-lag or discrepancy favoured by Dr Soal". I really do not know precisely what this sentence means and I cannot believe that the author has even read "Experiments in Precognitive Telepathy". The timing conditions of these Shackleton experiments as well as the whole experimental set-up were exactly similar to those of the 1945–1949 Stewart experiments. The Shackleton series produced on the whole no significant excess of hits on the "target" card but a highly significant excess on the card one ahead of the target which in 11,000 guesses corresponded to odds of 10^{35} to 1 against chance. On the other hand the main Stewart series of over 37,000 trials gave a highly significant number of hits on the actual (target) card, corresponding to odds of more than 10^{100} to 1, whereas on the cards 1 and 2 places ahead of the target there was a *deficiency* of correct hits that was also very significant.

It would be very hard to explain these anomalies in terms of timing differences and they would present a formidable problem for the "whispering" hypothesis.

Nor does Mr McLeish appear to have read my Myers Memorial Lecture of 1947 in which a preliminary account of the Stewart experiments is given, for his only knowledge of these experiments seems to have been derived from a somewhat inadequate description of a single experiment witnessed by the Science Editor of the *News Chronicle*. One does not in any case expect to find a scientific report of anything in a popular daily newspaper and I can take no responsibility for the account printed as I had no opportunity for reading it through.

Mr McLeish writes: "A closer examination of the experimental set-up would be necessary before Dr Soal's results can be accepted as evidence for the ESP hypothesis, especially in view of the fact that only two of his subjects—working in their own homes or studios—exhibited the ability to guess the cards in a fashion better than chance would suggest." But the experimental technique is well-known and has been described in minute detail in the Soal-Goldney report, in *Philosophy* by Professor Broad, in *Penguin Science News 9* by Dr Dingwall and Mr Parsons, in my Myers Lecture and elsewhere. Psychologists, philosophers and other scientists have witnessed it in operation without being able to find flaws in it. During the past nine years it has been discussed from every angle and still resists all serious criticism as Dingwall and others have pointed out. But Mr McLeish appears to be unaware of all this.

Nor is it correct to insinuate that Shackleton and Mrs Stewart could only produce their results in their own homes or studios. If Mr McLeish had read the Soal-Goldney report he would have known that Shackleton produced exactly the same effects when he was investigated at the rooms of the Society for Psychical Research in the presence of several persons with scientific training and Mrs Stewart on more than one occasion produced her most brilliant effects when shut up in the X-ray chamber of the Royal Free Hospital.

Since Mr McLeish makes "unconscious whispering" the spearhead of his attack on telepathy it is surely strange that he is unaware of either the experiments carried out by means of the telephone with Mrs Stewart and the agent in houses 150 yards apart or of the much more important experiments carried out in 1949 with the agents in London or Richmond and Mrs Stewart herself at Merksem near Antwerp. These last experiments, conducted with the aid of carefully synchronised stop-watches, and the 7-p.m. time signal produced a degree of success on the target card which corresponded to odds of 10^9 to 1 against chance. The successful results obtained on six occasions at a distance of over 200 miles were of precisely the same order as those obtained when Mrs Stewart and the agent were in adjoining rooms at a distance of about 20 feet apart. I do not know what Mr McLeish has to say about "unconscious whispering" or "changes of breathing" in connection with these experiments. If "unconscious whispering" is the probable explanation of telepathy there ought to have been a complete collapse in the results when Mrs Stewart migrated to Belgium!

Published reports of these experiments in detail were available to Mr McLeish six or seven months ago but rather than seek them out he prefers to cling to the exploded hypothesis that telepathy can be accounted for by unconscious whispering.

The author's discussion of experiments in "Pure Telepathy" designed to exclude clairvoyance is completely out of date. He is of course correct in his assertion that experiments in which the agent makes up a sequence of symbols in his head are vitiated by the fact that the guesser and the agent may possess similar order-preferences. But nowadays no serious investigator uses such methods whatever may have happened in the past. In all modern experiments in "Pure" Telepathy a code is used by the agent of which there is no written or other physical record. The symbols to be transmitted are decoded mentally by means of a set of different symbols arranged in random order. In attacking old experiments in which order-preferences and number preferences are not eliminated the writer is simply flogging a dead horse. Quite obviously he is unfamiliar with the experiment conducted by Mr Bateman and myself, and described in my Myers lecture which excluded not only clairvoyance in the present but also the possibility of precognitive and retrocognitive clairvoyance. Nor is there any mention of Miss McMahan's experiment on similar lines.

The writer asserts that "one looks in vain, however, in Rhine's books for the point-by-point discussion of the alternative views of particular experiments which have been put forward with considerable fire in the literature devoted to these problems". But this is not true of Rhine's most serious book, *Extrasensory Perception After Sixty Years*, a large part of which is devoted entirely to the discussion point by point of alternative hypotheses and their possible application to individual investigations.

The truth is that Mr McLeish is not sufficiently informed to make an effective criticism of extrasensory perception. He does not know the experimental literature sufficiently well.

If one confines oneself to attacking weak experiments and ignores all the really strong published reports it is quite easy to make out a case against ESP. One has only to describe a few of the ordinary sorts of error to which experiments have been often subject and then insinuate cleverly that even if these particular sources of error do not apply to certain experiments these may have been vitiated by other errors not postulated. But a quotation or two from Mr McLeish's article reveals only too clearly that his real motivation in writing this ineffective critique is a fear of the consequences which may follow the acceptance of ESP. He writes: "If therefore we subject Rhine's claims and conclusions to a closer examination, it is not because his views are inconsistent with materialism, but rather because the form in which they are expressed strikes at the root of psychological and other scientific experimentation. Rhine and his associates open the door to all sorts of incalculable forces, and his theories make the practical procedures of the scientific investigator in large part meaningless. The fashionableness of ESP in scientific circles in Britain today represents a form of intellectual *hara-kiri* symptomatic of a loss of nerve." Has it not occurred to Mr McLeish that the increasing attention which scientific men and philosophers are giving to ESP may have quite a different explanation? I suggest that, as Aldous Huxley remarks, the evidence for extrasensory perception is growing so strong that it can no longer be rejected. Or as Professor Hardy has proclaimed, it can only be rejected by the intellectually dishonest, and, I might add, by the intellectually timid and fearful. Incidentally I do not think Mr McLeish need worry about the possibility that ordinary psychological experiments may be vitiated by the intrusion of telepathy. If such a thing happened it would be of rare occurrence since the majority of people show only feeble evidence of telepathic powers or none at all. It is, however, quite probable that the recognition of telepathy and precognition may require us to make profound changes in our ordinary concepts of time and causation. But so far as the near future is concerned, the implications will be philosophical rather than practical.

Mr McLeish's critique abounds in misleading statements and half-truths. He remarks that "Coover, the Stanford University investigator, came to a negative conclusion after a long investigation under laboratory conditions (1917) and until his death resisted all attempts by others to 'cook' his results". But he carefully refrains from pointing out that on the whole 10,000 trials conducted by Coover there were 294 correct hits instead of the 250 expected by chance, and this excess, ignored by Coover himself, corresponds to odds of 200 to 1 against the operation of chance. As Professor Evelyn Hutchinson has remarked, papers are published every year in biology which support hypotheses with no greater odds than this. It was Coover himself who did the "cooking" by suppressing this fact, and not Professor Cyril Burt nor Dr Thouless nor Dr F. C. Schiller who drew attention to it. Odds of 200 to 1 in an experiment may not be conclusive but they are sufficient grounds for going on with the investigation, and Coover was too inert even to attempt to discover if there was any error in his experimental method.

But even had Coover's results been completely negative, the fact would have been of little consequence. It would only have shown that by taking 100 students at random one cannot be certain of finding a good telepathic subject. I did thirteen times as much work as Coover before I discovered two persons of outstanding telepathic gifts.

Mr McLeish does not fly to that last resort of the sceptic and accuse the investigators of fraudulent practices, but he suggests that in some cases the subjects may have been pulling the legs of the experimenters. Now while this might happen in a poorly controlled experiment it could not easily happen in, say, the recent Stewart experiments. For Mrs Stewart is seated in a different room from that containing the card-table and the agent. An experimenter sits beside her the whole time to see that she does not move from her place. She could only cheat if the agent or sender were in collusion with her. But several of the agents with whom she obtained most brilliant success were members of the staff of Queen Mary College whom she had never met until the hour of the experiment. Mrs Stewart often did not even know the name of the person who was coming to act as agent. Moreover the card sequences were determined from lists of random numbers prepared by an experimenter a few hours before the experiment.

Mr McLeish makes all sorts of suggestions as to what may happen, but he makes no attempt to apply his theories to well-controlled investigations and demonstrate how his suggestions could have worked in practice.

Again he hints vaguely that there is an atmosphere of excitement and emotion prevalent at experiments in extrasensory perception. This is simply not true of any experiments which I have attended and I have been engaged in such experiments for over twenty years. I am quite certain that everyone who has been present at either a Shackleton or a Stewart experiment would readily confirm that the whole procedure was humdrum and matter of fact from beginning to end. The results were not checked until the end of the experiment, and until then the subject did not know whether he was doing well or badly.

This is the kind of discrediting insinuation which in serious experiments has no basis in fact.

In support of his contention that the subjects themselves may be fooling the experimenters the author appeals to the Piddington performance. But the Piddingtons refuse scientific investigation and any competent experimenter could in half-an-hour satisfy himself that their results were not obtained by means of telepathy.

In certain circles we are constantly hearing rumours that a conjuror is going to duplicate the Shackleton experiments under similar conditions to those under which Shackleton worked. It is many years since the project was first mooted and my latest information is that the conjurors have not yet had sufficient practice but that matters are moving.

I would respectfully suggest that before Mr McLeish writes another critique of extrasensory perception he would do well to make himself more perfectly acquainted with the extensive modern experimental literature now in existence, for his present effort is at least ten years behind the times.

A very common remark about experiments in extrasensory perception is that the findings—unlike those of physics and chemistry—cannot be repeated by other workers. But this stricture requires considerable qualification. It would be truer to say that experiments in telepathy are difficult to repeat, and not that repetition is impossible. One reason is that the investigator is in the position of a chemist who wants to make experiments with some extremely rare element that is hard to obtain. In our case a human subject with high-grade extrasensory powers corresponds to the chemist's rare substance. In point of fact many of the findings such as displacement phenomena in card or picture guessing have been noted again and again by independent investigators, as I shall show presently. The same is true of the decline effects first noted by Miss Jephson and the negative scoring first discovered by Rhine.

But even were good telepathic subjects to become one day as common as they are now rare, we should not expect to obtain the precise verification of results that occurs in chemistry and physics. For mental phenomena, and more especially those connected with the higher-level functions of the mind, are extremely variable. So long as the psychologist restricts himself to simple experiments in sense-perception, learning by rote and so on, in which the physiology of the nervous system plays a prominent part, the findings will show a certain uniformity because these phenomena are partly subject to physical causation. Experiments in physics can be exactly repeated because the results are statistical effects concerning the average behaviour of immense numbers of elementary particles. One cannot say with any certainty what an individual electron is going to do. One can of course make mass predictions about the average behaviour of a large group of human beings. If fifty thousand persons chosen at random are asked to write down a letter of the alphabet, we can prophesy with some confidence that only a very small proportion will choose N, I, Y, or Z and that letter A and the labials B, M, P will be the most popular. But no-one can predict with any certainty the letter Tom Jones will select.

Psychical research explores the deep-level aspects of human personality, and human beings are only in part subject to physical causation. Most men believe that when faced with two alternative courses of action the conscious personality has the power to intervene and choose one of them. If this belief is illusory, then clearly there can be no such thing as moral responsibility. But if, as most of us believe, the power of conscious choice is a reality and we are not automatons, this means that our conscious minds can initiate physical processes within our bodies. The Will therefore is one aspect of human personality in which something that is non-physical comes into operation, namely the trigger action of conscious choice. We need not be surprised then to discover regions of the human mind where consciousness transcends time, space, and causality as we know them in the physical world.

Physical phenomena are characterised by their uniformity and psychological phenomena by their uniqueness. As we pass from physics to biology, thence to classical psychology, through psycho-therapy to psychical research we find an ever-lessening degree of repeatability in the results of experiment. But though physics and psychical research are at opposite poles they are like two ships moving in different directions on the same great circle and converging on the same objective. For both are leading us back to those fundamental problems of metaphysics which concern the nature of time and causation and the relation of man's mind to the physical world. Physics deals with groups of sense-perceptions which can be fitted into a certain mathematical framework; psychical research with those deeper level processes of the mind which refuse to fit into the framework. Both in their different ways bring us face to face with the problem of the human observer and the nature of consciousness itself. As Eddington has said, the footprints that we notice on the sands of the Universe turn out in the end to be our own. Just as Eddington was led to adopt a thorough-going idealism in his attitude to physics, so I believe that psychical research will have to abandon the Cartesian dualism with which it started off and espouse an idealistic philosophy which postulates different grades of mental substance that are capable of interacting with one another.

It has sometimes been urged that parapsychology has a plethora of facts and not enough working hypotheses. I do not think however that we can have too many facts, provided of course that they are well-substantiated. What the writer probably had in mind is that the facts are isolated and that we have not yet discovered significant relations among them. In the history of most sciences there is an intermediate phase between the collection of crude facts and the construction of fruitful hypotheses. This is the phase of systematisation and concise description in which isolated observations are brought into relation with one another and masses of detail are comprehended by some simple statement or formula. When Kepler found that the multitudinous observations of Tycho Brahe on the positions of the planets were consistent with the supposition that each planet moved in an ellipse with the sun at a focus and that the radius joining the planet to the sun swept out equal areas in equal times, and that for different planets the squares of the periodic times were proportional to the cubes of the major axes, this concise description created order out of chaos and paved the way for Newton's great hypothesis. I would not for a moment suggest that parapsychology is in sight of any such major clarification. But in a smaller way there are signs that certain workers are searching for relations among the facts of extrasensory perception. I have been much impressed by a series of papers by Dr J. G. Pratt and Mrs Esther Bond Foster dealing with the phenomenon of displacement which have appeared in recent numbers of the *Journal of Parapsychology*. Displacement occurs when a card-guesser names correctly and with significant frequency the card which is one or two places behind or ahead of the target card at which he is consciously aiming. If he scores significantly on the card one place ahead of the target card this is known as (+1) displacement; if on the card one place behind (−1) displacement. In the case where cards are arranged in a pile at the beginning of the experiment and lifted off one by one as they are guessed, there is no particular reason to suppose that the (+1) shift has anything to do with time. The displacement may be of a purely spatial kind. In the case of Basil Shackleton (+1) displacement occurred even when the card sequence was not in existence at the start of the experiment but was determined during the test by a person who selected coloured counters from a bag or bowl by touch one after another. Here there are reasons for thinking that we were dealing with a displacement into future or past time.

The earliest observation of displacement in card-guessing of which I have any knowledge was made in 1938 by Dr C. G. Abbott, an astrophysicist who was at one time Secretary to the Smithsonian Institution at Washington. Dr Abbott made a successful repetition of Rhine's card-guessing tests with the clairvoyance condition using himself as the subject. He noticed that when he was feeling tired or run-down he failed to guess correctly the card at which he was aiming but that his guess was often right for the immediately preceding or following card. In 1949 Dr Abbott tested himself again with the Zener cards and reported quite significant (+1) and (−1) displacements, these being of about equal magnitude. In 1940 Whately Carington published his classical paper on picture-guessing which produced evidence that when he pinned up a different drawing in his study on each of ten consecutive evenings the subjects in many cases got correct impressions, not of the sketch exhibited on the night on which they made their drawing, but of the one pinned up by Carington a night or two earlier or later. Carington's work, as is well known, led to the discovery that two of my own card-guessing subjects, Mrs Gloria Stewart and Mr Basil Shackleton, had produced highly significant $(+1)$ and (-1) displacements over a series of many hundred guesses. In a long series of further experiments carried out with Mr Shackleton by Mrs K. M. Goldney and myself, he continued to produce $(+1)$ displacement with each of two agents and both $(+1)$ and (-1) displacement with a third agent. When the rate of calling was speeded up to double the normal rate the (± 1) shift gave way to a (± 2) shift. Since the publication of our report on Precognitive Telepathy significant displacement effects have been recorded by several workers—notably by Thouless and Oram in England and by Birge, Bindrum, Abbott and Stuart in America. None of these effects, however, were comparable in magnitude with those observed in the cases of Shackleton and Mrs Stewart.

In 1943 W. Russell re-examined for displacement five records by American subjects who had produced high scores on the target card but discovered no evidence of displacement. Until the publication of the new work by Pratt and Foster the phenomenon of displacement appeared as an oddity—a queer exception to the rule that telepathic or clairvoyant subjects in general hit the target at which they are aiming. In the work carried out with Mrs Stewart from 1945–1949, however, F. Bateman and I observed for the first time a new type of displacement which we have called " negative " displacement. This must not be confused with backward or (-1) displacement. What happened was that Mrs Stewart showed a significant tendency to choose certain of her guesses so as to avoid scoring a hit on the card which immediately followed the target card or the card which immediately preceded it. That is to say she scored on $(+1)$ and (-1) targets many less hits than even chance might be expected to produce. This avoidance extends also to about the same degree on $(+2)$ targets and to a lesser degree on (-2) targets. On the actual target itself there is a very large positive score which on the main series of 37,100 trials amounts to over 25 standard deviations. Compared with this the negative deviations on displacement targets are small though highly significant—that is except in the case of the (-2) deviation.

Our first tentative explanation of these negative effects was to assume that they were the result of subconscious motivation on the part of Mrs Stewart. We had some reason to think that she was jealous of Shackleton and wished to avoid a slavish imitation of his positive displacement phenomena. As a consequence we assumed that some kind of censor in the subconscious stationed itself at the gateway and inhibited guesses which would be correct on the displacement targets from passing into her conscious mind.

Pratt and Foster have recently examined the original records of the extensive series of clairvoyance experiments carried out by Dorothy H. Martin and Frances Stribic at the University of Colorado prior to the year 1938. These are among the best-controlled of American card-guessing experiments. Up to date Pratt and Foster have been concerned with the work of Martin and Stribic's two best subjects known as D.W.

and C.J. Both these percipients, in the series studied, worked under similar conditions. A pack of 25 Zener cards was shuffled behind a screen and the subject guessed the order of the cards from top to bottom of the pack which remained undisturbed till the guessing was complete. This is usually known as the DT (Down Through) method. Each subject ran through a shuffled pack about 300 times. The averages of correct hits on the "target" card made by C.J. and D.W. were almost identical, these being 7.3 and 7.5 respectively for a pack of 25. These results are extremely significant. But Pratt and Foster have now disclosed a result which lay unsuspected for twelve years. They find that both D.W. and C.J. produced very significant forward (+1) displacement of a *negative* kind. That is, each tended to *avoid* scoring correct hits on the card which immediately followed the target card. In the case of D.W. the overall (+1) negative displacement amounts to 3.36 standard deviations but with C.J. it reaches the high figure of 9.1 standard deviations. The odds against chance corresponding to the last result are of the order 10^{18} to 1 and C.J.'s record constitutes the most remarkable case of displacement encountered since Shackleton. In making these estimates the high scoring on the target card in conjunction with the fact that Martin and Stribic used packs containing five cards of each symbol have been taken into account. The over-all backward (−1) displacement scores for these subjects are normal, that is what would be expected by chance.

But the most fascinating find of Pratt and Foster is the discovery that this tendency to avoid the card one place ahead is not an effect which is randomly or indifferently distributed throughout the whole of the scoring sheet. It has on the contrary a very definite positional relationship to the hits registered on the direct targets. In those regions where there is an absence of direct hits on the target forward negative (+1) displacement is a dominant feature. Thus in the case of a pair of consecutive misses or failures on the direct targets represented schematically as follows:

Fig. (i)

Guess	Target
A	B
C	D

there is a highly significant tendency for guess A which has missed its own direct target to miss also the (+1) target D, always provided that D has been missed by its own direct call C. That is to say in the case of a pair of direct failures in succession the second failure is frequently heralded by a (+1) precognitive failure. This might suggest that in those areas where ESP is not functioning directly there is sometimes a censored form of ESP still in operation. We may suppose that the function of the censor is to prevent the ESP impressions from slipping into the normal consciousness of the subject. In a world adapted to sense-perception ESP is a disturbing influence which must be suppressed in the interests of everyday living. We may think of the censor as trying to discourage this primitive awareness of the organism which we call extrasensory cognition. He may inhibit a card symbol known by ESP from emerging into consciousness by suggesting one of the other four symbols to the conscious mind. At the first of the two guesses in Fig. (i) the censor may know that target D is an ESP impression in the offing, as it were. He throws up a different symbol A into consciousness. At the second of the two guesses he is aware that D is now trying to slip into consciousness as a direct target, and this is again inhibited by the substitution of a different symbol C.

Another way of stating the statistical effect described above is to observe that in the case of pairs of consecutive misses on the direct targets the total (+1) forward displacement score is exceeded very greatly by the total (−1) backward displacement score worked out for these pairs and that this holds for both the subjects D.W. and C.J.

Quite a different situation arises in the case of pairs which consist of a miss followed by a direct hit, or conversely of a direct hit followed by a miss. These cases are represented schematically by (*a*) and (*b*) respectively:

Fig. (ii)

	Guess	Target	
(*a*)	A	B	Miss
	C	C	Hit
(*b*)	A	A	Hit
	B	C	Miss

The tendency now is for the forward (+1) displacement in case (*a*) to be greater than the backward (−1) displacement in case (*b*). This difference is significant, however, only in the case of the subject D.W. but still suggestive in the case of C.J. In fact the general trend in case (*a*) is towards a (+1) forward *positive* displacement and this is the reverse of the (+1) forward negative displacement noted with pairs of misses.

There is indeed a tendency for the subject to anticipate a direct hit precognitively one guess before he reaches it in the sequence.

Hence (*a*) becomes

Fig. (iii)

Guess	Target
C	B
C	C

Quite possibly the subject is unable to distinguish subconsciously between a correct impression that is precognitive and one that is direct. He may for instance in Fig. (iii) believe that he is guessing directly two consecutive cards bearing the same symbol.

As Pratt and Foster carefully emphasise, it is not at present possible to say which is the correct interpretation out of a number of alternatives which the authors suggest in order to cover these interesting findings. My own suggestion that there are regions of the scoring sheet in which ESP is functioning so strongly that the censor allows both direct hits and (+1) hits to escape into consciousness, and other regions in which ESP is so weak that the censor can cope with both direct and precognitive hits, may of course prove quite unacceptable in the light of further enquiry. The authors' work is not presented as a complete study but as a progressive programme of research in which more and more records will be examined and reported upon after they have been subjected to the same analysis as that applied to the work of D.W. and C.J.

Although the experimental conditions under which D.W. and C.J. worked are very similar, the results exhibit some curious differences. But this is only to be expected in view of what was said about the uniqueness of extrasensory phenomena. Each subject's performance represents his own personality reaction to the experimental situation, and this reaction is modified by the extra-personal relations of the group with which he is working. It was found for instance with C.J. that as the number of correct hits on the target card increased within the run, the average negative forward $(+1)$ displacement on a pair of consecutive misses also increased numerically in a significant regression. There was a similar though somewhat slower numerical increase on the average positive backward (-1) displacement on pairs of consecutive misses which ran parallel to the increase in the number of direct hits in the run. No such relationship between direct and displacement scoring was observed in the case of the other subject.

Again the authors found that the negative score on forward displacement was, with C.J. particularly, marked on pairs of consecutive misses that immediately followed after direct hits, but this was not true of D.W. Pratt and Foster suggest that C.J.'s strong negative forward displacement after a direct hit may be his own personal reaction to a successful hit on the target card at which he is consciously aiming. This would imply that C.J. was subconsciously aware of having succeeded by ESP and responded in a way that was peculiar to his own psychological make-up. Another subject might produce a quite different response. It is pointed out that there are chronological variations in types of ESP functioning to be observed even in the same individual when his records are studied over a sufficiently long period of time. In other series by C.J., both earlier and later than the one under present consideration, forward negative displacement on pairs of consecutive misses is absent though the subject continues his high scoring on the target card. This does not mean that in selecting C.J.'s (DT) series for special examination from the mass of his other work we have created a mere statistical artifact. For the authors show by means of a "t" test that if we work out the average difference per run of $(+1)$ and (-1) scores obtained from pairs of consecutive misses the corresponding "t" value is 7.5 instead of the expected 0 value with 299 degrees of freedom. Such a value would be expected to occur by chance only once in about 10^{11} such series.

Again, the displacement phenomena of Mrs Stewart showed a complete reversal in sign when we re-started our experiments in 1945 after an interval of eight years. That is, in 1936 she was scoring hits on the cards one place ahead or behind the card she was aiming at, but for the past five years she has been avoiding these two cards in a significant degree. It will be asked whether the new series has shown displacement effects similar to those discovered in the work of C.J. and D.W. As yet only a very cursory examination has been made of the Stewart records from this standpoint but it seems probable that the negative (+1) forward displacement is not uniformly distributed over the series and that it is related to success on the target card. We do not, however, anticipate the particular effects described by Pratt and Foster.

The appearance of both positive and negative displacement in different regions of the scoring sheet opens up interesting possibilities. It may well prove to be the case that displacement is less an exception than a normal accompaniment of scoring on a sequence of targets, as indeed Carington's work seemed to suggest. Quite possibly in series which show no over-all displacement there may exist both positive and negative displacements which are separately significant but which cancel each other on the whole.

The findings of Pratt and Foster are certainly thought-provoking, if very puzzling, and until many more cases of high scoring have been analysed for such effects one ought to be cautious in deciding which is the most satisfactory of a number of possible explanations. As Dr Pratt has remarked, the answer to puzzling facts is to be found in more facts.

The original experimenters, Martin and Stribic, do not even mention displacement in their long and careful report, and the discovery twelve years later of these totally unexpected results lends confirmation, if this were needed, of the authentic character of their work. One could hardly suppose that investigators or percipients would go to the immense trouble of faking these out-of-the-way effects without even drawing anyone's attention to them for a period of twelve years.

In our present state of knowledge it would probably be as futile to ask what extrasensory perception is as to ask what an electron really is. If, as Professor Price has suggested, ESP is a primitive kind of awareness which is characteristic of all matter, then it is probably not confined to living organisms. In a certain sense it might be said that two electrons in different states behave as if they were mutually aware of each other. If the basis of ESP is an all-pervasive fundamental awareness, it would not appear to be possible for the human mind to get behind it or to analyse it into any simpler elements. The various types of sense-perception, developed by living organisms as they ascend in the scale of evolution, would on the above view be regarded as highly differentiated and canalised forms of the primordial awareness. The primitive awareness thereby becomes imprisoned in space and time and thus subject to the laws of physics. As living organisms develop increasingly complex sensory apparatus, more and more of the general awareness will be directed into sensory channels and extrasensory awareness will tend to disappear. This suggests that psychical researchers who are competent biologists might devise experiments to detect extrasensory perception at various levels of the animal kingdom. Experiments with domestic animals and birds are obviously indicated, but more lowly creatures might also be used. For instance does ESP play any part in the homing instincts of newts? Do ants exhibit any extrasensory powers? Does ESP assist a salmon which returns to spawn in the same river in which it passed its early life even after the lapse of several years spent in the ocean, and upper migrations extending to hundreds of miles? These are fascinating problems which are too often ignored by biologists.

Any theory that we form as to the working of extrasensory perception must rest ultimately upon what view we take of the mind-body relation. If for instance we believe that all mental events are caused by physical events which occur in the brain and nervous system and that mental events can never cause bodily events or other mental events, then it becomes impossible to explain precognition and clairvoyance and extraordinarily difficult to give any satisfactory account of telepathy. But I think it would be correct to say that very few first-class philosophers either of the present generation or in the past have accepted epiphenomenalism as it is called. The only people who cling to it nowadays are the physiologists and the behaviourist psychologists. The pretensions of the latter have been mercilessly exposed by Professor Broad in his book, *The Mind and its Place in Nature*.

Any theory which hopes to interpret psychical phenomena must postulate I think that mental events such as ideas, images etc. exist in their own right and are not synonymous with chemical and electrical events that take place in nervous systems. This was conceded by Whately Carington in his Association Theory of telepathy. Following Bertrand Russell, Carington considers the mind to consist of a large number of elementary constituents—sense data and images. Images are of the same nature as sense-data only less vivid and constant, and Carington uses the word "psychon" to cover both sense-data and images. Certain tensions and relations exist among the members of a psychon system, and this is what is meant by saying that the system is conscious. Those groups of the system in which tensions are particularly strong at a given instant would constitute the normal consciousness of the mind, while the more quiescent groups would constitute its subconscious. Carington evidently supposes that the subconscious psychon systems of different minds have no relation to physical space and that they form one large subconscious system or common mind. The concept of a big mind composed of individual minds is of course an old one which goes back to the mysticism of ancient India, but Carington makes it somewhat more plausible by the atomic theory of mind which he adopts.

Carington then assumes that the laws of association of ideas operate within the large mind just as they do in individual minds. In a telepathy experiment the agent or sender associates the object he is trying to transmit with the general idea of the experiment. This idea of the experimental set-up is, however, also present in the mind of the distant subject or receiver. This mutual idea is therefore likely to call up by association from the common subconscious into the mind of the guesser the impression the agent is trying to send. Carington suggested that the more ideas the agent and subject have in common that are associated in the agent's mind with the transmission object, the better the experiment ought to succeed.

The theory would appear to be most plausible when applied to Carington's own type of experiment in which a single sketch, say of a ship, is pinned up in a room for several hours and is associated in the mind of the agent with the experimental set-up. But consider what happens in an ordinary card-guessing experiment in telepathy in which five different symbols are presented to the agent in random order at the rate of one every two or three seconds. After a hundred cards have been called, each symbol has been "associated" with the idea of the experiment about twenty times. Can we really believe that at guess No. 101 the power of association has any efficacy in calling up, say, the idea of a circle rather than that of any one of the other four geometrical symbols? If five *different* objects, one allotted to each symbol, were presented to the agent at the same time as the corresponding cards, and these same objects were presented in the same order to the guesser, this might give association a better chance to work. Yet it must be admitted that card-guessing experiments are at least as successful as picture-guessing experiments of the type carried out by Carington.

But there are probably other factors quite as potent as contiguous association which govern the recall of images from the subconscious. The ideas which emerge into our conscious minds most frequently are those around which our emotional interests are centred. If I see a tall church spire, that is associated in my mind with a great many things—with church bells, religious services, English history, and so on; but if I happen to be an enthusiastic mountaineer I am more likely to think of the Matterhorn than of any of these. This suggests that we might choose for our agent a person with five dominant interests which were also interests of the percipient, and employ symbols representing those interests.

Personally I have not found any evidence that by increasing the number of associative links between agent and guesser the experiment is any more successful. This certainly did not happen in the London-Cambridge experiments in which Mrs Stewart in London tried to get results with an agent in Cambridge who was unknown to her at the start of the test. First we provided her with a photograph of the agent, then with a photograph of the card-table showing a violin resting on it and finally with a detailed description of the room in which the agent sat at Cambridge, but her score showed no improvement.

As regards the psychon theory itself, I can only say that to a plain man like myself it seems very odd indeed. For, according to Carington, the mind consists of nothing but a fluctuating group of images and sense data, some arising from the body and some from the external world. No directive or controlling agency is postulated. But there are many things which happen in our minds besides the experience of sense and images. There are for instance judgements and discriminations, beliefs that certain propositions are true or false, inferences, aesthetic feelings and so on. Now these things appear to our introspection to be of a totally different nature from sense-perceptions and mental images. How then do they arise in a mind whose fundamental constituents are sensa and images? We might suppose them to be emergent qualities arising from groups and sensa and images. But if we have to assume that systems of psychons have properties whose nature or existence could not be deduced from the qualities of the individual psychons I cannot see that this is going to furnish us with any very intelligible theory of the structure of the mind.

On Carington's theory I cannot see how the mind would ever be able to transcend its immediate perceptions or how scientific knowledge and constructive imagination could have arisen.

But though the psychon theory seems inadequate (to me at least) to account for the whole range of mental phenomena, it may still provide us with useful suggestions in the study of telepathy. I agree with Professor Price that some kind of atomic theory of ideas and images is more helpful in psychical research than a unitary theory of the mind. We must be able to think of ideas and images as non-physical things which have an existence that is independent of the mind by which they were originally owned. That is, when an image is no longer present in consciousness it still continues to exist, but is now more loosely attached to the mind and under favourable circumstances is free to insert itself into another person's consciousness.

Whether the association theory of telepathy is really useful is a matter to be tested by experiment. In my own work I have not found much support for it, though I am not sure that I have found any evidence that actually tells against it. Last year Mr Bateman and I carried out with Mrs Stewart a good many tests in which two efficient agents focussed at each call simultaneously on different card symbols. On the assumption that ideas emerge by association from a common subconscious substratum it would be reasonable to expect that the percipient would receive impressions from both agents and would probably be unable to discriminate which came from A and which from B. That is, she would, since the agents were about equally good, now get a correct impression from A and now one from B and thus score significantly with both agents. But in practice this never happened. We found that she made a high score with one of the two agents but obtained nothing from the other. If she really received impressions from both A and B—though not necessarily from both at the same call—she was apparently able to distinguish, at some level of her mind, between impressions coming from A and those coming from B, and then ignore one set of impressions. But if she was receiving impressions from A only, this does not fit in very well with the association theory.

It was noticed, however, on certain occasions that when Mrs Stewart was informed beforehand that a person A was to be the agent but was left in ignorance of the fact that a second agent B was working in opposition to A, she succeeded with A but made no contact with B. In such a case it is possible, as Mr C. W. K. Mundle has suggested, that the idea of " A being the agent " which is common to both A and the percipient may serve as a " K " object in Carington's sense and cause A's card images to emerge by association into Mrs Stewart's mind. B of course also possesses this idea but to him it has not the same personal significance.

Whether there is anything in this suggestion could only be determined by suitable experiments.

Carington's theory fails of course to provide any satisfactory explanation of clairvoyance, a term which is usually understood to mean extra-sensory knowledge of an event in the physical world which could not be derived from an acquaintance with the present, past, or future states of some human or animal mind. The difficulties in the way of accounting for clairvoyance on any hypothesis of physical radiation from material objects has been sufficiently emphasised by both Professor Broad and Dr Rhine, and I need not recapitulate them. Nor can Carington's theory explain precognitive telepathy. Carington himself inclined to the belief that cases of apparent clairvoyance were to be explained in terms of precognitive telepathy, for which phenomenon he thought there was adequate independent evidence. It is certainly true that the great majority of clairvoyance experiments with Zener cards could be accounted for by assuming acts of precognitive telepathy on the part of the guesser. That is, we suppose that the guesser did not directly cognise the symbols on the cards themselves but that he became aware of the future mental images which would be present in the mind of the person who ultimately checked the order of the cards in the pack against the list of guesses. Experiments were therefore designed to eliminate the possibility of precognitive telepathy, and certain of the tests gave results which, it was claimed, could only be accounted for by postulating clairvoyance.

At the risk of re-opening a discussion of which I imagine most of us are heartily tired, I will state my own opinion that it is still possible to question the adequacy of certain of these tests as actually carried out.

Let us consider for instance the experiment recorded by Drs Humphrey and Pratt in the *Journal of Parapsychology*, December 1941. In this test a large number of Zener cards were taken and each card was enclosed in an opaque sealed envelope. There were five chutes, each provided with a kind of letter-box opening, and on each letter box was pinned one of the five Zener symbols. The guesser was asked to drop each envelope into the chute whose symbol he thought matched the card enclosed. The envelopes fell down the chutes into five boxes, but in falling the order in which the envelopes were dropped into the chutes was not preserved. The boxes were in another room and were unobserved by either the guesser or the experimenters. In order to check the success of this experiment the investigator had only to count the number of correct cards which fell into each box.

Actually the evidence for clairvoyance was indirect, for there were significantly fewer correct guesses than chance would predict. There was in fact a negative deviation, and the odds against this being due to chance coincidence amounted to about 1,000 to 1. The assumption underlying the experiment was that the guesser when about to drop, say, the tenth envelope into a chute, would be unable to obtain from the mind of the person who ultimately opened the envelope, the fact that this tenth envelope contained a circle, since when the checker opens it he cannot know that it is the tenth envelope owing to the original order having been destroyed. All that the guesser could obtain by precognitive telepathy from the checker's mind is that ultimately each box will contain so many circles, so many squares, etc., and this would be of little or no assistance to him in identifying the card in the particular envelope he is holding.

But the assumption that the envelopes were indistinguishable from one another was clearly not true in this particular experiment, since each envelope had affixed to it a band of brown paper on which a code number and letter were written. If therefore the guesser noted the number Q12 on an envelope before he dropped it into one of the five chutes, he could obtain by precognitive telepathy from the mind of the checker who ultimately opened the envelope, the fact that Q12 contained, say, a circle. But even had the envelopes borne no code numbers, certain of them might have been distinguished by, say, prominent specks on their faces or backs. We have plenty of evidence that persons with good eyesight and good visual memory are able to recognise a plain new postcard previously seen after it has been mixed with several others of similar make. Even new playing cards can be distinguished by means of specks and irregularities on their backs. To make the experiment completely satisfactory it should have been carried out in the dark and the envelopes dropped into the chutes by guessers who wore gloves.

The apparatus invented by Mr Denys Parsons and based on similar principles is free from these objections, but so far as I am aware no *successful* experiments in clairvoyance have yet been made with it.

I do not mean to imply by these comments that there is no good evidence for clairvoyance, but only that in such experiments as I am acquainted with there is room for some improvement.

May I in conclusion make a few remarks about future methods of experiment in extrasensory perception? I am not alone in the belief that the present card-guessing and picture-guessing tests will have to be supplemented by more delicate kinds of experiment if we are to learn anything really worth while about the true nature of the extrasensory process. The present methods can teach us very little for instance about the means by which the telepathic impulse emerges into consciousness. A greater use might be made of psycho-analytic techniques and I would suggest that the almost forgotten work of the psychologist Abramowski should be resumed on a statistical basis. Abramowski did experiments which bring out the close analogy between the realisation of an idea by a telepathic percipient and the revival of a memory. According to this psychologist unrealised memory states exist in the form of emotional equivalents. These emotional equivalents which he thought form an important constituent of the bodily co-aesthesia manifest their existence to introspection when we try to recall to memory some forgotten name. We are conscious of a certain feeling tone which seems, as it were, to contain the forgotten image at its core. But Abramowski was able to go further and compare experimentally the relative strengths of these memory equivalents by studying the different resistances which they offered to false suggestion.

Thus he would make one of his subjects read aloud in succession a list of twenty concrete names or twenty proper names, as the case might be. The subject would retain several of these words, but there would be certain words which he had definitely forgotten. Abramowski made him write down all the words he could definitely recall and then selected from his list one of the forgotten words. He would now make a false suggestion to the subject, and ask him if that was one of the forgotten words. The first suggestion would be very remote from the real word and the subject would probably refuse the suggestion. The experimenter would then proffer another suggestion nearer in association to the real word but still not the real word. The subject might refuse this word emphatically or he might feel doubtful. Various words would be suggested, each nearer to the real word, and if the resistance of the forgotten word was strong the subject would refuse every word until the true word was reached, which he would accept.

But if the resistance of the forgotten word was weak, the subject would either accept an associated word or not even recognise the real word.

Experimenting in this way Abramowski found that concrete nouns have a far stronger resistance to false suggestion than abstract nouns or numbers.

Abramowski next went on to make experiments in telepathy with forgotten words. He would first make his subject read aloud in succession a list of words consisting of concrete nouns, proper nouns, or abstract nouns, as the case might be.

The subject would note down as before the words he remembered. The experimenter would then concentrate mentally on one of the forgotten words, visualising its concrete meaning, etc. The subject would keep himself passive and if the experiment was successful he would be able to recall the word. Sometimes Abramowski made experiments by giving his subjects four or five words to choose from while he concentrated on one of them, thus anticipating the Rhine technique. It was found that the telepathy experiments were most successful with those words whose memory equivalents had the strongest resistance to false suggestion.

There was more success with concrete names than with proper names and least success in the case of numbers.

In order to show that the development of the telepathic image is introspectively similar to the development of an ordinary memory image, the experimenter made his subject read aloud a list of forty words consisting in equal numbers of concrete nouns, proper nouns, adjectives, and abstract nouns. The subject then wrote down the words remembered. Abramowski then made verbal suggestions on the forgotten words of the different categories. Sometimes it happened that when a true word was suggested the subject denied that it was in the list at all. On the other hand the subject declared that certain false words suggested were in the list. The experimenter then presented the true word that had been denied together with false words that had been accepted, and asked the subject to say which of the three words was in the list. Usually he chose one of the two false words. Without telling him whether he was correct or not the experimenter told him to shut his eyes and remain passive. Meanwhile the experimenter concentrated mentally on the true word which had been rejected. In the majority of cases the subject changed his mind saying that the rejected word now seemed to be in the list and that the false words had lost their feeling of being in the series. It happened a little later when the telepathic influence had disappeared that the subject frequently reverted to his original opinion and voted for the false words again.

It would at least be worth while to repeat Abramowski's experiments on a larger scale, and it should not be at all difficult to devise an appropriate statistical method for evaluating the results.

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PRESIDENTIAL ADDRESS

By Dr Gilbert Murray, O.M.

(Delivered at a General Meeting of the Society on 21 *May* 1952)

I observe that almost all the most impressive addresses given of late to this Society have begun with an apology. They come from highly qualified philosophers, like Professors Broad and Price, who can analyse the implications of the evidence, or at least of the current interpretations of it; or from long and exact students of the history of the S.P.R., like Mr and Mrs Salter; or from experts who have observed or conducted long series of scientifically controlled experiments. How much more humbly must a complete non-expert like me make his apologies, when attempting to discuss the whole field of Psychical Research, and in part to explain the one corner of it in which he has had some personal experience.

On his last visit to Oxford William James once said to me, in a discussion about the future of religious belief, that he thought it would be largely affected by the result of the researches of this Society. The statement made me reflect. If we take religion in a narrow doctrinal sense, there has certainly been a great liberalizing and internationalizing movement. The Pope has lately been addressing a collection of twenty-four thousand boys and girls, some Protestant and some Catholic. On committees at Geneva or Lake Success, Moslems, Jews, Hindus, Christians and Buddhists have worked together for various beneficient purposes without feeling any call to address each other as Unbelieving Dogs. But I do not think this Society played any part in that great change. If, however, we take " religion " in a very wide sense, as meaning what has been called the " inherited conglomerate " of beliefs, habits, expectations, approvals and disapprovals dominant in a given society at a given time, I think one's judgement would be rather different. There have been during the last two or three generations in England some large and surprising changes of outlook, in some of which I should judge that the S.P.R. has played a rather interesting part. We must not exaggerate. There is much truth in Andrew Bradley's statement that the supposed Victorian family is the greatest work of creative imagination that the twentieth century has produced. Nor must we forget that the " inherited conglomerate " is always to some degree in a flux, varying from old to young, from generation to generation, and of course varying widely in the same generation between the educated and uneducated. Still the changes of the last seventy or eighty years have been rather exceptionally marked.

Of course, the political, social and economic problems have greatly changed. But I doubt if Psychical Research has had any great effect on them. In science itself the advances have been almost revolutionary, especially, I suppose, in physics. I was brought up to believe that the types of certain truth were Euclid's theorems and Newton's law of gravitation. Einstein showed them both to be inadequate and in a sense unreal. What was one to believe after that? I was brought up to believe that the Earth was gradually cooling and that, like the Moon, she would become too cold to support life. Then, on the contrary, I learned that she would become intolerably hot; and later, that neither view was true, she would explode. I was told that the sun was a fixed star. Not at all; the whole universe was receding at enormous speed; that it was expanding; that it was coming into existence; and lastly, when the layman's mind was already reeling, that space itself was curved—whatever that might mean. The orthodox conglomerate was wonderfully open-minded and ready to welcome new ideas, except indeed just in the region that interests us most. There it was decidedly intolerant, would stand no nonsense. It would not listen. A belief in hypnosis, for instance, now a well-ascertained fact, was beyond the pale. The fact that various strange phenomena, which we now explain as hypnotic, were handed down in the popular tradition, told against them rather than for them to the scientific convention of the day. They seemed to be only old superstitions revived. The Viennese physician Mesmer, whose hypnotic cures had spread his fame all over Europe, was treated as a charlatan, examined in a hostile spirit and finally discredited. A generation later Elliotson, Professor of Medicine at London University, was ordered by the authorities to discontinue his hypnotic experiments, whereupon he resigned his chair. About the same time Esdaile, a surgeon in the Indian Service, performed some three hundred operations under hypnotic anaesthesia, but medical journals refused to publish his reports. In 1842 W. S. Ward in London amputated a thigh with the patient under "mesmeric trance", and reported the case to the Royal Medical and Chirurgical Society. The Society heard, but refused to listen; the patient was accused of being an imposter, and the record of any such paper having been read was struck from the minutes of the Society. It was not until nearly a hundred years after Mesmer's chief cases, when Charcot took up hypnosis at the Salpetrière, that hypnosis gained full credence and was accepted as a branch of medical practice.[1] A very similar history of intolerance could be told of faith-healing, telepathy, and various other phenomena. A great part of the whole science of psychology has developed from the systematic observation of phenomena which would have been scornfully set aside a hundred years ago as superstitions unworthy of attention.

Do not let us be unjust to this sceptical or negative attitude. We must not forget how close Europe still was to very cruel and revolting superstitions. England itself was comparatively safe. The statute prescribing the burning of witches had been repealed in 1738, though Ruth Osborn

[1] I have quoted the above almost verbally from M. Polanyi's *The Logic of Liberty* (Routledge, 1951).

was ducked as a witch and then murdered by a mob in Hertfordshire, close to London, in 1751. But in Ireland, in my own lifetime, a child, who was for some reason reputed to be a changeling, was beaten and burned with irons, the mother being locked out of the room while the invading fairy was exorcised, though unfortunately the child died in the process. A witch was burned in a village near Monte Cassino, so I am told by a friend who lived there, in 1912 or 1913. I knew an Englishman who somehow lost his memory in Italy, and was found some days after tied up in a market-place as a madman and beaten to drive out the evil spirit. In northern Greece a friend of mine found a madman tied up in a public place for anyone passing to beat as he chose. Such possibilities were near enough to have left behind them a real horror. So no wonder the men of science felt that the great need of the time was not to search sympathetically for such elements in old beliefs as might be really true, but firmly to reject the whole mass of degrading and inhuman nonsense.

None the less this excessively sceptical attitude provoked some reaction. There really are more things in the world than the science of any period can fully account for. One can see the feeling of this in many leading nineteenth-century thinkers, such as Carlyle and Ruskin, and even in J. S. Mill himself. And, of course, a much grosser kind of superstition makes a lasting appeal to human nature. The preacher who cries in Carlyle's words, " Come unto me ye who hunger and thirst to be bamboozled ", will always find a response. Among the remote and uneducated, particularly perhaps among the Celtic fringes, there was generally a survival, or sometimes a passionate resurgence, of unauthorised supernatural beliefs.

Much of the boldest and most uncritical offensive, however, came from America. The fact is patent, and I think one can see the explanation. In that great democracy the common man, however unqualified, has pretty full freedom of speech. The aristocratic tradition in England and Europe generally makes the uneducated rather timid about asserting their own ideas in public against those of the experts. The actual founder of American spiritualism is said to have been Andrew Jackson Davis of Poughkeepsie, who was thrown into genuine trances and expounded a mystical doctrine of spirit communion, called Harmonial Philosophy, in several large volumes.[1] But Spiritualism as a vigorous popular movement seems to have started chiefly from the Fox sisters. The two younger produced communications from the spirits by means of raps, and also various poltergeist phenomena. Both, it seems, were remarkably attractive young women, while the mother and eldest sister were experts in salesmanship. They had astonishing success both in America and in England, where so great a scientist as Sir William Crookes was convinced by them. Their father, however, had been a drunkard, and the girls eventually took after him. Kate wrote and withdrew confessions proclaiming that the phenomena were all a fraud—excepting, curiously enough, the actual raps, which were in some sense genuine. Margaret confirmed the confession, and the whole business ended in singular squalor. One cannot but suspect that if the arts of observation and detection had been better developed in the middle of the last century, many of the wonder-workers of the time

[1] F. Podmore's *Modern Spiritualism,* Vol. I, p. 158

such as the Foxes, Florence Cook and even the great D. D. Home himself would, like Mme Blavatsky, have had more interrupted careers.

Certainly a disproportionate number of mediums and thaumaturges and founders of new religions were American. Not many European countries can produce figures like Joseph Smith or Brigham Young, the founders of Mormonism, or Mrs Eddy, the founder of Christian Science, or even Dr Buchman, not to speak of Amy Macpherson and hundreds of persons of equally vulnerable pretensions. One popular preacher in Chicago who used to send me literature discovered that he was the prophet Elijah reincarnate, and in order to convince possible doubters, took to wearing large wings. One could quote such extravagances by the dozen. But one strange development which could not, I think, have occurred except in America was the history of the New Motor which was to save mankind. A man with an enthusiastic faith in machinery, though no great knowledge of its working, felt it to be obvious that Salvation, like everything else, could be much more effectively produced by machine power than by human labour. Mesmeric, or as we should say hypnotic, influences were then called "animal magnetism", and, since the earth was a great magnet, there was obviously a tremendous store of mesmeric force there ready to be tapped. By the guidance of a series of dreams this man succeeded in building a motor which was to concentrate in itself the magnetic force of the earth and so manufacture Salvation on a world-wide scale. With the help of subscriptions it was made and set up, but somehow would not work. Various expedients were tried. The Faithful gathered round it in prayer, but in vain. Presently a leading spiritualist was called in to advise. He decided that the dreams were genuine and came from the spirit world; but that any engineer would see that the machine could not move without breaking itself, and perhaps the spirits had wished to try or merely to tease the inventor. Meantime, however, a woman in the south, several hundred miles away, had it revealed to her that what the machine wanted was a mother, and that she was called upon to assume that sublime office. She came to see the inventor, who reports with obvious good faith: "I did not quite understand what she wanted, but I gave her the key of the shed." But even when mothered it would not move. At last someone suggested that it was in much too high a position. It was not near enough to the magnetic centre of the earth. So some hundred or so of the Faithful harnessed themselves to it and dragged it over miles of farmland to the bottom of a river valley. There, no doubt, it might have performed better, but the ignorant farmers of the neighbourhood broke it up and threw it into the river. The story is told in detail in Mr Podmore's book, *The New Motor*. Fashions change, but the interest in the supernormal continues. A book published in 1946 reports that there were then twenty-five thousand practising astrologers in the United States.

We must not forget the English newspapers which employ a regular astrologer, nor yet the temporary vogue among more intellectual circles of Mme Blavatsky and her brand of theosophists. My point is to illustrate the great mass of utterly unacceptable material which lay before the scientists of the mid-nineteenth century. First there was a vast respectable tradition of miracles and wonders, much of it supported by religious doctrine ; next, a large but indefinite remnant of primitive superstition among the uneducated ; thirdly, the constantly recurring interest, I had almost said the craving, among educated and uneducated alike, to discover, or hope to have discovered, some certainty behind the veil. It is also worth remembering that, overwhelmingly strong as the tradition is of the existence of prophets and *shamans* with superhuman powers, it is always accompanied by a suspicion of possible fraud or false pretensions. And the same suspicion accompanies the modern evidence. Mrs Salter records that in her childhood she saw something of the famous medium Eusapia Palladino, and though she did not attend any of the actual séances, remembers that in unprofessional moments Eusapia cheated at every game she played.

We can understand the indignant phrase of my old colleague Lord Kelvin, that all the phenomena were " half fraud and half bad observation ", as well as the wiser conclusion of Professor Sidgwick that, in the face of such a bewildering mass of remarkable and ill-attested phenomena, it was " a sheer scandal " that they should be left with no serious attempt to find out what parts of them, if any, were true. That, of course, was the purpose with which this Society was founded, and the quest on which it has been engaged for over seventy years.

What have we actually discovered? It is hard to say. Hypnosis is now accepted as a *vera causa* in medical science. Much akin to hypnosis are various forms of psychotherapy recognised and practised in hospitals. Going a step further, I think we are bound to admit the fact of actual faith-healing. The evidence from Lourdes and other Christian shrines is very strong, and is confirmed by similar or even more abundant evidence from Hindu shrines ; nor should we forget the successes of Christian Science. The actual limits of faith-healing must be left for medical science to determine. The fact of immediate relief is certain ; wounded men in great pain calling for morphia have fallen peacefully asleep on receiving an injection of pure water. Continuous relief in chronic cases seems certain, and by the relief of anxiety and a consequent lessening of the flow of blood to the affected part, has sometimes been hard to distinguish from actual cure. I have known one case of this in my own family. We need not therefore be quite as puzzled as the seventeenth century Dean of Wells in Aubrey's *Miscellanies*, who writes : " the curing of the King's evil by the touch of the King doeth puzzle my philosophie ; for, whether they were of the house of York or of Lancaster, it did." Similarly, when Freud released many patients from dangerous repressions by getting them to remember some forgotten incident of their childhood, in some cases it turned out that the incident had never really taken place. It was just imaginary. But the cure worked. Then, again, every anthropologist will remind us that Faith can kill as well as cure ; there are well-proven cases from New Zealand and the Pacific Islands of people who have died because they believed they had been touched by a magician or highly tabu chief. My brother in Papua saw a man give another a handful of pebbles. " What are these? " said the man. " They are sent specially to you by so-and-so," was the answer, mentioning the name of a well-known sorcerer. " Oh, then I am done for," said the victim, and died that night.

Of all the problems that faced the S.P.R. on its foundation, the first and greatest, I suppose, was the problem so confidently answered by the Spiritualists: do we in some sense survive our bodily death, and is there communication between the living and the spirits of the dead? The subject is too large and important to be treated in passing. We may note Mrs Sidgwick's conclusion, reached after considerable study, that the evidence of survival did not amount to proof, but was enough to justify personal belief.

Members of the Society will remember the two attempts that were made to obtain a wide Census of Hallucinations: it came out in both that roughly ten to eleven per cent. of those questioned had had hallucinations, and of the hallucinations about ten per cent. appeared to be veridical. A striking attempt was made by Mr Podmore to show that all such phantasms were phantasms of the living, not of the dead. This would apply to one striking case of which I had some knowledge, and which is in one point very remarkable. The phantasm of an intimate friend of mine appeared early one morning on September 3rd or 4th 1898, to a lady he knew in London. She felt, as usual in such cases, no particular surprise, and reported that he was smiling and said, "I am going on on the 11th." It was just after the Battle of Omdurman. My friend survived the battle, but was killed the day after. The phrase "on the 11th" had no ascertainable meaning; what he really did was to "go on *with* the 11th", that is, the 11th Lancers, a regiment to which he did not belong. Must he not really have said, "I am going on *with* the 11th", and been mis-heard? If so, it would seem to follow that the phantom was not merely a creation of the percipient's mind, but was carrying a real message. It is, of course, extremely difficult in many cases like these to prove either a positive or a negative. But my own impression is that most of the commonly reported wonders, both traditional and new-fangled, have so often been proved to be either misreported or misobserved or sometimes simply fraudulent, that they must be regarded, to say the least of it, with extreme suspicion; I would include in this category spirit photographs, haunted houses, extensions of the human body and the great majority of poltergeists.

A new standard of strictly scientific observation of these supernormal phenomena is evidently a great desideratum. And for one class of them such a standard has been successfully set by Dr Soal in his statistically controlled experiments with Mr Shackleton and Mrs Gloria Stewart, and on a greater and more elaborate scale by Prof. Rhine and his staff at the Duke University, North Carolina, where there is a special Institute of Parapsychology with fifteen rooms and a staff of six to eight whole-time parapsychologists.[1] I cannot criticise the work of the Institute, except to say that the good faith of the workers seems undoubted, and the accuracy of the methods well attested, yet the statistical results reported are—to me at least—quite incredible. In the field of Precognition especially there are parts as to which I find myself belonging to the class described by Dr Soal as "hopelessly prejudiced by some outmoded philosophy which they probably imbibed in their youth and which they are too old to abandon". I have always held, in accord with all my scientific friends,

[1] See the description by Dr West in the *Journal*, XXXV, Pt. 656, Jan.–Feb. 1950.

and with that admittedly dangerous guide, Common Sense, that the cause which produces an effect must come before the effect; the cause precedes, the effect follows. I know, of course, that Time is called a Fourth Dimension; and I fully recognise that the exact placing of any event requires three dimensions in space and one in time. But surely there is nothing magical in that. It is not the sort of fourth dimension which would enable us, for instance, to see and touch the inside of a solid. I quite see that our whole conception of events in the universe must be conditioned by the limitations imposed on our minds by our bodily structure; that the whole world would be exceedingly different to us if we could really

> "know, hear and say
> What this tumultuous body now denies,
> Feel, who have put our groping hands away,
> And see, no longer blinded by our eyes."

What repels me is the supposition that sometimes, some few of these limited minds should, for no ascertainable reason, completely overcome the limitations of human reason in one small point, while leaving all others unchanged. If I go with Alice into a Looking-glass world I shall expect to find that left is right, that people cry because they are going to be hurt, and pick themselves up because they are going to fall down. But I should at least expect some consistency. I look for some other explanation. I may add that I have read Mr Dunne's book twice in the hope of being convinced, but have not been. I think that the reasoners who do magic with a fourth dimension are like those who draw conclusions about the real world from the use of surds in mathematical formulae. I am, of course, ready to accept mathematical calculations which make use of the square root of -1, or the convention that A to the power of 0 equals unity, but I do not believe that the convention is more than a convention or that I shall ever meet the square root of -1 in real life. Consequently I feel enormous difficulty in accepting some of the statistical phenomena of precognition which, I confess, I am unable to explain otherwise. My old-fashioned mind notes with much comfort that Dr Soal himself feels doubts about precognition by pure clairvoyance with no help from telepathy.

What we are told is that in Prof. Rhine's experiments the percipient, while trying to guess the card that is dealt, happens, to a significant degree of frequency, to hit by mistake not that card but another card which has not been dealt, but is going to be dealt a few seconds later, which he is not trying to guess, and which is at the time not known to the dealer or any other human being. It is possibly made a little more plausible when we find that the guesses which do not hit their real mark are chiefly apt to hit the card next before or next after. This seems like normal shooting, a few shots hitting the bull, but more going just to the right or just to the left, but it would still involve some, to me, incredible hypotheses. I look hopefully towards the metaphysicians, such as Professors Broad and Price, to reveal some explanation which does not involve one of two incredible hypotheses: one that a man's naming of a card in an erroneous guess at another card should make a card of the sort named in another room make its way unobserved out of a pack and get itself dealt; the other, that the dealing of a card at a later time should cause a right guess to have been made some time earlier. To Dr Soal it would make all the difference if at the later time the dealer should see the card ; then it would at least not be the dead material card itself that caused the guess to have been made, it would be the thought which the dealer did not have at the time but was presently going to have which had influenced the mind of the guesser. Both views accept the conclusion which I find inacceptable, that an effect can precede its cause. I cannot feel much comforted by the explanation that the difference in time is only a matter of seconds and might be covered by that enduring moment which we commonly call " now " or " the present ". However, I know I may be wrong.

I feel on different grounds a similar incredulity about Dr Rhine's startling cases of telekinesis. Considering the vast experience of the human race in tossing dice and coins and the extreme interest which millions of gamblers have taken for hundreds of years in the way they fall, if human thought or will could really compel a die or a coin to fall in the position the agent wishes, I think we should have heard much more about it by now. Here, again, I note with relief that Dr Soal, too, is sceptical about telekinesis, and " sees no sign of a genuine physical medium on the horizon ".

As to telepathy, however, I cannot maintain this healthy scepticism. There are three numbers of the *Proceedings* which would confound me if I did. They contain accounts and criticisms of my own experiences as a percipient ; my Presidential Address in 1915, Mrs Verrall's " Report of a series of experiments in Guessing " (1916), and Mrs Sidgwick's " Report on Further Experiments in Thought Transference " in 1924. I have also several bundles of records of later sessions, though of late years, owing partly to the complete dispersal of my children and the rest of our old group, I have given up the experiments.

Let me say at once that my experiments belong to the pre-statistical stage of psychical research, when the experiments were treated almost as a parlour game. Still I do not see how there can have been any significant failure of control ; nor did Mrs Verrall or Mrs Sidgwick. The conditions which suited me best were in many ways much the same as those which professional mediums have sometimes insisted upon. This is suspicious, yet fraud, I think, is out of the question ; however slippery the behaviour of my sub-conscious, too many respectable people would have had to be its accomplices. I liked the general atmosphere to be friendly and familiar ; any feeling of ill-temper or hostility was apt to spoil an experiment. Noises or interruption had a bad effect. One question that arose was the degree to which the telepathy made use of real sights, sounds, smells, memories, to reach its goal. The general conclusion was curious. It seemed that I, or my sub-conscious, showed some anxiety to explain away the telepathy by seizing upon some such excuse. It said it had guessed Savonarola making the women burn their precious possessions because it smelt a coal which had fallen out of the fire ; that it had guessed Sir A. Zimmern riding on a beach in Greece because it said it had heard a horse on the road—when the rest of the company heard no horse. Memories, again, sometimes helped it, but more often hindered it in its search. At one time, indeed, I was inclined to attribute the whole thing to subconscious auditory hyperaesthesia. I got almost no successes if the subject was not spoken, but only written down. Two or three successes and at least one error could be explained by my having heard or mis-heard a proper name, e.g. by confusing Judge Davies and the prophet David. But, apart from other difficulties in this hypothesis, there were some clear cases where I got a point or even a whole subject which had only been thought and not spoken.

Of course, the personal impression of the percipient himself is by no means conclusive evidence, but I do feel there is one almost universal quality in these guesses of mine which does suit telepathy and does not suit any other explanation. They always begin with a vague emotional quality or atmosphere : " This is horrible, this is grotesque, this is full of anxiety " ; or rarely, " This is something delightful " ; or sometimes, " This is out of a book," " this is a Russian novel ", or the like. That seems like a direct impression of some human mind. Even in the failures this feeling of atmosphere often gets through. That is, it was not so much an act of cognition, or a piece of information that was transferred to me, but rather a feeling or an emotion ; and it is notable that I never had any success in guessing mere cards or numbers, or any subject that was not in some way interesting or amusing.

Let us consider what we mean by telepathy. I believe most of us in this Society are inclined to agree with Bergson that it is probably a common unnoticed phenomenon in ordinary life, especially between intimates. We all know how often two friends get the same thought at the same moment. Tolstoy, the most acute of observers, speaks of " the instinctive feeling with which one human being guesses another's thoughts, and which serves as the guiding thread of conversation." [1] Most teachers will agree that one of the marks of a good teacher is the degree of telepathy he can stir in his pupils. The same thought explains why a lecture or course of lectures, if good, can be more effective than the reading of a textbook, though the textbook almost always contains more information. And what about the impression people receive from the shared enjoyment of drama, poetry, music, or even, I think, some of the more imaginative branches of philosophy? Is there not some telepathy, some shared sensitivity, at work—not very different from that which a dog feels when he shares the trouble or anxiety of his master? [2] And shall I be wrong in suggesting that it is just in these cases that our main instrument, language, rather fails us and, like the dog, we have to appeal to something less perfectly articulate?

The point will be clearer if I take some typical examples of my own experiences, both successful and unsuccessful. I choose them from the unpublished bundles of which I spoke, which are later than Mrs Sidgwick's collection.

The method was always the same. I was sent out of the drawing-room either to the dining-room or to the end of the hall, the door or doors, of course, being shut. The others remained in the drawing-room : someone chose a subject, which was hastily written down, word for word. Then

[1] *Childhood and Youth*, p. 141. In another place he says of Nekhludoff that, " when a chord was struck in his mind, a chord in mine vibrated ".

[2] This sympathy is not, I think, entirely explained by the excitation of the man's adrenalin glands which is smelt by the dog : the smell of adrenalin seems generally to irritate or annoy a dog.

I was called in, and my words written down. I may add that, out of the first 505 cases, Mrs Verrall estimated the percentage as: Success, 33 per cent.; Partial Success, 27·9 per cent.; Failure, 39 per cent. But it may be remarked that as evidence for the presence of some degree of telepathy most of the partial successes are quite as convincing as the complete successes: this would produce something like 60 per cent. evidential and 40 per cent. non-evidential.

First, two perfectly ordinary cases, where the emotional atmosphere is obvious and strong, and then is developed into something more definite.

October 26 1924 (?).

My wife gave a subject:

M.H.M. " This is not a nice thing. What Nansen was describing the other day of the church yard at Buzuluk, where there lay the great pile of corpses, numbers of children who had fallen dead in the night."

I was summoned, and said:

G.M. " This is perfectly horrible. It's the Russian famine. It is the masses and masses of bodies carted up every night in the Church yard at . . . " (The scribe did not catch the name.)

M.H.M. " Any particular bodies? "

G.M. " Oh yes, children. I associate it with Nansen's lecture here."

Here came memory in as a help. The subject was an incident that I remembered. In the next it was an obstacle: that is, a remembered incident thrust itself in and had to be rejected before I could get the real subject. I should explain that my mother had a story that when she was at a school in France, she had been made to wear a placard labelled " *impie* ".

November 24 1929.

MRS DAVIES. " Jane Eyre at school standing on a stool, being called a liar by Mr Brocklehurst. The school spread out below her and the Brocklehurst family 'a mass of shot purple silk pelisses and orange feathers'."

G.M. " . . . (I think of) my mother being at her French school, being labelled '*impie*'. . . . I reject that. But a sense of obloquy. Girl standing up on a form in a school, and the school there, and people coming in, and she is being held up to obloquy in some way or other.—A thing in a book certainly. I think they are calling her a liar. I get an impression of the one girl standing up and a group of people or a family coming in and denouncing her. I think it's English."

Question. " Colour of the people's dresses? "

G.M. " I can't get the colour of the people's dresses."

I take another with a very marked but extremely different atmosphere.

January 22 1928.

STEPHEN MURRAY. " George Rickey and me riding the motor-bike past the inhabitants of Moulsford Lunatic Asylum, and one cheery-looking man with gold spectacles on his forehead barking furiously at us, like a dog."

G.M. " A curiously confused and ridiculous scene. You and someone on a motor bicycle, and a scene of great confusion ; . . . perhaps the bicycle is broken down. But there is a confused rabble and, I know it sounds ridiculous, but someone on all-fours barking like a dog." (Then after a little encouragement) " Are they lunatics by any chance? "

Then two where the atmosphere is fainter and more subtle. The first came on a bad evening after two or three failures, and I was inclined to give up.

MY DAUGHTER ROSALIND. " I think of dancing with the Head of the Dutch Foreign Office at a *café chantant* at the Hague."

G.M. " A faint impression of your journey abroad. I should say something official ; sort of official soirée or dancing or something. Feel as if it was in Holland."

The second occured on May 14 1927.

R.M. " I think of walking in the Park at Belgrade and meeting the English governess."

G.M. " I'm getting a different feeling. It's somebody who is in rather a state of mind. I should think escaped from Russia. You are meeting her in some curious country. Wait a bit! It's not anyone at Robert College or Constantinople College. It's some queer country where you seem to be alone, and you are meeting some sort of Englishwoman who has been driven out of Russia, and hates the place where she is. . . . Oh yes. I do remember. It's when you went out to Constantinople by the express alone, and met the English governess in the Park."

The history and " state of mind " of the English governess was correct, but had not been mentioned. I had some faint memory of the incident. The " queer country " was Serbia.

Next I will take two cases where I received a feeling or thought that had not been spoken, and was not in my memory at all.

November 17 1924.

R.M. " A scene in a book by Aksakoff, where the children are being taken to their grandparents, and the little boy sees his mother kneeling beside the sofa where his father is lying, lamenting at having to leave them."

G.M. " I should say this was Russian. I think it's a book I haven't read. Somebody's remembrance of childhood or something. A family travelling, the children, father and mother. I should think they are going across the Volga. I don't think I can get it more accurately. The children are watching their parents or seeing something about their parents. . . . I should think Aksakoff. They are going to see their grandmother."

Note. They did just afterwards have to cross the Volga, and Rosalind said she had been thinking of that, though she did not mention it.

Much more curious is the next, though at first sight it is a mere failure.

May 15 1927.

EDITH WEBSTER. " I think of the Castalian spring at Delphi and how we drank the water there."

G.M. I don't think I shall get it. But I've got a slight feeling of atmosphere, as if there were something terrible going to happen ; as if it were the night before something . . . an atmosphere of suspense."

Note. R.M. commented : " I had been thinking of saying goodbye to someone who was going (to the war) to be killed, Hugo? Rupert? I got the feeling of ' This is the end '."

R.M. had not spoken. She had evidently intended or expected to give the next subject, but E.W. was asked instead.

I add another failure which is, I think, equally significant.

November 24 1929.

MARGARET DAVIES. " Medici chapel and tombs : sudden chill : absolute stillness. Marble figures who seem to have been there all night."

G.M. " I wonder if this is right . . . I've got a feeling of a scene in my *Nefrekepta*, where the man goes in, passage after passage, to the inner chamber where Nefrekepta is lying dead with the shadows of his wife and child sitting beside him . . . but I think it's Indian."

(My poem was translated from an Egyptian story ; I suppose I felt the subject was not Egyptian.)

Sometimes the subject was a bit of poetry : I was then apt to answer at once without any groping or hesitation.

January 22 1928.

MARGARET COLE. " The man in Browning who is dying and sees the row of bottles at the bed, and it reminds him of where he met his girl when he was young."

G.M. (Instantly on entrance.)

" How sad and mad and bad it was,
But oh, how it was sweet."

JOHN ALLEN. " I think of the priest walking by the shore of the sea after he had been to Agamemnon and been refused."

G.M. *Βῆ δ' ἀκέων παρὰ θῖνα πολυφλοίσβοιο θαλάσσης*

[*Iliad*, I. 34.]

Now, granted that this curious sensitivity which we call telepathy exists, how shall we best analyse or describe it? In the first place, as far as my own experience goes, it does not quite feel like cognition or detection ; it is more like the original sense of the word " sympathy ", *συμπαθεία* the sharing of a feeling, or " co-sensitivity ". I seem to be passive, and feel in a faint shadowy way the feeling or state of mind of someone else. Tolstoy's metaphor of the chord which vibrates when another chord is struck seems to express it.

If we follow the general lines suggested by Bergson we may suppose an original store, so to speak, of vague undifferentiated sensitivity belonging to all gregarious creatures, which is then " canalised " into particular clearer and more efficient forms as the creature develops definite sense organs. These senses again become keener and more effective as they are needed and used, but fade away if they are not used, while some remnant of the original weak uncanalised sensitivity is still there to be drawn upon.

I have tried to get some guidance from books on the intelligence of animals. But the books are unfortunately all written by human beings, who keep testing their examinees by the intelligence tests to which they themselves are accustomed—reasoning, inference, speed in learning and the like. No wonder the poor victims cut rather a miserable figure. I wonder what sort of show we should make if dogs tested us for our powers of finding our way or following a trail, or if spiders criticised our power of symmetrical web-weaving. Mr Romanes, for example, told his dog, a setter, to follow him. He then set off in Indian file, with eleven men following his footsteps, his gamekeeper, a special friend of the dog, coming last. After two hundred yards, he turned off to the right with five men, the other six going to the left. The dog was then let loose, and, by smell or otherwise, made out his master's track with eleven other footprints over it. Among many much stronger sense impressions it discerned unfailingly the one that it needed, that it craved. We civilised white men are hardly at all dependent on our sense of smell or our power of following a trail, and are consequently very helpless in such matters. The Australian blacks, however, are almost as clever as dogs. " The Arunta ", I read, " have extraordinary aptitudes for all that pertains to the quest of food. Their skill in following a trail is wonderful. Not only does a black know the tracks of all animals and birds, but when he has examined a burrow he can say at once from the direction of the latest tracks, or even by smelling the earth at the entrance of the burrow, whether the animal is at home or not. He will also know the footprints of all his acquaintance." [1]

Take again the sense of direction. A bat can find its way in the dark, flying fast this way and that among obstacles without hitting them. One bat at least still found its way when some scientific inquirer put out its eyes. Good authorities, such as Bethe and Fabre, consider that a " special sense of direction, not dependent on sight ", exists in bees and wasps.[2] We all know how a cat or dog taken a hundred miles away in a train often finds its way back to the home it has left. There must surely be some element of the same faculty in homing pigeons. That power has fallen into disuse with civilised man. But Lapps, Finns, Eskimo and Red Indians seem to have it. I have read of a Lapp setting forth confidently over a trackless and treeless country covered with snow, to meet another Lapp who was coming to see him at a spot about 150 miles off. He knew exactly the direction in which to go, and was sure of keeping it. Even more surprising is a record quoted by Galton from the *Journal of Captain*

[1] Sommerfelt, *La Langue et la Société*, p. 80 ; based on Spencer and Giller, and Strehlow.

[2] Washburn, *The Animal Mind*, p. 26

Hall, published in 1879 by the U.S. Government. An Eskimo who had been travelling for several months, covering a distance of more than 1100 miles, was asked to draw a map to show where he had been; he drew a map of his journey stage by stage, which is described as rather more exact than the Admiralty Chart of 1870.[1] There are probably here also a few special sensitives among us; but on the whole the faculty has been lost by disuse. In hearing, again, the animals that are habitually hunted by beasts of prey have generally developed great acuteness of hearing, at least for the sounds that may mean danger. So have the Red Indians, who in the midst of much irrelevant noise from wind and water, will instantly detect the crack of a small twig that may mean an enemy's footstep. One may wonder why many small animals—but not as a rule large ones—can hear the cries of a bat and other sounds which are too high for the human ear. Galton tells us how he once walked out in London with an instrument producing strong super-auditory sounds. No one took any notice except cats and small dogs, who became highly excited. Some creatures, again, are sensitive to ultra-violet lights which the human eye cannot see; and some low forms of life without eyes are nevertheless affected by light, and seem to have a vague uncanalised sensitiveness in their bodies, to such things as magnetic currents and balance. This is not, of course, telepathy, but it seems to show the reality of an undifferentiated sensitivity capable of taking different forms.

And I must say it is very difficult not to see telepathy in the action of flocks of birds and of many gregarious animals. We have all seen a flock of rooks or of field fares start suddenly and simultaneously in flight. There is a similar collective and sudden movement in migrations, though no doubt there are external reasons of date or of weather which have contributed to the joint decisions. There is a wonderful description by Kropotkin of a Siberian lake, crowds of ducks on the water and big birds of prey watching hungrily all round, but never daring to touch the flock because it all defends itself as one unit. Stampedes of cattle, buffaloes, zebras, and other gregarious animals seem to be the result of a sudden emotion sweeping through the herd, not of detailed individual obedience to an order or imitation of a leader. A panic in a human army seems to be much the same, an inrush of undefined and unexplained terror communicating itself telepathically from man to man. And surely the so-called language of animals is just this contagious emotion. When a dog invites another dog to come off after rabbits, when a bee by means of its curious dance urges another bee to fly off for honey in a particular direction, when an ant by a well-known movement of its antennae explains that its load is too heavy and that it needs help, we cannot suppose that there is any mention of rabbits or honey or loads; a cry of emotion, an expression of feeling in a particular situation is enough. The feeling is infectious and is shared. This faculty is needed by the animals just because they have no language proper; man, possessing that vastly superior means of exact communication, has not the same urgent need for this faculty, but seems to share in it to some extent.

These are only conjectures, and the conjectures of one who has little claim to any special knowledge of psychology or philosophy, or any

[1] Galton, *op. cit.*, p. 72.

knowledge at all of zoology. They represent merely an attempt to find a place in the coherent world that we know for these strange powers and qualities of the human mind. The points on which I speak with some conviction are, first, that telepathic communication does take place, and secondly that as far as my own experience goes it seems to me to be a communication of feeling rather than of cognition, though the cognition may follow as the feeling is interpreted. To these I add the conjecture that our ordinary everyday telepathy may be a faded and greatly intellectualised form of a sensitivity which exists much more simply and widely among many birds and gregarious animals and primitive races of men.

But there may be more than that in it. The differences between the human and non-human are very great. Our whole range of sensitivity has been so widely increased by our possession of such tools as hands and language. We cannot see like a hawk or track like a dog or hear like a hunted deer ; but we can see a Rembrandt picture and feel the thrill of a Beethoven sonata or a great poem. And surely it is noteworthy that just here our sensitivity passes beyond the realm of mere observation into that of feeling ; beyond the facts that you observe there is the sense of other things, not fully known, which have value and importance. I have already noticed that our faculty of telepathy, such as it is, seems to operate best in just those spheres where our normal instrument, language, either fails or works with difficulty. It is certain, I suppose, that there still are more things in heaven and earth than are at present mastered by science. And Bergson has reminded us that millions of men have lived for thousands of years in a world vibrating with electricity, without ever suspecting that there was such a thing. Are we not probably now in the presence and under the influence of unknown forces, forces concerned with deeper or more remote values or beauties or loyalties, which are beyond the range of our exact knowledge and power of definition, but by no means beyond the reach of an undefined but strong and even passionate feeling : " This is what I value," " This is what I love," " This is what I must obey"; or negatively, " This is what I reject." I suspect that what we call genius is a special sensitiveness in this region of art, poetry, thought and the like : a sensitiveness which according to many critics is apt to be deadened and disregarded by our all-absorbing material civilisation, and if so, is disregarded at our peril. It is in that region that our great tool, language, fails us and we have most highly developed our ancient pre-linguistic or supra-linguistic sympathy. If this is so, it may well be that William James was right in his forecast that the work of this Society may ultimately render great service to the religious gropings of the human mind.

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For the sake of brevity such qualifications as 'supposed', 'alleged', etc., are omitted from this index. It must, however, be understood that this omission is made solely for brevity, and does not imply any assertion that the subject-matter of any entry is in fact real or genuine.