PROCEEDINGS

OF THE

Society for Psychical Research

VOLUME XLVI

(CONTAINING PARTS 161-165)

1940-1941

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Agents for America: THE F. W. FAXON CO.,
83 Francis Street, Boston, Mass,

PRINTED IN GREAT BRITAIN BY ROBERT MACLEHOSE AND CO. LTD,
THE UNIVERSITY PRESS, GLASGOW

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For the sake of brevity such qualifications as "supposed", "alleged", etc., are generally omitted from this index. It must, however, be understood that this omission is solely for the sake of brevity, and does not imply any assertion that the subject-matter of any entry is in fact real or genuine.

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// PROCEEDINGS

OF THE

Society for Psychical Research

PART 161

PSYCHICAL RESEARCH AND THEOLOGY[1]

BY

W. R. Matthews, D.D., Dean of St. Paul's

The subject which I propose to discuss in this lecture is a large one and I must apologise at the beginning for the superficiality of what I have to say on many topics which need prolonged and profound discussion. My excuse is that I believe it may be useful to take a rapid survey of a territory which has been but little explored and try to gain some conception of the work which needs to be done. One limitation I must insist upon in order to avoid misunderstanding. My title is " Psychical Research and Theology "—not Religion. Though it is impossible to draw a clear line of distinction between religion and theology, or indeed between theory and practice in general, in a rough way we can distinguish the experience of religion from the intellectual activity of the formulation of religious doctrines and concepts. In this lecture I shall have nothing to say, either in criticism or approval, of Spiritualism in so far as that is a religion and I shall confine myself to Christian theology, partly because it is the only one about which I am competent to speak and partly because it is the only one with which psychical research in this country comes into contact.

The attitude of the Christian Church to psychical research in the past has been, on the whole, one of antagonism, or at least of suspicion. This has not been, in the main, due to scepticism concerning the phenomena which are the subject-matter of the enquiry,

[1] This lecture, being the Sixth Myers Memorial Lecture, was delivered at a General Meeting of the Society on 17 January 1940.

but rather to a conviction that they were real and that they came from a source only too well known. Though at least one of the Hebrew prophets regarded the idols of the heathen as simply illusions and some of the Christian Fathers held the same view, the general opinion has been that abnormal and supernatural phenomena could be produced by evil spirits and that the pagan altars were erected to devils not too deeply disguised. Only in the eighteenth and nineteenth centuries did any considerable number of Christians adopt a sceptical position with regard to the possibility of witchcraft and commerce with the devil.

In recent times the reluctance of Christians to see any good in psychical research has, I suppose, arisen from two different causes. On the one hand, there remained the traditional feeling that such investigations were dangerous and partook of the nature of communicating with " familiar spirits " ; on the other hand, there was the attitude of disdain commonly adopted by the scientific world towards psychical research. The two orthodoxies, religious and scientific, so different in other respects, concurred in regarding psychical research as disreputable. It would, of course, be both inaccurate and ungrateful to forget that this general attitude of suspicion has had notable exceptions. Christian divines have contributed to the study of paranormal phenomena and that eminent Victorian ecclesiastic, Bishop Boyd Carpenter, held the presidency of our Society. In very recent years a notable change may be observed. The spread of interest in the subject, sometimes in questionable forms, led the Archbishops to appoint a Commission to consider the question of the relation of psychical research and of spiritualism to the Christian faith. This Commission reported some time ago and the delay in the publication of its findings suggests that they may have found their final resting place in the archiepiscopal pigeon holes, if not in the archiepiscopal mind. In these circumstances I feel myself at liberty to disclose only two facts about the work of that Commission. No one will be surprised to hear that strong difference of opinion was manifested in the Commission. That might have been predicted. What was perhaps unexpected by many members of the Commission was the evidence that a number of people had found in psychical research a confirmation of their Christian faith and even a way from agnosticism to belief. This has received a very limited support from Professor Broad who, in a recent essay on the future of Christianity, mainly pessimistic in tone, expresses surprise that theologians do not pay more attention to the implications of the results of psychical research. In attempting to contribute something to this enterprise I feel that I shall be acting in the spirit of the great man whose name this lecture commemorates, for Frederic Myers never concealed the fact that he hoped to gain from psychical research new light on the mystery of man's life and destiny.

The area in which there seems to be some contact between theology and psychical research is far wider than the one question which occupies the horizon of many—that of survival of bodily death. The problem of survival is, in some respects, the most important and the most controversial and I propose to leave it to the end, first making some remarks upon some topics which are, from the theological point of view, prior to that of personal immortality.

The scope of theology in modern times is larger than is suspected by those who have not studied the subject. The growth of scientific knowledge has presented it with new problems and new tasks. Among these is that of the nature and development of religion in general. For theologians of an earlier time these problems scarcely existed, or, if they were admitted at all, seemed capable of finding a summary solution. The development of the study of Anthropology and Comparative Religions has changed the situation profoundly. There are still Christian theologians who think it not only possible but essential to treat Christianity as a revelation so distinctive and unique that it has no significant relations with other religions, and of late, under the influence of Karl Barth, they have become both numerous and influential; but, on the whole, it remains true that the modern theologian, while holding that in Christianity is contained a unique revelation, recognises that it has a place in the general development of the religious experience and of religious ideas.

A great deal of importance has been attached to the definition of religion and the fact that among the hundred or more suggested definitions not one has secured general acceptance does not indicate that the whole discussion has been futile. There would be no great value in possessing a logical definition of religion; a formula which would cover the beliefs and experience of all religious persons from the fetish worshipper to the philosopher-saint would tell us little about the significance of religion. The real interest in the discussion is the enquiry into the nature and significance of religion as an aspect of the continuing and developing life of humanity, to decide how it can be distinguished from magic and, at a later stage of its evolution, how it differs from and how it is related to other modes of spiritual activity, such as morality and art.

In my opinion this question of the nature of religion and its status as a "moment" in the life of spirit cannot be decided by any empirical method, but is one of the central problems of metaphysics. Nevertheless, within limits, assistance has been gained from psychology—though not I think so much assistance as some psychologists would allege. It is difficult to say whether psychical research can help us much in this department of theology, but there is one special aspect of the general problem of the nature of religion where it is conceivable some important contribution might come from researches such as this Society is concerned with. It has been held by some distinguished writers that mysticism is the essence of religion or, alternatively, that in the mystical experience we have religion in its most concentrated form. Others have held, on the contrary, that mysticism is a form of religious experience which is doubtfully Christian and certainly not the highest. It is a well-known fact that some of the physical phenomena related on good authority as happening in connection with saints of the mystical type have also occurred in connection with mediums—this at least is alleged on equally good authority. The most notable example is levitation. This obviously suggests some relation between the mystical and the mediumistic trance. But this remark does not take us very far, for clearly the two types of experience might have this characteristic in common and yet be, in all important respects, quite different. It appears to me that many writers on religion assume too readily that mysticism, as such, is good and that, when once we have described a man as a mystic, we are absolved from the duty enjoined on us by the Apostle to "try the spirits whether they be of God." I am inclined to think that there are mystical states which are morally and spiritually either indifferent or evil. There are persons who have what we can only call "spiritual power", who are apparently in contact with some source of energy which reinforces their natural endowments and gives them a unification of purpose which makes them most formidable, but these persons are often evil and their mystical experience is a heightening of their will and their capacity for destruction. In other words, I believe that there is meaning in the word "demonic". We should not have to look very far for an example of a mystic of this type or for the evidence of his power for evil. It occurs to me that some light on this question might be gained by a study of the effect of mediumship on the personality and character of the medium. Probably something has already been done on these lines, but if so I am not acquainted with it and there are obvious reasons why a publication of results would be difficult. I should expect to find that, in cases where there was already a formed character, that character would be enhanced by the mediumistic experience.

There is another element in the problem of religion—its development. The theologian of the liberal type has generally held that the history of religion, in spite of its very questionable pages, has been, on the whole, a progress in the apprehension of Reality. He has contended that the crudest creed had some scintilla of truth and that the development of the idea of God, culminating in the Christian revelation, was the evolution not of an illusion but of a concept which is, in Leibniz's phrase, *bene fundatum.* The theologian has not found it an easy task to interpret the story of religion as one which has a denouement, though I do not think he has been defending a hopeless cause even on the assumption that the commonly accepted presuppositions of Anthropology are correct. He may at least claim the support of Dr Whitehead, who has said that religion is the one sphere of human activity in which progress is undeniable. But there has been one formidable difficulty. Religion has been associated with beliefs and practices which, from the standpoint of modern scientific common sense, are illusions and superstitions. It has believed that it had experiences which in fact it could not have had. The question is obvious : Where so much is admittedly superstition and error may we not infer that the central belief, that in God, and all the experience associated with it are infected with the same illusiveness? The inference is natural enough, though illogical. There would be a gain, if not from the standpoint of pure reason at least for the imagination, if we could push back the frontiers of mere phantasy in the history of religion.

Professor Price in his Presidential Address expressed the hope that anthropologists would learn something of psychical research and psychical researchers something of Anthropology. It is indeed much to be wished. Scientific research on the history of religion has taken for granted that nothing can happen which is undreamed of in Herbert Spencer's philosophy. It is often said that spiritualism is the revival of ancient superstitions and there is obvious truth in the remark, but it may be that the revival is due not only to the persistent needs of the human mind and its readiness to be deceived, but also to the fact that there was some basis in genuine experience for these ancient beliefs. Certainly the phenomena of the medium's trance, clairvoyance, and haunting, which have been the subject of careful study, seem to be analogous to what we hear of oracles and of the sacred places. We must add that the mixture of fraud with authentic paranormal phenomena seems to be testified both in ancient and modern times. There is good evidence that pious deception played a part in the temple-worship of more than one ancient religion. It is, I understand, the opinion of experienced investigators that mediums who have been detected in trickery have sometimes been possessed of genuine powers of producing paranormal phenomena.

Though it is evident enough that these observations are strictly relevant to one branch of theological study, it is exceedingly difficult to say precisely what consequences they might be expected to have. I have suggested that psychical research may throw light on the development of religion and perhaps do something to remove the impression that, at least in its earlier stages, it consists entirely of illusions, but evidently the theologian will be left with a new problem on his hands. What relation have these psychic experiences with the growth of the knowledge of God and how are they to be connected with the idea of revelation? I certainly have no solution to this problem and I suggest that the theologian cannot really make much progress in its solution until there is some measure of agreement among investigators concerning the causes of the phenomena. If the spiritistic hypothesis should eventually be accepted as the most likely one, we should be back at a position very much like that of the primitive Church, which, while repudiating the whole of pagan religion, did not question the existence of the heathen deities or the reality of their influence. St. Paul, at least in one phase of his thought, seems to have regarded the whole of the religion of the world prior to the advent of Christ as partly the work of evil spirits and partly due to the mediation of spirits which were not evil but certainly lower than God. Christ, according to this view, triumphed over all these " powers and principalities " not by showing that they did not exist but by making them unnecessary, by opening direct access to God. The horror of falling away from Christ to pay honour to spiritual powers was not that they did not exist but that they did, and the backslider was to be reprehended, not because he was superstitious but because he was under the influence of spiritual forces which were either evil or at least lower than the highest. It seems not impossible that a conception of this kind may once again appear as worthy of the consideration of philosophers.

The central question for theology is the reality of God and His nature. Every other problem is subordinate to this and must be treated in relation to it. Though there are undoubtedly persons, as I have remarked, who have had their faith in God increased and enlarged by their study of psychic phenomena, I do not see how Theology can gain any direct assistance from this source. No one who understands what he is about would suppose that there could be a purely empirical argument for Theism or that God could be the conclusion of an investigation by scientific methods. This does not mean that empirical data do not play a necessary part in the argument for Theism; they are the starting point, for example, of the famous Cosmological and Teleological proofs. The most which we can expect from psychical research is some significant addition to the data. The alleged communications of departed spirits deal frequently with this central problem of theology, but I must confess that, with few exceptions, the sermons of the departed seem worse even than those of preachers still in the flesh—more dogmatic and less coherent. That some personality purporting to be the spirit of a great man or of a friend should believe in God is not necessarily more convincing than the fact that Mr H. G. Wells does not believe in God. We do not know what opportunities the alleged communicator has for arriving at a rational conclusion. Nor again, so far as I have been able to judge, is there any unanimity in the utterances of "spirits", either on the nature of God or even on His reality. Even if we accept the spiritistic hypothesis, there is nothing of any importance to be gained with respect to belief in God from the testimony of the departed.

Nevertheless I think the results of psychical research have a certain indirect value for the theologian in his unending debate on the question *An Deus sit*. The great enemy of theistic belief is materialism in the wider sense—that naïve philosophy which is the natural initial assumption of both common sense and science. We need not define materialism further than by saying that it covers any philosophy which seeks to explain the higher in terms of the lower and, more specifically, regards mind as wholly dependent upon the physical order or even as a kind of shadow cast by it. I do not say that a refutation of this theory is equivalent to a demonstration of Theism, for there are other views which escape the absurdities of materialism, but I suggest that to expose its inadequacy is an important element in the case for Theism.

I do not wish to be misunderstood. I do not hold that psychical research has provided us with a disproof of materialism nor that it can do so; the claim which I would make is more modest. It seems to me that a survey of the evidence, on the whole, suggests that materialism will not do. It makes the hypothesis not more plausible but less.

Let us glance briefly at the data. I suppose that the fact of telepathic communication is about as widely accepted as anything in this highly controversial field. It would, of course, be absurd to allege that no explanation is conceivable which would not harmonise with a materialistic philosophy, but the facts as we have them seem highly recalcitrant to such treatment. On the surface, what we have is a direct communication between mind and mind, in some instances at least, apparently instantaneous over long distances. So far as I know, no physical hypothesis which even pretends to explain the phenomena has ever been put forward. The general statement that it is " all done by waves " seems about as enlightening as the explanation that the vanishing trick by conjurers is " all done by mirrors ", for even if there were a vibration detected, which seems unlikely, a vibration is not a thought or a feeling.

In the present circumstances it seems wise to say nothing about " extra-sensory perception ", because the experiments in this country have failed to confirm Dr Rhine's results, and it would seem unlikely that the atmosphere of Duke University is specially favourable for the development of this faculty. It is sufficient to observe that, if the existence of this power should be proved, even in one instance, it would be very difficult to fit in with any materialistic philosophy.

The phenomena of prediction, in the various forms in which they are alleged to occur, are admittedly perplexing, but so far as my reading goes it is difficult to doubt that authenticated cases are not very uncommon. Assuming that there are experiences which seem to presuppose acquaintance with events which have not yet occurred, they would have indirectly an important bearing on belief in God. In the first place, they may be said to lend some support to the belief itself. Personally, I do not see that theories about the nature of time, interesting and important as they are, can clear up the mystery. There may be many time-series, but the event which is foreseen or predicted is an event which will happen in the time-series of the persons concerned, the one in which they are now and which will continue until the predicted event comes to pass. The problem is, how can an event, which has not yet happened, be an object of knowledge, not by inference, but by acquaintance? Probably there are several philosophies which could incorporate this alleged fact, but at least it may be said for Theism that it has a ready explanation. Events may exist before they happen in the mind of God. But this brings us to the second theological implication of prediction. Must we not hold, if we adopt the theistic explanation, that all events are necessary, that the divine Providence orders all things in such a way that nothing is really contingent and freedom of choice is an illusion? Some Christian theologians have not shrunk from these conclusions, but whatever may be said for them on other grounds, I do not think they are necessary consequences of prediction. It would be equally plausible to conceive that all the possible courses of events are present to the divine Mind and that the one predicted is only one of the events which may occur. Most of us know of instances where an apparently predictive experience has indicated an event which might have happened and which nearly happened but which did not happen. Possibly the prediction hindered its own fulfilment. The theologian would have no difficulty in supposing that some predictive experiences are warnings which may or may not be heeded.

I shall have to refer to the problem of survival later in the lecture, but it is obviously relevant to this question which I am at present discussing. It would, I suppose, be accepted by all psychical researchers that the hypothesis of survival is one of the " live options ", to use a phrase invented by one of our most eminent Presidents. Nothing has emerged which rules out the hypothesis, and I think we may go further and say that several facts have been established which would certainly not have been anticipated by one who holds that consciousness is a function of the body. The persistence of the personality after the dissolution of the physical organism not only remains a possibility but there are indications that the purely empirical investigation renders it more plausible than it was when these super-normal phenomena were regarded as nothing but illusion. That I believe to be a cautious statement of the position and, if it is correct, we may say that psychical research has here again failed to confirm materialism and has, within limits, afforded some support to a contrary theory ; for it seems to me to be beyond question that the establishment of survival would be a fatal blow to every type of materialist hypothesis.

If I had unlimited time at my disposal I should now proceed to discuss the bearing of physical phenomena, such as telekinesis, on the question before us, but the facts are so perplexing that I have nothing of any importance to contribute to their explanation. On the whole, they seem to suggest once more the existence of force which is not, in any ordinary sense of the word, physical.

I do not doubt that hypotheses of a materialistic kind can be framed to cover all these phenomena which I have briefly touched upon, but they become so complex that they lack persuasiveness. The general impression, I suggest, which an unprejudiced observer would gain from a survey of the various classes of well-authenticated super-normal phenomena would be unfavourable to a materialist philosophy and thus, up to a point, favourable to the theistic conception, but, I must repeat, I do not think for a moment that any direct support for belief in God can be gained from them. Perhaps we might put it roughly in this way : the phenomena in question are much less surprising and disconcerting to one who holds a theistic philosophy than to one whose presuppositions are materialistic.

The plan of my lecture obliges me to pass rapidly from one subject to another and I must now make some remarks on a matter which has great importance for Christians—the reliability of the New Testament narratives as a whole. I believe that one of the indirect effects of psychical research has been to encourage a new attitude on the part of critical scholars to the so-called "miraculous" element in the Gospels and Acts. In this field, as in the wider field of the history of religion, we have been under the domination of a narrow rationalism which assumed in advance that "miracles do not happen"; by this is meant that no event could possibly have occurred which transcended commonplace experience. Thus it became necessary to relegate all the supernatural incidents of the New Testament to the category of mistake, illusion or legend. The consequences of this supposed scientific necessity were embarrassing, because the earliest sources for the life of Christ plainly represent Him as the possessor of abnormal powers and, if the whole of this element in the Gospels must be rejected, we are left with a residuum which must itself be suspect by reason of its association with such questionable matter. A great accumulation of knowledge, not all of it derived from psychical research, has altered the situation. Spiritual and mental healing, the study of divided personalities, clairvoyance, telekinesis, all these have suggested very forcibly that the limits of the credible were too narrowly drawn by the older rationalists. It appears to me that the Society for Psychical Research is a standing refutation of Hume's famous argument against miracles. You remember that, taking a miracle to be an event contrary to common experience, he urged that it is more probable that testimony should be false or mistaken than that a miracle should happen. But all the phenomena which our Society investigates are contrary to common experience, yet we are persuaded that some at least of them occur. In view of these facts I do not believe that any reasonable person would be inclined to assign limits to what phenomena might accompany the appearance of a personality which, in any view, was among the most potent influences which history records. Professor Saurat has recently illustrated the vision of St. Peter which revealed to him the necessity of admitting Gentiles into the Church by analogies with modern instances of telepathy and no doubt other detailed applications to New Testament narratives have been made. But the chief interest of all this to the theologian, apart from its bearing on the reliability of the records, is the light which may be thrown on the significance of the supernatural in the life of Christ.

The attitude of Jesus towards His own " mighty works " is not easy to determine. On the one hand, when confronted by the messengers of John the Baptist with the question : " Art thou he that should come? " He points to works of healing culminating in the raising of the dead and puts along with them the fact that " the poor have the good news proclaimed to them ". It may be that these " mighty works " are to be understood in a figurative sense, that the lame in spirit walk, the spiritually blind see and the spiritually dead are raised up, but I think this is unlikely and that the Evangelist at least meant the words to be taken literally. On the other hand, Jesus says that it is an evil generation which seeks for a sign. We may perhaps conclude that He attached a quite secondary importance to the supernatural powers which He exercised, but that He considered them to have value as arresting attention and giving confirmation of his unique character and mission. The result of a fresh consideration of the miraculous narratives in the light of modern psychical research would go some way to confirm this point of view. Obviously the old-fashioned " argument from miracles " receives no support. That depended on the assumption that the miracles of the Gospels were unique, and evidently we shall not help that assumption by finding analogies to them. But the occurrence of these remarkable phenomena is at least congruous with the belief that the Person who caused them was unique. Spiritual power might be expected to manifest itself, not only in teaching and heroic action, but by the use of psychic capacities for spiritual purposes.

I now turn to the subject which to many appears the only important one—that of survival. Among those who are convinced that psychical research establishes, or goes near to establish, survival of bodily death there is much natural impatience with theologians, because they have been so slow to avail themselves of the help which was at hand. I have already said that I admit the theologians have been excessively timid on the whole in this matter, but I must add they have reasons for hesitation which are worthy of respect. The most important reason is that they are not sure of the ground ; they do not know whether the belief in survival, which might be supported by psychical research, is really a spiritual belief at all. Christian thinkers of many different schools have expressed this doubt. They would say, the belief that human personality survives death is not, as such, a religious belief, nor has it any spiritual value. The only immortality which is worth having and which forms a part of the Christian hope is life in the presence of God. That this personality just as it is will go on after the dissolution of the body is not necessarily good news—it may be very bad news. The religious man should desire a future life only in so far as it offers new opportunities of spiritual progress and closer communion with God. Some of these theologians lay stress upon the distinction between the future life and eternal life, by which they mean life interpenetrated by the eternal values. Some, it seems, regard the question of a future life almost with indifference on the ground that the eternal life which is not subject to the limitations of time may be ours here and now.

We must admit that there is some ground for the suspicion with which many theologians regard the empirical proof of survival. Heine said that most believers would be perfectly happy without God if they could be sure of their own immortality ; they needed Him only as a guarantee that they would not perish. No one would doubt that Christians of this kind exist and no one would doubt that they are very bad Christians. History and modern experience show that excessive concern with the question of one's own survival and that of one's friends may have a deleterious effect on the life of the spirit. Historians of Hebrew religion have pointed out the paradoxical fact that it was partly because the Hebrews had no belief for a long time in personal survival that their conception of God was able to develop along lines quite different from those of the other Semites.

But when all this is said, I cannot agree that Theology may safely ignore the empirical evidence. In my opinion, the only arguments for personal immortality which have any value depend upon the belief in the existence of God ; and further no conceivable accumulation of evidence could prove the reality of everlasting life. Plainly the evidence could, at the most, show that some persons survived bodily death, but it could not show that all persons did, nor could it show that any persons triumphed over death so that it had no more dominion over them. The possibility would remain that extinction overtook them in the end. Nevertheless, within these limits, the existence of empirical evidence for survival would, I think, have important religious and theological consequences. There are some minds which are wellnigh impervious to philosophical and theological reasoning, but which capitulate to facts. I do not doubt that a demonstration, if such a thing could be imagined, which convinced everyone that a particular individual who had died was communicating with his friends would have a profound effect on the outlook of the majority of people and that this effect would be to incline them to a religious view of life. But I would go further. It seems to me that, however firmly persuaded a man might be on other grounds that the soul is immortal, he ought to welcome facts which tend to confirm belief that death is not final. For the standing difficulty is this, that death appears indeed to be the "bourne from which no traveller returns". Beyond there is silence. The voice which we once heard we hear no more, the thoughts which we once shared we share no longer. However great our faith or our reliance on arguments of reason, we cannot stifle the conjecture that there "all their thoughts perish". To me at least it would be a momentous thing if I could be sure that a thought had come to me from one whom I had known on earth and was now no longer among those we call "the living".

Has this happened? or to ask a more modest question, Has psychical research produced any evidence which makes belief in survival more probable, from a strictly scientific point of view? You will not expect me to sum up at the end of a lecture a complex and controversial problem, nor do I imagine that my personal views are likely to have any considerable value, but since the relation of psychical research to Theology does depend, to some extent, on the question of what hopes of a positive result from these enquiries may be entertained, I feel that I should be shirking an important issue if I did not give my opinion for what it is worth.

One is sometimes tempted to believe that some power—whether beneficent or malevolent I do not know—has determined that we shall never reach certainty on the subject of the life beyond and that to secure this it has sent a lying spirit into the prophets. The records of psychical research are full of deceit, fraud and illusion. But when one has discounted all this there remains a residuum of established facts which, prima facie, suggest the hypothesis of survival; that at least is my opinion. I am confirmed in this conclusion by observing that several eminent philosophers, who for various reasons are reluctant to accept the survival theory, have recently been led to put forward alternative theories which, in effect, admit some kind of persistence after death, though not personal persistence. I refer, of course, to the various forms of the "psychic factor" hypothesis. I suppose that these philosophers have convinced themselves that telepathy between the living does not account for all the phenomena, and in this they are surely right.

Professor H. H. Price, in his Presidential Address, developed a theory on the lines of the psychic factor to explain certain types of haunting in which he used the conception of an "aether of images". With modesty as rare in a philosopher as it is praiseworthy, he conceded that his theory might turn out to be "nonsense" and that it could be torn in pieces by a competent second-year student of philosophy. I am certainly very far from thinking that the theory is nonsense and I have had enough experience to know that many ideas which suffer annihilating dissection at the hands of second-year students survive as useful guides along the path to truth in the minds of wiser men. No doubt this is the case with the President's fascinating hypothesis, but I confess I found myself saying, " How much simpler to believe in ghosts! " Of course, the reflection was stupid, because it is by no means simple to believe in ghosts or indeed to say what believing in ghosts means. Nevertheless there was perhaps a serious thought behind the frivolous exclamation.

I think it was this. Unless I am mistaken, every theory which employs the concept of a " psychic factor " existing apart from a psyche must hold the possibility of what Mr Bradley used to call " floating ideas ". The word " factor " is a vague one and I confess I do not know what it means in this connection. The word " psychic " is not vague. It means, I suppose, anything that may be an element in the experience of a psyche, such as thoughts, emotions, feelings, conation and acts of choice. The phrase " psychic factor " seems to be a rather confusing way of saying that thoughts, emotions, feelings and all the rest have a kind of independent existence, so much so that they may persist when there is no longer any consciousness which possesses them. The theory is generally developed on the basis of the persistence of images, and here perhaps it offers least difficulty, but, to confine ourselves to the subject of haunting, these images seem, in some cases, to be charged with emotion, and a feeling which is no longer felt has a mode of existence which baffles me.

One is left with the question whether the idea of thoughts which are not thought, of images which are not conceived and of experience which is not experienced is really an intelligible one. There is at least no evidence of the existence of such " floating ideas ". If the believer in survival is confronted with the alleged fact that we have no knowledge of any consciousness which is not associated with an organism, he may reply, with even greater force, that we have no knowledge of psychic factors except in relation with a psyche. In view of the perplexities which arise when we try to make clear the meaning of a " psychic factor " I should be inclined to expect that this very limited advance towards the hypothesis of survival will have to be amended and that any amendment will bring it nearer to the ordinary conception of the survival of the self.

Since in this part of my lecture I am perforce giving simply my own opinions, I may be allowed to add a comment on the idea of survival. One of the objections made against it as an explanatory hypothesis is that it is vague, indefinite and obscure, that its meaning needs to be defined. After grappling with some of the alternative hypotheses, I confess that this particular objection strikes me as odd. Indeed I should have said that one of its chief virtues is its simplicity and intelligibility compared with the complexity and abstruseness of its rivals. No one questions that the application of the hypothesis to the facts offers many problems, but the hypothesis itself could be stated, I should have thought, in words to which everyone attaches a meaning. Personal survival is the hypothesis that the centre of consciousness which was in existence before death does not cease to be in existence after death and that the experience of this centre after death has the same kind of continuity with its experience before death as that of a man who sleeps for a while and wakes again. The difficulty that our experience is largely that of states and changes of our bodies is certainly formidable but not, I think, decisive. We have at least in Professor Price's Presidential Address some valuable suggestions towards the conception of a " spiritual body " which has a real continuity with the body of flesh. I repeat, the survival hypothesis may be hard to accept, but I cannot think it is hard to understand.

This lecture has, I fear, been rather of the nature of observations on several topics and I should be the last to claim that it has established any conclusions. It is no excuse to say that I have laboured at it, for I fear in the result I have been like the poet spoken of by Alexander Pope, " sleepless myself to give my hearers sleep ". I must plead that the subject is new and very large. It may have been worth while to try to map out the country which, as I think, waits for theologians to explore.

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OBITUARY

ALICE JOHNSON

ALICE JOHNSON was a member of a large family well known in Cambridge, and entered Newnham College in 1878. She became a scholar of the College in 1880, gained a First Class in Natural Science in 1881, and was elected Bathurst Student in 1882. Shortly after this the Balfour Laboratory, founded to commemorate the work of Professor Francis Balfour, was inaugurated as part of Newnham mainly through the efforts and generosity of Mrs Sidgwick, and Alice Johnson was appointed in 1884 the first Demonstrator in Animal Morphology there, a post which she held until 1890.

It was natural that anyone brought closely into touch with Prof. and Mrs Sidgwick should develop an interest in psychical research. In 1889 she took part as a sitter in the first series of sittings given by Mrs Piper in England, and in the following year she assisted Mrs Sidgwick in carrying out the well-known " Brighton experiments in thought-transference " recorded in *Proceedings* Vol. VIII.

About this time the Society took in hand one of its most important pieces of work, the Census of Hallucinations, the report on which, published in 1894 in Vol. X of *Proceedings*, is one of the recognised classics of psychical research. Alice Johnson was a member of the Committee which took the Census and presented the report, her colleagues being Prof. and Mrs Henry Sidgwick, Frederic Myers and Frank Podmore. Her work on the Census gave her invaluable experience in the critical analysis and valuation of evidence in " spontaneous cases ".

After being for several years private secretary to Mrs Sidgwick, she became in 1899 Editor of the Society's *Proceedings*, the proposal for her appointment being made by Frederic Myers. In 1903 she was appointed Organising Secretary of the Society, and at the end of 1908 Research Officer, a post she held until her retirement owing to ill health in 1916. How large a part she played in the Society's work during those years may be gauged from the fact that in the Combined Index for the *Proceedings* and *Journal*, covering the years 1901 to 1913, the entries under her name occupy more than four pages.

Of any period in the Society's history it may be said that it was a period of great difficulty. The particular difficulties confronting the Society during Alice Johnson's tenure of office arose largely through the death of Henry Sidgwick in 1900 and of Frederic Myers in 1901. The loss in rapid succession of two of the principal founders and leaders of a still young society might easily, but for the energy of those on whom the guidance of affairs then devolved, have led either to a decline in the quantity and quality of the Society's work, or to the Society developing tendencies in a direction contrary to the founders' intentions. To Alice Johnson belongs no small share of the credit for preserving the S.P.R. from either of these disasters.

Frederic Myers did not live to complete *Human Personality*, and the work of preparing the book for the press was entrusted to Richard Hodgson and Alice Johnson, the latter having already been occupied with the general supervision of the press work and the marshalling of the Appendices, which were a most important part of the book.

Most of Alice Johnson's research work related to the " mental phenomena ", as her grasp of the problems of evidence can have left her in little doubt that progress was most likely to be made in that direction. But two papers by her in Vol. XXI of *Proceedings*, written shortly after a visit to the United States to investigate " physical " phenomena there, contain valuable discussions as to the possible hallucination of sitters under the conditions of the séance room.

Spontaneous cases of " mental " phenomena were at that time reported to the S.P.R. in a profusion that the present officers can only envy. The persons concerned in these cases are often in a highly emotional state, and need to be handled with tactful sympathy. Alice Johnson possessed this quality in a high degree, but she was able to combine with it a strict adherence to the evidential standards on which the Society has always insisted. Carelessly reported and badly documented cases met with a polite but firm rejection at her hands.

The automatic writings of the " S.P.R. group of automatists ", which began shortly after Myers's death, presented entirely new problems of investigation. Nothing could have been made of them without most careful and systematic documentation on the one hand, and on the other great enterprise as regards interpretation and theory. Subsequent developments have shown how well and truly were laid the foundations of this novel and difficult study, the importance of which was on the increase throughout Alice

Johnson's tenure of office. With this study she was from the first intimately associated, and several of her most important papers in *Proceedings* deal with automatic scripts, particularly Mrs Holland's, and the theory of cross-correspondences: see *Proceedings* Vols. XXI, XXIV and XXV.

After her retirement through ill-health in 1916 she retained the liveliest interest in psychical research, and gladly allowed herself to be consulted on matters of interest to the Society. It is appropriate that her last paper in *Proceedings* (Vol. XLIV) should have been a tribute to Mrs Sidgwick's work in psychical research.

Below are printed contributions from two former colleagues of Alice Johnson, Mrs W. H. Salter and Miss Isabel Newton.

I first came to know Alice Johnson well in 1909, when I was appointed as her Assistant in the Society's Research Department. I found her daunting in the early months of my apprenticeship. I had a fair general knowledge of psychical research but little previous experience in research. Miss Johnson was a stern exponent of the Society's most rigorous traditions in exactitude; a fault or omission never escaped her eagle eye and the most one seemed to get in praise was "Yes, that will do". But I realised after a time that it was only a matter of adjusting oneself to a rather low scale in the expression of approval. A good piece of work would always find recognition (though the words might be few and the manner dry), and when I was criticised, I was bound to admit on reflection that criticism was deserved.

Miss Johnson was (as every scientific worker should be) meticulously accurate in ascertaining and recording all relevant facts; she was never satisfied with anything less than the whole truth, so far as it was discoverable, and she expected others to conform to her own high standard. To work with her was an admirable training in scientific method.

I also came to realise very soon the human sympathy and understanding that underlay that dry manner. It fell to Miss Johnson's lot (and to mine as her Assistant) to interview many of those who came to the Society either to report personal experiences or to seek advice. She was impatient of pretentious claims and a good judge of character, not easily deceived, but where she felt that there was a prospect of getting good material for the Society, or a need of help that she could give, she never grudged time or trouble. She had also in dealing with the human side of the Society's work a delightful sense of humour, which must often have stood her in good stead.

I cannot imagine any one being associated with Alice Johnson without coming to feel for her not only great respect, but also great affection. I shall always keep a very pleasant memory of the years we worked together, and I feel that whatever value my own work may have for psychical research it owes largely to her teaching.

HELEN de G. SALTER.

Mrs Salter has asked me to add to the Obituary of Miss Alice Johnson a note on some of my own personal impressions concerning her. These were formed during the years 1903-1916, when I worked with her, first as an Assistant in the office, and later as Secretary of the Society. At this distance of time the characteristics that stand out most clearly in my memory are her courtesy, her consideration for all who worked with her, her absorption in the work of the Society, her sympathy, her sensitiveness and her integrity.

To a complete and very unsophisticated novice—as I was in 1903—who dreaded the expected brusqueness of office manners, Miss Johnson's gentle courtesy was not only a thing of beauty, but it was to me an incentive to put forth my best efforts. It conveyed encouragement and confidence, and it never failed, notwithstanding mistakes and ignorance on my part. Looking back, I see myself as ever conscious and appreciative of it.

It had certain modulations, however, and by these she expressed displeasure. Here I am reminded of her dislike of pretensions, and of every form of overstatement. Her reaction to these not uncommon weaknesses was immediate: her manner would stiffen, her courtesy become chilling, and a faint air of hostility would spring up between herself and the offender. I have a vivid recollection of an occasion when, describing an interview she had just had with one of the most persistent offenders in this respect, she confessed to having been "quite rude". This being my one and only association of "rudeness" with her is probably the reason why this trivial incident was not forgotten years ago.

My earliest memory of the office is of being asked by Miss Johnson to write more clearly, so that the postmen should be able to see at a glance the destination of the letters and so be spared unnecessary trouble. I was to find later that this was typical of the consideration she showed for everyone with whom she was in contact, directly or indirectly. 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. With this in mind she took particular pains to cover every point in the enquiries and to make her meaning clear. Her letters were singularly free from ambiguities.

When I joined the office staff in October 1903, it was obvious that her advent as Secretary the previous March had opened a new era in the routine work. We generally found it difficult to trace references before that time, but with those of a later date it was a simple matter. In a few months she had gathered the loose ends together and had welded them into a system in which there was no confusion and no uncertainty. The contrast gave meaning to her almost meticulous insistency on accuracy. I remember that when, moved by her disinterestedness and earnestness of purpose, I resolved quite definitely to "support" her, my part in the co-operation seemed to me to consist in nothing more nor less than to be accurate in every particular!

In the office her manner was gentle and rather shy. But her rule was absolute. She was exacting to a degree in the matter of accuracy and thoroughness. Her displeasure was expressed by the slight but unmistakable coldness of manner to which I have alluded; never by words of blame. She was invariably reasonable and just. She showed thoughtful consideration for her staff in every possible way. And she created, probably quite unconsciously, a strong bond of unity between herself and her colleagues. There was never any friction. I cannot recall one instance of a member of the staff uttering a word of complaint against her.

Her devotion to the interests of the Society impressed me greatly. I thought she sacrificed much for it, as it absorbed all her energy. She was a slow worker; this, I think, was owing to the pains she took to perceive clearly, and to impart her knowledge clearly. She worked quietly and steadily, and was never hurried. Pictures of her slip in and out of my mind as I write, but there is one that predominates. It is as I saw her evening after evening, when at six o'clock I closed the office and went to her rooms for her letters. She was seated at her desk writing, her grey head bent low under the light of an electric lamp at her elbow, her short-sighted eyes close to the paper. She was always intent. I got the impression then that her mind was never in flight: that she held it captive, forcing it to record with exactitude what she wished to impart.

Lunching recently with Mrs Thatcher, a former Assistant-Secretary of the Society, I asked her what stood out most in her memory of Miss Johnson, and she answered at once, "Her shyness, and her absorption in the work of the Society." "I have a mental image of her," she added, and went on to describe it. It was of

Miss Johnson writing at her desk, and it tallied with mine in almost every detail.

When the Rooms of the Society were closed during August and September for the vacation, we understood that Miss Johnson did not wish to be approached on S.P.R. matters. She attached great importance to good holidays, and I took it for granted that she then relaxed. She always came back looking refreshed, but she did not talk about her holiday. She rarely spoke about herself. It was our custom to have tea together every afternoon, but even then we generally discussed the affairs of the Society, and were more or less impersonal. (It is odd, but I cannot remember ever having heard Miss Johnson's " voice in laughter ".) I think that this lack of social intimacy was due partly to her inherent shyness, and partly to a kind of mental decorum on my side, imposed by her seriousness and by the fact that I felt, rightly or wrongly, that she invested the office of Secretary (which I then held) with the dignity of a high calling. I think now that we probably might have had more fun. She belonged to the academic world, and her knowledge of life and human nature outside it was limited, but I remember several incidents which suggest that she enjoyed seeing other outlooks, when they were sincere and natural.

She had a great capacity for sympathy, and a gift for expressing it in letters simply and sincerely. But it was shown mostly in deeds. I received so many kindnesses from her that, looking back, I see them rather as evidence of an attitude to life than as detached actions. It was as if she had assumed a certain moral responsibility for my welfare as a colleague. Her kindness was spontaneous, and always practical. In various ways she greatly added to my comfort and well-being, and it was not until I enjoyed the benefits of her forethought that I realised my former need. In particular, I can never forget her great kindness during a long and serious illness I had in 1912. She thought of everything to help me. Again when I was laid up the following year, she visited the Nursing Home and assured herself that the best arrangements were made for me. I can say without being guilty of an overstatement that, outside my own family, no one showed so much solicitude for my well-being. She continued to show it after she left the Society, until a comparatively short time before her death ; fearing that this or that arrangement might " fall heavily upon " me, reminding me to take a " good holiday ", and " not to overwork ". " And try not to be anxious as to how things go." I think that this extract from one of her letters not only shows sympathy, but suggests that things were not always easy for her.

Few members of the Society knew her as we knew her in the office. Some saw her as a rather austere little figure, with an academic manner which alarmed them. Some in closer contact accused her of a love of power and resented her authority. This was inevitable, for where there are two diametrically opposed approaches to a subject, the personal and the scientific, as in psychical research, there must be conflict. Here I see her with a firm grip upon the affairs and the policy of the Society, opposing tooth and nail everything that threatened to weaken its scientific character or lower its scientific prestige. She was a formidable foe, with an indomitable will. She had no vulnerable spots which might have deceived and diverted her. She held herself intact, and subordinate to her incorruptible integrity.

Sensitiveness and incorruptible integrity combined often lead to unhappy hours and a continual bracing up of courage. I thought on some occasions that this was Miss Johnson's experience, and several memories spring up which support the impression. The contrast between her weakness and her strength inspired in me a strong partisanship. Even now I am touched by her heroic qualities and frail physique. I count it a great privilege to have worked with her.

ISABEL NEWTON.

[*We print below a notice of the Studentship recently established at Trinity College, Cambridge, out of Mr F. D. Perrott's bequest. We believe this Studentship to be the first of its kind in this country, and we welcome the close connection thus formed between Psychical Research and Trinity College, of which three of the Society's Founders, Gurney, Myers and Sidgwick, were Fellows. Hon. Ed.*]

TRINITY COLLEGE

Perrott Studentship in Psychical Research

Trinity College, Cambridge, has established a Studentship for the study of Psychical Research out of a bequest left to the College for that purpose by Mr Frank Duerdin Perrott as a memorial to F. W. H. Myers.

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

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

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

The Studentship is tenable for one year, and the Student will be required to devote a substantial part of the period of his tenure to investigating some problem in Psychical Research.

The Studentship will be of such value, not exceeding £300, as the Electors may award after considering the nature of the research which the candidate proposes to undertake. The emolument will, in general, be paid half-yearly, and the first instalment will be paid on the quarter-day on which the tenure of the Studentship begins.

The Student shall, during the tenure of his Studentship, pursue to the satisfaction of the Electors the course of research proposed by him in his application ; provided that such course may be altered with the consent of the Electors. If the Electors shall report to the Council of Trinity College, Cambridge, that he is failing to pursue his course of research with due diligence, the Council may, if they think fit, deprive him of his Studentship.

The Student shall not, during the tenure of his Studentship, follow any business or profession or engage in educational or other work which in the opinion of the Electors would interfere with his course of research.

Applications from candidates should be sent to Professor C. D. Broad, Trinity College, Cambridge, before 6 May 1940. In making his application a candidate should state his qualifications and claims, and his proposed course of research ; he may also submit any work which he has written, published or unpublished. No testimonials are required from candidates who are graduates of Cambridge University or women students on whom a title of a degree has been conferred by that University. Other candidates must submit the names of three referees, and the Electors will not award the Studentship to any such candidate until they have had a personal interview with him.

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

31 *January* 1940.

PROCEEDINGS

OF THE

Society for Psychical Research

PART 162

INTRODUCTION TO MR WHATELY CARINGTON'S AND MR SOAL'S PAPERS

By Professor C. D. Broad, Litt.D.

The Editor has asked me to write a brief introduction to the two papers, by Mr Whately Carington and Mr Soal, which form the main contents of the present number of the S.P.R. *Proceedings*. I am very glad to do so. Mr Whately Carington's work has been done in Cambridge in consultation with a committee composed of himself, Mr Gatty, Dr Irwin, Dr Thouless, and the present writer. This committee has held its meetings in my rooms in Trinity; so, although my lack of expert knowledge and practical experience in statistical method has prevented me from contributing anything of importance to their debates, I have at least been in constant touch with this investigation from the outset. I have had no such direct contact with Mr Soal's work. But I have read all that he has published on the subject with great interest, and I have always admired the patience, thoroughness, and accuracy of all his many-sided contributions to psychical research. It is an ill war that blows no one any good, and the disaster that has befallen Europe has at least had the good result of making Mr Soal a temporary resident in Cambridge, where the members of our committee live.

I suspect that the first reaction of many members of the Society on opening this number of the *Proceedings* will be to cry "Another mass of boring statistical stuff!", and that the second reaction of some of these will be to leave the contents unread. This kind of behaviour may be natural, but it is foolish on general grounds and it would be most unfortunate in this particular case. I will now give my reasons for these two assertions.

In almost every branch of psychical research the first question that arises is this :—Have coincidences of a certain kind happened much more often than they might reasonably be expected to do by chance? This question crops up in investigating mediumistic communications which purport to give information about a dead person, and in investigating the claim that a house is haunted, just as much as it does in experimental researches on alleged para-normal cognition, such as are reported in the present number of *Proceedings*. In the first case we want to know, before going any further, whether considerably more of the medium's statements about the alleged communicator agree with the facts about him than might reasonably have been expected by chance. In the second case we want to know whether coincidences between staying in this house and having sensory hallucinations of a certain kind are more numerous than we might reasonably have anticipated if chance alone were operating. It is only when these questions have been answered in the affirmative that there is anything worth investigating further.

But, although this kind of question arises everywhere, and although in many cases we understand vaguely what it means and we have a stronger or weaker personal conviction as to the answer, it is only where statistical methods can be applied that the question becomes precise and that a completely definite and objective answer becomes possible. What exactly is a " coincidence "? What is meant by a coincidence " happening by chance "? How often might coincidences of a given kind be reasonably expected to happen by chance? What amount of deviation from the most probable chance-frequency would it be reasonable to ascribe to chance? There is one field of human activity in which precise answers can be given to all these questions, and it is one that is perfectly familiar to all of us, viz. what we call " games of chance ", such as are played with cards, dice, roulette-boards, and so on. These answers can be carried over into other fields without loss of relevance or intelligibility just in proportion as those other fields are analogous in all relevant respects to actual or conceivable games of chance.

Let us take an example from ordinary playing-cards. No one competent to express an opinion would question, *e.g.*, any of the following statements, except on the ground that a mistake might have been made in the calculations which led to them. The most likely number of hearts in a whist-hand properly dealt from a properly constructed and shuffled pack of ordinary playing cards is 3. The probability that such a hand will contain exactly that number of hearts is about ·286. The probability that it will contain exactly 4 is only slightly less, viz. ·238. The probability that it will contain more than 6 hearts is ·0112. Suppose that 1000 hands were dealt. Then the number of them which contain more than 6 hearts apiece is more likely to be 11 than any other number. The probability that it will be exactly 11 is not great; but the probability that it will be less than 16 is ·9332, and the probability that it will be less than 20 is ·9965. Therefore, if in a thousand hands there were considerably more than 20 which contained more than 6 hearts apiece, it would be reasonable to feel almost certain that there was something wrong with the pack or the shuffling or the dealing.

Now contrast this example with cases where statistical methods cannot be applied, and one can only make such remarks as "It is incredible to me that Mrs X, that simple and ignorant woman, should by chance have made so many true and striking statements about the deceased Mr Y". Possibly one's incredulity may be quite justified, and possibly the situation may have been such that a majority of fair-minded readers with no strong prejudices against mediumistic communications will come to share it. But it is all terribly personal and subjective, and experience shows that it leaves most plain men and scientists completely unmoved.

For such reasons as these I believe that experiments in psychical research which are capable of precise statistical treatment are of the utmost importance. I do not think that we shall ever get orthodox experimental psychologists to attend to our work unless and until we can produce results of this kind. We must remember that experimental psychology is very much in the position of a woman with a shady past who has at length, after a hard struggle, settled down to a respectable life and got on visiting terms with the doctor's, the solicitor's, the vicar's, and even the squire's wife. (The scientific equivalent of this apotheosis is being admitted to form a section at the British Association.) She is fanatically determined to keep her hard-won respectability unsullied by the slightest breath of scandal. Physics, which has been honoured for centuries, can afford, like the scion of some noble house, to throw her cap over the mills; but poor dear psychology feels that she dare not take risks. Now the work of contemporary orthodox experimental psychologists is very largely statistical, as anyone can see who looks at their journals. Since this ground is so very familiar to them, there is a hope that a few of those extremely shy birds may be tempted to hop over the wall which separates them from us, if we can offer them some statistical crumbs, of unimpeachable wholesomeness, to peck at. They certainly ought to be investigating the problems which interest us, instead of ignoring them or unhelpfully nagging at us for the crudity and amateurishness of the methods which we have to use in default of their advice and assistance. But I am sure that they will continue to neglect that duty until we can bring to their notice properly conducted researches of a statistical kind, such as those of Mr Whately Carington and Mr Soal.

There are two remarks of an historical kind which it may be worth while to make before we leave generalities. The first is this. Some readers may be tempted to complain that such researches as these can at best lead only to trivial and pedestrian results. What interests them, and what induced them to join the S.P.R., was the hope of finding answers to questions of fundamental human importance, such as the survival or extinction of the individual and the destiny of the race. Investigation of trance-mediumship, they will say, has at least a chance of throwing light on these important questions; but these experiments on drawing pictures and guessing cards are at best *difficiles nugae*.

I think that the history of science shows this attitude to be entirely mistaken. It is precisely the attitude against which Galileo, Descartes, Bacon, and other great men who assisted at the birth of modern science, constantly and energetically protested. What could seem more trivial than Galileo dropping balls down inclined planes and noting the times of their descent? Yet those experiments are the basis of the science of dynamics, and without them Newton's all-embracing theory of the planetary system would have been impossible. Similarly, if we could establish the fact and disentangle the laws of extra-sensory perception by experiments on ordinary persons in artificially simplified situations, we might hope eventually to form comprehensive and satisfactory theories about mediumistic communications. But, if we insist on delivering blind frontal attacks on unanalysed problems of immense complexity, we are likely to share the fate of the scholastic physicists.

The second point is this. Any science starts by consisting mainly of " natural history ". If it develops, the proportion of natural history becomes smaller and the proportion of theory and deduction becomes greater. As this happens, the science grows more technical, and it becomes more and more difficult for the interested amateur to contribute anything of value to it, or even to understand the contributions of experts. Just contrast, *e.g.*, the early meetings and the early *Transactions* of the Royal Society with those of the present day. If we may compare small things with great, we must expect that something of this kind will happen with the S.P.R. unless it be engaged on a wild-goose chase. Undoubtedly there is still an immense field for " natural history " in our subject, and for many years to come the intelligent and curious amateur will be able to make most valuable factual contributions to psychical research. But already, in my opinion, the investigation of alleged physical phenomena in the séance-room has ceased to be a job for anyone but a trained specialist with an elaborate knowledge of electrical and photographic technique. I am equally convinced that we have now reached a stage in the study of para-normal cognition under experimental conditions at which statistical methods must be constantly and increasingly used. Members of the S.P.R. would do well to reconcile themselves to this inevitable tendency, and to prepare themselves for it by making that quite moderate study of the elements of statistics which would enable them to follow intelligently the reasoning contained in such papers as Mr Whately Carington's and Mr Soal's. They would find that the effort was rewarded, not only in connexion with psychical research, but also in the many other departments of life in which statistical concepts and methods are now applied.

I hope that I have now shown adequate cause why the two papers which follow should not be ignored off-hand on the general ground that they contain a good deal of statistics and that the results which they establish make little direct appeal to our higher emotions and aspirations. It remains for me to state some positive reasons why they deserve to be read with special care and attention.

In the first place, both sets of experiments have been conducted with a degree of care and thoroughness which has probably not been equalled and has certainly not been surpassed in any previous work on the subject. The writers have been at pains to exclude every possible kind of normal " leakage ". They have stated exactly what precautions they took, and have given so clear and full an account of the conditions under which the experiments were performed that anyone who chooses can repeat them exactly. A critic who wishes to show that there was opportunity for normal leakage will have to indicate some specific defect in some recorded detail of the technique ; he will not be able to base his criticisms (as in many cases he quite justifiably can) on the negative ground that " we are not told whether the percipient had such and such chances of acquiring knowledge normally ". There may have been other experiments on paranormal cognition in which the conditions were *in fact* as rigid as they were in these ; but I do not think that there have been any in which *we know* them to have been so rigid as *we know* them to have been here.

It is important to notice that Mr Whately Carington has devised a kind of experiment which ought to give *positive* results, if repeated, in a *fair proportion* of the repetitions. Of course something may depend on the mental attitude of the agent or transmitter, but there should be no insuperable difficulty in finding suitable agents who are sufficiently interested and sympathetic. To have achieved this result is a real step forward. Provided that later reflexion and discussion do not reveal unforeseen sources of error, Mr Carington has (probably for the first time in the history of the subject) produced a *repeatable* experiment.

Secondly, it is most unlikely that there is any flaw in the statistical technique. Objections on this ground have been brought by certain statistical pundits in America against Dr Rhine's results, though Mr Soal and Dr Thouless have shown that these objections are in the main captious and nugatory. In the present case it is doubtful whether even the most captious statistical critic can have much to say. In Mr Soal's experiments the statistical problem is perfectly simple and straightforward, and it does not differ essentially from that of Dr Rhine's experiments. By carefully randomising the cards in the way which he describes, and by using Mr Stevens's formula, which allows for the actual preferences of the guessers among the five kinds of Zener cards, Mr Soal has obviated the only criticisms on Dr Rhine's statistical assumptions which ever had any substance.

A careless reader, on a first hasty reading, might be inclined to think that Mr Whately Carington's " method of palpable hits " is open to criticism because it makes use of the opinion of a certain individual judge as to whether such and such a drawing does or does not resemble such and such an original enough to be counted as a " palpable hit " on the latter. This criticism is fallacious, as Mr Carington has most carefully, lucidly, and conclusively shown in his paper. I will not discuss the matter further here, because I have tried to explain the statistical situation by means of an analogy which Mr Carington has embodied in an appendix. It will suffice to say that, if there be any statistical fallacy in Mr Whately Carington's paper, it has eluded, not only myself (a very feeble defence), and not only Dr Thouless and Mr Gatty (who have constantly to use and to appraise statistical reasoning in the course of their psychological and biological work), but also Dr Irwin, who is an expert professional statistician.

A third merit of these experiments is that we know that we have *all* the results, good, bad, and indifferent, before us. There is no room for the suspicion which attaches, rightly or wrongly, to some long series of experiments on para-normal cognition, viz. that the experimenter began to record his results only after they began to be exciting, that he stopped recording them when they ceased to be so, and that he discarded results which were got when the subject was "off-colour".

The fourth reason for attending carefully to these two sets of experiments is that they led to positive results of a high degree of statistical significance. Consider, *e.g.*, Mr Whately Carington's answer to the question whether or not his percipients tend, to a significant extent, to score relatively more hits on the originals of the experiment in which they are engaged than they do on those of experiments in which they are not engaged. He finds, on the most conservative method of scoring, that the actual deviation from the most probable number of successes on the hypothesis of chance is positive and is 3·572 times the standard deviation. What precisely does this mean? Suppose we compare the whole of Mr Carington's material in all these experiments to a single "deal" or "throw" in a game of chance which is fairly played with properly constructed apparatus. Suppose we imagine a similar set of experiments to be repeated, with the same amount of material on each occasion; and suppose we compare each such repetition to a new "deal" or "throw" in the same game of chance. Suppose that the positive results which Mr Carington actually got were like some big chance deviation from the normal in a single deal, *e.g.* holding 10 or more hearts in a fairly dealt bridge hand. Then one could not reasonably expect to get so great a deviation more than once in 1000 such "deals", *i.e.* in 1000 repetitions of such an experiment as is here described.

Now take, *e.g.*, Mr Soal's figures for the successes scored by his subject Mrs S. on the actual card, the one immediately before it, and the one immediately after it. The divergences of these from the most probable numbers were all positive, and were respectively 2·627, 3·309, and 4·164 times the relevant standard deviations. The *meaning* of these statements can be interpreted as above. The actual *numbers* concerned will be different; in the first case somewhat less than 1000, and in the third case very much more than 1000, repetitions of the experiment would be needed before it would be reasonable to expect so great a deviation. Moreover we have to take into account the further fact that all these large deviations are *positive*, whereas in a game of chance, they might just as well have been negative; and that they are *clustered about* the card actually aimed at by the percipient.

I will now say something about the connexions and the disconnexions between the two papers. The two investigations began and continued for a long time in complete independence of each other. Mr Soal worked for five years with Zener cards and individual percipients in London. Mr Whately Carington has been working for about a year and a half with drawings and groups of percipients. The drawings have been exhibited in Cambridge, the groups of percipients have been in various places. It was not until November 1939 that the two sets of experiments converged. Mr Carington had found a highly significant degree of post-cognitive and pre-cognitive success among his subjects, and he suggested to Mr Soal that the latter should look through his results and compare the guesses made by his subjects with the cards exposed immediately before and immediately after the card at which the guess was deliberately aimed. The remarkable outcome of this comparison, in the case of two of his subjects, forms the content of Part II of Mr Soal's paper. It certainly adds to the dramatic interest of these two investigations that a suggestion, based on the results of one of them, should have revealed that the other had led to a highly significant positive result which would otherwise have been overlooked.

The following two points of difference should be noted. (i) Mr Whately Carington is led by his experiments to conclude that the power of para-normal cognition is *widely distributed* but *very weak*, so far as concerns the subjects whom he has tested and the tasks which he has assigned to them. Among Mr Soal's subjects, who are engaged in a very different kind of activity, the situation seems to be quite different. When the scores were re-investigated for signs of pre-cognitive and post-cognitive knowledge only two of his subjects were found to have shown it to any appreciable extent. These two seem to possess it strongly ; and they are also outstanding, at any rate in their earlier scores, at guessing contemporarily exposed cards. (We must not, at this stage, ignore the possibility that, if the guesses made by the other subjects were tested for successes on more remote cards than the three central ones, they might be found to have scored significantly.) (ii) As Mr Soal has pointed out, the guesses which he has called " pre-cognitive " need not have been so in the sense of involving present knowledge of a future event. The cards which will be turned up after a given guess has been made already exist and are already on the table covered up by other cards. If the subject can clairvoyantly cognise cards which have not yet been exposed in the course of the experiment but are already in the pack, he will be able to make guesses which are " pre-cognitive " in the sense required by Mr Soal's results. Now this is not so with the pre-cognitive knowledge which appears to be manifested in Mr Whately Carington's experiments. This is knowledge of a drawing which has not yet been made, a drawing whose subject has not yet been chosen.

In this connexion the two following remarks are worth making. (i) Dr Thouless, who had done a number of experiments on guessing Zener cards with students at Glasgow and had got entirely chance results, was encouraged to look over his records for post-cognitive and pre-cognitive successes. He found that there was no trace in them of significantly non-chance scores. This is exactly what Mr Soal would have found if he had not been lucky enough to include among his 160 subjects those two white blackbirds Mrs S. and Mr B. S. (ii) Mr Soal found no significant evidence for pure clairvoyance, either of the card at which the subject was aiming or of cards which came immediately above or below it in the pack, in the experiments which were specially directed to this question. He has not as yet had time to make elaborate counts for more remotely displaced successes. Of course it is possible that Mrs S. and Mr B. S. would have been exceptions, for in fact they tried only undifferentiated extra-sensory perception. If we suppose that they would have failed at pure clairvoyance, as did all the subjects who tried it, we shall have to hold that their pre-cognitive successes really did involve foreseeing what the agent was going to see when he afterwards turned up the next card, and that they did not just clairvoyantly perceive that card while it was still lying covered on the table. In that case their pre-cognitive results would be in line with those of Mr Whately Carington's subjects.

EXPERIMENTS ON THE PARANORMAL COGNITION OF DRAWINGS

I. Experiments I to IVb

By Whately Carington, M.A., M.Sc.

GENERAL ABSTRACT : Five experiments (Nos. I to IV B) have been carried out, using simple drawings as test material. About 250 percipients took part. In no case was any percipient in the same room with the drawing he was required to reproduce, and careful precautions were taken to prevent knowledge being obtained by sensory means or by rational inference. Marking was done by an independent judge, who was not given sufficient information to enable him to produce a spurious positive result.

It was found that percipients tended, to a highly significant extent, to score relatively more ' hits ' on the drawings (originals) used in their own experiment than on those used in other experiments.

It was also found that hits were not by any means always scored on the occasions on which the originals to which they referred were displayed, but tended to be displaced to an earlier or later occasion. Both these tendencies appear to be significant, indicating the occurrence of precognitive and retrocognitive effects.

A control scoring of the same drawings against a set of randomised ' dummy ' originals gave null results.

SECTION I

Introductory

1. *Genesis of the Experiments :* The experiments described below arose almost inevitably out of my re-examination of various earlier researches, which I described in a Paper read to the Society in June 1938, and was summarised in the *Journal* for December of that year. These studies convinced me that, despite the machinations of the malevolent hoodoo which apparently dominates the subject, the case for supposing that significant and genuine positive results had been obtained in the past from experiments of this kind was very strong. On the other hand, there seemed to be a general rule that the more carefully experiments were carried out, the less ' successful ' they were likely to be. Still, if the main conclusion were correct, there should be no reason inherent in the nature of the phenomena why, given a modicum of good fortune, results satisfying the necessary criteria should not be obtained again.

I accordingly decided to undertake as soon as practicable a new series of experiments which should at least be free from the weaknesses noted by myself and others in earlier work, and to press the attack home to a point at which it would be possible to give a definite answer one way or the other.

2. *Objects of the Experiments and Choice of Technique:* There are four criteria to which any successful experiment in this field must conform. In approximate order of importance these are: (1) The conditions must be rigid, (2) The scoring must be unbiassed, (3) The results must be statistically significant, (4) The experiment must be repeatable.[1]

In all cases the general situation is essentially the same. That is to say, someone or other is required to display knowledge of some object or event, which he could not have obtained by normal sensory means or by rational inference from normally perceived facts; the accuracy or frequency of the knowledge is then assessed by some method of marking, scoring, judging or the like; and finally an estimate is made, by one means or another, of whether the extent of the knowledge shown is greater than can reasonably be attributed to chance or luck.

It is evident that if the knowledge displayed *could*, by any reasonable stretch of imagination, have been obtained by the normal processes of perception and inference, the experiment has been just so much waste of time, at least so far as establishing the reality of the phenomena is concerned; for those who are unwilling to accept them will, not unnaturally, maintain that a known cause, even if intrinsically unlikely, is to be preferred to an unknown. And it is difficult to say at what point this attitude becomes unreasonable.

Similarly, if the process of assessment is in any way subject to the prepossessions of the assessor, the results, though not necessarily invalid, are bound to be suspect; indeed, a very conscientious judge might well bias the outcome against his own views, just as a wishful enthusiast might bias it in favour of them. It is, in fact, essential that the judge should be as incapable of influencing the outcome in either direction, however strong his prejudices, as the percipients of obtaining knowledge of the material by normal means.[2]

Consequently, although it would be an overstatement to say that watertightness of conditions was the primary *object* of the experiments, it was certainly my chief preoccupation, in the sense that I was completely determined from the start that, whatever else might happen, there should be no room for argument as to whether

[1] It might well be argued that, since the repeatable experiment is the very foundation of science, the last criterion is the most important of all; but the question of repeatability does not arise till the other criteria have been satisfied, so I make no excuse for putting it last.

[2] In absolute strictness, this ideal is not necessarily possible of attainment in all cases; for it is often possible for a judge to 'sabotage' an experiment by a suitable display of irrationality. Such extremes need not be considered here; for the remedy is simple—discard the judge—while, under a proper procedure it is never possible for a judge to generate a falsely significant effect.

the percipients could have obtained by normal means any knowledge of the material they might display. It has all too often happened that someone has carried out experiments with an enthusiasm and diligence amounting to devotion, and has obtained results which he reasonably believed to be convincing, only to have some critic point out that there *might* have been a leakage of information through some normal, if unlikely, channel which he had omitted to block. I viewed with great distaste the prospect of having months of work nullified in some such way as this, and with not less the alternative of being dragged into interminable and inconclusive arguments as to whether the observed results might or might not be due to involuntary whispering, olfactory hyperaesthesia, the subconscious interpretation of subaudible pencil scratchings, the purloining of cards by corrupted housemaids, and all the rest of it.

I accordingly laid it down as a first principle that, until after a percipient had done his work and had duly handed in his efforts, he should in no case be in the same room with the material he was required to 'guess'. In the experiments here discussed this condition has been rigidly adhered to throughout. With the exception of the second experiment, *q.v.*, no percipient, to the best of my knowledge and belief, has so much as been in the same building during the preparation or use of any of the material; and even in the 'Individual Experiments', which are not discussed here but may be reported later, percipients never entered the experimenter's room, or even that adjoining it, until after the experiment was over.

I was well aware that, in insisting on this extreme rigidity of conditions, I should, in the opinion of some students, be running the risk of inhibiting altogether the effects I was interested in establishing. But against this two considerations weighed. In the first place, the whole contextual evidence of the subject suggests that such factors as distance and brick walls are no impediment to the occurrence of the phenomena in question, if they are going to occur at all; in the second, I think I would much prefer to do a null-resulting experiment under rigid conditions than one which yielded exciting results under conditions which were sloppy—one would not merely have the satisfaction of knowing that one had done the job properly, but might fairly hope to be spared the indignities of controversial disputation.

Having settled this point, two further decisions had to be made, namely as to the type of percipient and the kind of material to be used.

As regards the first, beggars cannot be choosers; but there is always the question of whether to attempt to find and concentrate on a few specially gifted performers, or to rely on the 'random samples' likely to be met with in the course of mass experiments. The first plan has obvious labour-saving potentialities and may well prove the best when it comes to examining the effect of particular changes of conditions, etc., after the reality of the phenomena has been established, always assuming that specially gifted performers exist; but at the present stage, when that reality is still gravely in question, I have no doubt at all that the second is not only preferable but almost obligatory. Certainly from the point of view of repeatability there can be no two opinions about it; for history indicates that so soon as an investigator has obtained magnificently successful results from some specially gifted sensitive, she either developes moral scruples or some similarly fell disease, or 'disappears into the Middle West', or gets married, or just loses her powers, and we are left to speculate as to whether the results were genuine or whether, after all, the experimenter had left some loophole unguarded. It therefore seemed to me that, if I could get significant results from more or less randomly selected people under rigid conditions, I should be putting the phenomena on a very much wider and firmer basis than by obtaining perhaps more spectacular performances from one or two special sensitives who might easily become inaccessible for future work. It will be seen from what follows that at the time of writing (Dec., 1939) about 250 different percipients have taken part in the experiments, none of them having, so far as I am aware, any special antecedent claims to being abnormally 'psychic'.

As regards the second question, the decision is not so easy to make. Broadly, the choice is between 'restricted' and 'free' types of material. In the first case the percipient knows in advance that the object he is required to 'guess' will be one of a limited range, such as a two figure number, or a card from a particular sort of pack; in the second, the problem is more in the nature of 'What?' than of 'Which?', for he may be asked to record his impressions of what is in the experimenter's mind, or what is the subject matter of a book or a drawing, which leaves a field of almost boundless extent open to him.

It goes without saying that the restricted type of material is, on the face of it, very much the easier to deal with from the point of view of assessment; in fact relatively few attempts have been made to treat free material in a quantitative manner at all.[1] But this seems to be in reality something of a delusion, for the method of assessment described below appears to be, in some respects, actually easier to apply than the very error-liable scoring of successes with cards or the like.

[1] But see such contributions as J. G. Pratt, *Jour. Parapsy.* I, 4, p. 248 *seq.*, or Saltmarsh and Soal, *Proc. S.P.R.*, XXXIX, p. 266 *seq.*

As a matter of fact, my first intention was to use standard 5×5 packs of Zener cards. I had the idea of placing ten such shuffled packs face downwards on some suitable shelf in my study every evening, asking my percipients, working in their own homes, to guess all ten 'down through' for, say, ten successive evenings in each case, and to go on doing this until either a statistically significant overall result emerged or we all gave up in despair.

Fortunately, however, it was strongly represented to me (as, indeed, I had realised for myself) that this would be a procedure singularly lacking in human interest—in fact, about as tedious as could be devised; whereas it is not unreasonable to suppose that, if such phenomena occur at all, they may well be to some considerable extent influenced by the degree of interest, emotion or the like associated with the material used. I was accordingly very ready to adopt the view that it would be better to use a type of material a trifle livelier than the somewhat arid austerity of Dr Zener's symbols.

The use of drawings in one form or another at once suggested itself, partly on account of the considerable degree of success which, by inspection and assuming conditions to have been as rigid as described, appears to have attended many attempts on these lines;[1] partly because I felt that, in any event, a good deal more fun would be had by all than with the dreary task of guessing and scoring cards; while, if the worst were to come to the very worst, I should at least salvage from failure a good collection of free drawings which could hardly fail to be of some general psychological interest.

Moreover, this sort of material promised to allow subjects a degree of latitude impossible with the restricted type, and this might, it seemed to me, prove of decisive importance in enabling any real effect there might be to show itself. It might well happen, I reflected, that, with many people if not with all, an impression might have difficulty in reaching consciousness in its original and undistorted form but might readily appear in a disguised or symbolic shape. If this were so, it would presumably lead to null results in the case of cards, particularly perhaps Zener cards with which guesses cannot be partially right, whereas with drawings some element might be recognisable, or the reproduction might represent something more or less closely associated with the original; thus, given a method of assessment which could take account of such modifications without abandoning impartiality, it might be possible to detect genuine cognitions which would otherwise escape notice.

[1] *E.g.* Mr and Mrs Upton Sinclair, Warcollier, and others.

Finally, the decision to insist on the utmost severity of conditions as regards the possibility of 'leakage', by not having the percipients on the premises at all, in most cases, enabled me to introduce what I believe may have been a valuable relaxation by allowing them to make their attempts in the familiar surroundings of their own homes, instead of coming to a laboratory or to a stranger's house, and to do so, within fairly wide limits, whenever they felt most inclined.

Thus the final plan, which was followed in all experiments except the second (*q.v.*), may be summarised as follows:

Each experiment lasted ten nights; on each night a fresh drawing, made by either myself or my wife, was put up at 7.0 p.m. in my suitably curtained, etc., study, and left there till 9.30 a.m. the next morning; the subjects of the drawings were determined immediately before their production by a substantially random method; the percipients were allowed to make their attempts at 'reproduction' wherever they happened to be and at any convenient time within the indicated limits.

This procedure, which appears to combine the maximum of freedom for the percipient with the utmost rigidity of experimental conditions has yielded results which I cannot regard as other than extremely satisfactory.

3. *Arrangement of Discussion:* In dealing with these experiments, I shall first give, in the next main Section, a detailed account of the experimental procedure with especial reference to the precautions taken to ensure that no knowledge of the drawings could be obtained by normal means. In Section III, I shall discuss the question of assessment generally, and shall describe the three methods, of which the first two were abandoned after trial, which were actually used. In Section IV, I shall first present the results of the Main Calculations, by which the success or otherwise of the experiments must be chiefly judged; and shall then go on to discuss such other points of interest and importance as have been investigated at the time of writing (Dec. 1939). In Section V, I shall give some account of a kind of dummy experiment intended to serve as a control of the outcome of the experiments proper. Next, I propose to try to save time and trouble all round by anticipating some of the more obvious criticisms; and I shall conclude by discussing in a tentative manner the conclusions which it seems legitimate to draw from the results so far obtained.

4. *Two categorical Statements:* Remembering how often I have been distressed by the omission of other workers to give essential facts, and the allegations of improper selection that have not

infrequently been made against them, I wish to make the two categorical affirmations following:

A. The whole of the work of all the percipients who took part in Experiments I to IV B here discussed has been included, except (*a*) two sets eliminated by Dr Thouless from Expt. I because they contained clues which might have been helpful to the judge, and (*b*) the masterpieces of two persons who sent in ten completely blank sheets each in Expt. IV. As none of my original drawings was intended to represent the Bellman's Chart, I had no scruples about consigning these, unhonoured and unsung, to my wastepaper basket. The Private Experiments, not reported here, consisted of eight ten-drawing tests, each conducted in a single evening, with five percipients selected on account of their success in one or other of the main experiments. My wife (six times) and I (twice) also acted as percipients in these, as a matter of interest, and the results will be discussed in due course. In addition, a fortuitous visitor was on one occasion invited to participate: but his results will not be counted. Thus we have a maximum of 170 drawings not here discussed, as compared with 2,193 which are. This is a negligible proportion anyway; but as a matter of fact, the 170 are, by inspection,[1] quite up to the general standard, if not better.

B. To the best of my knowledge and belief (and, as will be seen, it would be very strange if I were in error) only two persons other than my wife and myself have ever entered my study while an original drawing was displayed. One was the occupant of the flat below; he was present for only a few seconds and declares (I have no doubt correctly) that he did not notice the drawing at all. The other was the lady who attends to our domestic needs; she, on two occasions during Expt. IV, came into the room before I had taken the drawing down in the morning. It is not, of course, mathematically excluded from possibility that she may represent an extensive system of espionage, and some people would have us believe scarcely less fantastic propositions; but I do not think the suggestion need be taken very seriously.

5. *Acknowledgements:* The reader will soon see for himself how deep and extensive is my indebtedness to others. I believe that I have duly acknowledged (in the appropriate passages of the text) the many particular acts of help received; if anywhere I have inadvertently omitted to do so, I can only extend here my regretful apologies to whoever may have been so neglected.

But in addition to these specific acknowledgements, I must

[1] *I.e.* without applying formal tests of significance. The words 'by inspection' are used in this sense throughout.

express my more general though not less sincere gratitude to the members of the Cambridge Committee, Professor Broad, Dr Thouless and Mr Oliver Gatty, who have aided, abetted and supervised my labours ; to the first named in particular I am especially indebted, not merely for the several long and tedious tasks he has undertaken, but for much personal as well as official encouragement and support. I am also very deeply obliged to Dr J. O. Irwin, who kindly consented to join the Committee in October, 1939, for invaluable guidance and help, which it has been impossible adequately to acknowledge in the text, in the matter of statistical treatment. I am also much indebted to Dr E. J. Dingwall, not only for constant and stimulating criticism, but for moral support and practical help at many stages of the work. To Mr M. T. Hindson, whose perspicacious judging has been a factor of prime importance in the success of the work, I must pay a very special tribute, and similar acknowledgements are due to Mr H. F. Saltmarsh, who undertook the equally laborious task of 'control marking'.

Last, but scarcely least, my thanks are due to the percipients, without whose disinterested co-operation nothing could have been done at all. Theirs has been the somewhat thankless task of doing a not very interesting job for the benefit of someone whom they usually had never so much as heard of, and without even having, in most cases, the trifling satisfaction of knowing what degree of success had attended their efforts. I hope that any of them who may read this will realise that it has been quite impracticable to render individual reports, or even thanks, except in a very few special cases, but that their good offices have been none the less appreciated.

SECTION II

Experimental Procedure

Preliminary : I have found it a good deal more difficult to write this Section in anything like a coherent form than might be expected. It is of course easy enough to describe factually what I did, and what precautions I took to avoid this pitfall or that ; but it is not nearly so easy to make clear why I did it, without entering upon a variety of digressions which are now irrelevant and out of date.

The trouble is that, in order to keep things moving at a reasonable speed, it has usually been necessary to plan experiments in advance before the outcome of those already performed was fully known or adequately digested. If I had not done this, which involved a certain amount of would-be intelligent anticipation, I should probably still be trying to squeeze the last drop of juice from Expt. I, and still not quite sure that there was any juice to squeeze.

On the other hand, the procedure had the drawback that probably no experiment would have been planned quite as I did plan it, if I had known at the time what I knew later. Consequently, although certain features, notably that of preserving completely watertight conditions, are common to all, the policy behind them has had to be to some extent modified as the work progressed and as the nature of what appeared to be happening was more fully appreciated.

In particular, I originally intended to assess my results by the method of matching the drawings made by the percipients in any experiment against the originals used in that experiment. This method, which is discussed at some length below, is based on the assumption that if a percipient, as the result of a ' paranormal ' cognitive process obtains a correct impression of an original which he cannot see, he will do so (or at least is most likely to do so) on the same occasion as that on which the said original is displayed. This assumption is now known to be untrue, at any rate so far as the material and percipients involved in these experiments are concerned ; but realisation of this was delayed by the fact that the first ten sets of drawings examined were exceptional in showing a significant positive result [1] when this method was used. Naturally enough, therefore, the second and third experiments were designed primarily with a view to continuing with this technique ; and both parts of the fourth were arranged so as to permit of it, though the experiment as a whole was intended to be based on a comparison between the two parts, each taken *en bloc*, rather than on the matching of drawings against originals within the parts.

In these circumstances, I have decided that the best plan will be as follows : In the present section I shall describe the experiments, mainly from the point of view of procedure and precautions against leakage, adding only a few notes on the outcome by way of a kind of running commentary. In Section III I shall deal with the three methods of assessment which were tried out at different stages, giving fairly full results for the first two, as a matter of interest. Only after this shall I embark on a discussion of the results obtained by the third method, which alone are submitted as positive evidence for the occurrence of paranormal cognition.

Experiment I ; 1, *General :* So far as the points of procedure and technique here considered are concerned, this first experiment is so much the prototype of all others except the second that it will be necessary and sufficient to describe it in detail, indicating only where

[1] Cf. p. 72 below.

necessary the details in which Expts. III, IV A and IV B differ from it.

The general procedure has already been outlined and may be summarised as follows :

On each of ten successive evenings, beginning on that of Feb. 1st, 1939, a different simple drawing, made by either my wife or myself, was exposed at 7.0 p.m. in my study (suitably curtained and guarded) at 5 Fitzwilliam Road, Cambridge, and was left in position till 9.30 a.m. the next morning. Percipients were asked to draw, in books provided for the purpose and at any time within the period of exposure convenient to themselves, the best ' reproduction ' they could manage of what they thought each drawing represented, or (which probably came to much the same thing) whatever came into their minds when they made the attempts.

Inasmuch as it was at this time intended to assess the degree of success achieved by the method of Forced Matching,[1] it was vitally important that the judge, in this case myself, should have no normal knowledge of which drawing by any percipient was intended to represent which original. Accordingly, the books were handed in by the percipients at the end of the experimental period to Dr Thouless, who detached the sheets from them, assigned to each sheet a suitable ' code ' number, and shuffled them before passing them to me.

The results of the matching, which started by appearing very promising but ended by giving no significant result, will be discussed in the next Section. At present we are concerned only with those points of experimental technique which are of fundamental importance irrespective of what method of assessing the results is finally used. The following details should be carefully noted and should serve to make all clear.

2. *Percipients' Books and Instructions :* Each percipient was provided with a specially printed book consisting of a cover and ten pages measuring 13″ by about $8\frac{3}{4}$″. These pages were perforated about $\frac{3}{4}$″ from the left margin, so that when they were torn off at the perforation they were just foolscap size, *viz.* 13″ × 8″. Suitable spaces were provided at the top of each sheet for the percipient's name, the hour of making the attempt, the code number to be inserted by Dr Thouless, and for notes by the percipient as to his degree of confidence and the occurrence of visual imagery. Another space for general notes and impressions was ruled off at the bottom, leaving an area of about 7″ × 8″ for the actual drawing. The ordinal number of each sheet was printed on it to the left of the

[1] Cf. Section III, 2.

perforations. Full instructions for the percipient were printed on the cover ; these are reproduced in Appendix I. To guard against the possibility that some percipients might press so heavily on the paper as to indent the sheet below, and thus give some clue to the order in which the attempts were made which would vitiate the process of matching, a sheet of stiff card, about ·5 mm. thick, was provided with each book, and percipients were instructed to insert this between the sheet they were using and the next. No such indentations were in fact noticed, but I think the precaution was worth taking.

3. *Photograph of the 'setting' :* In order to give the percipients some idea of the setting of the experiment, and to form some kind of a link between them and the location of the drawing to be reproduced, photographs were also distributed showing the relevant parts of my study with a blank sheet of paper pinned in the position to be occupied by the drawings in the course of the experiment. I am much indebted to my friend Mrs Ramsey, of Ramsey and Muspratt, for the trouble she took in this matter. In the later experiments it was not always possible, owing to irregularities of supply, to ensure that every percipient had a photograph to himself ; but most had, and I think there were very few, if any, who had no opportunity of looking at one.

4. *Percipients :* A total of 37 percipients took part in this experiment, and the number was made up as follows :

(*a*) The bulk of the group was formed by 27 students from Dr Thouless's lecture class, all of whom were training for the teaching profession. Of these 19 were women and 8 were men, and their ages ranged from about 22 to 25, though no exact data were sought ; the other participants were appreciably older than this.

I am particularly obliged to Dr Thouless for setting the ball rolling by obtaining the co-operation of these percipients, as well as for undertaking the work of randomisation already mentioned.

(*b*) The S.P.R. was represented by Mr and Mrs Salter, Dr and Mrs Thouless, Mr and Mrs Tyrrell, Professor Broad, and Dr E. J. Dingwall.

(*c*) In addition, two ladies resident in Cambridge, friends of my own, were induced to take part.

5. *Production of the Originals :* To determine the subject matter of each drawing I opened a copy of Chambers's Mathematical Tables at random, noted the last digits of the first three or four entries encountered,[1] turned to the corresponding page of Webster's

[1] 'Three or four' because the terminal digits of the first four entries met with might form a number greater than the number of pages in Webster.

Dictionary, and took as the subject for the drawing the first reasonably drawable word found on or after that page.

This method was by no means perfect; in particular, it led to the use of certain originals which I later came to regard as unsuitable for the purpose, on account of their vagueness or unfamiliarity. But it served the purpose for which it was intended, namely that of ensuring that no percipient could possibly forecast by rational means what the nature of the drawing would be, and of guarding against the effects of possible coincidental thinking prompted by contemporary events.

In general, the drawings were made between 6.30 p.m. and 7.0 p.m. on the evening each was to be used; but on one occasion (Feb. 9, 1939) the necessary absence of both my wife and myself [1] during the afternoon and evening led to the drawing being prepared at about 1.30 p.m. The room was, of course, carefully curtained and locked (see below) from that time onward.

6. *Nature of the Originals:* The pages in Webster found by the above described procedure and the words selected for illustration, together with notes on the choosing, are given below:

1. p. 323: 'Bracken sickness' was rejected, a BRACKET, illustrated in the dictionary, was drawn.

2. p. 2886: The first word on the page is Water Ox or Water Buffalo; a picture of a horned bovine animal (not conspicuously of the genus *Bubalus*) was drawn and labelled BUFFALLO (*sic*).

3. p. 385: Various words such as Embalm, Embank, Embark, Embarrass and Embassy were rejected, and an illustration of an EMBATTLED FESS (heraldic) was drawn.

4. p. 1496: A considerable number of words such as Maniac, and others beginning with Mani- were rejected; but Manicure suggested hands, and a left HAND was drawn with fingers spread.

5. p. 632: A great number of compound words beginning with Cross was passed over, and an illustration of CROSS STITCH was copied.

6. p. 2811: Vacillator, vacillatory, vacoa, and a number of words beginning with Vacu- were rejected; the illustration given of a Vacuum Bottle was judged too complicated for the purpose, and an ordinary BOTTLE was drawn, with a label marked VINO, by way of preserving the V.

7. p. 969: Flite (Miss) and various words in Flit- were passed over. FLITTERMOUSE was illustrated by a Bat with outstretched wings.

8. p. 1644: The first word was Net Blotch. H.S.C. decided to illustrate a NET and, to make it more interesting, drew a sketch of a man pulling a net with fish in it out of water.

[1] Hereinafter referred to as H.S.C. and W.W.C., respectively, when convenient.

(This subsequently proved to be a somewhat unfortunate original, for opinion was considerably divided as to whether the man, the fish, the waves, the net or the beach should be regarded as the principal feature.)

9. p. 1519 : Two earlier pages were rejected as providing nothing suitable. On this page, the illustration of a Meal Moth was rejected as being too like the Bat already drawn, and finally an illustration of a Meal Worm BEETLE was approximately copied.

10. p. 97 : The word ANCHOR was the first suitable for representation, and a picture of an Anchor was drawn.

The drawings illustrating these words were drawn by W.W.C. and H.S.C. alternately, the former starting with No. 1. They were done on sheets of white paper substantially identical with those issued to the percipients and were of a size to fill the $7'' \times 8''$ space more or less completely. All except the last were line drawings in Indian ink made with a broad nib ; in the case of the Anchor, the outlines were filled in with ink, thus producing the effect of a silhouette with shaft and limbs about 10 mm. broad.

Whatever objections may be made either to the objects finally selected or to the method of selecting them, and I have already said that I do not consider either to have been ideal, I trust that all will agree that there was no possible means whereby the percipients could possibly have inferred what the originals were, or even what class of object they were most likely to represent. This is all that matters from the evidential point of view, and such questions as whether the process of selection was truly 'random' in a strict mathematical sense are irrelevant from this point of view.

Personally, I do not now consider the method to be a very good one. I think it would be much better to use an artificial dictionary consisting of words specially picked for their suitability, and to apply, perhaps, a more convenient and more truly random method in selecting from it. In Expt. VI, for example, which will be reported later, I have used a list of 216 'suitable' words, arranged in six blocks, six rows and six columns, so that selection can be made by throwing three dice.

As regards 'suitability' : Speaking quite provisionally and from inspection only, I am now fairly sure that there is usually no question at all of percipients in any sense *copying* the original ; and it seems as if it is the 'idea' rather than the form of the drawing that is cognised—though admittedly the word 'idea' is unpleasantly vague. If this is so, then the first criterion of suitability is that the idea should be reasonably familiar, for otherwise it will not be recognised and cannot be reproduced ; while the second, I think, is that it should be unambiguous. But this is a digression.

7. *Location, etc., of the Originals :* The room in which the originals were exposed is a kind of study-bed-sitting-room on the first floor of a house looking south over a relatively unfrequented road. There are no houses immediately opposite, and the nearest that could be said to ' overlook ' the room is at a distance of, I suppose, about half a mile. In addition to the ordinary defences of the house, the outer door of which is always locked in winter, a Yale type lock was specially fitted to the door of the study at the beginning of the experimental period, and the keys did not leave the possession of my wife and myself. The precaution was highly supererogatory, but I made a practice of always locking the door whenever I left the room for more than a very few minutes, and even after retiring for the night. The chance of anyone making an unauthorised entry during any period of exposure may be regarded as completely negligible.

The curtains with which the room is normally provided are sufficiently opaque, as I have tested by careful observation, to prevent anyone seeing from the road outside so much as whether there is a sheet of paper in the position where the originals were placed—let alone distinguishing any design there might be on it. As a further precaution, however, an additional thickness of fairly heavy rep was drawing-pinned over the lower half of the window, through which alone a glimpse of the drawing might be supposed to be obtained, during the exposure periods. This was always put in position, and the ordinary curtains drawn and secured over it, before the original was displayed.

Each original was pinned in turn to the centre of the top shelf of a bookcase which stood against the wall to the west of the window ; this brought the upper edge of the paper to a height of about 5′ 8″ above the floor. The room was not prepared in any way, except by taking down two or three portraits from the wall near the bookcase and removing (subject to the one exception noted below) a calendar which normally hung just below the position selected.[1] The portraits were not removed for the later experiments, though the calendar was.

In the case of this experiment only, intending percipients from Dr Thouless' class were invited to visit the room before the experiment started ; but only two did so.

[1] It is not uninteresting to note that on one occasion I rehung this calendar for reference during the day and inadvertently omitted to remove it in the evening. On the night in question, and on no other, an excellent picture of a calendar of the same general type and proportions as mine was drawn by one of the percipients.

At the end of each exposure period, namely at 9.30 a.m. on the morning after exposure, or a little later, I was careful to remove the drawing and lock it away in a steel box before any 'outsider' had access to the room or the curtains were drawn back. With the trivial exceptions noted in the Introduction above, I can say with complete confidence that no one except my wife and myself saw, or could have seen, any of the originals during the course of the experiment or before the great bulk of the books had been received by Dr Thouless.[1]

Finally it may be just worth noting that the lights were not left on in the study if no one was in it.

I need hardly say that both my wife and I were scrupulously careful not to mention or hint at the nature of the originals to anyone at all during the course of the experiment, or to make any remark that might give a clue to their nature. This was easy enough, for we were not at that time acquainted with any of Dr Thouless' students, while our contacts with the other percipients were very slight.

8. *Concentration, etc.:* No special effort was made by us to 'concentrate' on the originals during exposure, nor were any instructions to this effect given to the percipients. But the original for each evening would naturally be more or less in the minds of my wife and myself, while the position selected ensured that I at least would be fairly often reminded of it.

While on this topic, I may record the wholly provisional and personal impression that attempts at 'concentration' by percipients are likely to do more harm than good, except in so far as they denote no more than trying to free the mind from thoughts of which the origin can be identified. In the fourth experiment, for example, more than one percipient reported in such terms as "Complete blank, even after fifteen minutes Intense Concentration".

9. *Results:* As I have already indicated, and shall be obliged to emphasise almost *ad nauseam* later, significant positive results have only been obtained by inter- as opposed to intra-experiment comparisons. These will be fully described and discussed in due course, while some account of the results of the matching technique will be given in the next Section. There is accordingly not very much to be said here, particularly as we cannot say whether an experiment

[1] The slight reservation implied in the last sentence is necessitated by the fact that three sets of drawings came in very late, and a few people, not in touch with the percipients concerned so far as I am aware, were shown the originals before these sets arrived. I mention the point only for the sake of meticulousness, for it certainly has no bearing on the results.

of this type is successful or not, taken as a whole, until we have something to compare it with. But a few first impressions may be worth recording, if only as a matter of historical interest.

Speaking personally, I shall not easily forget the thrill I received when I opened the very first set of the first batch of randomised drawings passed to me by Dr Thouless and found a fine sketch of a Hand (Original No. 4) backed up by an unmistakeable Fisherman and a kind of slender Jug, which were pretty good shots, in the circumstances, at Net and Bottle respectively; and the next set contained a battlemented archway, which was by no means bad for Embattled Fess. Nor were these mere isolated examples, for throughout the 37 sets of the experiment H.S.C. and I kept on finding unmistakeable 'winners' of one kind and another, which soon made us feel that there was something going on which 'mere chance' would be unlikely to account for. Of course we knew very well that these impressions were purely subjective, quite likely to be due to wishful thinking, and in no sense evidential. It was only a matter of personal judgement that made us think it unlikely that people would have drawn as many Hands, Cows (for Buffalo) and so forth as we found, if there had been no Hand or Buffalo among the originals; but it is perhaps just worth recording the fact that our immediate reaction to the drawings, before we in any way 'knew the answer', was that the experiment had been very definitely a success.[1] This feeling was confirmed and enhanced when we learned the result of our matching of the first ten sets; it dwindled to little more than a conviction based on a belief in the soundness of our own judgement as that result was gradually whittled away to nullity by the outcome of subsequent matchings; but it has been more than justified by the application of the method of 'palpable hits' and inter-experiment comparison.

Experiment II; 1, *General:* At a very early stage of the investigation it had been suggested that percipients might be able to match their drawings for themselves against the originals they were intended to represent, or which had in some measure influenced their production, a good deal better than any outsider could do. This seemed reasonable enough, for it might very well happen that the person who produced the drawing might recognise some element in the original which had formed part of his impression, but had been imperfectly portrayed; or he might have personal associations, unknown to the experimenter, which would lead him to connect one of his drawings with an original in a way which no one else could do.

[1] At this stage, it will be understood, we had no information as to which drawing of any set was intended for which original.

Since at this time (Feb. 1939) I was still proposing to rely on the method of matching for the assessment of results, I was naturally anxious to see whether this was the case.

It was accordingly arranged, by the courtesy of Professor Bartlett, to carry out an experiment on Feb. 11th, 1939, in the Cambridge Psychological Laboratory, with the contemporary 'Part II' Psychology class. I am very particularly indebted to Mr Grindley for organising this experiment, as well as to others mentioned below for the parts they played.

2. *Procedure :* Although, in this experiment, the percipients were in the same building as the experimenter during the production and 'exposure' of the drawings, the conditions were not less rigid than those for the other experiments. The class, numbering 7 women and 13 men,[1] was assembled in the practical class room on the second floor of the building, and Professor Broad and Dr Banister kindly consented to invigilate it. The originals were produced by me in a small room on the ground floor and the opposite side of the building (which, it may be noted, is unusually well insulated acoustically) and were done under the supervision of Dr Rawdon Smith and Mr R. C. Oldfield. Communication between the two rooms was maintained by Mr Grindley with the aid of a buzzer and telephone, and the relevant corridors were patrolled by two of the laboratory assistants. When the time came for the originals to be taken upstairs to be matched by the percipients, as explained below, this was done by Mr J. C. W. Craik, who had not been in either room during the progress of the experiment. Even Mr Grindley, who informed the invigilators, and through them the percipients, when to start and stop attempts to reproduce each original, was not actually in the same room as myself but in a partitioned recess, and could not see what I drew. So the possibility of leakage from experimenter to percipients may be regarded as completely excluded.

Each percipient was supplied with a pad of ten $6'' \times 4''$ cards, numbered 1 to 10. Each card had spaces in which the percipient was to enter his name, the identifying letter of the original to which he would eventually match his drawing, and the degree of confidence of the matching.

I began by briefly explaining the nature of the experiment to the class, and at once retired to the room downstairs in which the originals were to be produced. Since at this time I had no idea of what these originals would represent, it was impossible for me to indicate their nature to the percipients, even inadvertently.

[1]This included one percipient who had taken part in the first experiment.

The subjects for the originals were determined as follows: A set of cards, numbered 1 to 10, was randomised by Mr Oldfield and Dr Rawdon Smith, using log tables, and were then inserted by Mr Grindley, face downwards and in a haphazard manner, between the pages of a copy of the Concise Oxford Dictionary, which was also held face downwards. Care was taken to ensure that Mr Grindley did not see between what pages the cards were inserted, and, as already mentioned, he was not in a position to see what I was drawing or had drawn until after I had finished all drawings. Moreover, to make assurance doubly sure, I was careful not to mention the subject of any original aloud or to make any remarks concerning it, though I occasionally wrote down some explanatory comment and showed it silently to Dr Rawdon Smith.

To select the subjects for the various originals, I turned the dictionary right way up, opened it successively at the cards numbered 1, 2, 3 . . . etc., and took the first reasonably drawable or 'illustrable' word that came in each case.

The words found and drawings made to illustrate them were as follows:

1. SPINNING: I drew a Spinning Top.
2. PARNASSUS: Illustrated by a roughly outlined Mountain at the foot of which was drawn a Greek Temple with pillars, steps and pediment.
3. JENNET or small Spanish Horse: A Horse was drawn.
4. EXFOLIATE: I drew some Leaves.
5. BRIM: Illustrated by a sort of Goblet or Chalice with a heavy brim or lip.
6. SHOOTING: I thought of drawing a field gun on wheels, and made a note to this effect on the bottom of the sheet; but finally drew a Sporting Gun with indications of a puff of smoke.
7. ANCESTOR: Illustrated by an Old Man, with a bald head and long beard, leaning on a stick.
8. PRAWN: I drew as good a Prawn as I could, but it might equally well have been a shrimp.
9. STANDARD: I tried to draw the Royal Standard, and produced an unmistakeable flag; but the Lion rampant was more suggestive of a demented monkey.
10. THRONE: I drew a kind of wooden Arm-chair with upright back and a very bad indication of a Crown on the top.

These originals were then randomised by Mr Grindley and Dr Rawdon Smith, were lettered A to J for purposes of identification, and were taken up to the class room by Mr Craik. They were there pinned up in view of the percipients, who were asked to write on each of their cards the letter of the original which they thought it most closely resembled and to grade this assignment α, β, or γ, according to the degree of confidence they felt in it. When they had done this, they noted their matchings on a separate slip of paper and handed in their pads to Professor Broad. Meanwhile, Dr Rawdon Smith, Mr Oldfield, Mr Grindley and I remained *incommunicados* in the downstairs room; but so soon as we had been informed, by telephone, that all pads had been duly handed in, we went up and the true order in which the originals had been produced was disclosed.

3. *Results:* From the point of view of assessment by matching, and particularly as regards the question of whether the percipients could match their own drawings better than an outsider could, this experiment was a most discouraging failure; for the percipients contrived to make only 17 correct matchings between them, which is three below expectation.[1]

On the other hand, when I come to view this experiment in retrospect, it is clear to me that it was of vital importance, and marked the turning point of the whole investigation.

To start with, of course, I consoled myself for the apparent failure by reflecting that the conditions under which the percipients—and, indeed, the experimenter—worked were so different from those of the first experiment as amply to account for any deterioration of results.[2] In the first place, the elaborate precautions taken to exclude leakage, admirable and necessary as they were, inevitably produced an atmosphere of bustle and strain likely to be much more inimical to success than that prevailing in percipients' own homes; in the second, a number of persons forming a group might not behave in the same way as they would if they were functioning independently; in the third, there might be a big difference between producing a drawing at one's own time and inclination, and doing it to order whether one felt like it or not; finally, whereas the trials of the first experiment were spaced at intervals of a day, those of the second were separated by no more than some five minutes. Any of these factors, or perhaps others not enumerated, might easily be held to have militated against success.

But as soon as I began to examine the drawings themselves at all

[1] For slightly greater detail, see Appendix II, p. 138.

[2] As it happened, the first ten sets of Expt. I, which gave a significant result, were matched and the outcome was known, before the second experiment was carried out; thus the drop to a null result appeared a sad contrast to what had gone before.

closely, I received the impression that the appearance of failure was illusory and due to the breakdown of the matching technique rather than to the percipients not receiving satisfactory impressions. So far as superficial indications went, indeed, they seemed to have done rather better than those of the first experiment. As a rough and ready test, we find that they themselves assessed 31 drawings as 'alpha' grade resemblances to originals—an average of 1·5 per set —which was confirmed by H.S.C. working independently, who gave 30; whereas, in the first experiment, H.S.C. and I working together gave 39 'alphas' in 37 sets, an average of only just over 1·0 per set. Since the assignment of 'alphas' depends solely on the personal opinion of the judge, this is far from being conclusive; but there is certainly a strong suggestion that, so far as intrinsic resemblance to the originals was concerned, the percipients of the second experiment were not inferior to those of the first.

But there was very much more to it than that, for inspection left little doubt that the drawings which 'caught the eye' as manifest successes in the second experiment were not of objects commonly drawn in the first, and *vice versa*. Among the second experiment drawings, for example, was a really beautiful Spinning Top—almost a point to point replica of my original—but there was nothing of the kind in those of Expt. I. Again, there were some five or six guns of sorts, or mention thereof, in the second experiment drawings, but nothing at all definite in the first. Again, whereas the first experiment drawings showed about as many hands and cows as there were guns in those of the second, the latter produced no more than a glove and a finger-nail in the one case and two cows in the other.

I am deliberately using indefinite language here, corresponding to the qualitative impressions obtained from inspection; but the conclusion was very strongly indicated that, *if the order of the drawings were ignored and the various sets considered as wholes regardless of the positions of their constituents, the drawings of the second experiment resembled the originals of the second experiment, which they were intended to resemble, much more closely than they resembled the originals of the first experiment, which they were* NOT *intended to resemble; and that the drawings of the first experiment resembled their own originals much more closely than they did those of the second.*

This was an enormous and vital step forward, though it took me some little time to realise its full implications; for it involved jettisoning altogether the natural assumption that success would necessarily take the form of good resemblances being produced synchronously[1] with the originals which they resemble.

If I had not performed this experiment and been impressed by the Spinning Top and the relative profusion of 'Shootings', coupled with the paucity of Hands, and similar observations, I might well have gone on for an indefinite period vainly trying to repeat the remarkable 'flash in the pan' matching success obtained with the first ten sets of the first experiment.[2] But as soon as I realised that the basis of enquiry had to be shifted from comparisons within an individual experiment to comparisons between different experiments, progress became possible; and as soon as I began to study seriously the displacement of drawings from their expected positions, results of major importance began to emerge.

Experiment III; 1, *General:* This was the smallest of the 'mass' experiments so far conducted and, as will be seen later, conspicuously the least successful. Only eleven percipients (4 women, 7 men) took part. These were members of Mr O. L. Zangwill's Workers Educational Association psychology class, to whom he was so kind as to introduce me. I was glad to take an opportunity of having a few minutes' talk with this class on the evening before the experiment started, for I thought that the personal contact so obtained might increase the chances of success; the results suggest, however, that its effects, if any, were in the opposite sense.

2. *Procedure:* The procedure adopted was precisely that of the first experiment, with the trifling exception that H.S.C. did the odd-numbered drawings in this case, and I the even, instead of the other way round. That is to say, ten originals were exposed in my study, in the same position as before, from 7.0 p.m. till 9.30 the next morning on each of ten successive evenings, starting with that of Wednesday, March 8th.[3] Exactly the same precautions as regards curtains and door-locking were taken as before. The books used were some that had been left over from the first experiment, so I need not describe them again; and they carried, of course, the same instructions of which only the dates needed alteration. At the end of the experiment they were handed in to Mr Zangwill, who very kindly carried out the necessary randomisation and numbering of the sheets before passing them on to me.

[1] That is to say, within the same period of time as that during which the original concerned was displayed; or alternatively, showing a one to one correspondence as to order with the original concerned.

[2] Cf. pp. 72 below.

[3] In the interests of strict accuracy it may be noted that these periods of exposure were twice interrupted for an hour or so while I carried out two 'individual' experiments. Percipients were warned not to make attempts during the periods of these interruptions.

3. *Originals :* The originals were selected by precisely the same process, using Chambers's Tables and Webster's Dictionary, as was used in Expt. I. The subjects thus chosen and illustrated were :

> 1. VIOLIN ; 2. A BIRD (supposed to be a 'Corn Bunting', but actually of a non-specific passerine appearance) ; 3. A FISH ; 4. CLEOPATRA'S NEEDLE ; 5. A SKULL ; 6. The Planet SATURN ; 7. A FROG ; 8. FLEUR-DE-LYS ; 9. COTTON APHID (this was blacked in by H.S.C.) ; 10. A pair of SPECTACLES.

4. *Results :* The matching was done by H.S.C. and myself, jointly, as before. I had hoped that we might be able to repeat the success of the early sets of the first experiment ; but, as will be seen on reference to p. 138, this hope was sharply disappointed.

None the less, the drawings sent in were far from lacking in interest. For example, there was one out and out success for the Spectacles, whereas I had found only one pair of spectacles in the 57 sets of the first and second experiments and the 12 sets which, up to this time, had been produced in the Individual experiments not reported here. Again, the association between Saturn and Rings is, in my mind at least, extraordinarily close ; and in these 11 sets there were three instances of rings being drawn, though not on the right day, two being of the jeweller's variety and one a very solid-looking annulus, as compared with only one—and that as a quite secondary ornament on one of the Hands—in all other sets ; that is to say, rings were about 19 times as frequent in these sets as in those among the originals for which Saturn did not figure. Further, there were three pyramids, which may be regarded as pretty closely associated to Cleopatra's Needle, as compared with one rather doubtful example in the other 69 sets mentioned. No one knows better than I that this sort of thing is not coercive evidence ; but it served to support to some extent the view to which the outcome of the second experiment was leading me.

In view of this, and of the fact that three of these percipients showed great promise (one was of outstanding interest) when subsequently tried out under the conditions of the Individual Experiments, it is not at all easy to understand why this experiment should have been so very unsuccessful, not only absolutely but relatively as will be seen in due course. So far as I can judge, the percipients were above rather than below the average in intelligence and good will, while my personal contact with them, if this be supposed potentially deleterious, was scarcely greater than that with the class of Expt. II. On the whole, while content to leave the point for the present as a minor mystery, I am inclined to suspect over-conscientiousness operating by way of a kind of 'reversed effort'.

Experiment IV, A and B: 1. *General:* The results of these first three experiments left certain quite clear and definite impressions on my mind. These were, first, that although I was not yet in a position to prove it formally, successes were being obtained which could not plausibly be attributed to chance; second, that the matching technique was no good for the purpose, because most of the best 'hits' were made on the wrong occasion; third, that the only way to bring out the effect I was sure was there was to adopt some quantitative plan for comparing experiments as wholes, so as to see whether there was, as seemed plainly visible to me, a significant tendency for drawings to resemble their own rather than other originals, when sets of the one and series of the other were considered as wholes.

I also had to face the difficulty, which is likely always to be embarrassing, that I could not decently rely to an indefinite extent on the good offices of other people for scoring results. I accordingly planned a large scale experiment in two parts, A and B, each forming as it were an experiment in itself, and arranged it in such a way that, after suitable randomisation, I could score each set of drawings sent in against both series of originals used, without knowing to which it was supposed to correspond. The plan did not work very well, because, although I used different percipients in the two parts (only one worked in both), which I imagined would keep them satisfactorily distinct, displacement appears to have taken place *between* the two parts just about as freely as it had previously done *within* the previous separate experiments, though the double experiment *as a whole* was highly successful. But we shall consider all this in full at a later stage and need say no more about it here.

2. *Percipients:* This experiment was on a very much bigger scale than any that had preceded it. About 370 books were sent out to various people whom I thought might be willing to participate, and 183 came back.[1] These 183 percipients were made up as shown in the following Table:

Table 1

Part A

Group	Women	Men	Total
1. Cambridge Students -	—	8	8
2. ,, S.P.R. -	7	3	10
3. ,, Residents -	—	2	2
4. Edinburgh ,, -	3	2	5
5. Dutch S.P.R. - -	34	34	68
6. Duke University Group -	6	6	12
Totals - -	50	55	105

[1] This is not counting the two books of ten blank sheets already referred to.

Part B

1. Cambridge Students	-	4	9	13
2. ,, S.P.R. -	-	1	3	4
3. S.P.R. Members -	-	13	4	17
4. Edinburgh Students	-	12	14	26
5. ,, Residents	-	2	2	4
6. Dutch Spiritualists	-	2	12	14
Totals - -	-	34	44	78
Grand Totals -	-	84	99	183

This table is probably somewhat inaccurate as regards the distribution between Women and Men. This is because a considerable number of percipients, despite explicit instructions, omitted to write their full names on the cover of the book ; consequently, as I had not asked them to state their sex in so many words, I was often left in doubt on this point. Provisionally, I have counted all such delinquents as men, on the assumption—which has at least the merit of gallantry—that no woman would be so negligent as not to read the instructions.

My thanks are due to Dr Thouless in securing the participation of most of the Cambridge Students, to Mr Fraser Nicol for the Edinburgh Residents, and especially to Dr Mary Collins for the handsome contribution of 26 Edinburgh Students.

The outstanding features are, of course, the splendid effort of the Dutch participants, and the Duke University Group arranged by Dr Rhine. I am particularly grateful to Mr J. J. Poortman for the immense amount of trouble he took over the Dutch S.P.R. percipients, not merely in circularising his members and in translating my instructions into Dutch, but in translating all the key words in the 'Notes and Impressions' subjoined to the various drawings, so that there should be the minimum of difficulty in judging. I am also much indebted to Mr H. Nont, of the Nederlandsch Spiritualistisch Genootschap, for analogous good offices in respect of his Association.

3. *Books, etc. :* The drawing books used in this experiment were substantially identical in form and arrangement with those used in Expt. I. A few trivial alterations were made in the instructions with a view to extra clarity, but the only important difference lay in shifting the spaces for Name and Age to the outer cover instead of the inner sheets, so that I should have no clues from these sources as to whether the book I was scoring was an 'A' or a 'B'. For the same reason I resisted the temptation to use two different colours for the covers, for I feared that in the process of trimming or the like

particles of cover might be transferred to the pages and thus give an indication of which kind of book it was.

4. *Originals :* The subjects for the originals were selected by the same substantially random method as was used in the first and third experiments. They were :

For Part A : 1. Dodo. 2. Flag (This was drawn as a *black* Flag with a conspicuous *white* Latin Cross). 3. Castle (very ' schematic ' and toyshop-like). 4. Moustache. 5. Stop-cock or water tap. 6. Butterfly. 7. Boot (The dictionary word was Shoe, but this was illustrated by what is commonly known as a Boot in England, so a Boot was drawn). 8. Fan. 9. Balance (A Chemical Balance was drawn). 10. Scissors.

For Part B : 1. Tree (Illustrated by a solitary Tree of indeterminate species). 2. Fish-spear (Commonly known and thought of as Trident). 3. Bench (A kind of Garden Seat was drawn). 4. Geissler Potash Bulbs (Commonly known as Bulbs, *tout court*). 5. Hammer. 6. Ewer, illustrated by a single-handed Jug with a constricted neck. 7. Boat (The dictionary word was Ship, illustrated by a drawing of a full-rigged Ship ; but H.S.C., whose turn it was, thought this too difficult to draw and drew a fore-and-aft rigged sailing boat instead. This was shown in silhouette). 8. Windmill. 9. Arrow. 10. Shell (A more or less Whelk-like Shell was drawn.)

These originals were drawn, as before, by W.W.C. and H.S.C. in turn, the former starting, except that external circumstances made it impossible for H.S.C. to take her turn on the sixth occasion ; W.W.C. accordingly drew both Stop-cock and Butterfly, and H.S.C. did Boot and Fan.

These twenty originals were drawn and put up (from 7.0 p.m. till 9.30 a.m. as before) in two sequences of ten consecutive evenings each, from Wednesday, April 26th to Friday, May 5th, and Wednesday, May 10th, to Friday, May 19th, 1939, inclusive, respectively. I hope I need hardly say, except as a matter of form, that the same precautions were maintained, as regards locking of doors and curtaining of windows, as were used in the previous experiments.

5. *Randomisation :* Despite the failure of the Method of Direct Matching used in Expts. I, II and III, it was decided to persevere with this, in the hope that H.S.C. or I might recapture her or my initial virtuosity. It was accordingly necessary to effect a double randomisation, first of the books themselves, so that I should not be able to tell which was intended for A and which for B ; and second of the sheets in each book, as was done for the books of Expts. I & III.

This very considerable labour was most kindly undertaken by Professor Broad, to whom I am greatly indebted for carrying out the work. Each book was identified by means of a pair of letters, such as *Em* or *Pt*, taken from a suitable key table, in which the A or B character of the book was entered. These letters were written on the original cover of the book, which was detached, and on a temporary paper folder in which the torn out sheets were placed. Another key table, giving a large choice of random sequences of the numerals 1 to 10, suitably prepared from logarithm tables, was also supplied, and the sheets of each book were numbered in accordance with one of these sequences before they were torn out; they were then re-arranged in the order 1, 2, 3, . . . 10, which constitutes effective randomisation for this purpose. Identification was ensured by writing the appropriate two letters in the margin of the table, and the serial number of the sequence used on the cover of the book. As a final precaution against the matching judge being able to draw any rational inferences as to the most probable true order of the sheets from a study of the distribution of digits in the logarithm tables, approximately half the books were numbered backwards instead of forwards, suitable indications being given in the key table.

Since none of this is relevant to the results later presented as evidence, though some of the matching work may be usefully informative, I shall not go into further detail here.

6. *Results:* It seems worth recording that during the period of this large double experiment, extending over more than three weeks, both H.S.C. and I were extremely 'stale' and—not to put too fine a point on it—more than a little bored with experimenting in general and with the necessity of being on the spot to produce an original every evening in particular. Several times we nearly forgot; once we were twenty minutes late (WINDMILL), and I fear that our thoughts throughout were more on an approaching trip abroad than on paranormal cognition. I accordingly half expected that the experiment would be a failure, or at least comparatively so; and if success or failure depended on the conscious mind of the experimenter, I do not see how it could have failed to be affected, even though I was no less anxious than before that it should succeed. But, as will be seen, it was at least as successful as the others, at any rate in the sense that it played its full share in producing the high significant result obtained from the work as a whole.

Probably the most notable features by inspection were: A fine crop of Scissors (9 in the whole experiment, and none previously); Five Balances (including Scales and Steelyards) also unprecedented; Five Boots (only two or three slippers before this); Eleven Shells (counting Snails) as compared with two in earlier experiments; and a relatively great number of Trees, Boats and Jugs (Ewers), though all these had been fairly common.

These features will, of course, be incorporated in the calculation of the main over-all results and there is nothing to be added by gloating over them here, striking as they were when first observed. On the other hand, there was another feature, of a different kind, which appears to me of such outstanding general interest as to deserve a section to itself.

7. *Special Note on Dr Rhine's Group:* In due course I hope to make a complete comparative study of the various groups who have engaged in these experiments, but I have not been able to undertake this as yet. I feel it incumbent on me, however, to put on record here the extremely striking results obtained by the group of 12 percipients from Duke University.

By inspection this group is outstandingly good; in fact I have no doubt at all that it will be found to be easily the best of all which have taken part. At the moment I can say definitely:

1. When tested in a 2×2 table against all percipients who did not take part in Expt. IV A, *viz.*, the percipients of Expts. I, II, III and IV B, it yields an *intrinsically significant* positive result, for we have

	Duke Group	Others	Totals
Hits on Originals of IV A -	18	94	112
Hits on other Originals -	60	729	789
Totals - - -	78	823	901

whence χ^2 is found to be 7·853 with P less than ·01.

2. When tested against all other percipients who did take part in Expt. IV A, it yields a *significantly better* result, for we here have

	Duke Group	Others	Totals
Hits on Originals of IV A -	18	40	58
Hits on other Originals -	60	268	328
Totals - - -	78	308	386

giving χ^2 4·204 with P less than ·05.

(*N.B.* In so far as the above calculations are not self-explanatory, the reader must wait for full enlightenment till we have discussed Methods of Assessment and the calculation of results; they may, however, be taken as correct pending explanation of these matters.)

3. It is better, though not significantly better ($P \sim \cdot 09$) than the other percipients of Expt. IV A at discriminating between the Part A and Part B originals.

In view of the great amount and intensity of criticism—mostly stupid—to which Dr Rhine's work has been subjected, I think it is only fair that these very remarkable facts should be noted at the earliest practicable moment. They in no way invalidate, of course, the considerable legitimate criticisms which might be, but usually have not been, brought against the work in question; still less do they guarantee that all the results reported by Dr Rhine and his colleagues or followers, or even any particular examples thereof, are veridical. But they do go a very long way towards substantiating Dr Rhine's main contentions in a general fashion. It will be seen later that the results of my experiments taken as a whole are very significantly positive; and in so far as they may be accepted they indicate the occurrence of a 'paranormal' mode of cognition. But this might perfectly well be true and Dr Rhine's results still due to a mixture of carelessness, practical joking and so forth. On the other hand, the fact that members of the Duke Group did so well in this experiment suggests to the point of demonstration that they possess in good measure the ability revealed by the investigation as a whole; and if this is so it seems to provide circumstantial evidence for supposing that they may also possess the presumably closely allied abilities claimed for some of them by Dr Rhine.

Summary: The important points to note in this Section are not the trivial details, which I record only for the sake of completeness, but the following:

1. In all Experiments, the possibility of any percipient *seeing* any original, or of his *hearing* anything, such as pencil scratchings, involuntary whisperings or the like, was completely excluded.

2. In no Experiment was it possible for any percipient to forecast or infer the nature of any original by any process of rational inference.

3. The process of random selection which assures the second point also assures the elimination of coincidental thinking prompted by contemporary events.

4. If anyone can show that, despite the precautions described, there was, in fact, scope for the operation of sensory clues, rational inference, or coincidental thinking, to any appreciable extent, the experiments are automatically suspect; and if anyone can show that any of these things did in fact take place to a material extent, then the experiments are invalidated. *But, if not, then not*; and, in such circumstances, critics must transfer their attentions either to the Method of Assessment employed or to the statistical treatment.

SECTION III

Methods of Assessment

1. *General :* From various comments I have made in the preceding pages it will have been gathered that I had substantially no difficulty in obtaining what appeared to be genuine positive results when judged by inspection and assessed by the persuasive if intangible criteria of common sense. Fortunately or unfortunately, however, the findings of ' common sense ', like the sayings of the immortal parrot, are not evidence ; and I found it by no means so easy to devise a satisfactory method of assessment which should not only be completely free from any suspicion of bias and proof against all manner of wish-thinking, but also sufficiently sensitive and capable of doing justice to the material.

Somewhat correspondingly, I find that whereas most people with whom I have discussed the matter have little difficulty in understanding the necessity for the precautions taken against leakage, and their efficacy, they tend to go astray so soon as questions of judging and scoring arise. I shall deal faithfully with some of the commoner pitfalls and fallacies at a later stage, but it will be desirable here, as a preliminary to what follows in this Section, to consider the problem in general terms.

The difficulty arises, both technically and in the mind of enquirers, with the transition from the restricted to the free type of material. Almost anyone can understand that there is just one chance in 52 of guessing correctly a playing card drawn from a normal shuffled pack ; or that, if a pack of 25 Zener cards contains 5 specimens of each of five varieties, the chance of any guess being right is one fifth, and five the most probable number of successes in 25 trials. And most people can at least grasp the idea that it is possible to calculate the probability of any given number of successes arising, in these circumstances, by ' chance alone ' or ' pure guesswork ', even though they may be incapable of performing or even following the requisite calculations themselves. But so soon as we begin to deal with drawings, which may be of anything under the sun, where there are no such convenient antecedent probabilities to guide us, and where opinions may very well differ widely as to what constitutes a ' success ', they seem to imagine that the whole business must necessarily degenerate into a mere matter of opinion to which the application of precise methods is impossible.

This is very far from being the case, and the various methods described below, though of widely differing practical value, are all as logically impeccable as any that have been used in the assessment of restricted material. Indeed, the boot is, if anything, rather on the other leg; for the antecedent probabilities mentioned above are not, as is commonly but erroneously supposed, god-given *a priori* certainties: they are hypotheses based on assumptions (usually justifiable in the circumstances) to the effect that the packs of cards, etc., which are used approximate very closely to ideal packs and suffer negligibly from defects, such as differences of stickiness, which might cause their behaviour to diverge from the ideal pattern. I think I am right in saying that all cases where these facts do not, in principle, need to be taken into account are special cases—e.g., that of guessing all the cards in a pack of known composition.

In practice, however, scoring situations are pretty sharply divided into two types, in the first of which there can be no doubt as to what constitutes a success, while in the second there may be. In the first, the scoring is truly objective, so that it does not matter whether the judge or scorer 'knows the answer', provided he is accurate and honest; whereas in the second it is imperative, as a rule, that he should not know, lest his personal prepossessions should bias the outcome. All experiments with playing cards or Zener cards fall into the first class, and all experiments with free drawings into the second. There can, for example, be no doubt that a success has been scored if the Queen of Hearts is guessed when the Queen of Hearts is drawn, or that a guess is a failure if the percipient says 'Square' when the card in question showed a Star. Even if we take partial successes into account, giving so many points for rightness of suit, colour, number, and so forth, the procedure remains perfectly objective, for there can be no reasonable doubt that a percipient who guesses Nine of Diamonds is thinking of a red card, so that there can be no question, once the system of scoring has been settled, as to how many marks he should be given in any particular case.

But the moment we turn to unrestricted material, such as Drawings where the percipient is not asked to say *Which* of a number of known things the object to be cognised is, but *What* it is, the situation becomes very different, and is still more so if the percipient is required to draw what he thinks it is instead of stating its nature in a word or two. For in these circumstances the question of whether a success has been scored or not may easily be a matter of opinion or even of acute controversy. Is it to be counted a success, for example, if the original is an Arch Bridge and the drawing is a Suspension Bridge; or if the original is a Monkey and the drawing a Gorilla, or a Lemur or a Baboon? Sceptical purists might say No, wishful enthusiasts would say Yes ; and the only thing certain is that, regardless of which might be the more reasonable view, no assured conclusion could be reached.

It is accordingly clear that, unless our efforts are to be stultified before we start, we must base our enquiry on a somewhat wider conception than that of the simple ' right or wrong ' antithesis applicable to restricted material. This, of course, most emphatically does *not* mean that we are to allow any one-sided relaxation of standards such as would allow the wishful to claim mediocre resemblances as successes and to discard others as failures, just as it suited their purpose. On the other hand, it would evidently be absurd to demand an exact point to point correspondence between Original and Drawing before conceding a ' hit ', for this would merely ensure that we should never record a ' hit ' at all, either in the right place or the wrong.

Clearly, our proper plan will be to allow whatever latitude we see fit as regards what shall constitute a resemblance, or ' hit ', but to arrange our procedure in such a manner that this will cut equally both ways and be as likely, if chance alone is operative, to increase the number of hits in the wrong places as in the right. The first is necessary in order to secure any material to work on ; the second is not only necessary *but also sufficient* to guard against warping the outcome in one direction or the other.

It is important to get this last point clear, for one of the commonest delusions in this context is that lowering the standard of acceptance (i.e., the closeness of resemblance demanded before a hit is scored) must *ipso facto* favour the production of spurious positive results. I do not know what the origin of this belief may be, but I suspect that it is based partly on a false analogy with experiments using restricted material, and partly on a failure to realise that hits in the wrong place as well as in the right are, and must be, taken into account in the assessment of any material of the type we are discussing. Obviously, for example, in an experiment with playing cards, we should soon generate spurious results if we counted a guess of Knave or Queen as a full success when the card drawn was a King, on the ground that these were court cards and ' very like ' the King. No sane person would do this, of course, without taking into account the fact that the antecedent probability of success has been materially increased ; though it would be perfectly legitimate to do so if appropriate adjustment were made. I suspect that some people cannot get away from the idea that analogous antecedent probabilities must be used in assessing free material and that lowering the standard must be equivalent to the kind of thing just indicated, without the possibility of applying the corresponding correction; but this, as will be seen, is not the case. Alternatively or additionally, persons unfamiliar with this class of work are apt to think only of one side of the situation and to forget that although lowering the standard is likely to increase the number of hits in the right places, it is also likely correspondingly to increase the number in the wrong, just as raising it will diminish the number in the wrong as well as in the right.

Perhaps the point at issue may best be clarified by reflecting that the relation between the original and the drawing, or the card and the guess, etc., is always of a two-fold character; there is not only a relation of likeness but also a relation of position. That is to say, we not only demand of a 'successful' guess or drawing that it shall be 'like' some card or original used by the experimenter; we require also that it shall occur in a position related in some definite way to that in which its prototype occurs. This may, at first sight, appear too vague; actually it is no more than stating our second requirement in accurate if general terms. Usually, of course, we require that the positional relationship shall be one of identity; that is to say, just as we demand that the percipient shall guess Square and not Circle when a Square is concerned, so we insist that he shall do so on the 10th occasion, say, if that is the occasion on which a Square is drawn, and not on the 11th or the 9th or any other. But there is no kind of theoretical necessity for doing this, as regards either part of the relationship. In practice we usually do it because it seems to us more likely on common sense grounds (not always an infallible guide) that if paranormal cognition occurs at all it is most likely to do so in a certain particular form, namely that which will produce a strong positive relation of likeness and an identity of ordinal position between the 'prototype' (original, card, etc.) and the 'reproduction' (drawing, guess, etc.). But this is based on a judgement, not necessarily correct, as to what is likely to happen, and we are perfectly entitled to modify either part of the relationship, or both, in any way that seems good to us, as by enquiring whether there is a significant tendency to guess black cards when red are drawn, and *vice versa*, or whether correct guesses tend to occur seven places later than, or within a range of three places before or after, the prototype. I am not suggesting that it would often be wise or profitable to go out of our way to hunt for peculiar 'configurations', to adopt a convenient term, such as these or others more fantastic; but it would, in principle, be perfectly legitimate to investigate the frequency of their occurrence, if there seemed any object in doing so, provided always that we make the necessary allowances and apply the appropriate safeguards in estimating the probability of their happening by chance.

My purpose in the preceding remarks was to introduce and emphasise the notion that there are more places (positions) than one in which a 'hit' may occur. Colloquially speaking, there are 'right' places and 'wrong' places; the right places are those specified by the positional part of the compound relationship defining the configuration we are considering, such as 'the same ordinal position', or 'within the same experiment', and all other places are wrong. Once this idea is grasped, it is easy to see that what interests us in the general case is not the absolute number of hits scored, which may vary greatly according to the intrinsic popularity, so to say, of the prototype and the strictness or otherwise of the scoring, but the question of whether relatively [1] more hits are scored in the 'right' places than in the 'wrong'. Further, it should be clear that, provided the process of scoring or marking is applied impartially, that is to say without any systematic bias in one sense or the other, any increase or diminution in the total number of hits scored, as produced by lowering or raising the standard, will correspondingly inflate or diminish the numbers of hits in the right and wrong places indiscriminately.[2]

In dealing with restricted material there is, of course, no difficulty about ensuring impartiality, because there can be no difference of opinion about whether a hit has been scored or not; but as soon as we begin to consider Drawings, where acute and perfectly legitimate differences may easily arise, the position is quite otherwise. Cases about which there can really be no two opinions are the exception rather than the rule, and it would be no more than a time-wasting engenderment of controversy if we were to conduct our process of assessment in such a way that any positive results which might emerge could legitimately be attributed to bias on the part of the judge.

There is only one way of ensuring absolute impartiality in these circumstances, and that is by arranging that the judge is wholly

[1] The word 'relatively' is necessary because, as a rule, there will be more wrong places available than right; so that if chance only is at work, the absolute number of hits in the wrong places will be greater than that in the right. What we want to know is whether the right places get more than their chance-indicated share of the hits recorded.

[2] In what follows, I shall frequently use the term 'winners' for hits in the 'right' places, and 'losers' for hits in the 'wrong', the words 'right' and 'wrong' having the meaning given above; I shall reserve the word 'success' for the particular case in which the ordinal position of the reproduction in the series considered is the same as that of its prototype.

ignorant of which places are ' right ' and which are ' wrong ', so that he cannot possibly favour the one category at the expense of the other in his allocation of hits. Under these conditions, the most that wickedness or stupidity can achieve is a voluntary or involuntary sabotaging of the experiment, either by recording so many hits as to swamp any real effect there may be, or so few as to prevent its emergence.

For this reason, no less care has been taken throughout this work to prevent leakage of the relevant information to judges than to prevent leakage of sensory and inferential clues to the percipients.

In the following sections I describe three methods of assessment which have been tried. Only the third has proved satisfactory, but I have thought it worth while to give some account of the other two, partly for the sake of completeness, and partly in order to emphasise the importance of not deciding too rigidly in advance the form which a real effect, if any ' must ' take. The third method is neither more nor less theoretically valid than the first, but it revealed the significance of previously unsuspected facts indicated by inspection of the material ; the first was just as good a way of tackling the job in the light of antecedent ignorance, but since the effect it was designed to bring out was not detectably present it led to null results.

2. *The Method of Forced Matching :* The matching technique discussed below is not new to psychological practice, for it has been fairly freely used in cases such as the connection of character with handwriting where ordinary quantitative methods cannot readily be applied. The principles involved are likely to be more easily grasped by some readers if we begin by considering an illustrative example of this kind.

We will suppose that we want to know whether a graphologist can form, from specimens of handwriting, estimates of the writers' characters which cannot plausibly be discounted as no more than lucky shots ; and we will suppose that we are rightly anxious to eliminate any bias due to our own preconceptions as to the possibility of doing so. We select, say, ten subjects, A, B, C . . . I, J, for experiment and obtain from each a specimen of handwriting which we submit to the graphologist, asking him to make out the best character sketch he can of each person concerned and to give us the ten sketches shuffled up so that we cannot tell which is meant for which person, but so numbered that they can later be identified. At our end, we may either rely on our personal knowledge of Messrs A, B, C, etc., or we induce some competent observer acquainted with them all to prepare ten other character sketches based on ordinary experienee; these may either be randomised or not, but we will suppose they are not. When the graphologist's versions arrive, we eompare them with those of the competent observer or with our own knowledge, and try to pair the two sets off—graphologist's A against observer's A, graphologist's B against observer's B, and so forth; but of eourse, as I have just said, we do not know whieh of the graphologist's sketehes is meant for A and whieh for B, so that we are foreed to rely exelusively on the resemblanees, if any, between his version and that of the other observer. When we have paired off the two sets to the best of our ability, we ask the graphologist for the key to his numbering, and thus ascertain how many correet pairings we have made. We might find this sort of thing for example:

Graphologist's estimate of:	J	C	H	D	E	A	G	B	I	F
is paired with										
Observer's estimate of:	A	B	C	D	E	F	G	H	I	J

Here there are four eorrect pairings, namely those of D, E, G and I, which is a good deal more impressive than it looks, for we should obtain four or more eoineidenees only onee in fifty sueh trials, on the average, if ehanee alone were operative—if, for example, we had simply drawn the two sets of sketehes at random out of two hats.

The point to grasp here is that, if charaeter and handwriting (strietly speaking, as estimated by the partieular observer and graphologist eoneerned) have *no* systematie conneetion with eaeh other, and the graphologist has no other souree of information than the specimens submitted to him, then the matehing proeedure is preeisely equivalent to the hat drawing ritual so far as the ehanees and expeetations of finding eoineidenees are eoneerned. For, *ex hypothesi*, there is no more than a random relation between the graphologist's only guide and the eharaeters he is trying to assess; and even if the observer's estimates were perfeet, the relation of the graphologist's versions to these eould still be no more than random.

It is very important to note here that we are not eoneerned in a situation of this type with the question of how good the graphologist's estimates are, in any absolute sense, but only with whether they are good enough to enable us to identify eaeh as *more* like some one of the other sketehes than it is like the rest.

Now let us replaee the known estimates of eharacter, as made by the observer or ourselves, by the Originals in one of our experiments, and the graphologist's efforts by the Drawings of one of the percipients suitably shuffled and eode numbered so that we do not know whieh is whieh. Then the situation as regards matehing the

Drawings to the Originals is formally the same as that of matching the graphologist's character sketches to the observer's estimates. Most people seem to have no difficulty in understanding the graphological parallel, but many come to grief when they try to think of the drawing-original situation. This appears to be mainly due to an insistent confusion between the probability of the judge correctly matching the *n*th drawing to the *n*th original by chance alone, when he has nothing but the intrinsic resemblance to rely on, and the probability of the percipient drawing an ' X ', or an ' X-like ' object, on the same occasion as an ' X ' is represented by the original.

However, since the method is well established, and I am not relying on its results for evidential purposes here, there is no need to spend more time on demonstrating its validity.

The procedure itself is simple enough. The judge (in these cases usually H.S.C. or W.W.C.), with the originals of the experiment concerned before him and knowing, as a rule, the order in which they were used, receives the drawings of each percipient arranged in a random order by some third party (*vide supra*) and code-numbered for subsequent identification. He accordingly has no external clue to guide him in deciding which drawing was intended for which original and must rely solely on such intrinsic resemblances as he can detect. His task, of course, in respect of every percipient, is to assign each of the (normally) ten drawings to whichever of the originals he considers it most closely resembles, or is least unlike. Sometimes this is easy enough, as with the Hands and Cows of Expt. I, which could be assigned to Hand and Buffalo without hesitation, or with the Tops and Guns of Expt. II and Scissors and Balances of IV A ; but more often it is necessary to look for some more or less recondite similarity of form, or for associations of greater or less remoteness, in order to come to a decision ; for example, in Expt. I, we frequently assigned drawings of Boats to the original Anchor, on the ground that both were ' nautical ', or vases, etc., to Bottle, as being ' containers '. The judge is obliged, it will be understood, to assign every drawing to *some* original (hence the name ' Forced ' Matching) and it not infrequently happens that the utmost ingenuity fails to discern any plausible resemblance at all, so that one or more drawings are finally placed by elimination or simply at random. After all the assignments have been made, on one basis or another, and duly noted, the occasion on which each drawing was actually produced is ascertained by reference to the key held by the third party aforesaid, and the number of correct matchings made in the case of each percipient is counted. It is then only a matter of some not very advanced mathematics to determine the probability of the observed number in each case being due to chance alone. I need not inflict these on the reader here, but the following Table, for which I am indebted to Dr Thouless, shows the probabilities of making the various possible numbers of correct matchings for series of ten originals and ten drawings, such as we are concerned with here. The column headed R gives the number correct, which may have any value from 0 to 10 inclusive, except 9. Under N is shown the number of ways in which 10 drawings can be arranged so that R are rightly matched[1]; column P gives the probability of getting exactly R right by chance alone, while P′ shows that of obtaining R *or more* right by chance, which is what interests us here.

Table 2

R	N	P	P′
0	1,334,961	·367,9	1·0
1	1,334,960	·367,9	·632,1
2	667,485	·183,9	·264,2
3	222,480	·061,31	·080,3
4	55,650	·015,34	·019,0
5	11,088	·003,056	·003,66
6	1,890	·000,520,8	·000,600
7	240	·000,066,1	·000,079
8	45	·000,012,4	·000,013
9	0	0	—
10	1	·000,000,276	·000,000,3
Total :	3,628,800		

These figures apply to any single set of ten matchings; when, as in any of these experiments, we wish to combine the results from a plurality of percipients, it is more convenient to apply the Stevens matching formula (Cf. pp. 83-84 below), from which it is easily seen that the expected number of correct matchings is equal to the number of percipients, with variance the same; for in each case the expectation is 1 with variance 1; and the expectation and variance for the whole group are equal respectively to sum of the expectations and the sum of the variances of the constituents.

The actual results of matching the first three experiments in this way are summarised in Appendix II, from which it will be seen that, when all results are taken together, no conclusion of serious evidential value in favour of a cognitive effect can be drawn. There are, however, one or two points of interest which are worth mentioning.

[1] These necessarily add up to 3,628,800, or ‘ factorial ten ’, which is the total number of different ways in which ten things can be arranged.

Chief of these is the very remarkable score of no fewer than six correct matchings obtained with one of the percipients of the first experiment. Reference to the Table just given will show that such a result, or better, could be expected, on the average, only about six times in 10,000 such trials, or once in about 1,600 attempts. This, of course, would be highly significant if it were the only observation to be considered, and the probability of its occurring by chance even as the best among the 68 sets of the first three experiments is no more than 1 in 25, which would itself be considered tolerably significant in normal contexts.

But the attendant circumstances rendered this result even more remarkable than it appears at first sight. The percipient was the fourth whose drawings were matched by H.S.C. and myself, and it so happened that we completed a batch of ten, and ascertained the number of matchings correctly made, before going on to deal with the remaining sets. In this first batch of ten, we scored as many as 20 successes, which is an excess of 10 over expectation with standard error 3·162. Such an excess or greater would occur only once in about 660 such groups of 10 by chance alone, and no more than once in almost exactly 100 times as the best of 6·8 such groups (i.e., in the whole of the material so far dealt with by matching). It is quite legitimate to single out this group for special consideration, because it is, so to say, isolated from the rest by considerations other than its high score: it was the first we matched; we stopped when we had done it; and we did not resume the work till after we were aware of the success of our efforts; thus the group is sharply distinguished from all others on both chronological and psychological grounds. The success achieved *may*, of course, have been no more than a fluke, either in its entirety or as regards the six-success set only, and if anyone wishes to write it off as such on the ground that we failed to repeat it, I cannot reasonably object; but it will be his loss, not mine. I might have felt constrained to do so myself if the Method of Palpable Hits had not yielded the very much more significant results reported below. As it is, I think it much more probable that it is genuine, and represents an unrepeated (though I hope not unrepeatable) display of what I can only call "insight" by H.S.C., who was almost wholly responsible for the matching at this stage. Certainly it was not due to these ten percipients having drawn things more clearly like the originals than others did; in particular, the hero of the six successes produced no unmistakeable resemblance at all, and H.S.C.'s assignments in this case appeared to me (I must confess) to border on the far-fetched and unconvincing. This, however, does not alter the facts or the probabilities, which clearly suggest that the achievement was more in the nature of a *tour de force* than a stroke of luck. If this is so, the implications are of very great psychological interest from two points of view: in the first place it is suggested that a genuine cognitive process may be subject to such distortion as leads to expression in almost unrecognisable form; in the second, it would appear that to detect the relation between drawing and original in such cases calls for something more or other than rational perspicacity can provide; presumably it involves a kind of intuitional process at the same mental level as that at which the distortion or transformation took place.

As a matter of fact, we shall later find evidence for supposing (p. 95) that drawings having no more than a very feeble resemblance to their originals are yet to some extent determined by them, which tends to support this view; and it is greatly to be hoped that H.S.C. may succeed in recapturing her initial virtuosity, the loss of which (if real) is probably accounted for by the feeling that the success achieved settled the question at issue beyond need of further effort.

The other point that requires mentioning is the remarkable tendency we noticed for the best and most convincing resemblances to occur on the wrong occasions. We made it a practice to grade our matchings as α, β, or γ according to the degree of confidence we felt in them, which very approximately corresponded to the degree of resemblance discernible, and the Table below shows the numbers of α's given in each of the first three experiments and how many of them were displaced early, late, or not at all.

Table 3

Displacement

	Early	Zero	Late	Total
Expt. I	9	2	28	39
,, II	11	3	17	31
,, III	9	1	9	19
Total	29	6	54	89

We should of course expect about 9 zero displacements out of 89 awards if chance only were operative, and the difference is not significant. On the other hand the excess of Late over Early displacements is quite definitely so, both for the first experiment ($P < \cdot 01$) and for the Totals ($P < \cdot 02$). This might be due, on the chance-only hypothesis, to the more popular originals (i.e., those more commonly drawn under truly random conditions) happening to have come early in the series. I did a considerable amount of work, by control matchings and otherwise, to investigate this possibility; but concluded that though the effect was probably a real one, it was to some extent due to this cause, while the data available were insufficient to settle the matter decisively. But we shall be dealing with the whole question of Displacement, which is extremely important, at a later stage and by rigid methods, so that I need not go into details of these early explorations here, beyond remarking that these observations went far to support the belief engendered by the success of the first batch of matchings that something other than chance was at work.

For similar reasons I need not describe here the investigations I undertook to test sundry hypotheses, of varying degrees of far-fetchedness, advanced by assorted critics to account for our initial success on normal grounds; for example, the suggestion that percipients might tend to do their drawings later and later as the experiment progressed, and that we might have been unwittingly guided in our assignments by the time data recorded on the sheets. All these yielded null results.

On the whole, I think there is very little to be said for the Method of Forced Matching for this purpose, or probably for any other. Apart from the fact that it is liable to be completely wrecked by the phenomenon of displacement, it does not seem to yield any information that would not be given by a suitable system of marking, while it automatically precludes the possibility of giving recognition to the influence of more than one original on the same drawing, and is hopeless for dealing with multiple or composite drawings. The chief advantage gained is that the assessment of each experiment, and indeed of each percipient, is self-contained, and does not require reference to any other drawings; and it would be of outstanding value for cases, if ever they occur, in which very faint resemblances, such as would probably be ignored in any ordinary system of marking, preponderantly appear on the correct occasions.

2. *The Method of Decimal Scoring.*

The results of the first experiment suggested, and those of the second and third confirmed, that the Method of Forced Matching was not likely to be successful. This was not because the drawings produced by the percipients bore no discernible resemblance to the originals at which the set as a whole was aimed, or only resemblances so infrequent or so feeble as to be of insignificant importance: on the contrary, so far as common sense inspection and estimation of probabilities could tell, they seemed to be representing at any rate the general idea, if not the exact form, of the originals a good deal more successfully than was likely to be due to chance alone—for example, by the appearance of Hands and Cows in Expt. I and of Shootings and Tops in Expt. II. The trouble, as already indicated, was that the most successful reproductions tended to come in the wrong place, so that the Hand displayed as an original on Saturday, say, would appear as a percipient's drawing on Sunday or Monday instead of on its proper day. Since the whole Method of Matching depends on the assumption that there will be preponderantly a one to one correspondence of occasion between an original and the drawing most closely resembling it, it is pretty well bound to be defeated by displacements of this kind ; at the very least, its utility is likely to be seriously impaired. On the other hand, it is far from unreasonable to suppose that an impression subconsciously received might remain latent for some little time until either an internal process of gestation, or the incidence of some extraneous stimulus brought it into the conscious field ; alternatively, a process of gestation in the mind of the agent might be necessary prior to transmission, if the cognition were dependent on telepathic factors.

I accordingly sought for a method which should enable us to answer the broader question of whether the sets of drawings produced by the percipients, taking each set as a whole, significantly tended to resemble the series of originals at which they were aimed (*i.e.*, the series of originals used in the experiment in which those percipients were concerned) more closely than they resembled series of originals at which they were not aimed. The idea, in other words, was to match each set of drawings, as a whole, against two or more series of originals, as wholes (one of them being that at which the drawings were aimed) instead of matching individual drawings against individual originals.

There is, of course, no theoretical reason why this should not be done in the literal manner suggested by the words used above. We might, that is to say, present the judge with the originals of perhaps half a dozen experiments, divided into the six series in which they were actually used, together with all the sets of drawings produced in those experiments, suitably randomised and coded, and ask him to assign each set to the series which, on the whole, he thinks it most closely resembles, working by inspection and qualitative judgement alone. But the practical difficulties of such a procedure are great, as was discovered when Prof. Broad kindly attempted to apply this plan to six sets of drawings and originals produced in the course of the Private or Individual Experiments. Apart from the difficulty of bearing a large number of originals in mind at a time, there is that presented by the conflicting claims of drawings in the same set, of which some may resemble originals in one series, while others resemble originals in other series ; and it may become necessary to decide whether the strong resemblance of a certain drawing to originals in series A should or should not outweigh the fainter resemblances of two or more other drawings to originals in series B. Considerations of this kind necessitated the introduction of some quantitative method of expressing estimated degrees of resemblance, as opposed to making a purely qualitative decision to the effect that one drawing, or set, was more like one original, or series, than the others with which it was compared.

The plan I thought most promising was as follows : Judges were to be given a collection of shuffled and coded sets of drawings taken from two (or possibly more) experiments together with the originals, also shuffled, belonging to those experiments, and were to be asked to assign to each drawing as many points, from 0 to 10 inclusive,[1] as they thought it deserved for its resemblance to any original or originals. In cases where two or more objects appeared in the same drawing the marks which would have been given to any of them if shown singly were to be reduced to an extent corresponding to the relative importance of the object in question in the drawing in which it appeared : To quote from the instructions " If the relevant object shares the drawing more or less equally with one, two, three, etc., other objects of apparently equal importance, it should be given a half, a third, a quarter, etc., of the points it would have been given if it had appeared alone ". This regulation was probably unwise, and was discarded in the Method of Palpable Hits finally adopted ; for further reflection suggested that the important point is not likely to be that of whether the idea depicted in the original is the *only* impression received by or present to the mind of the percipient, but of whether the drawing provides good evidence for supposing that the idea was prominently present in his mind *at all.* If we add up the points allotted in this way to the originals of each series concerned, for each set so scored, we shall obtain totals which will, in general, be unequal ; and if the total for the series at which any particular set was aimed is greater than for that or those at which it was not aimed, we may say that the set has been correctly matched to its own series.

Thus, to take an imaginary example, a particular set from Expt. I,

[1] Hence the name given to the method.

say, scored against the originals of Expts. I and II, might yield results like this :

Drawing No.	Original	Points
1.	Parnassus	3
2.	Hand	10
3.	Horse	5
4.	Buffalo	8
5.	Net	6
6.	—	—
7.	Throne	7
8.	Bottle	10
9.	—	—
10.	Bat	3

Noting that Hand, Buffalo, Net, Bottle and Bat, scoring 37 between them, are originals of Expt. I, while Parnassus, Horse and Throne, scoring only 15 between them, belong to Expt. II, we conclude that in the opinion of the judge the set, taken as a whole, resembles the originals of Expt. I, at which it was aimed, more closely than it resembles those of Expt. II at which it was not aimed. If we have a number of sets scored in this way, we can calculate the probability that any observed proportion of them should be correctly allocated to their own originals as the result of chance alone.

It is clear that the process of allotting points to the originals of the various series concerned according to the degree of resemblance, if any, that each drawing of a set is judged to show to them, and then adding the scores so obtained by each series, is only a roundabout way of 'matching' sets as wholes to series as wholes, which is what we set out to do.

A rough and ready trial of the method yielded promising results ; but exploration soon showed that it had serious disadvantages. The chief of these may be summarised by saying that it proved extremely difficult to frame instructions elastic enough to give room for the exercise of common sense and at the same time sufficiently precise to avoid leaving the result too much at the mercy of the individual caprices of the judge. Such idiosyncrasies could not, of course, systematically falsify the outcome, nor would they be likely altogether to obscure a genuine positive result in the long run ; but, by introducing factors other than the straightforward resemblance or close association of drawing to original, they in effect increase the variance, and may thus inordinately lengthen the 'run' necessary to secure a definite answer to the questions studied. As an example of the kind of thing I have in mind, one potential judge of whom I had great hopes, insisted, in flagrant violation of the instructions, on giving the maximum of 10 marks to Hand for a drawing expressly described by the percipient as representing a fingernail; two others seemed to think that they were engaged in a puzzle picture competition, on the lines of 'In this drawing of a bird's nest find five hidden pirates'; while in another instance it was seriously contended that a drawing of a girl on a surfboard should be given points for Incubator, on the ground that the words 'poule' in French and 'chicken' in American are colloquially used to refer to the female young of the human species. This kind of thing might be of interest in pursuing the more abstruse ramifications of the subject, but so long as we are in doubt as to whether any paranormal cognition takes place at all it can only obscure the issue.

Another and sometimes more serious trouble is that if the objects depicted in one series of originals are appreciably more popular, in the sense of being more frequently drawn under chance conditions, than those of the other series they will tend to attract, as it were, an undue proportion of points, so that only sets resembling the other series so strongly as to outweigh this difference will escape the attraction and have a chance of being allocated to the less popular series. This effect is very marked in the case of Expt. IV B, which contains some highly popular originals, notably Tree and Boat; reference to Appendix III will show how it ruins my scoring of 80 sets from Expt. IV.

Finally, if the method is applied, as I applied it, to drawings and originals taken from only two experiments at a time, it is liable to be extremely insensitive, because so high a proportion as 50% correct assignments might be expected to result from guesswork alone; while, on the other hand, the task of attempting to decide what fraction of a maximum of 10 points should be allotted to which, or perhaps each of several, of more than twenty originals is too difficult to expect any ordinary person to attempt with success.

In view of these handicaps it is hardly surprising that application of the method was next door to being a failure. Mr and Mrs Oliver Gatty, to whom I am very much indebted in this matter, working on the suitably grouped, randomised and camouflaged drawings of the first three experiments, obtained a result slightly beyond the ·07 level of significance, which looked reasonably promising, though hardly a rich reward for the examination of over 650 drawings. But even this was reduced to no better than ·12 when my own scoring of 80 sets from Expt. IV, A & B, was included, and was only raised to a level a little better than ·03 by the incorporation of the 16 sets of the Individual Experiments. Details are given in Appendix III.

These results were disappointing, especially in view of the amount of labour involved, and by contrast with the degree of success apparently visible to the naked eye. It seemed clear to me that the method was at once too delicate and too cumbrous, too elaborate and too insensitive, to be of permanent utility, and that something considerably better would have to be devised.

3. *The Method of Palpable Hits.*

General: This, the third and (so far) last method adopted was designed to overcome the difficulty of Displacement which stultified the first, while avoiding the defects of vagueness and insensitivity which, so far as I could judge, were chiefly responsible for the poor results obtained from the second.

It must be realised that all the time the collection of material and attempts at assessment described above were going on I was continually impressed by the plain, commonsense evidence that the experiments seemed to thrust upon my notice. For example: The originals of the first experiment contained a Hand, and the 37 sets of the first experiment showed at least six obvious drawings of hands, apart from several with plausible claims as partial successes; in contrast, the 31 sets of Expts. II & III produced no more than one glove and one finger-nail, which might perhaps count as one success for Hand between them. For Buffalo, I found five obvious cows in the first experiment, but only two in the second and third together. Similarly, I found five unmistakeable guns, cannon, etc., for Shooting among the drawings of the 20 sets of Expt. II, and only one very doubtful candidate in the 48 sets of Expts. I & III. Again, when I began to examine the drawings of Expt. IV, I found a very satisfactory number of Scissors and Balances, neither of which had appeared at all in the drawings of the first three experiments, together with an apparently undue proportion of Ewers (or similar vessels) and isolated Trees. It seemed to me almost incredible, on common sense grounds, that these effects were solely due either to chance or to the wishful selectivity of my own mind, or that, if they were genuine, it should be impracticable to demonstrate them significantly by some tolerably simple method.

I accordingly determined to abandon, for the time being at any rate, my ideas on giving full scope to possible associations, distortions of form, symbolisms, etc., and to concentrate on the simplest and most straight-forward method of assessment possible. I similarly resolved to discard the niceties of gradation afforded by giving anything from 0 to 10 points to a resemblance, together with the system of scoring in proportion to the importance of the object in the drawing, and to base the assessment as nearly as possible on the simple principle "Do this drawing and that original plainly and unmistakeably portray the same thing? If they do, give one mark; if they do not, give nothing." In other words, only 'palpable hits' were to be counted.

This sounds simple enough on paper, but in practice it is not nearly so easy as might be supposed, and I found it quite impracticable (somewhat fortunately, as it proved) to induce others concerned to adopt and maintain the high standard I originally envisaged, and extremely difficult to adhere to it myself.

In the first place, I realised even before I started, that it would be necessary, to modify the basic rule, so as to cover composite drawings containing more than one object, by adding "or does the drawing provide plain and unmistakeable evidence that the object portrayed in the original was prominently in the mind of the percipient when he made the drawing", or words to that effect; and the introduction of the word 'prominently' at once opens the door to a certain amount of difference of opinion and ambiguity; yet without this modification a number of drawings which common sense would indicate as clearly 'hits', such as horses in carts, or cows standing under trees, might be rejected by too literal a follower of instructions.

In the second place, even the words "the same object" cannot be wholly unambiguous; for example, a drawing of a child might be held to represent Ancestor just as well as the old man I actually drew, for very many children have descendants and all ancestors were young once. In cases such as this it seems necessary to keep pretty closely to the actual drawing; whereas with an original like Net (Expt. I, 8) it is more important to consider what was in the agent's mind (namely, illustrating a *net*) rather than all of what was drawn (namely, a *man* pulling a *net* with little *fish* in it out of the *sea*) for it would be obviously absurd, or so I think, to give a mark to every drawing which shows a man, a fish or the sea, without any indication of the net. On points such as these opinions may well differ, and it does not very much matter what we decide to do, provided our decision is not manifestly nonsensical and that we hold to it with reasonable consistency; indeed, failure to conform to even these conditions would not necessarily prove fatal. It will be understood, or so I hope, that no convention of a *general* kind can possibly produce a spurious positive result; this could only be done by forcing upon the marker some arbitrary *specific* convention based on a preliminary study of the material. For example, I might notice that there was a large number of *keys* drawn among the sets of Expt. IV, as was the case, and then suggest or insist that keys should be given a point for, let us say, Castle ; this, if followed, would lead to a number of unjustifiable winning hits being recorded : but a general instruction such as ' clearly identifiable parts of objects should be treated as if the whole object were drawn ' could do no harm ; for there is evidently no reason why such a rule should increase the proportion of hits registered on originals in the series at which percipients were aiming (*i.e.* ' winning hits ' or ' winners ') compared with those on series at which they were not aiming (' losing hits ' or ' losers '), and it is by the relative frequencies of these, not by the total number of hits of all kinds, that the issue must be judged.

On the other hand, it behoves us to frame our rules with intelligence, because, if we make them too lax, we shall tend to blur whatever pattern the data might otherwise reveal, while, if we make them too stringent, we shall risk having no pattern to blur. Thus, if we gave a mark to Arrow for every drawing with a straight line in it (' straight as an arrow '), and one to Shell for every drawing with a curly line in it, we should be likely so to overweight these two originals with marks as completely to swamp out the kind of thing we are looking for ; while if we insisted on a drawing being an exact point to point counterpart of the original before giving it a mark, we should ensure a perfectly null result by being unable to award any marks at all.

Our business, clearly, is to steer between the two extremes by framing the simplest set of common-sense rules we can, and to leave the doubtful cases to the discretion of the marker aided by whatever suggestions of a general or non-tendentious character we can supply. I shall refer again shortly to the instructions and guidance actually used, but the underlying principle remained substantially that given above, namely : ' Stick to the obvious ; avoid the recondite ; give one mark for a palpable hit, nothing for a miss, and half a point if there is real doubt '. Thus three degrees of resemblance were recognised—hit, miss and doubtful—compared with the eleven permitted in the method of decimal scoring.

In the interests of sensitivity I also decided that all drawings must be marked against all originals, within any self-contained group of experiments,[1] as opposed to working by pairs of series of originals as I did with the preceding method. This involved trying to bear the fifty originals in mind at once, or else adopting the terribly tedious

[1] I shall discuss elsewhere the question of what is to be done when we are dealing with isolated experiments, or small groups ; one cannot go on marking all drawings against all originals indefinitely, nor can one be sure of always having a suitably sized group within which to work.

procedure of comparing each drawing with each original in turn; but on the credit side there was no longer any need to think about exact gradings or to look for associations and the like, so that it was not quite so formidable as it sounds, and experience showed that it could be done without undue difficulty.

I have already emphasised the necessity for basing our enquiry on an empirical study of the frequency with which people do in fact draw the objects depicted in the originals, on occasions when these originals are not exposed, but a few remarks in amplification may not be out of place here. It is no use congratulating ourselves on the fact that Hands and Cows, Guns and Spinning Tops, Scissors and Balances turn up where they should and not where they shouldn't, if at the same time, unremarked by us, other originals are scoring a profusion of hits in winning and losing positions indiscriminately. If our originals consisted preponderantly of objects which, like Trees and Boats, are very commonly drawn, it might quite well happen that a few lucky hits on the less popular originals, in the right place and not repeated elsewhere, would look extremely impressive to the expectant eye, whereas they would really be no more than some of those insignificant aberrations with which any large collection of random data is almost certain to be embellished. We have no right to pick out the plums, however conspicuous they may be, and to use them alone in a test of significance; though we may quite legitimately test them mentally, so to speak, in the light of our general experience and common sense, with a view to forming a qualitative judgement as to what is going on. For a statistical test we must take account of *all* hits, whether 'winners' or 'losers', within the group concerned, before we can decide on the likelihood of the observed proportion of winners being the result of chance alone.

Mathematical Treatment. The statistical treatment necessitated by the method does not, fortunately, involve any very recondite process: indeed, the first stage is a matter of no more than common sense arithmetic. We will suppose that an unbiassed judge has given us the number of hits scored by all the percipients of each experiment on the originals of that experiment and of all others in the group considered; that is to say, the number of hits scored by all drawings on all originals, suitably subdivided under experiments. A certain number of these will be 'winners', namely those scored by the percipients of any particular experiment on the originals used in that experiment, and the remainder will be 'losers'. Our task is first to calculate, from the empirical data provided, how many winners we should expect, and then to apply a test of significance to show how likely it is that the observed number differs from the expected number as much as it actually does (or more) if this difference were the result of chance alone.

In dealing with the first of these stages we ignore 'misses' altogether; for these are of no more interest to us than would be calls of 'spade' or 'club' by a percipient trying to guess cards in a pack consisting (unknown to him) solely of hearts and diamonds. All we are interested in is the question of whether percipients tend to draw Hands, Horses, Violins, Scissors, Trees, etc., significantly more often when these objects are represented among the originals used in the course of an experiment than when they are not, and the frequency with which they draw armadillos, razors, kite-balloons, or other objects which have not been presented at all, can throw no light on the problem.

Now suppose, for the sake of illustration, that our judge has given a grand total of 1,000 hits by all drawings on all originals, and that these include 25 hits on Boat; and suppose also that 400 hits of one kind or another were scored by the percipients aiming at the series of originals in which Boat appeared. It is clearly no more than a matter of the simplest sort of proportion sum to ascertain how many of these 400 hits may be expected to be Boats, if chance alone is operative. For we have just found that boats make up twenty-five thousandths of all hits, or 2·5%, and on the chance hypothesis there is no reason for supposing that this proportion will be greater or less in any particular sample we may happen to choose; so we should expect to find 2·5% of 400, namely 10 Boats, appearing among the drawings of the percipients in question. But precisely the same reasoning will apply to the series of originals used in any experiment, taken as a whole, as applies to any particular original within it, except that we shall use the attribute of 'being like one of the originals of such-and-such an experiment' instead of the attribute of 'being like the original Boat' in making our calculation. For example, if 280, or 28%, of the total hits recorded were on the originals of Expt. I, and the percipients of that experiment scored, as before, 400 hits on the originals of all experiments together, we should expect 28% of 400, or 112, to be the number of hits scored by these percipients on their own originals, if there were no factor other than chance tending to make them score more. Repetition of the process for all experiments gives us the total expected number of 'winners'.

If we find that the observed number of winners is considerably in excess of the expected number, we shall have grounds for suspecting that the chance hypothesis is insufficient to account for the facts, and that the frequency with which drawings representing the various originals appear is not independent of whether these originals occurred in the series at which the percipients were aiming. But we cannot form an accurate judgement until we have determined the 'variance' of the expected number, that is to say, a quantity which serves as a measure of the extent to which the theoretically calculated expectation is likely to vary under chance conditions, and accordingly affords a means of judging whether any actually observed deviation therefrom is likely to be due to chance alone.

This is not such a simple matter as that of determining the expectation, and the interested reader should refer to Mr Stevens' paper, "Tests of Significance for Extra Sensory Perception Data," *Psychol. Rev.*, Vol. 46, No. 2, March 1939, where it is shown that the value of the variance, σ^2, is given by an expression of the form

$$\frac{1}{N^2(N-1)}\left\{S^2(a_jb_j)+N^2 . S(a_jb_j)-N . S\overline{(a_jb_j)(a_j+b_j)},\right\}$$

which is not, in practice, nearly so formidable to deal with as it appears. From this quantity we derive the Standard Error (σ) of the Expectation, by taking the square root; dividing this into the observed excess of winners we obtain the value of the 'normal deviate', whence, by reference to the appropriate Tables in the usual way, we find the probability of such an excess, or a greater, arising as a result of chance alone. An example will be shown worked in full when I come to discuss the outcome of the experiments as a whole. A discussion of the applicability of Mr Stevens' method to the present problem, by Dr Irwin and Mr Gatty, together with a physical analogy due to Professor Broad, will be found in Appendix V.

Marking the Drawings. Meanwhile we must consider the very important matter of how the drawings were in fact marked, and the precautions taken to ensure that the data to be used in subsequent calculations were wholly or substantially unbiassed.

To start with, all drawings were marked against all 50 originals by myself. This was done partly as a matter of exploration, in order to see whether the method seemed promising, and partly to find out at first hand what sort of difficulties or ambiguities were likely to be encountered. I did this work as carefully and conscientiously as I could; but, since I knew which drawings were aimed at which originals, the highly significant result I obtained cannot be accepted as reliable evidence. I do not think I erred in the direction of giving winning points where none were deserved—that, I found, was fairly easy to avoid; but it was only natural that, knowing a given batch of drawings to have been aimed at a particular series of originals, I should have other series less prominently in mind than this, and should thus tend to miss a certain number of ' losers ' which a wholly uninformed judge might find. On the other hand, since I had drawn 30 of the 50 originals myself, and had assisted H.S.C. in the preparation of the remainder, it is reasonable to suppose that I was in a better position than any outsider to know what the agent actually had in mind at the time, and thus whether any particular drawing successfully reproduced it ; and this might well be of importance from certain points of view. Be this as it may, however, my own markings must evidently be ruled *hors concours* : they are given below as a matter of comparative interest only and no conclusions are based on them.

For the next, and all-important, stage I was so exceptionally fortunate as to secure the help of Mr M. T. Hindson, the value of whose contribution to the work cannot be overestimated. Mr Hindson not only had the leisure and critical interest necessary for undertaking the task of marking, but had pursued until recently the career of Bank Inspector—a vocation well calculated, one may suppose, to promote just those qualities of objective judgement and indifference to tedium which are most necessary here. To say that I am very deeply indebted to him is seriously to understate the case, for without his aid the work might well have come virtually to a standstill for lack of a suitable judge. As it was, his marking, carried out under carefully planned and maintained conditions of ignorance, yielded a highly significant result of the utmost importance.

The 50 originals were given to Mr Hindson arranged in alphabetical order, that is to say *randomised* or *shuffled* from the point of view of deciding which belonged to which experiment ; and I was careful to remove completely, by cutting off the tops of the sheets, such numbers or letters as had been used at earlier stages, which might possibly have identified certain originals as belonging to the *same* experiment.

Instructions were contained in a ' Guide to Scoring Hits ', which seems of sufficient importance, in the circumstances, to be worth reproducing as Appendix IV. This ' Guide ' was supplemented by a number of ' Notes on Originals ', which was intended to draw attention to various points I had noticed in the course of my own marking. The idea, which Mr Hindson adopted, was that the judge should go through the Notes first, before tackling the work of marking, and make up his mind what policy he would adopt with regard to each of the points indicated, before actually meeting with them, so as to reduce the risk of varying his standard in the course of the work.

In preparing these notes, I was careful always to use either an interrogative or an imperative form, in drawing attention to the points raised, in order to avoid giving any suggestion as to how I thought they ought to be dealt with. Examples are :

> " BOTTLE. This was based on the dictionary word VACUUM BOTTLE, which was rejected as too difficult. What will you give to Carafes, Medicine bottles, Rubber hot-water bottles, Jars, Vases, etc., etc.? "
>
> " EWER : In the agent's mind the distinguishing features were the handle and the constricted neck. Should full or half hits be given for two-handled vases? For watering cans, teapots, etc., with handles but also spouts? For teacups, etc., with handles but no constriction? For saucepans, etc., with a different sort of handle? For vases, etc., with constriction but no handle? "
>
> " FLAG : It will be necessary to distinguish this from Royal STANDARD. The flag drawn is a black flag with a strongly marked White Latin Cross. Consider Black Flags (without crosses), Plain Flags, Striped Flags, Union Jacks ; Latin Crosses without Flags ; Other sorts of Cross ; also pennants, burgees, etc."

It will presumably be agreed that notes such as these, while they may serve a useful purpose in directing the attention of the judge to the kind of problems he will have to solve, contain no suggestion as to which solution he should adopt in any instance, and still less (if possible) as to which solution would be favourable to the experiment. These Notes are given in full in Appendix IV.

I hope I need hardly add that, when I discussed the matter with Mr Hindson, I was most scrupulously careful not to say anything that could possibly be interpreted as a specific recommendation, in respect of any original or type of drawing, one way or the other.

I must add that Mr Hindson specifically authorises me to say, which is important, that he had no clue, at any time during the marking, as to which Originals corresponded to the various batches of drawings submitted to him, and that neither my written instructions or verbal comments influenced him in any way towards assigning marks to drawings in respect of one Original rather than another.

In other words, he did the marking completely ' blind ' in every relevant sense, so that his results may be accepted as wholly without bias, and I venture to assert without much fear of contradiction that data obtained under these conditions cannot plausibly be assailed even by the most exacting criticism. I accordingly take Mr Hindson's figures as fundamental in the whole of the investigation of this group of experiments that follows. Only these figures, be it noted, and such others as may be derived from them by rigidly objective processes, are eligible for consideration from the strictly evidential point of view ; and only results obtained from them will be claimed as evidence : though, as we shall see later, figures derived from other judgements, into which subjective factors might theoretically be supposed to have entered to some extent, if in practice they probably or apparently did not, may be of considerable interest from the standpoint of information as opposed to evidence.

Alternative Markings : The next points I wish to discuss will be best understood if I describe the circumstances which gave rise to them.

When the drawings of the first experiment, with their assigned points, were returned to me by Mr Hindson, I found that he was marking on a very much more generous scale than I myself had done ; for on these 37 sets he had given a total of 272·5 points where I had given only 93·0—a ratio of almost exactly 3 to 1. This meant that he had been using a very much lower standard, or ' level of acceptance ', than I had used ; but it did not, of course, mean that his marking was necessarily in any sense ' worse ' (or ' better ') than mine, for the optimum standard to adopt for detecting a real effect, if there is one, can clearly only be found by experiment. It might quite well happen, indeed—and apparently does—that many resemblances much less noticeable than those which I myself would reckon as ' palpable hits ' are yet to some extent inspired, so to say, by the originals and, if this were so, a standard of marking which counted them would be preferable to one that did not. But at the time in question I was full, to the point of obsession, with the idea that it was very important to keep the standard as high as possible ; this was partly in reaction from the disappointing results of the Method of Decimal Scoring, in which marks so low as one tenth of the maximum could be given for faint resemblances and associations, and partly because I knew from my own marking that the use of a high standard would lead to significant results.[1] It was not unnatural, therefore, that I became apprehensive lest the adoption of so much lower a standard might result in a serious and possibly fatal dilution of the effect I had good reason to suppose was there.

[1] I suppose I must point out here that the fact of my own marking being inadmissible as strict evidence in favour of the occurrence of a real effect, does *not* render it useless as a guide ; on the contrary, the fact that it was very carefully and conscientiously done by the person who knew more than anyone else about the originals might well compensate for the possibilities of unwitting bias already mentioned. I knew very well that though I could not claim it as evidentially rigid, there was uncommon little wrong with it. W.W.C.

I accordingly asked Mr Hindson to raise his standard somewhat in future, and at the same time I cast about in my mind for some method of eliminating the feebler resemblances, if need be, on as objective a basis as possible.[1]

As regards the first, I may say that Mr Hindson adjusted himself to my demands with great success ; in marking Expts. II to IV B, he gave 576·0 points as compared with my 466·0, which is a ratio of only about 1·25 to 1.

As for the second, I finally decided on the plan of picking out all the drawings on the marking of which Mr Hindson and I did not exactly agree [2] and submitting them to independent arbitration. This seemed the best practicable expedient in the circumstances, which were intrinsically somewhat difficult (August 1939), even though it was not absolutely rigid evidentially, for reasons which I shall discuss in a moment. I wanted, first of all, to be quite satisfied in my own mind that the results of the marking I had done were not due to pathological aberrations on my part ; so that in the event of Mr Hindson's marking yielding null results I could be reasonably sure that this was due to his employing too generous a standard and not to there being nothing to detect. If, on the other hand, Mr Hindson's unarbitrated marking were to lead to a significant positive result (as it did) no harm would be done ; on the contrary, a very approximately rigid alternative assessment of this kind, using an appreciably higher standard than Mr Hindson's, could hardly fail to be of considerable interest from the comparative point of view.[3]

There are two reasons why these arbitrated figures may not be perfectly rigid. First : It might be suggested that I myself, wittingly or unwittingly, had marked potential winners with generosity and potential losers with meanness. If this were so and if, as was the case, Mr Hindson marked much more generously all round, the result would be that relatively few potential winners would come up for arbitration, because he would have passed most of my judgements ; but there would be many potential losers, because I should have disagreed in advance with his losing selections. If then, as was also

[1] This raising of the standard could not affect the evidential value of the results obtained, but it was unquestionably an error as regards extracting the maximum of information from the material.

[2] *I.e.* including those to which one of us gave a full mark and the other a half, as well as those to which one gave a full or half mark and the other nothing.

[3] External circumstances, I may remark, made it impracticable to postpone action until after all Mr Hindson's figures had been received, collated and computed.

the case, the arbitrators were urged to adopt a high standard, these losers would be extensively rejected. As a result, the proportion of winners would be unfairly inflated. This supposition is contra-indicated by the evidence. Mr Hindson's 1,209 original entries contain 280 or 23·16% potential winners, while the 934 entries submitted for arbitration contain 213, or 22·81 ; the difference is trivial by inspection.

Second : Professor C. D. Broad and Professor H. H. Price very kindly consented to do the arbitrating. Of these, the former had considerable knowledge of the originals, though the latter had not, and it is not altogether impossible that this knowledge might have introduced some slight measure of unconscious bias into some of the judgements. But this also lacks material support. If such a tendency were appreciably operative, we should expect to find Professor Broad giving considerably more points to winners and fewer to losers as compared with Professor Price. There is a very slight tendency to this effect to be observed in the 586 cases in which independent judgements from both are available ; [1] but when the figures are tested for significance by the ordinary 2×2 method, we find that a discrepancy as large as that observed, or larger, would occur a shade more often than not by the operation of chance alone.

We may accordingly conclude that, although the arbitrated figures are not evidentially impeccable, they are none the less entirely satisfactory for all practical purposes, and may be safely used wherever information rather than the highest grade of evidence is in question ; for the latter, we may rely on the unarbitrated Hindson figures and their derivatives.

Derived Figures : I have already pointed out that a genuine effect, if present, might be obscured by either too generous or too strict a standard of marking, and a moment's reflection will show that there must be some optimum standard which will bring out to the best advantage whatever effect there may be. It is clearly of considerable practical interest to ascertain, if we can, whether we are approximating to this optimum, or whether, in future work, we should do better to employ appreciably higher or lower standards.

This can, of course, only be done by varying the standard in both directions and seeing whether the best result (after making due allowance for differences in the size of the sample) lies between these limits. It is fortunately possible to do this very easily, and perfectly

[1] Professor Price was unfortunately unable to complete the whole of the work. For the latter part of Expt. IV A and the whole of IV B, the onus of arbitration fell on Professor Broad alone.

objectively, in the case of any set of data scored on the full mark, half mark, or zero method; for changing all half marks into full marks will give us the effect of using a lower standard, while changing them into zeros will be equivalent to raising the standard. In the one case we treat all doubtful assessments as full fledged winners, which may well be over-generous; in the other we treat them all as misses, which is likely to err on the side of strictness.[1] These figures, which will be discussed in due course, were easily obtained from the original Hindson data, but to obtain the arbitrated figures and their derivatives called for considerable care and labour in tabulation and checking.

Tabulation, etc.: I began, as already implied, by preparing lists of all drawings about which Mr Hindson and I did not exactly agree; but to these I added a considerable number about which we did agree, either because I was not altogether confident that even our agreed judgement was sound, or because it seemed to me that our policy had not been wholly consistent. From these lists, after the arbitrators had recorded their judgements on them, I prepared 'Summaries of Arbitration' for the drawings of each experiment in turn. Each such summary showed, for the drawings of the experiment concerned as compared with each of the 50 originals, (*a*) the number of hits on the original as agreed by Mr Hindson and myself and not submitted to arbitration, (*b*) the number of drawings submitted which were unanimously discarded by the arbitrators, (*c*) in detail, the mark given by each arbitrator to those drawings they did not unanimously discard, and (*d*) the lower of these two marks, if different. This last was because it was agreed at the time that, 'in the interests of conservatism' and to maintain a high standard, the lower of the two arbitrations should be taken; but there was, of course, nothing binding or sacrosanct about this decision, and I shall also give the results obtained by taking the higher of the two: note that, since there was only one arbitrator for the latter part of IV A and the whole of IV B, the 'high' and 'low' arbitrations are identical in this region. From these Summaries it was easy enough to extract the requisite information, namely the number of hits scored by the drawings of each experiment on the originals of their own and of the other experiments.

When I had done this, I retabulated the whole of the data *de novo*, on a larger and more elaborate scale, in a kind of Master Table arranged by Originals in alphabetical order instead of by Experiments in chronological order, and showing every entry made by

[1] *Mutatis mutandis*, a similar procedure may be applied to all data obtained from a graduated system of marking.

Mr Hindson or myself and the judgements of both arbitrators whether unanimous or not. By collecting the same figures from this Table as from the Summaries and comparing the two, I was able to detect any discrepancies there might be and to track them to their origin. At the same time I checked through all the arbitrators' decisions (and a great many of Mr Hindson's and my own) with a view to detecting inconsistencies of marking. A few instances were found and submitted to Professor Broad for a ruling, which was then applied to all relevant cases. A few more drawings were discovered which had somehow managed to slip through the net, *e.g.* had inadvertently been omitted from arbitration ; there were not more than ten or a dozen of these, and nearly all could be confidently marked by analogy with others of the same kind on which the arbitrators had given decisions ; in only some three or four cases was I obliged to rely on my own judgement, and in these I was careful, so far as was compatible with common sense, to weight the marking against rather than in favour of the effect looked for.

Thus the arbitrated figures, while lacking the perfect evidential rigidity of the Hindson unarbitrated, represent the product of an extremely careful and conscientious attempt to form considered composite judgements, using a somewhat higher standard than Mr Hindson, on the part of judges who, where they had knowledge that might bias the result, did their best to discount it. For certain purposes, such as determining the magnitude of the effect at the level of acceptance adopted, these figures, despite their lack of complete rigidity, are preferable to any single judgement however ' blind ' it may have been.

To Sum up : The Method is based on the very simple plan of marking all drawings against all originals, giving a full mark if a drawing plainly resembles any original, nothing if it does not, and a half mark if there is real doubt. Mr Hindson had no kind of clue as to which drawings were aimed at which originals, so that his figures and their derivatives may be taken as completely rigid. The arbitrated figures, though theoretically less perfect, are practically reliable and amply good enough for informative purposes.

SECTION IV

Results

A. Main Results

1*a*. *Calculation of the Main Results from the Hindson original Figures :* We are now in a position to approach the central and crucial point of the whole enquiry, namely the question of whether or not percipients tend, to a significant extent, to score relatively more 'hits' on the originals of the experiment in which they are engaged than they do on those of experiments in which they are not engaged.

The simple mathematical technique necessary for this purpose has been discussed above, and we will start by applying it to Mr Hindson's unarbitrated and unmodified figures. These are shown tabulated in the appropriate manner at the top of Example I, p. 132, where the whole calculation is given in full.

This Table should be interpreted as follows: When the originals were removed from the alphabetical sequence in which they had been given to Mr Hindson and were regrouped according to the experiments in which they were used, it was found that the drawings done by the percipients of Expt. I had been credited with 51·5 hits on the originals used in Expt. I, with 77·5 on those used in Expt. II, with 36·5 on those of Expt. III, with 25·0 on those of Expt. IV A, and with 82·0 on those of Expt. IV B, making a total of 272·5 assorted hits by these percipients. Similarly the percipients of Expt. II scored 7·0 hits on the originals of Expt. I, 18·0 on those of their own experiment, 12·5 on the originals of Expt. III, and so on throughout the Table, which shows a grand total of 848·5 hits scored on all originals by all percipients put together. Alternatively, we may read along the lines instead of down the columns to the effect that the originals of Expt. I had 51·5 hits scored on them by the percipients of Expt. I, 7·0 by the percipients of Expt. II, 5·5 by those of Expt. III, and so forth, showing a total of 127·5 scored on them altogether. Either way, it is clear that the figures in the diagonally placed cells, set in heavy type, are the 'winners' in which we are interested, while all the others are 'losers'. Summing the figures in the diagonal cells, we find 201·5 as the total number of winners scored; but this is of no use to us unless we also know (*a*) how many winners we should expect to find, given the known number of hits by each group of percipients and on each series of originals, if chance alone were operative, and (*b*) the extent to which this expected number is likely to vary under these conditions.

Recapitulating an earlier passage, we know that 127·5 hits out of a grand total of 848·5 are scored on the originals of Expt. I; that is to say, the fraction $\frac{127{\cdot}5}{848{\cdot}5}$ of all hits. Since, on the null hypothesis, there is no reason to suppose that one group of percipients would score relatively more or less on these originals than any other, we should expect this fraction of the total hits scored by any group of percipients to be hits on the originals of Expt. I. In particular, we should expect $\frac{127 \cdot 5}{848 \cdot 5}$ of 272·5 to be the number of hits scored on the originals of Expt. I by the percipients of Expt. I. This comes to about 41 hits expected, so it is clear that, in this case, expectation has been handsomely exceeded. Thus, to find the expected number of winners in any case, we multiply the appropriate column total by the corresponding row total and divide the product by the Grand Total.[1] But since we are only interested here in the total expected number of winners, and not in the expectation in each experiment separately, it is quicker to multiply the column and row totals in pairs, sum the products, and divide the sum by 848·5. This is shown in the first step of the Example, where it will be seen that the sum of the products concerned amounts to 138,545·25; dividing this by 848·5 we find the total expected number of winners, E_w, to be 163·28. Subtracting this from O_w, the observed number, which we have already found to be 201·50, we obtain for D, the difference between them, a value of 38·22. That is to say, according to Mr Hindson's marking, the percipients have succeeded in obtaining between them some 38 more hits, or 23%, on the originals at which they were aiming than we should expect them to do if chance alone were responsible. This sounds very fairly impressive, but whether it really is so, or whether it is the kind of fluctuation we might reasonably expect under chance conditions, cannot be decided until we have found the appropriate measure of chance fluctuation, namely the variance, in the manner already indicated.

We start by summing the corresponding column and row totals in pairs and then multiply each of these sums, $a+b$, by the previously determined product ab, thus obtaining a new series of products $ab(a+b)$, which are listed in the Example and have a total value of 57,863,437·000. The work of calculating the variance is then more or less self-explanatory. We write down the square of the sum of the ab products and add to it the result of multiplying the sum of those products by the square of the total number of hits; from this we subtract the sum of the compound products $ab(a+b)$ multiplied by the total number of hits. Dividing this by $N^2(N-1)$

[1] It should be noted that this procedure automatically allows for the varying 'popularity' of the originals of the different experiments; for this is measured empirically by the total number of hits scored on the originals concerned by all the percipients taken together, *i.e.* by the marginal totals 127·5, 181·0, etc., and the expected numbers calculated are proportional to these. *Mutatis mutandis*, the same is true of the differing numbers, or activity, of the percipients in the various experiments.

gives us the variance, σ^2, which we find comes to 114·468. Taking the square root of this gives the Standard Error, σ, of 10·699, and when we divide this into the value of 38·22 already determined for D, we obtain a 'normal deviate', D/σ, of 3·572; that is to say, the observed deviation of 38·22 hits in excess of expectation is more than three and a half times its standard error. Reference to the appropriate Tables, and interpolation, shows that *a deviation of this magnitude or greater may be expected less than once in a thousand such cases, or about once in 2,944, as the result of chance alone.*

The result is highly significant, and the chance hypothesis would be regarded as disproved in any normal context.

1*b*. *Main Results from Hindson 'derived' figures:* We may next apply precisely the same method to the figures derived from Mr Hindson's original data (*a*) by turning all his half points into full points, that is, by treating all his entries as of equal value, (*b*) by counting all his half points as zeros (*i.e.* 'misses') and using only those entries to which he gave a full point in the first instance. As already observed, these figures are every whit as rigid evidentially as the unmodified version, but correspond to a lowering and raising of the standard in the two cases respectively. For comparison, we will also work two results of some interest from the half points taken by themselves and treated simply as 'entries'. There is no need to tabulate these data in full, or to reproduce the calculations, which exactly follow the procedure of Example I.

The results are of considerable interest, particularly when compared with that already obtained, which is included in Table 4 below for ease of reference. The successive lines of the Table show: The total number of points given, the expected number of 'winners', the observed number, the difference between these, the value of the normal deviate, the probability of such a difference (or a greater) arising by chance alone, and the difference expressed as a percentage of the total number of points, the use of which I shall explain later.

Table 4

	Half and Full Points			Half Points only	
	Halves as Wholes[1]	As Marked	Halves as Misses	All Halves	Rejected Halves
N	1209	848·5	488	721	546
E_w	227·01	163·28	100·62	128·90	93·76
O_w	280	201·5	123	157	110
D	52·99	38·22	22·38	28·10	16·24
D/σ	4·166	3·572	2·772	2·924	2·043
$P<$	·000,1	·001	·01	·01	·05
100D/N	4·383	4·504	4·586	3·897	2·974

[1] These are known as the 'All Entries' figures and are extensively used

It will be seen that all the first four results are handsomely significant, and that the effect of lowering the standard is to push the value of P beyond the one in ten thousand point. No one can reasonably ask more from a test of significance than this, and the supposition that the observed excess of winning over losing hits is due to chance may be dismissed with considerable assurance.

I hope no one will imagine that the foregoing treatment of the material is no more than an illegitimate monkeying with the data undertaken with a view to extracting a more impressive significance from them than they properly contain. My object is, rather, to throw light on the method of assessment adopted, both intrinsically and as handled by Mr Hindson ; and this, I think, the figures given succeed in doing with some success.

It is at once evident from an inspection of the Table that I maligned Mr Hindson in suspecting that he was using too low a standard, and that I was wrong in supposing that a very high standard was necessary or even desirable ; on the contrary, as will be seen, the improbability of the result being due to chance alone is raised more than ten-fold by ignoring the difference between half and whole points and treating all entries as of equal importance, as is done in the first column of the Table.[1] If, on the other hand, we throw the half points out altogether, as under 'Halves as Misses', the significance is greatly diminished. So it is clear that the halves make a very useful contribution to the result instead of being, as I had feared, a dangerous diluent. Moreover, if we separate out the half points and treat them simply as 'entries', by themselves, as is done under 'Half Points Only : All Halves', we find that they yield a strongly significant result. Finally, even those half points which were rejected by myself and at least one arbitrator as not up to so much as 'doubtful' by our standard ('Rejected Halves' in the Table) give a result just better than the conventional level of significance.

We are accordingly led towards the very interesting conclusion that the process of cognition involved is not an all or none affair, but that, on the contrary, the more remote resemblances may often represent just as genuine an effect as the closer, even if it be more difficult to detect. But before accepting this conclusion, even provisionally, there are two possibilities which must be considered.

First : May it be that the drawings receiving half points are largely those which are badly drawn? I am fairly sure that this is

below. For this reason, and as a matter of interest, details of the data, with the various expectations and differences, are given in full in TABLE I, p. 134, *q.v.*

not the case, though I do not see how I could easily justify my belief. But it is scarcely more than a matter of common sense to realise that the veriest scrawl of a Tree, a Boat, a Jug, an Anchor, a Balance, a Cow, a Butterfly, and so forth, will be unmistakeably *recognisable*, which is all that is necessary to gain a full point. Moreover, the instructions (*q.v.*) were explicit as to disregarding lack of skill in draughtsmanship and, I am confident, were duly followed. The situations which led to the giving of half points were rather those in which it was necessary to decide whether the clearly depicted object was near enough to the original in nature to justify a point being given. For examples: Is the outline of a Latin Cross to be given anything for Flag, in which a cross is the most conspicuous feature? Should a snail have a point for Shell? Should a steamer, or a rowing boat, or a gondola, have one for Boat? What sort of leaves are like enough to those in Exfoliate to justify a point? In very many cases such as these the judge would end by giving a half point; and it is they, together with a certain number of vague, as opposed to ill-executed, drawings which are mainly responsible for the large number of half-point entries.

Second, there is the possibility that Mr Hindson may have been exceptionally chary of giving full points at all, and may have diffidently given half points where a less cautious marker would have given wholes. I think I am prepared to go so far as to say that this is quite definitely not the case. Mr Hindson gives a total of 488 full points; I appear to have given 453, so that he is slightly more lavish than I am—not less—though only by about $7\frac{1}{2}\%$. On the other hand, only 175 of his 721 half points survive arbitration at all, and only 5 are converted by the arbitrators into full points; so he clearly has not marked a large number of 'palpable hits' too low. What he has done is to be appreciably more generous in the matter of full points than I, and to give no fewer than 721 half points to my 212. This is in no sense a reflection on his marking, which, in my opinion, would be difficult to improve upon. On the contrary, because he was quite free from bias, as no one who 'knew the answer' could possibly be, and could approach the work with a free and open mind devoid of guess-founded obsessions, he was able to recognise a large number of resemblances which I was afraid to admit for fear of 'dilution', though they were in fact to an important extent non-fortuitous.

On the other hand, his half-point entries are of a lower quality, as is to be expected, than his full-point; and it will be well to devote a few paragraphs to this question of 'quality' before we go on to discuss the arbitrated data.

2. *Measures of Quality :* At the beginning of an investigation of this kind we are, very properly, chiefly interested in deciding whether any real effect occurs at all ; but as soon as our basic experiments have given a sufficiently significant result to satisfy us, even provisionally, that this is the case and that there is something worth studying in our material—and we may certainly claim to have achieved this here—we shall want to answer questions of a much less simple character. For example, we shall want to know whether one group of percipients is relatively more successful than another, whether women do better than men, and so forth. In all such cases it will often be necessary to use some measure of success which is independent of the total number of hits recorded, for in general this will merely be proportional to the number of percipients concerned. At the moment, it would be of interest to know whether some standards, or methods of judging, are more *efficient* than others, in the sense of enabling us to select from the raw material a group of drawings containing a higher ratio of genuinely cognised specimens to fortuitous resemblances, as opposed to being merely more *effective*, in the sense of yielding a higher level of significance.

For it is not to be imagined that all 'winner' resemblances, however close, in a sample showing a significant result, however high, are necessarily the result of genuine cognition ; some are, we may be sure, for otherwise the significant result would not be obtained, but others will be purely fortuitous, and I see no means at present of telling which are which with any kind of assurance. And it might well be of importance, for some purposes, to employ material having the highest possible ratio of genuine to fortuitous hits.

Now it is easy to see that, if there is a real effect, then of two samples of different sizes but identical constitution the larger will show the higher significance, simply because it *is* larger ; indeed, a large sample of low quality may even prove more significant than a small sample of higher quality. To a close approximation, if there is a real effect and N is large, the effect of multiplying the size of the sample by n, will be to multiply the value of the normal deviate by $\sqrt{n}$; thus, we may reduce two samples of different sizes to level terms by dividing the normal deviate in each case by $\sqrt{N}$. But this gives a result approximately proportional to D/N. It is for this reason that I have here adopted the percentage of *excess* winners, 100D/N, as the most appropriate criterion of quality to use here.

Referring now to Table 4, let us see what light this throws on what has been going on. In the first place, we see that, although the significance falls as the standard is raised, the quality rises ; but the rise over the first three columns is very small, and some way from significance, though in the expected sense. This would appear to imply that the full points given are not worth very much more than the half points, and to a certain extent I think this is true ; for I have the impression that some of Mr Hindson's full-point markings were a trifle optimistic, and we shall see later that a considerably higher value of Q (as I may conveniently call all such measures of quality) is obtained from those full-point entries of which no one concerned had any doubts. On the other hand, we find a noticeable drop in quality, as we should expect, when the half points are isolated and treated by themselves, and this is still more marked when we consider only the halves rejected by the arbitrators.

From these facts we may fairly confidently draw the following conclusions, which are not without importance :

1. The cognitive process involved is not of an all or none character. That is to say, it may manifest itself in the production of drawings which are by no means unmistakeable likenesses of the originals (witness : ' All Halves ') and even in such as bear what many people would consider no more than faint resemblances to them (witness : ' Rejected Halves ').

2. The lower limit of useful resemblance, if I may put it so, is probably being approached by Mr Hindson's more optimistic half-point markings ; for the Rejected Halves, of which we have the very considerable number of 546, only just creep over the conventional significance level. If the standard were to be made much lower, it seems probable that either there would be no effect at all, or at best that serious ' dilution ' would begin to set in. I may easily be wrong here, and further work alone can show where, if anywhere, the ultimate effective limit lies ; but at present it looks as if the mesh of Mr Hindson's net had been cunningly chosen so as to bring in pretty well all the worth-while fish.

3. The fact that the values of Q vary just about as we should expect them to do goes some way towards assuring us that the effect is a well-behaved citizen of the scientific world : it would have been very disconcerting if we had found, for example, that the fainter resemblances were of higher quality than the stronger!

3. *Main Results from Arbitrated Figures :* As I have emphasised above, results obtained from the Hindson unarbitrated figures are alone to be regarded as completely rigid from the strictest point of view ; but, for reasons explained, the arbitrated figures have certain merits of their own. Since the process of arbitration has already been fully described, we may proceed at once to the results, which

are given in Table 5 below. To the arbitrated results proper I have added, as a matter of interest, those obtained from my own marking and by treating all entries, whether by Mr Hindson or myself, on the same lines as in the first column of Table 4: these must be treated as suspect, and no conclusions are based on them. The calculations were, of course, as shown in Example I.

Table 5

	W.W.C.	W.W.C. and Hindson	Arbitrated High	Arbitrated Low	Full Points
N	559	1329	558·5	486	314
E_w	128·88	260·53	122·24	113·46	71·41
O_w	166·5	323	160·5	140·5	90
D	37·62	62·47	38·26	27·04	18·59
D/	4·052	4·564	4·054	3·158	2·770
P<	·000,1	10^{-5}	·000,1	·01	·01
Q	6·730	4·701	6·850	5·564	5·920
Mean P			<·001 (1/3,588)		
,, Q			6·252		

The first two columns are self-explanatory; the third gives the results obtained by taking the higher of the two arbitraments where there is a choice; the fourth those of taking the lower: the fifth is obtained by discarding all entries about which anyone concerned had indicated a doubt by giving a half point or less, even if others had given a full point.

Note first the high values reached as regards both significance and quality by my own marking; it is fairly clear that either my knowledge was too much for my efforts to discount it, or that the quite legitimate factor of my understanding of what had been in the agents' minds proved valuable; unfortunately we cannot distinguish with certainty between the two hypotheses. But note also that these values are fractionally exceeded by the 'High' arbitrated figures; as there is no real reason for suspecting these at all seriously, it looks as if my efforts to discount my own bias had been reasonably successful and that the second alternative exerted (as I personally believe) an appreciable influence on the outcome.

Since there is nothing to choose so far as I can see, between the High and the Low versions of the arbitrated figures, except that the latter is the more 'conservative'—which is not necessarily a virtue —I have given, at the bottom of the Table, values of P and Q obtained from the means of the two sets of figures. It will be noticed that they closely agree with the unmodified Hindson figures as regards significance, but achieve this result with the use of only

522 points (mean) instead of 848·5; that is to say, the quality of the sample obtained by the exercise of collective judgement is better than that from a single marker. This is only what one would expect, on the general ground that two heads are better than one; but it may be of some practical importance. It suggests the possibility that the best way to extract the most significant result from a given number of drawings may be to employ a plurality of judges working to a low standard rather than a single judge working to a high standard, and to test successively the data on which they all, all but one, all but two, etc., agree—thus finding empirically the optimum cut-off point, which is presumably that at which good resemblances begin to pass into fanciful.

4. *Summary:* The results just discussed, above all those of Table 1, are of fundamental importance to the whole enquiry. They show clearly that it is in a very high degree improbable that the observed excesses of winning over losing hits are due to chance alone. If chance alone were operative, we should expect to find such a result as that of the Hindson 'All Entries' occurring on the average only once in some thirty-three thousand similar groups of experiments. It is accordingly very unlikely that we are being led astray by fortuitous resemblances. Sceptics must either find a flaw in the experimental procedure or conditions of marking, or show (which seems to me an impossible task) that the antecedent improbability of such a phenomenon is at least comparable with the figure just given, or take refuge in avowed intransigeance. Meanwhile, we may regard the occurrence of a genuine cognitive process under the conditions described as provisionally demonstrated, to a point which amply justifies continued study and repetition by others of experiments of the kind described.

We also conclude that the cognitive process is not of an all or none character, so that comparatively remote resemblances may be genuine in the sense of being non-fortuitous; and that, consequently, the use of a very high standard is not necessarily the best way of separating the wheat from the chaff.

B. Displacement

1. *General:* The apparent tendency for an original to be reproduced by the percipient on some occasion other than that on which it was exposed was noted in the first experiment and has already been mentioned. If this apparent tendency is real (as we shall see that it is) there can be no doubt that it is a matter of the first importance. If it occurs in one direction only, namely that of making

the reproduction appear *after* the occasion of exposure of the original, there would be a suggestion that the impression received may require some period of gestation in the subconscious before it can emerge into consciousness and be recorded, or perhaps that it only emerges when some relevant extraneous stimulus gives it the necessary fillip ; and either of these or similar conclusions would be of appreciable psychological interest. But if it occurs in the opposite sense, so that reproductions tend to appear *before* the exposure of the originals which they resemble we should be confronted (in so far as the effect was significant) with empirical evidence of a new type for the occurrence of a phenomenon of a precognitive character ; and this would be of the very greatest interest from all manner of viewpoints beside the purely psychological.

The matter is accordingly one which must be investigated with the utmost care and rigour, and it will be well to begin by clearing the air of one or two potential misconceptions.

The one thing we must *not* do, of course, is to claim as precognitive every resemblance [1] that occurs before its original and as retrocognitive every one that occurs after it ; for this would be merely begging the question. In other words, we must decide before we start on what the words precognitive and retrocognitive may reasonably and usefully be supposed to mean.

It is, as usual, a matter of observed as compared with expected occurrences. We can usefully and validly speak of retrocognitions only if we find a number of resemblances, significantly greater than the hypothesis of chance would lead us to expect, on some date or dates later than the relevant event ; and *vice versa* for precognitions. Beyond this we cannot safely go before we have studied the facts, though our general experience might lead us to expect a continuous falling off in the frequency of the resemblances as the remoteness from the event increases. And we should have no right to complain, if some to be surprised, if it were found that the falling off were of a periodic or fluctuating character. But our general knowledge of the way in which all kinds of phenomena occur suggests that, if pre- and retro- cognition be both phenomena of nature, the relative frequency of hits on a given original is likely to increase as the occasion on which a drawing was made approaches that on which the original was displayed, to reach a maximum at or near the coincidence of the two, and to fall off again as the former recedes from the latter.

[1] The words ' resemblance ', ' reproduction ' and ' hit ' may be regarded as interchangeable and as denoting a drawing sufficiently like an original to have been scored by the judge. The degree of resemblance does not concern us at this stage.

Our business therefore is to calculate from the observed data the number of hits expected to occur on occasions one, two, three, etc., places before, and one, two, three, etc., places after, the event concerned, namely the exposure of an original, and to compare these calculated values with the numbers actually observed.

2. *Illustration of Procedure :* To illustrate the procedure, which becomes laborious when large numbers of occasions and originals are involved, we will take the relatively simple case, of no special importance intrinsically, of the originals and occasions of the first experiment.[1]

The tabulated data and the results of the calculations are shown in Example II, p. 133, the first part of which should be read as follows : On the first occasion one hit was scored on the first original (Bracket), on the second and third occasions no hits, on the fourth occasion one hit, on the fifth two hits, etc., . . . a total of 7 ' hits ' or resemblances to the original Bracket being found among the drawings of the first experiment percipients. Or alternatively : Among the drawings done on the first occasion there were found one resembling Bracket, one resembling Buffalo, none resembling Fess or Hand, two resembling Cross-stitch, etc., etc., . . . a total of 5 hits on one or other of the originals of Expt. I being found among the drawings done on the first occasion.

It will at once be realised that the figures in the ' leading diagonal ' shown in heavy type, represent the numbers of hits scored on the several originals on the occasions on which they were exposed ; that is to say, hits not displaced in either direction but coinciding with the event—or, more accurately, occurring within the period of exposure, which constitutes coincidence for our purpose. It is also easy to see that the next diagonal upwards and to the right, parallel to the leading diagonal, contains the numbers of hits scored on the occasion immediately *after* the event, of which there were one for Buffalo, one for Hand, two for Cross-stitch, two for Bottle, etc. ; whereas the diagonal below and to the left of the leading diagonal shows the numbers of hits scored on the occasion immediately *before* the event, of which there were only one for Buffalo, one for Bat and two for Net. Similarly, as we proceed outward from the leading diagonal, we find diagonals containing the numbers of hits scored on the second, third, fourth, etc., occasions before or after, as the case may be, that on which the original they resemble was exposed. Our task is to determine whether there is any significant tendency for the observed numbers in these diagonals to exceed their

[1] I again use the Hindson ' All Entries ' data, and shall do so henceforward unless otherwise expressly stated.

expected values as we approach the leading diagonal from either direction, or both.

The procedure is simple enough in principle, and is basically the same as that which we adopted for obtaining the expected numbers of hits on the originals of a particular experiment by the drawings of that experiment when we were calculating the main results, *q.v.* The number of hits to be expected in any 'cell' or compartment of the 10×10 Table of Example II is obtained by multiplying together the two marginal totals concerned and dividing the product by the total number of hits. Taking the extreme bottom left-hand corner as an example, we note that only one hit was scored by these percipients on the original Anchor, being 1/81 of the total; altogether, five hits were scored on these ten originals on the first occasion; so, if no cause other than chance is operative, we should expect the same proportion of Anchors among these first occasion hits as in the total, *viz.* 1/81, from which it follows that the actual expected number of hits will be 5/81, or ·062. In practice, of course, one cannot have ·062 of a hit, and the expectations in individual cells are far too small to be useful or interesting, but this does not affect the principle involved. For the next diagonal nearer the centre, the expectation will be $(3 \times 5)/81$ for the Beetle cell, and $(1 \times 7)/81$ for the Anchor cell, making a total of 22/81, or ·272. In short, to determine the expected number of hits in any diagonal we sum the products of the successive pairs of marginal totals concerned and divide by the total number of hits in the Table. When we do this, we obtain the figures given under E in the lower part of the example, where 'Diagonal $-$ 9' means the diagonal containing the hits made nine places before the original to which they refer, and so forth. O is the observed number of hits in these diagonals, as may be verified from the Table, while the quantity $(O-E)/\sqrt{E}$ gives a measure of the deviation from expectation, reduced to a standard level, on the hypothesis that chance alone is responsible for the distribution in the various diagonals.

The Example, as I have said, is only illustrative of procedure and I do not propose to discuss the not very interesting results in detail here. The only points worth noting are, first, that the observed number of hits of zero displacement, *i.e.*, made on the same occasion as that on which the original was exposed, is fractionally *below* expectation, instead of being handsomely above as we might perhaps have expected from the fact that the experiment as a whole scores 16·54 hits above the expected value; second, that, although the quantities $(O-E)/\sqrt{E}$ are distressingly ragged and not very informative as they stand, they do contrive to show a noticeable 'peak' at diagonal 2, while the observed numbers are greater than expectation in the positive (retrocognitive) part of the Table and less in the negative (precognitive part); these last two facts accord, though somewhat feebly, with the qualitative impression first formed to the effect that hits tended to be deferred. The difference may be attributed to the fact that the impression was formed on the basis of the stronger resemblances only, whereas the data of Example IV include the feebler.

3. *Compilation of the* 50×50 *Table*: As a matter of historical fact, I first satisfied myself that there was a displacement effect worth investigating by a study, on the lines indicated above, of the inter-experiment data given in TABLE I, p. 134, but it will be more convenient to defer consideration of these for the present and to proceed at once to the crucial large-scale investigation of all occasions and all originals taken together.

This involved the preparation of a 50×50 Table, a task which was complicated by the fact that in all cases except Expt. II it was necessary first to 'decode' the random numbers which had been given to the 1,209 drawings involved for use in connection with the matching technique. In this Table the cross-headings of the rows from top to bottom were the fifty originals in the order in which they were exposed in the five successive experiments, and the headings of the columns from left to right were the fifty successive occasions on which drawings were made in these experiments. In each cell, formed by the intersection of a row with a column, was entered the number of drawings, made on a certain occasion, which the judge had assigned to a certain original. Nothing would be gained by presenting the full Table here, particularly as we are not interested in individual cells, the expectations for which I have not calculated, but only in the total numbers expected and observed in the diagonals. Nor do I think it would be worth while to give full details for all the 99 diagonals; but TABLE II shows an illustrative sample consisting of three groups (the second, central and last but one, counting from the extreme bottom left-hand corner of the 50×50 Table) of nine diagonals each, and the sub-totals for the remaining eight groups of nine into which the 99 diagonals may conveniently be divided. This will be sufficient to show the procedure adopted and the kind of results obtained. It should be noted that the values of $(O-E)/\sqrt{E}$ and of χ^2 given in the lines for Sub-totals are obtained from the sub-total values of O and E, and not by summing the results for individual diagonals; that is to say, in computing these quantities, the data are pooled for each group of diagonals.

It will be seen that the individual values of $(O-E)/\sqrt{E}$, in which we are chiefly interested, are distinctly irregular ; but the sub-totals leave little doubt as to what is going on.

Note, however, before we pass on to discuss these, the very remarkable ' dip ' at the centre (diagonal O), with ' twin peaks ' either side of it at -2 and 2. We will discuss the significance and interpretation of this at a later stage, but we may note here that the peak at diagonal 2, where there is an excess of no fewer than 19·384 hits over expectation, is very definitely significant even in the context of 98 other diagonals, for the value 4·269 gives P less than ·000,02, and even the symmetrically placed peak at -2 passes the 1 in 4,000 mark.

To bring out the salient features to better advantage, I retabulate the Group Sub-totals in Table 6 below, and show the values of $(O-E)/\sqrt{E}$ plotted in Figure I.

Table 6

Group	$(O-E)/\sqrt{E}$ [1]	
-5	$-1·031$	Precognitive Groups.
-4	$-1·973$	
-3	$-1·678$	
-2	·524	
-1	1·186	
0	3·873	
1	·760	Retrocognitive Groups.
2	$-$ ·922	
3	$-1·262$	
4	$-2·083$	
5	$-$ ·542	

The null hypothesis is, of course, to the effect that no diagonal or group of diagonals is more likely than another to show an excess or deficit of hits as compared with expectation. According to this, the values of $(O-E)/\sqrt{E}$ should be randomly distributed about a mean of zero with unit standard deviation. This, by inspection,

[1] The quantity $\sqrt{E}$ is, of course, only an approximation to the standard error of $(O-E)$. For the extreme case of a diagonal containing only a single cell, it is clearly very nearly true, since the probability of a hit falling in it is so small ; for the leading diagonal of the whole 50×50 Table it is 4·883, while the exact value of the standard error, calculated by Mr Stevens' formula is 4·764. The discrepancy is negligible, but on the safe side in the sense of tending to make the values of $(O-E)/\sqrt{E}$, and consequently of χ^2, somewhat too small. On the other hand, it should be noted that the values of χ^2 are not necessarily strictly additive, since the distributions in the various diagonals may not be independent. This, however, will not affect the implications of the highly systematic arrangement of the points in Fig. I.

they quite evidently are not, so that we hardly need the value of 32·09 for $S(\chi^2)$, which gives P just beyond the ·001 level of significance, to tell us that the hypothesis is untenable. Moreover, deviations from expectation are very fairly symmetrical, as may be seen at a glance from Figure I, so we may conclude that the precognitive effect is just about as strongly indicated as the retrocognitive.

If we wish to satisfy ourselves as to the significance of the two sides separately, we may conveniently group the diagonals in seven groups of seven on each side of the leading diagonal and find the value of $S(\chi^2)$ with 7 degrees of freedom in each case. This gives

	$S(\chi^2)$	P
Precognitive side	20·979	·01
Retrocognitive ,,	14·481	·05

Curiously enough, the precognitive effect, which most people would consider the less probable antecedently, appears to be the more strongly marked, or at least somewhat the better substantiated.

4. *Introduction of the Quantity Λ :* The quantity $(O-E)/\sqrt{E}$ is admirable for testing significance, but has certain disadvantages from other points of view. Since, from the nature of the calculations, the total observed and expected frequencies must be equal, it follows that any excess of the first over the second in one region must be balanced by a deficiency in another. Thus the fact that the line in Fig. I goes up to a peak in the middle necessitates a depression or depressions at one end or both ends; consequently the line is unlikely to represent to the best advantage the true relation between displacement and incidence of hits.

It would clearly be a matter of the greatest general interest if we could establish this relation definitely; if, that is to say, we could determine the 'law' connecting the magnitude of the real effect with displacement. This might well throw light on the nature of the process involved, or at least enable us to say that the facts are incompatible with certain hypotheses. But before we make even the most tentative steps in this direction we must be clear in our minds as to the senses in which we are using the terms 'magnitude of effect' and 'displacement'.

If we were dealing, let us say, with magnetic phenomena, there would be little room for ambiguity; the 'magnitude of effect' would be measured by the pull in dynes exerted by one magnetic pole on another or on a piece of iron, and 'displacement' would be the distance in centimetres between the two, and we should have little difficulty in ascertaining that the one varied inversely as the square of the other. In this case there is no ' magnitude ' of this nature that we can measure ; the phenomenon is a matter of the relative frequencies with which hits occur in different positions with respect to the originals on which they are made, and it is in terms of such frequencies, therefore, that we must conduct our study. The question of ' displacement ' is not quite so simple, for we may either consider *order* alone, without reference to *time*, as we have done hitherto ; or we might transform the horizontal scale of Fig. I into a true time scale (*e.g.* astronomical time) and work in terms of that. For the present, I shall continue to use the bare *order*, mainly because it is simplest and has yielded a reasonable looking arrangement of points ; but it will be necessary, sooner or later, to substitute the mean values of the astronomical time intervals as abscissae, and to try to determine whether these, or order alone, are most relevant to the facts.[1]

In these circumstances, the ideal would be an empirically determined measure of the *probability* of a hit occurring on an occasion at any given distance (measured in terms of order) from the original concerned ; we could then test various hypotheses regarding the nature of the phenomenon, leading to correspondingly different theoretical relations between probability and displacement, and see whether some were significantly more compatible with the facts than others.

Another way of approaching the point is to suppose that, in general, it is easier for the percipient to score hits on the originals nearer to the occasion on which he is working than on those more remote, and to enquire how ' easiness ' varies with ' remoteness ' ; from this point of view it should at once be clear that the only possible measure of ' easiness ' is the relative frequency with which percipients do in fact, on the average, score hits at different remotenesses, other things being equal.

The sting of this is found in its tail, for our chief difficulty lies in the fact that other things are very far from being equal. In the first place, of course, the numbers of occasions of different type vary greatly ; that is to say, with fifty originals, there are fifty occasions of zero displacement, 49 each with displacement plus one or minus one, 48 of displacement plus or minus two, and so on down to only one each with a displacement of plus or minus 49 positions. This would not matter particularly if equal numbers of hits were scored on all occasions and on all originals ; all we should have to do would

[1] At a guess, and as a matter of rash prophecy, I have little doubt that it will not be astronomical time that will be found most relevant ; but I do not wish to embark on an examination of this point at the present stage.

be to divide the number of hits occurring on each class of occasion by the number of occasions in that class (*i.e.* by the number of cells in the appropriate diagonal of the 50×50 Table), and this would give us a series of numbers representing the relative frequencies of hits with respect to displacement, which is what we want.

Unfortunately, from this point of view, originals differ very widely in popularity, in the empirical sense that some are drawn very much more often than others. Moreover, the number of percipients working on different occasions may also vary between wide limits while neither the 'markability' of their attempts nor the generosity of the judge can be assumed constant. Clearly, we shall have to make allowance for all these factors in attempting to estimate the quantity in which we are interested.

Note before we go on that any diagonal, *e.g.* of the 50×50 Table, or of Example II, is built up, so to say, of the hits scored on a number of different occasions and on an equal number of different originals; the common feature is that all the occasions are displaced to the same extent from the original on which the hits are scored. Consider in Example II, for instance, the 'plus 2' diagonal, which is that starting in the top line and the third column, running downward and to the right, terminating opposite Net, and reading . 4 2 1 1 3 3 2. Each of these entries represents the number of hits scored two occasions later than the original concerned; but both the occasion and the original necessarily vary from entry to entry. Thus, no hits are scored on Bracket on the third occasion, which is two occasions later than that on which Bracket was used as an original, while 4 were scored on Buffalo on the fourth occasion, which is two occasions after that on which Buffalo was used. But Buffalo was much more popular than Bracket (15 hits altogether as compared with 7), while the percipients of the experiment scored altogether 10 hits of sorts, on the originals of the experiment, on the fourth occasion as compared with five on the third. This should serve to make it clear why, in considering the relative merits of different positions considered *qua* displacement, we must correct the observed absolute number of hits, not only for the number of occasions of each type considered but also for the 'popularity' of the originals and the 'activity' of the percipients concerned. The first of these terms is self-explanatory, and is measured by the total number of hits made on the relevant original; by the second I refer to the combined effect of all those factors mentioned above which tend to increase or diminish the total number of hits scored by the percipients of that occasion on all originals, and is measured by that number.

Now it is evident that any increase in these numbers is likely to result in a proportionate increase of hits in the 'cell' or diagonal concerned, quite apart from its displacement; hence, as we are here only interested in the last named variable, the sensible thing to do is clearly to divide the observed number of hits in any cell by the appropriate 'activity' and 'popularity', or the number of hits in any diagonal or group of diagonals by the mean values of these, as well as by the number of cells. This will give us an estimate of what the number of hits per cell would have been if all the originals concerned had been of equal popularity and all the occasions productive of equal activity. For any diagonal, then, our estimate will be

$$\frac{\text{Observed number of hits}}{(\text{Number of cells})\times(\text{Mean Activity})\times(\text{Mean Popularity})}$$

and since Mean Activity and Mean Popularity are found by dividing Total Activity and Total Popularity by the number of relevant Occasions and Originals respectively, each of which is equal to the number of cells, this reduces to

$$\frac{(\text{Observed number of hits})\times(\text{Number of cells})}{(\text{Total Activity})\times(\text{Total Popularity})}$$

The terms in the numerator need no expansion; those in the denominator are obtained by summing the relevant marginal totals of the Table concerned, for relevant Occasions and Originals respectively. Thus for the $+2$ diagonal of Example II, we have 8 cells and 16 hits; the relevant occasion totals are 5, 10, 10, 7, 10, 6, 9, 12, giving a Total Activity of 69; and the relevant original totals are 7, 15, 4, 9, 16, 9, 9, 8, showing a Total Popularity of 77. Hence the value of our estimate, Λ, is $(8\times 16)/(69\times 77)$ or ·024.

The values so obtained are not probabilities; but they may be regarded without much risk of being misled as measures of or indices to them, so that we may conveniently regard Λ_r as proportional to the frequency of hits that would have occurred on an occasion r places distant from that on which the original concerned was used if all originals had been equally popular and all occasions equally active.

In Figure II, I show the values of Λ corresponding to those of $(\text{O}-\text{E})/\sqrt{\text{E}}$ in Fig. I, for the eleven groups of nine diagonals each. It will be seen that, as we should expect, the general tendencies are very similar to those of Fig. I; but we can now say definitely that, ignoring for the moment the upward twists at the ends, the probability of scoring a hit on any original, other things being equal, increases as the occasion of display approaches, reaches a maximum in the near neighbourhood of that occasion, and diminishes as it recedes. The actual values of Λ (multiplied by 10,000 for convenience) are

Table 7

Group		
–5	7·027	Precognitive Groups.
–4	6·484	
–3	5·290	
–2	8·480	
–1	10·148	
0	10·276	
1	8·271	Retrocognitive Groups.
2	7·812	
3	7·359	
4	6·125	
5	7·286	

It may be thought that I have devoted an undue amount of space to the introduction of this quantity, which tells us but little more, now we have evolved it, than we could infer by looking at Fig. I. My reason is that, although the quantity $(O-E)/\sqrt{E}$ is perfectly satisfactory mathematically, it is too much of an abstraction to be a useful aid to thought; whereas the concepts of the differing *proportions* of hits occurring at differing remotenesses from the occasion of display, or of their relative frequencies, or of the probabilities of their occurrence (which are virtually interchangeable) are relatively intelligible and should enable us to form some idea of the kind of thing that is going on.

6. *Summary and Comments:* I fear I cannot claim that the foregoing has done more than scratch the surface of the enormously important subject of Displacement. We have, to be sure, established beyond any reasonable doubt (if the authenticity of the data be accepted) that displacement does occur in both the precognitive and retrocognitive senses, and this is a far from trivial conclusion. Not only is it likely to have interesting repercussions in the cosmological field and for psychology generally, but it affords a basis for a plausible explanation (other than fraud, mal-observation, etc.) of why there has been such difficulty in securing reliable repetition of card-guessing experiments. Displacement is easy enough to detect by inspection when we are dealing with drawings of Buffaloes, Hands, Guns or Windmills, which cannot well be intended for anything else; but the successes of a percipient guessing Zener cards, for

example, who consistently scored high, but a place or two early or late, might easily escape notice unless specially searched for.[1]

On the other hand, the number of questions we are not yet able to answer is legion. For example: Is the 'central dip', referred to on p. 105, genuine or chance determined? (Probably genuine.) Are the upward twists at the ends of the $(O-E)/\sqrt{E}$ and $\varDelta$ curves genuine, and do they foreshadow lateral maxima? (With considerably less assurance, I think they probably are and do; but this is almost entirely based on inspection.) Is the asymmetry of these curves genuine? Does the retrocognitive side decline to a higher 'fixed value' than the precognitive? (Probably 'Yes' to both these.) Is the peak for the whole curve, neglecting any local disturbance due to the central dip, significantly displaced in either direction? (Probably not.) Have the curves substantially the same shapes for strong and weak resemblances? And for near and distant percipients? Is bare order, or astronomical time, or something that we might describe as 'relevant psychological time' the proper basis for the horizontal scale?

Probably in all these cases, and undoubtedly in some, we could obtain a certain amount of information by an intensive analysis of the existing data; but I think it will be better to defer the investigation of these questions until more material is available, or until further consideration of other points has suggested the most profitable lines of attack.

SECTION V

Results of Control Marking

1. *General:* When the results described in the preceding Section began to emerge—particularly, of course, the Main Results of Table 4 and those for Displacement embodied in Fig. I—it was suggested that it might be of considerable interest, as a matter of comparison, if a 'control' marking of the same drawings against different originals could be arranged, and this suggestion was approved by those most closely associated with the work. This

[1] It is very gratifying to find that a scrutiny of some of Mr Soal's results from this point of view shows clearly that such an effect was very significantly present in some of his subjects. Such confirmation of the effect, coming from a completely independent worker, using entirely different material, a different method, and different percipients, constitutes as strong supporting evidence as could well be desired; and, of course, reciprocally.

For these results, see Mr Soal's paper in this number of *Proceedings*.

was not because any of those concerned had detected, or thought he had detected, any flaw in the procedure adopted, which is, indeed, logically impeccable. But it was felt that logical impeccability is not the only means, or even always the best means, of commanding assent and allaying doubts; and there was also, of course, the theoretical possibility that the combined intelligences of Professor Broad, Dr Thouless, Dr Irwin, Mr Gatty and myself, not to mention others who had considered the problem, had failed to notice some serious methodological fallacy. This did not seem very probable; on the other hand many people would consider the results obtained to be also not very probable, and I think we were all agreed that we should feel easier in our minds—even though we might not be able to give logical justification for it—if a control marking were to be carried out and were to yield null results.

For this purpose we were so fortunate as to secure the co-operation of Mr H. F. Saltmarsh, to whom I am very much indebted for his labours in this connection.

2. *Procedure:* For the purpose of this control a fresh series of fifty originals (or perhaps I should say 'pseudo-originals') was used. These were drawn by my friend Mrs Aletta Lewis, to whom I am very much obliged for undertaking the work, and were primarily intended for use in the next experiment of the series, which lies outside the scope of this report. Inasmuch as some of them have been and others may be used for experimental purposes it would be undesirable to mention their nature here; it is sufficient to say that they were drawn in Indian ink on white paper in the same general style as the true originals, that they did not duplicate any of these used in Expts. I to IV B, that their subjects were selected by a strictly random method from a list of 216 possibilities submitted by me to the artist, and that they represented on the whole, rather more everyday objects than did those already described. The following are the points relevant to the present issue.

As received from the artist, each original was enclosed, in accordance with my instructions, in a separate envelope and further protected by a sheet of paper against its nature being inadvertently ascertained in the course of ordinary handling. The envelopes were shuffled by Dr Dingwall and myself, and ten of them were drawn at random for use in the further experiment mentioned above. These were later returned to the pack, so to say, and the whole 50 were re-shuffled by Dr Dingwall, who then opened them one by one, calling out the title of each to me as he did so. It was agreed that the first ten thus arbitrarily selected should be *deemed to be* the originals of the first experiment for the purpose of the control marking ; similarly, the second ten were allocated to the second experiment, the third ten to the third, and so forth. The allocations thus determined were listed by me at the time of selection under the heads of Expt. I, Expt. II, etc., in Dr Dingwall's presence, and initialled by us both. All this was to guard against the possibility that, in the event of the control yielding a null result, as I confidently and rightly expected that it would, someone might suggest that I had arranged the pseudo-originals so as to secure it. As a matter of fact, it would be extraordinarily difficult to devise a better method of arrangement from this point of view than the shuffling procedure actually adopted, as a moment's reflection should show, but there seemed no harm in adopting a full precautionary ritual.

The pseudo-originals were then re-arranged in the alphabetical order of their titles, just as had been done with the true originals submitted to Mr Hindson, and were sent to Mr Saltmarsh in this order. I also sent him, by instalments, the same 2,193 drawings which Mr Hindson had marked against the real originals ; that is to say all the drawings of Expts. I to IV B.

As regards instructions, these were substantially identical with those given to Mr Hindson ; and the reservation ' substantially ' is made only because, as reference to Appendix IV will show, it was necessary to alter or delete a few words here and there, in the ' Guide to Scoring Hits ', which would otherwise have indicated that it referred to originals other than those submitted. Apart from these trifling alterations, which were made in consultation with Dr Dingwall, the instructions issued were identically similar in the two cases ; but the ' Notes on Originals ' were naturally not sent, nor was it thought necessary to fabricate a corresponding document.

Needless to say, Mr Saltmarsh was not told that he was engaged on a control or dummy experiment ; on the contrary, by a piece of innocently ambiguous verbiage such as is frequently necessary in this class of work, I was at pains to convey the impression that he was repeating rather than paralleling the work of an earlier marker.

The scoring sheets, showing half and whole points awarded to the various originals in respect of the drawings, were sent direct to Dr Dingwall, and the data necessary for working the ' All Entries ' figures, corresponding to the most important main result obtained from Mr Hindson's marking, were extracted by us together. The remainder of the work was done by myself alone ; but the original scoring sheets and the relevant calculations are, of course, available for inspection if desired.

3. *Results :* As was expected, the results were, if not quite ' icily regular ', at any rate ' splendidly null '. In Table 8 below I give

the values of D/σ and of P for what I may term the four main modes of scoring, together with the corresponding figures for Mr Hindson's marking against the true originals, taken from Table 4 for ease of comparison.

Table 8

	Mr Hindson (Experiments)		Mr Saltmarsh (Control)	
	D/σ	P	D/σ	P
Full points only	2·772	$<$·01	− ·613	$\sim$·54
Half ,, ,,	2·924	$<$·01	−1·699	$\sim$·09
All Entries	4·166	$<$·000,1	−1·318	$\sim$·19
Score [1]	3·572	$<$·001	− ·984	$\sim$·33

There is evidently nothing at all significant about the control figures. Even the modest value of ·09 must be to some extent discounted as being one of several, even though not fully independent, results ; conversely, the interdependence robs the fact of all four results showing the same sign of whatever slight interest it might otherwise possess. In ordinary language, the percipients have scored on these pseudo-originals fractionally fewer hits in the right places, as compared to the wrong, than we should expect, but only to a quite non-significant extent entirely compatible with the hypothesis that no factor other than chance was involved.

In spite of this, I thought it worth while to make a job of it by repeating the calculations of the 50×50 Table, at least so far as was necessary to obtain the analogue to the eleven point Displacement graph of Figure I. The values obtained are shown below :

Table 9

	$(O-E)/\sqrt{E}$	
Group	Experiment	Control
−5	−1·031	·834
−4	−1·973	·152
−3	−1·678	·530
−2	·524	·606
−1	1·186	− ·502
0	3·873	− ·476
1	·760	− ·274
2	− ·922	−1·089
3	−1·262	−1·288
4	−2·083	1·197
5	− ·542	1·427

[1] This merely means that in this case the full and half points have been allowed their due relative weights, instead of one or other or both being treated merely as "entries", as in the first three lines of the table.

These values are shown by the broken line in Figure I, which, by contrast with the full line, well indicates the relatively random nature of the result. Inspection of the Table suggests a fairly strong central depression followed by an upward jink at the positive end; but the effect is not significant. Testing in the usual way, I find an initial variance of 8·1210; reduction of variance associated with the first two parameters of the regression line, 3·8610 with mean 1·9305; residual variance 4·2600 with mean 0·5325; variance ratio 3·622 ($n_1=2$, $n_2=8$); P>·05 or about ·1.

That is to say, the Displacement results, like the Main results, show nothing at all that is not quite easily explicable on the hypothesis of chance.

4. *Comments:* Some readers may be inclined to think that this process of control marking was no more than a gratuitous waste of Mr Saltmarsh's time and my own; that it did no more than exemplify the obvious by showing that if you carry out a random process properly you will obtain a random result; and that it no more illumines the questions at issue than making ten thousand throws with a substantially true die would illumine the question of whether another was loaded. With such critics I have the liveliest sympathy, for these were precisely my own reactions when the project was first mooted. But I am now inclined to think that these reactions, though natural, were wrong, and that the procedure, though emphatically a psychological luxury rather than a methodological necessity, has a rational justification and a positive value greater than meet the superficial eye. Here are my reasons.

I implied just now that it would be waste of time to do a long series of throws with a substantially true die as a check on one which had shown significant bias. That is true, but it is only true because we know a great deal from practical experience about the behaviour of dice; it would emphatically not be true if no one had ever thrown a die before, and if all we had to go upon were an 'intuitive' conviction that because the thing was a cube it 'must', *ipso facto*, behave in a particular way; this would be just as rash, in principle, as to conclude that because a steel needle is symmetrically suspended in a mechanical sense it 'must' be indifferent to its orientation; which is false if it happens to be magnetised.

Now the application of statistical methods (*i.e.* of probability theory) to any experimental data is no more and no less than the transference to one type of empirical situation of systematised knowledge gained originally from experience of other empirical situations; for there are no such things, as was once erroneously supposed, as god-given *a priori* probabilities. And if the situation to which the methods are applied is not strictly analogous in its relevant features to those on which our theory was based, the transfer may be illegitimate, things may go wrong, and invalid conclusions be drawn. In most cases we are on reasonably safe ground, for common experience tells us that the behaviour of, for example, properly made cards or dice, approximates sufficiently closely for all practical purposes to that of the idealised versions usually postulated by theory. But it is not the less important to realise, as a matter of principle, that the assumptions we may then, quite legitimately, proceed to make are ultimately based on practical experience and reasoning therefrom by analogy, not on any kind of divine right. Usually, of course, our ' intuitive ' judgements, to the effect that in the absence of some systematic cause one thing is as likely to happen as another, will be reliable ; at any rate in so far as they are expressions of properly assimilated if previously unformulated experience. But it would clearly be imprudent to rely on them too implicitly, or to pursue the chain of analogical reasoning too far ; consequently, there is nothing to be lost and a certain assurance rightly to be gained by making fresh contact every now and then with the real world of empirically observable facts.

In this particular case, I have no doubt at all that we were completely within our logical rights in assuming that, in the absence of any systematic cause, the distribution of marks by an unbiassed judge would be in accordance with the terms of the postulates underlying the statistical treatment, or that it would be possible to justify this assumption with virtual rigidity. But I am also pretty sure that no one has hitherto taken the trouble to mark two thousand odd drawings against fifty thoroughly randomised and arbitrarily grouped pseudo-originals ; so that the control marking has certainly established a much closer link between empirical experience and the assumptions of the experiment than could otherwise have been held to exist.

It seems to me, therefore, that although the work may well be deemed to have approximated to supererogation, it cannot possibly be considered to have been waste of time.

SECTION VI

Anticipation of Criticism

1. *General :* It is very meet, right and our bounden duty that we should subject experiments of this kind to the utmost rigour of all reasonable criticism. On the other hand, life is short and the

amount of work to be done in the subject prodigious, so that it is to nobody's advantage to replicate controversy or to waste valuable time in arguing about matters which ought not, properly speaking, to come into dispute at all. I have accordingly thought it expedient to deal here with some of the types of criticism which private discussion has shown to be most likely to be raised, and to attempt some kind of logical analysis of the various ways in which an experiment of this general character might fail to satisfy scientific and evidential criteria. It is hoped that prophylactic treatment on these lines may prevent subsequent discussion from drifting into irrelevant backwaters. I need hardly say, I hope, that there is not, in my opinion, *any* valid criticism of a fundamental character which can be brought against the work—otherwise I should not be publishing it; but there seems to be quite a number of points which people find it difficult to grasp and these may better be dealt with in advance than in arrear. This is not intended to imply that the experiments could not have been better carried out; on the contrary, there are many details which I think I could now improve upon in the light of experience. But I do not believe there is any weakness either of theoretical method or practical procedure which is competent to invalidate, or even appreciably to weaken, the main conclusions reached.

Broadly speaking, we may expect to find critics divided into two main classes. First, there will be a few who, while admitting that there is nothing wrong with the experimental evidence, will contend that the existence of a cognitive relation between any person and a drawing the nature of which he cannot determine by any sensory process or by rational inference is impossible, or at least so antecedently improbable that no evidence could be considered convincing. Second, there will be a much larger class who will suggest that there is some flaw or weakness either in the general method adopted or in the manner in which it was carried out.

The first class are presumably of greater interest to the pathologist than they are ever likely to be to me; but for the benefit of the small and reasonable sub-class who are genuinely and semi-rationally troubled about antecedent probabilities two or three points are worth briefly noting which I do not think have hitherto been made.

First, it seems quite clear that all estimates of probability are either based on empirical experience or are worthless; hence there can be no such thing as a truly *a priori* probability, though there may be well-founded estimates of probability formed antecedent to the undertaking of any particular piece of investigation. Second, any empirical estimate of a probability must be deduced from the study of a sufficiently large aggregate of comparable instances, *e.g.* throws of a die or drawings from a pack, to enable a reasonably accurate estimate to be formed of the limiting ratio of the relative frequencies of the occurrences and non-occurrences of the event in question ; it follows that the 'antecedent improbabilitist' has the unenviable negative task of showing that the many indications of paranormal cognition which have from time to time been obtained are *not* in excess of what can reasonably be attributed to chance, fraud and the like ; at the very best he must show (*a*) that so many cases are explicable in these terms that there is a strong probability that any new and similar case is thus explicable, and (*b*) that this case is similar. Third, if he declares that the occurrence of a faint but widely diffused power of paranormal cognition, such as I have found in the course of this work, is contrary to common experience, one can only reply that it is not ; on the contrary, every second person one meets has a story suggestive of paranormal cognition in some form or other, either at first or second hand, while the whole literature, not merely of Psychical Research, but of History, Anthropology and Biography, is crammed with instances of varying degrees of authenticity. Fourth and finally, what would be contrary to common experience, to the point of incredibility, would be if no concatenation of circumstances had ever resulted in the kind and degree of ability I have found ever resulting in an incident sufficiently striking to be put on record.

In short, criticism on these lines, even in its most reasonable form, does not seem to me to have a leg to stand upon ; for the whole of the antecedent and contextual evidence appears far more consonant with the supposition that what we now call 'paranormal' modes of cognition constitute a fact in Nature, often enough "twisted by knaves to make a trap for fools" but none the less real for that, while attempts to explain them away may be due to a wide range of motives—from a laudable desire to discourage superstition, through a vague dread of their ill-deduced implications, to a petty dislike of anything not smugly subsumable under the critic's own conception of how Nature ought to behave.

Turning to the second class of critics, with whom I have every sympathy (for have I not been of their company where much other work has been concerned?) it seems to me that not-obviously-stupid criticism can again be divided into two main types, of which one is concerned with what I will call Real Possibilities of Error, while the other arises solely from misapprehensions of the logical basis of the work. The possibility of sensory perception would be a perfect example of the first, while the delusion that the use of originals depicting common objects might possibly foster a spurious positive result would typically represent the second.

2. *Real Possibilities of Error :* Using the term ' error ' in a somewhat wider sense than usual, these possibilities may conveniently be dealt with under the heads of Fraud, Misreporting, Sensory leakage, Inference, Marking and Statistical Treatment.

As regards Fraud, it is entirely proper that, in the present state of the art and in view of the unfortunate history of much of our subject, the possibility of deception should be seriously considered, without any regard at all to personal feelings, wherever it seems plausibly applicable ; but I do not think that this is a case where it is. It certainly would have been if I had been working with professional music-hall telepathists, or if H.S.C. and I had set up as a pair of modern Zaneigs, or if I had relied wholly or mainly on the performance of some one especially gifted percipient, or even on those of a very few ; whereas none of these circumstances obtain. Apart from this, I very much doubt whether I could have put through so monumental a deception without arousing the suspicions of some at least of my various colleagues and consultants ; and even if I could, it would still have been necessary either to conspire with a large number of my percipients, or else to fabricate drawings on a fairly extensive scale. The last would probably have been the best plan, and a skilled forger could presumably manage it. On the other hand, all drawings, markings, tables and calculations are, of course, available for inspection by any interested and responsible person.

But considerations of this kind do not constitute the proper rebuttal of any such suggestions. As I indicated at the very outset of this paper, the development of a repeatable experiment—the most urgent desideratum in the subject—has been one of my most important objectives throughout. I believe I have succeeded in doing so ; consequently, I am in the position of being able to make the only completely satisfactory reply (even better than pistols for two and coffee for one before breakfast), namely, " If you don't believe me, go and try for yourself."

Great stress has often been laid on the importance of predictability as the acid test of genuine phenomena. No sane person, of course, would agree with the pronouncement " Only when conditions can be so controlled that, *e.g.*, a teacher can announce beforehand that, on such a day, hour, and place he will demonstrate these things " [*viz.*, telepathy or clairvoyance] " can they or will they be accepted by any sound scientific mind ", but the general principle is sound enough.

I accordingly venture to predict to the following extent: Take a group of about 30 or more university students of mixed sexes and perform a ten day experiment as described in my account of Expt. I: be scrupulously careful to exclude all sensory clues: Select the subjects of originals from some much larger list of reasonably common objects, avoiding ambiguity, by any substantially random method: repeat the experiment, preferably with different percipients, two or three months later, using different originals: have the drawings scored 'blind' against the 20 shuffled originals, by groups as described above, using a rather generous standard of scoring: if necessary repeat the experiment a third time. I am fairly confident that at least promising results will be obtained, though I would not care to guarantee significance; and when I say 'promising' I mean sufficiently good to justify any reasonable person in continuing the work. Naturally, a dozen small points come to mind, which can be gathered from the details given in the preceding pages; but there is one possibly important reservation which I think it fair to make. Inasmuch as the process may be and probably is of a 'telepathic' character it might very well be upset by a hostile attitude on the part of the experimenter, so I think it legitimate to insist that such an experiment should be undertaken with a reasonable amount of good will. That is to say, I doubt whether an experimenter would be likely to obtain good results if he started with the attitude "This man is an adjectival charlatan, but I suppose it's my duty to show that there is nothing in it". Subject to this reservation, I fail to see why anyone should not repeat the experiments with success, substantially at any time and in any place and with any percipients he pleases—unless, of course, I happen to be an exceptionally good telepathic agent, which there is no reason for supposing and would be a singularly harsh piece of misfortune.

The possibility of Misreporting, though one of the most important in some branches of our subject, is hardly one that is likely to be applicable here. There is no question of whether I did or did not hold hands in the dark, or of the squirmings of ectoplasmic pseudopodia in a dim light; nor even, to come a little nearer home, of making statements of the type "The percipient was seated so that she could not see the cards", which would need very careful expansion before acceptance. I think I am right in saying that all evidentially important statements made are of such a nature that they must be either lies or substantially accurate; but I shall be very pleased to elucidate any ambiguity which has escaped my notice.

The likelihood of percipients having obtained knowledge of the drawings by sensory means [1] may, I believe, be dismissed with even greater brevity. Not even Sam Weller's million magnifying power telescopes would enable people to see through brick walls, hundreds of intervening objects, or even curtains—let alone three thousand miles of bulging Earth between here and North Carolina. I don't know whether anyone will be so rash as to try to attack on this front ; but I'm afraid he will meet with but little success if he does.

Similarly, I find it hard to imagine that anyone will suggest that the percipients were able to infer by any rational process what the nature of the originals was likely to be, either in respect of any particular occasion or of any experiment taken as a whole. If a percipient had known (as none did) the precise method of selection we were using in the first, third and fourth experiments, and what dictionary was employed, he might have successfully inferred that originals as a whole were somewhat more likely to be drawn from one section of the dictionary than from another ; but this would not have helped him to decide which page was most likely to be used on a given occasion or in a given experiment.

Personally, if I were coerced into leading a forlorn hope against an impregnable position of this kind, I should concentrate on the Marking, and should try to make out that the Instructions and Notes provided, wittingly or unwittingly, clues and guidances calculated to ensure an undue assignment of ' winning ' points. It is to forestall attempts of this kind that I have published the relevant material in full in Appendix IV, and I do not think that any plausible case can be made out on these lines.[2] In the very unlikely event of this occurring, we can always fall back on the results of the Arbitrated marking, or on the 314 drawings dealt with in the fifth column of Table 5 on page 99. In principle, I should be only too pleased to see these 314 drawings scored blind by any reasonable panel of unbiassed (*i.e.* ignorant) judges ; though in practice I should suggest that their time would be far better spent in repeating the experiment.[3]

We are left with the question of Statistical Treatment, and here,

[1] By ' sensory means ' I refer to any physiological process involving the stimulation of a peripheral receptor and the transmission of an impulse along an afferent nerve fibre to the central nervous system.

[2] In case anyone wishes to make such an attempt, I hasten to point out that it must be completely specific ; that is to say, expressions of opinion are not enough ; it must be explicitly stated which Instruction or Note is thought likely to generate a spurious positive result, and how.

[3] I have said " these 314 drawings ", but strictly it should be the whole 2,193 unless the critic concerned is prepared to accept my assurances as to which the 314 were, and on other relevant points.

I think, I am on ground no less firm than elsewhere. The procedure whereby we calculate the expected number of Winners, explained on p. 83 above, is no more than the simplest rule-of-three arithmetic; the method of obtaining the variance is due to Mr W. L. Stevens and has not been challenged[1]; or alternatively, where the numbers are as large as they are here, we may use a simple χ^2 method familiar nowadays to almost every scientific worker. The only point I can imagine being raised in this connection is the very silly and trivial one that my first method of assessment did not work, so that I was forced to shift the basis of the enquiry from particular occasions to experiments as wholes; on this ground, in a narrowly literal sense, I might be accused of having 'selected my test of significance after I had seen the data'. The answer to this is "So then what? Do you suggest that the results are due to chance, or are you merely indulging a taste for dogmatic psittacism?" However, we need not worry about this unless someone is unwise enough to start it.

3. *Misapprehensions:* As will have been gathered from the foregoing I feel I have nothing to fear from attacks directed on legitimate objectives, that is to say on any feature of the work which is, in fact, relevant to its outcome. If there was no *mala fides* or gross misreporting, if the percipients could not obtain knowledge of the drawings by sensory means or by rational inference, if the marking was truly unbiassed, and if the statistical treatment was correct, then the experiments stand and the results must either be attributed to some mode of cognition not covered by the above, or to chance: and chance alone would yield such results only once, on the average, in some tens of thousands of such investigations.

On the other hand, I am very frightened indeed of the much more dangerous type of critic who does not (or sometimes will not) fully grasp what has been going on, but none the less avers, with much grave and pseudo-judgmatic head-wagging, that he is "not altogether satisfied" about something or other—which usually he is incapable of stating clearly. To describe this kind of thing as 'irresponsible' is to carry courtesy to the point of fulsomeness; for it is not, in other contexts, considered honest to make disparaging allegations without being able at least to define even if not to substantiate them.

A typical example is to be found among those whose resistance to accepting the plain implications of the work is expressed by a melancholy mewing to the effect that "it *may* be chance". Of course they *may* be chance; anything *may* be chance; the behaviour of such persons themselves *may* be chance—in fact this is often the

[1] Cf. Appendix V.

most charitable interpretation of it. But some things are very much more likely to be due to chance than others, and events of the 'once in ten thousand trials' order are not strong candidates for the former class. This kind of thing is exasperating, but I believe that behind it there is often a genuine bepuzzlement which deserves a few lines of consideration. It arises, I believe, from a failure, or refusal, to understand the kind of thing we are trying to do and the object of the tests of significance we apply. Let me try to explain.

Common experience tells us, before we start any experiments of this kind, that innumerable and unspecifiable factors of varying potency will be at work in the percipients' minds and *may* cause them to draw anything whatever, including some of the objects depicted in the originals we use. We may accordingly expect with some confidence that we shall obtain a certain number of apparent 'successes' (*i.e.* resemblances between drawings and originals) which will actually be due to these factors and not to any true cognitive relation between percipient and original. If we do not fully realise this we are very liable to be led by enthusiasm into over-rating what happens and attributing to 'paranormal' cognition effects which are really due to these other factors. The object, and strictly the only object, of a test of significance is to tell us whether this is likely to have happened—and about how likely. When the level of significance is found to be high, as in these experiments, we conclude that the effects observed are *not* likely—indeed, are very unlikely—to be due to such causes; though, of course, it is always open to anyone to contend that, in his opinion, they are even less likely to be due to any alternative cause. This last, however, is of no importance; it would be of interest only if we were attempting to 'prove' the occurrence of the phenomena once and for all by means of a single experiment or group of experiments. But this is not, or should not be, the case. Rare and obscure phenomena are not 'proved' in this sort of way; they gradually become established and accepted through familiarity and through a gradual elucidation of the conditions of their occurrence and the way in which they work. If experiments of the kind here described are never successfully repeated, if we never succeed in discovering the 'laws' concerned, then the critical historian of the future will be perfectly correct in writing them off as some kind of a *lusus naturae.* But in the meantime we are perfectly entitled to say that the likelihood of their outcome being a *chance* effect is so remote that no one need fear wasting his time in pushing the investigation further.

Of the more specific misapprehensions that I have encountered there are two which seem to me to merit fairly detailed discussion here. Of these, the first is concerned with the desirability of employing a plurality of judges, the second with the question of whether the result can be swayed in one direction or the other by the use of common or rare originals.

The first can be made to sound extremely plausible, if it is put in some such form as "Surely you will not allow results of such importance to rest on the opinion of a single judge?"; but none the less it arises solely from confusion of thought as to the issues involved. It is true that I am relying, and am perfectly content to rely, on the *markings* of a single judge, but I am not relying at all on his *opinion* in any sense that would make this type of criticism valid. Yet, in another sense, I am relying on his opinions to such an extent that if they were usually ill-founded no significant effect would be likely to emerge. If we can clear up this apparent paradox, we shall not only settle the particular point at issue but a number of others also of a somewhat similar nature. We can put it very briefly by saying that the kind of opinion on which we do rely, and which is of great importance, is a judgement to the effect that "This drawing is sufficiently like that original to be worth giving it a mark (or half a mark)"; and that the kind of opinion on which we do not rely is a judgement to the effect that "This drawing is a winner (*i.e.* a hit in the right place)". But this is unduly cryptic and calls for careful elucidation.

I think I can best clarify the issues, which are exceedingly important, by considering two apparently contradictory statements, both of which are true. The first may be put in the form "It makes no difference what the judge does", and the second as "A good judge makes all the difference". The catch here is this, that the first statement is true and the second false if there is no real cognitive effect, but only chance, at work; while the second is true and the first false, if there is. The point will become clearer if we expand the first to the form "If there is *no* real effect, a judge who does not know the answer [1] cannot generate a spurious one by any kind of wishful or misguided marking", and the second to the form "If there *is* a real effect, a judge who is intelligent and discriminating will bring it out better than one who is neither".

Even these expanded forms need further consideration, and I think the best way to ensure it is to invite the critic to imagine himself in the position of a judge intensely anxious to secure (or prevent) the obtaining of a spurious positive result. How is he

[1] I use the phrase 'know the answer' as a convenient condensation of 'know which originals belong to which experiment', or, more generally, 'know what allocations or markings will favour a positive result.'

going to set about it? He is given one batch of drawings which he knows to be those of the percipients of Expt. I, another of Expt. II, and so on: he is also given 50 originals arranged in alphabetical order—Apple, Beaver, Cat, Cormorant, Distaff, etc., down to Xylophone, Yoke and Zeppelin, or whatever they may be. But (and this is vital) he does NOT know which of these originals were used in Expt. I, which in Expt. II, etc., nor has he any clue to guide him. He starts work on the first batch of drawings and soon comes across, we will suppose, a number of rather indeterminate birds. Is he to reckon these as hits on Cormorant, or as half hits, or as not worth a mark at all? Which policy will best serve his ends? If Cormorant is one of the originals of Expt. I, then reckoning all these as full hits will tend to produce a positive result; but if it happens not to be, then the reverse. But this is exactly what he does not know, so that he will be quite unable to decide whether generosity or strictness will pay him best. And the same applies to every original of the 50 and to every batch of drawings. Thus it is literally impossible for any degree of fancifulness, of prejudice in either direction, or of eccentricity on the part of the judge to generate anything but a chance effect if there is no non-chance relationship between the drawings and the originals.

The same applies, though not quite so obviously, to capriciousness or lack of consistency. I have heard it asked "Suppose it just happens that [1] the judge is feeling generous when he is considering a drawing of Expt. I and looking at an original of Expt. I, and strict when he is considering a drawing of Expt. I but looking at an original of Expt. II?". The answer is "Suppose it 'just happens' the other way round, the effect will be reversed. Have you any reason for supposing that the one situation will arise appreciably more often than the other (if so, state it); and are not these fluctuations of the standard exactly what we refer to, among other things, when we use the word 'chance'?". It goes without saying that fluctuations of this kind will frequently occur and that they may often be the deciding factor in cases of doubt; but unless we postulate relevant knowledge on the part of the judge (excluded by hypothesis), or suppose him endowed with paranormal powers (which would be begging the question) they can have no more systematic influence on the outcome than would decisions obtained by tossing a halfpenny or throwing a die.

Now turn to the other side of the question, and suppose that there *is* a real effect to which we are anxious to do justice. In these circum-

[1] This is a common involuntary trick whereby the notion of chance is smuggled incognito into a sentence ostensibly not containing it.

stances the intelligence and ability of the judge is likely to be of the very greatest importance. The material, on this hypothesis, will be made up of three types of drawing; there will be some which have no resemblance or connection with the originals at all; there will be some which show resemblances or associations determined by chance alone; and there will be a certain proportion of which the nature or form has been to some greater or less extent influenced by the cognitive process postulated. Now, unless we can hit on some infallible sign whereby we can distinguish lucky shots from genuine cognitions, we shall never be able to eliminate the second class of resemblances, which will always be present to dilute the real effect in some degree. The ideal judge would therefore be he who would reject all specimens of the first class, but successfully detect and correctly mark all members of the third; this would bring out to the fullest possible advantage whatever real effect the material might contain. At the other extreme, even if the material consisted wholly of genuine cognitions (class three) it could scarcely survive marking at the hands of a blind imbecile, who would presumably allot points in a completely haphazard manner having no reference to the resemblances actually observable.

It is in this sense, and in this only, that what the judge does is of importance; just as it is in the other sense, and in that only, that what he does can make no difference. If this is not now clear, I fear I must despair of ever making it so, and must be content to wait for the spontaneous growth of comprehension in the minds of those concerned.

It is worth noting in passing, however, that the evidence suggests that Mr Hindson was a singularly 'good' judge in the sense just discussed; for it will be remembered that even those of his half-points which the arbitrators rejected as implausible resemblances showed a just significant result when treated alone. This is very remarkable and strongly suggests that Mr Hindson has a definite 'flair' for detecting the kind of remote resemblance which results from a very imperfect cognition. I shall always regret that I asked him, as recorded on p. 88, to raise his standard after scoring the drawings of the first experiment; if I had not done so, we might well have obtained much more information about what kind of distortion takes place and what the limit of 'genuine' resemblance is.

This brings us back to the question of the plurality of judges from a fresh angle. I greatly fear that, in spite of all that I have said above, I shall be urged to have the marking repeated by an independent judge or judges with a view to strengthening the validity of the results obtained; and if I decline to do so (as I shall) I am likely to be told that I am afraid of such a re-marking giving a null, or much less significant, result. This, of course, is rubbish, for a much less significant result by another judge would no more invalidate that already obtained than a much more significant result would strengthen it; it would only show the second judge to be inferior (in the sense discussed above) to Mr Hindson, or *vice versa*. Chance, as an explanation of the effects observed is already out of court, except from the apriorist standpoint dealt with earlier, and no reduplication of marking will bring it in again. Nothing is to be gained by wasting the time of judges in doing what we know to be unnecessary, when they might be doing something useful; or by bowing oneself in the house of unreason when one's logical position is impregnable.

The position would be very different, and re-marking desirable to the point of necessity, if it could be shown that the Instructions, etc., gave biassing guidance to the judge, or if it could be plausibly maintained, in face of his testimony and my own, that Mr Hindson had, in fact, any notion of which originals were used in which experiment, or even any clues by which he might have known. Apart from this, re-marking in this case, or plurality of markings in general, will only throw light on the relative merits of the judges, not on the validity of results.[1] Such questions as the extent to which judges differ, and why, or of what principles of judging are best calculated to bring out real effects in varying types of material, are of no small intrinsic interest, and will undoubtedly demand investigation in due course. But they have nothing whatever to do with whether the results of these or similar experiments are valid, and it is wholly illegitimate to contend that they have.

A great deal of what has just been said is applicable, *mutatis mutandis*, to the cognate suggestion that the outcome of the experiment may be influenced by the kind of originals used. If we substitute 'ignorance of percipients' for 'ignorance of the judge', we shall find a close parallel between the two situations, the second of which accordingly does not need detailed discussion here. Just as, in one sense, it makes no difference what the judge does, so, in the corresponding sense, it makes no difference what originals are used; and just as, in the other sense, some judges are better than

[1] To cover a point which might be raised: If we were to submit to a judge, under the conditions described, material which we believed to embody a significant real effect, and were to obtain a null result, we should be perfectly entitled to re-submit to another or others in the hope that the first was incompetent—always provided that we made due allowance, in the usual way, for the number of judges so employed when making our final estimate of probability.

others, so, in the corresponding sense, some originals are likely to be better than others. And just as it is impossible to show how a judge who does not know the answer can fabricate a spurious result by any process of mis-marking, so it is impossible to show how an experimenter can fabricate a spurious result, through the use of one sort of original rather than another, provided the percipients have no possibility of telling, by sensory means or rational inference, what originals are likely to be used on which occasions. Finally, again as in the preceding case, the best way of convincing oneself that this is true is to try to work out a plan for generating a spurious effect, by the use of any type of originals whatever, subject to the restrictions that the original to be used on any occasion is selected by a random method, and that the percipients are given no clue as to what it is.

4. *Summing-up :* I have no desire to appoint myself dictator-like, as judge in my own cause, though it seems fair enough to insist, as I have just been doing, that criticism should be directed towards points which might, in principle, be vulnerable rather than against those which could never be. It is true that my most anxious scrutiny has failed hitherto to find any source of systematic error in either the method or procedure adopted, and that I do not believe that there is one. But it is not by my opinion, or even by that of others however eminent, that the work must be judged as successful or otherwise ; it is by the test of repeatability alone that it must ultimately stand or fall. If properly conducted attempts to repeat my results consistently fail to do so, no amount of argument on my part will off-set the fact ; and if, in a fair proportion of such cases, similar results are obtained, argument will be superfluous.

So we may end this Section very much as we began it : I have tried to produce a repeatable experiment ; I believe that I have succeeded ; if, for whatever reason, you distrust the conclusions reached—Try it yourselves.

SECTION VII

Summary and Conclusion

This paper is already depressingly lengthy, though I doubt whether I could have shortened it appreciably without damaging omissions of important details. I will accordingly do no more by way of bringing it to an end than run over the essential features of the work in briefest summary and add a very few comments of general interest for which no appropriate place has been found elsewhere.

Five experiments[1] in the 'paranormal' cognition of distant drawings have been conducted. In each of these, ten different originals were used, and ten fresh originals were used in each experiment. All originals were selected by a substantially random method. About 250 percipients of both sexes, producing about 2,200 drawings, took part. Very few percipients, probably not more than ten or twelve, took part in more than one experiment. In no experiment was there the smallest possibility, humanly speaking, of any percipient obtaining any clue to the nature of any original by normal sensory means or by rational inference. The drawings were marked against the whole fifty originals by a judge who had no clue or information as to which originals were used in which experiment. A total of 1,209 drawings were found which were judged to be sufficiently like one or other of the originals to deserve mention. From the data it is possible to calculate how many of these resemblances or 'hits' would be 'winners', *i.e.* hits on originals used in their own experiment, if chance alone were operative, and how often this value would be exceeded by any given amount. It is found that the excess is such as would be equalled or surpassed only about once in some thirty thousand such investigations if chance alone were responsible. In other words, percipients' drawings resemble the originals (considered as a group) at which they are aiming more closely than they resemble originals at which they were not aiming to an extent which cannot plausibly be attributed to chance.

Examination of the data from another point of view shows that these resemblances do not occur exclusively, or even most often, at the same time as the display of the original concerned. But there is a fairly regular tendency for drawings which resemble a given original to occur relatively more frequently on occasions which are near to that on which it was displayed than on others which are more remote. This effect is observable to a significant extent in both directions.

The main conclusions indicated by the facts are, first that there is a real cognitive relation of some kind (direct or indirect) between percipients and originals, second that this may be either of precognitive or retrocognitive form.

The above summary, if somewhat arid, appears to cover ade-

[1] I say 'five' here because it will be more convenient henceforward to think of the experiments here described as Experiments I to V, rather than I to IV B; otherwise we may find ourselves getting confused at some later stage by having, say, seven experiments with eighty originals. The next experiment will be numbered VI.

quately the essential facts and inferences therefrom, but I may be permitted a few comments of a slightly more speculative nature.

I suppose most readers will want to ask " What kind of a process do you think is involved? ". I do not think the time is ripe for saying, or even suggesting, what the process *is* ; but I am prepared to record a few quite tentative impressions which suggest fairly strongly what it is *not*. First and foremost, I am as confident as I could be without special experimentation that it is in no sense a matter of the percipient copying or in any way 'seeing' the drawing. In contrast to M. Warcollier's recently published views, I have the strong impression that it is the ' idea ' rather than the form that is cognised. For example, it is as if the percipient were told ' Draw a Hand ' rather than ' Copy this Hand ', for we get left hands and right hands, open hands and closed hands, apparently quite indiscriminately, which we surely would not do if it were a matter of copying something seen. Very seldom indeed have I received the impression that it is the uninterpreted form itself that has ' got across ', and even then I fancy that it has been the ' idea of the form ' rather than the lines themselves that have been concerned. In fact, on the strict understanding that this is entirely conjectural and ' for purposes of entertainment only ', I increasingly incline to the view that the lines on the paper have nothing to do with it at all, except perhaps as a focus for the thought or attention of the experimenter.

The astute reader will correctly deduce from this that my impression at present is one of a ' telepathic ' rather than a ' clairvoyant ' phenomenon. This is true as far as it goes, but only subject to the very important reservation that I am not at all sure that the current conceptions of either the one or the other will necessarily fit the facts. The truth of the matter is that we do not know what kind of a process is involved, and it would be a mistake to handicap ourselves by trying to tie it up prematurely with any preconceived notions.

It seems pretty clear that the percipients show a cognitive relation to the originals which cannot be attributed to any ' normal ' cause ; and I have no doubt at all that the experimenter's mind (H.S.C.'s or mine) plays an important part in establishing that relation. Indeed, certain work now in progress suggests on inspection (I will not say more) that the mind of a third party, neither experimenter nor percipient but still connected with the experiment in certain respects, may significantly influence the results ; and the bare suspicion that such an effect may be exercised in a manner determinable by experiment should make us very cautious about formulating even the most tentative explanations.

I will end by drawing attention to the fact that the effect found, though highly significant, is intrinsically very faint. On the basis of the Hindson 'All Entries' data, which I have so extensively used, a total of rather more than 2,000 drawings yields a crop of just over fifty hits above chance expectation; so we may reasonably speak of a $2\frac{1}{2}\%$ effect. Presumably this is an underestimate, partly because I artificially restricted Mr Hindson's natural judgement, partly because it may fairly be supposed that a perfect marker would have done a little better. But I should be inclined to doubt whether any marking however perfect would raise the figure much above 5%.

This, to my mind, is a very gratifying order of result. It is admittedly small, but so is the amount of radium in pitchblende which no one on that account denies is important and interesting. On the other hand, if I had found an effect of some ten times the size, I should have felt it too good to be true and have suspected the presence of many large flies in the ointment. As it is, it seems to me that what I have found (especially when we add the complications due to displacement) is eminently compatible with both sides of common experience—with the knowledge that on the whole people very seldom show signs of paranormal cognition, and with the knowledge that none the less they occasionally do. Finally, the fact that, so far as I can judge by inspection, the ability concerned is pretty widely distributed, or at least not concentrated to any startling degree among a very few specially gifted persons, suggests that it is likely to prove an attribute common to all humanity, with nothing alarmingly magical about it; so that perhaps the adjective 'paranormal' is something of a misnomer after all.

EXAMPLE I

MAIN RESULT FOR HINDSON FIGURES

TABLE OF POINTS AWARDED:

Hits by the Drawings of Experiments

On the Originals of	I	II	III	IV A	IV B	Total
I	**51·5**	7·0	5·5	40·5	23·0	127·5
II	77·5	**18·0**	7·5	51·0	27·0	181·0
III	36·5	12·5	**6·5**	45·0	36·5	137·0
IV A	25·0	7·0	4·0	**41·0**	24·5	101·5
IV B	82·0	19·0	10·0	106·0	**84·5**	301·5
Total:	272·5	63·5	33·5	283·5	195·5	848·5

Let $a \ldots a_5$, $b \ldots b_5$ be the row and column Totals, taken in order, respectively (or *vice versa*). Then

a_1b_1	34,743·75 ;	a_1+b_1	400·0 ;	$a_1b_1(a_1+b_1)$	13,897,500·000
a_2b_2	11,493·50 ;	a_2+b_2	244·5 ;	$a_2b_2(a_2+b_2)$	2,810,160·750
a_3b_3	4,589·50 ;	a_3+b_3	170·5 ;	$a_3b_3(a_3+b_3)$	782,509·750
a_4b_4	28,775·25 ;	a_4+b_4	385·0 ;	$a_4b_4(a_4+b_4)$	11,078,471·250
a_5b_5	58,943·25 ;	a_5+b_5	497·0 ;	$a_5b_5(a_5+b_5)$	29,294,795·250
Sums:	138,545·25 ;		1,697·0 ;		57,863,437·000
E_w	163·28 ;	O_w	201·50 ;	D	38·22 ;
N	848·50 ;	N^2	719,952·25 ;	$N^2(N-1)$	610,159,531·875

Whence

$S^2(ab)$	19,194,786,297·5625
$N^2 . S(ab)$	99,745,964,464·3125
Sum	118,940,750,761·8750
$N . \overline{S(ab)(a+b)}$	49,097,126,294·5000
Subtracting	69,843,624,467·3750
Dividing by $N^2(N-1)$ gives	σ^2 114·468
Whence	σ 10·699
D/σ	3·572

$P < \cdot 001$ or 1/2,944 v.n.

EXAMPLE II

Distribution of Hits on Originals of Expt. I made on Occasions of Expt. I

Originals	Occasions 1	2	3	4	5	6	7	8	9	10	Total
1. Bracket	**1**	.	.	1	2	.	1	.	.	2	7
2. Buffalo	1	**1**	1	4	2	1	1	.	2	2	15
3. Embattled Fess	.	.	**1**	.	2	.	.	.	.	1	4
4. Hand	.	1	.	**1**	1	1	2	2	.	1	9
5. Cross-stitch	2	2	3	.	**3**	2	1	.	1	2	16
6. Bottle	.	2	.	2	.	.	2	3	.	.	9
7. Bat	1	.	.	2	.	1	.	1	3	1	9
8. Net	.	.	.	.	.	2	2	.	2	2	8
9. Beetle	.	.	.	.	.	.	1	.	**1**	1	3
10. Anchor	.	1	.	.	.	.	.	.	.	.	1
Totals :	5	7	5	10	10	7	10	6	9	12	81

Then we have

Diagonal	O	E	$(O-E)$	$(O-E)/\sqrt{E}$
−9	.	·062	− ·062	− ·249
−8	1	·272	·728	1·396
−7	.	·815	− ·815	− ·903
−6	1	1·556	− ·556	− ·446
−5	.	2·321	−2·321	−1·523
−4	4	3·763	·237	·122
−3	4	4·975	− ·975	− ·437
−2	9	5·371	3·629	1·905
−1	4	7·013	−3·013	−1·137
0	**8**	**8·025**	**− ·025**	**− ·088**
1	10	7·630	2·370	·858
2	16	8·383	7·617	2·630
3	6	7·692	−1·692	− ·610
4	6	6·432	− ·432	− ·170
5	3	6·124	−3·124	−1·262
6	2	3·753	−1·753	− ·905
7	3	2·778	·222	·133
8	2	3·000	−1·000	− ·577
9	2	1·037	·963	·946
Totals :	81	81·002	− ·002	
Totals for				
− ve part :	23	26·148	−3·148	− ·616
,, + ve ,,	50	46·829	3·171	·463

TABLE I: Hindson 'All Entries' Data, Showing Expectations and Differences

Hits by the Drawings of Experiment

On the Originals of		I	II	III	IVA	IVB	Total
	O	81	10	9	53	33	186
I	E	64·46	13·23	7·69	59·38	41·23	185·99
	O − E	16·54	−3·23	1·31	−6·38	−8·23	
	O	125	26	11	76	44	282
II	E	97·73	20·06	11·66	90·03	62·51	281·99
	O − E	27·27	5·94	− ·66	−14·03	−18·51	
	O	61	15	9	66	52	203
III	E	70·35	14·44	8·40	64·81	45·00	203·00
	O − E	−9·35	·56	·60	1·19	7·00	
	O	43	11	7	58	33	152
IV A	E	52·68	10·81	6·29	48·53	33·69	152·00
	O − E	−9·68	·19	·71	9·47	− ·69	
	O	109	24	14	133	106	386
IV B	E	133·78	27·46	15·96	123·24	85·56	386·00
	O − E	−24·78	−3·46	−1·96	9·76	20·44	
Total	O	419	86	50	386	268	1,209
,,	E	419·00	86·00	50·00	385·99	267·99	1,208·98

Note : The expectation in any cell is given by the product of the appropriate marginal totals divided by the Total number of hits (1,209).

The Total Expectation for the leading diagonal is 227·01 ; the total number of hits observed is 280 ; the difference is 52·99. Thus the value of χ^2 for the leading diagonal, using Yates' correction, is

$$52{\cdot}5^2/227 + 52{\cdot}5^2/982 = 14{\cdot}95, \text{ with P about } {\cdot}000{,}1.$$

TABLE II: Samples and Group Sub-Totals of the 99 Diagonals of the 50 × 50 Table

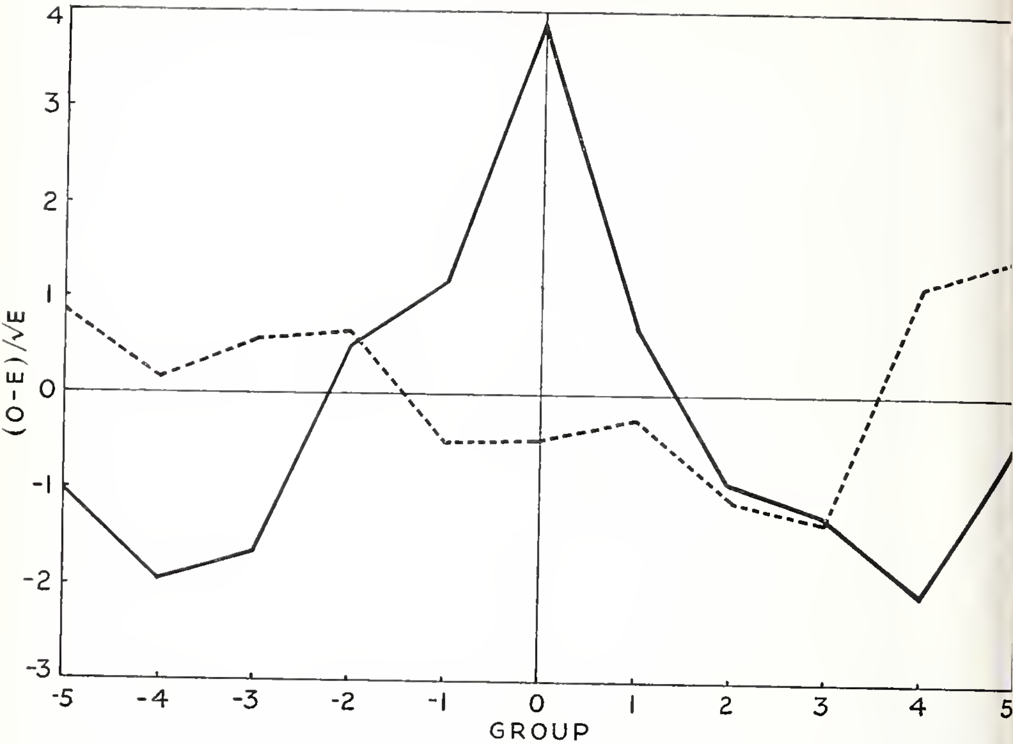
(Groups of Nine Diagonals)

Diagonals	*O*	*E*	*(O − E)*	*(O − E)/√E*	χ^2
−49 to −41					
Sub-totals	43	50·312	− 7·312	−1·031	1·06
−40	11	12·900	− 1·900	− ·529	
−39	9	14·084	− 5·084	−1·355	
−38	14	13·474	·526	·143	
−37	14	14·371	− ·371	− ·098	
−36	12	11·778	·222	·065	
−35	6	8·566	− 2·566	− ·877	
−34	4	9·143	− 5·143	−1·701	
−33	3	8·933	− 5·933	−1·985	
−32	10	9·776	·224	·072	

TABLE II (*continued*)

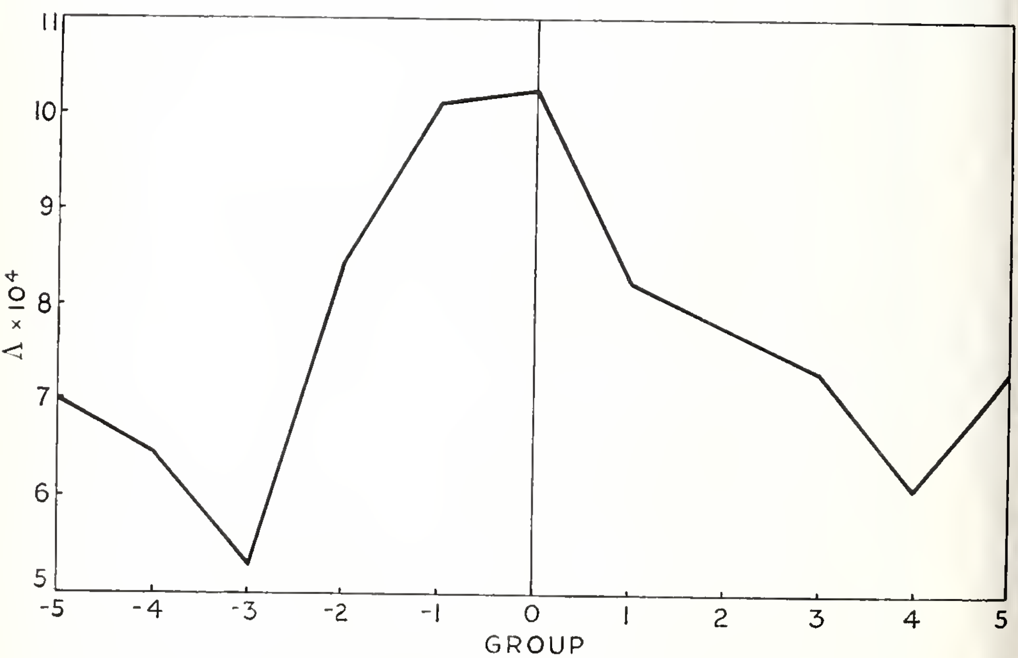
Diagonals	O	E	$(O-E)$	$(O-E)/\sqrt{E}$	χ^2
Sub-totals	83	103·025	−20·025	−1·973	3·89
−31 to −23					
Sub-totals	63	77·801	−14·801	−1·678	2·82
−22 to −14					
Sub-totals	121	115·370	5·630	·524	·27
−13 to − 5					
Sub-totals	220	203·100	16·900	1·186	1·41
−4	30	24·298	5·702	1·156	
−3	19	23·995	− 4·995	−1·020	
−2	40	22·544	17·456	3·676	
−1	33	24·288	8·712	1·767	
0	**28**	**23·848**	**4·152**	**·850**	
1	31	21·460	9·540	2·059	
2	40	20·616	19·384	4·269	
3	18	20·555	− 2·555	− ·563	
4	15	17·721	− 2·721	− ·646	
Sub-totals	254	199·325	54·675	3·873	15·00
5 to 13					
Sub-totals	139	130·325	8·675	·760	·58
14 to 22					
Sub-totals	113	123·239	−10·239	− ·922	·85
23 to 31					
Sub-totals	103	116·631	−13·631	−1·262	1·59
32	8	11·278	− 3·278	− ·976	
33	10	10·448	− ·448	− ·139	
34	10	10·088	− ·088	− ·028	
35	5	9·422	− 4·422	−1·441	
36	6	8·001	− 2·001	− ·708	
37	3	6·641	− 3·641	−1·413	
38	3	5·596	− 2·596	−1·098	
39	6	4·840	1·160	·527	
40	2	4·173	− 2·173	−1·064	
Sub-totals	53	70·487	−17·487	−2·083	4·33
41 to 49					
Sub-totals	17	19·386	− 2·386	− ·542	·29
TOTALS	1,209	1,209·001	− ·001		32·09

FIGURE I



N.B. The dotted line indicates the corresponding results for the Control Markings, $q,v,$

FIGURE II



APPENDIX I

Copy of Instructions printed on the Outer Covers of Percipients' Books used in Experiment I

Instructions

1. Certain Drawings (none of which will be elaborate) will be displayed in the room of which a photograph is provided, and in the position of the sheet of paper shown pinned to the bookcase. Only one drawing will be shown at a time and a different drawing will be shown on each of ten successive nights. Each drawing will be in position from 7.0 p.m. till 9.30 a.m., starting on the evening of Wednesday, February 1st and ending on the morning of Saturday, February 11th.

You are asked to try to reproduce these as well as you can. Attempts to reproduce a picture may be made at any convenient time during the period of its exposure.

In order to be of use for the purpose of the experiment, it is essential that an attempt should be made on each of the ten nights. Please make an attempt of some kind, whether or not you feel that you are succeeding.

2. Drawings should not be altered or 'finished', and it does not matter if you cannot draw well. If you have several impressions, however crude or vague, make several drawings, *on the same sheet*, writing 'Best' against the one you think most likely to be successful. Please make *all* drawings on the sheet provided, not on rough paper first.

3. Write your full name and the time of making each attempt in the spaces provided on each sheet. Indicate under 'Visual Imagery' whether you had a clear picture 'in your mind's eye'. Write + or 0 after 'Confidence' according to whether you did or did not feel that you were getting a genuine impression of the picture.

4. Be careful to use the sheets in the order which they occupy in the book.

5. Do NOT write the date or any serial number on the sheet, or make any mark which would enable the order of the sheets to be ascertained if they were removed from the book. Do NOT make any Note which would give a clue to the date or ordinal position of the sheet.

6. Please insert the card provided between the sheet you are using and the next, so as to prevent the possibility of marks made on one sheet showing on the other.

7. The Space for Notes and Impressions is not intended for elaborate introspections, but for verbal recording of any 'ideas' which seem relevant but are too difficult to draw. There is no need to make any entry here unless you wish.

8. It is desirable that the reproductions by the various participants should be quite independent of each other; so please do the experiment by yourself and do not discuss your impressions until after the books have been handed in.

9. Return the sheets, in their cover, to Dr Thouless when finished. On no account detach the sheets from the book or use them in the wrong order.

APPENDIX II

Results of the Method of Forced Matching

The Method of Forced Matching and its outcome have been fully discussed in general terms in the text: the subjoined Table shows the actual scores obtained in the first three experiments by various judges. H.S.C. and I did the first and third experiments jointly, but with H.S.C. predominating; she necessarily worked alone in the case of Expt. II, because I knew the orders of both originals and drawings. For this experiment I give also the results obtained by the percipients themselves, which are fractionally worse than H.S.C.'s. As a matter of interest I add the figures for an independent matching of the 37 sets of Expt. I, which was very kindly undertaken by Mr O. L. Zangwill.

TABLE A II. 1

Showing Numbers of Successes obtained by Forced Matching

Correct Matchings	Number of Sets Scoring 0	1	2	3	4	5	6	>6	Total Percpts.	Total Score	Mean Score
	A. Matched by H.S.C. and W.W.C.										
Expt. I	14	12	6	4	.	.	1	.	37	42	1·135
,, II	5	11	2	2	.	.	.	.	20	21	1·050
,, III	5	2	2	2	.	.	.	.	11	12	1·091
Total	24	25	10	8	.	.	1	.	68	75	1·103
	B. Matched by Mr O. L. Zangwill										
Expt. I	12	16	8	1	.	.	.	.	37	35	·946
	C. Matched by Percipients										
Expt. II	7	9	4	.	.	.	.	.	20	17	·850

The Table should be read as follows : When the 37 sets of drawings constituting Expt. I were matched, set by set, against the originals by H.S.C. and myself, and the number of correct matchings ascertained as described in the text, it was found that in 14 cases we had failed to match any drawing successfully against the original used on the corresponding occasion ; in 12 cases we made *one* correct assignment, in six cases we made *two*, in four cases *three*, and in one case *six*.

Since the expected number of successes is in all cases equal to the number of percipients, there is evidently nothing of interest here except the solitary score of six in the first line, which is fully dealt with in the text.

APPENDIX III

Application of the Method of Decimal Scoring

1. *Scoring of Experiments I, II and III.*

This was done by Mr and Mrs Oliver Gatty in two batches. The first consisted of the eleven sets of Expt. III mixed with eleven sets (Nos. 1-10 & 12) taken from Expt. I ; the second included the remaining 26 sets of Expt. I and the 20 sets of Expt. II. Since I am relying for my conclusions solely on the results of the third method of assessment, I need not detail here the innocent subterfuges I adopted to prevent the scorers wittingly applying processes of rational inference to the work. I am, however, satisfied, and the internal evidence suggested, that these were sufficient even if they may not have been necessary.

When the points were separated and added in the way illustrated in the text (p. 77) the results were found to be

(*a*) Experiments I & III :

	Judged to resemble I	III	Total
Sets of Experiment I	10·0	1·0	11·0
,, ,, III	6·5	4·5	11·0
Total	16·5	5·5	22·0

Using the general matching formulae (pp. 83-4) this gives

E 11·0 ; O 14·5 ; D 3·5 ; σ^2 4·321 ; σ 2·079

which is not intrinsically significant, for $D/\sigma = 1{\cdot}684$ with $P \sim {\cdot}09$.

(*b*) Experiments I & II :

	Judged to resemble I	II	Total
Sets of Experiment I	10·5	15·5	26·0
,, ,, II	5·0	15·0	20·0
Total	15·5	30·5	46·0

(*N.B.* A set is counted as half right and half wrong when the points awarded to the two series of originals are equal.)

This leads to

E 22·02 ; O 25·50 ; D 3·48 ; σ^2 10·327.

For the two batches taken together we have

E 33·02 ; O 40·00 ; D 6·98 ; σ^2 14·648

whence we find $D/\sigma = 1{\cdot}824$ with $P \sim {\cdot}07$, which is still not significant.

2. *Scoring of* 80 *sets from Experiment IVA and IVB :*

As explained in the text, the procedure of this experiment was specially devised to enable the somewhat onerous task of scoring the three hundred odd sets which it was hoped to obtain to be undertaken by myself, and special precautions were adopted, as there described, to ensure that I could have no normal knowledge of whether any particular set was aimed at the A or B originals. As it turned out, however, many fewer sets were obtained than had been expected, while of those that were sent in a considerable number showed internal evidence which, as it happened, would have enabled me to judge that they were 'A' sets apart from any intrinsic resemblances of the drawings. However, when these had been weeded out by Prof. Broad, there remained 80 sets on which I could safely work. I again abstain from detailed discussion of the points involved on the ground that, although these results are perfectly reliable, I am not depending on them for my conclusions, and therefore need not establish every point meticulously here.

These 80 sets were given me in two groups, the first consisting of 20 'A' and 20 'B' sets, the second of 10 'A' and 30 'B' ; but, by a not unfortunate misunderstanding, I received the impression that the composition of the second group was the same as that of the first. Thus any bias I may have had in dealing with it was liable to mislead rather than to help : actually, it could hardly do either, for knowledge of the relative numbers of A and B sets in the group could not enable me to decide which were which.

The results obtained were

	Judged to resemble A	B	Total
Sets of Experiment IV A	9·0	21·0	30·0
,, ,, IV B	13·0	37·0	50·0
Total	22·0	58·0	80·0

Applying the same methods, we obtain

E 44·50 ; O 46·00 ; D 1·50 ; σ^2 15·142

which is very nearly as bad as it could be, for D/σ is only 0·386 with P as large as ·7. The result must be incorporated, but it is only of interest as showing the difficulty of separating the A and B portions of Experiment IV, which we shall find persisting even with the much more successful third method of assessment.

3. *Scoring of* 16 *sets from the Individual Experiments :*

In each of these eight experiments which I do not propose to describe in detail here, a ' selected ' percipient was tested, together with either H.S.C. (first six experiments) or W.W.C. (last two). All H.S.C.'s drawings were scored by Dr E. J. Dingwall, the six sets of the selected percipients in the first six experiments by Mr B. E. Parr (to whom I should like to take this opportunity of expressing my gratitude), and the remainder by myself. All judges were kept in appropriate ignorance either as to which originals were which or as to which drawings were aimed at them, or both, as the circumstances required and permitted.

The scoring was done by pairs of experiments and need not be given in detail here. Combining the returns from the three judges

E 8·0 ; O 12·0 ; D 4·0 ; σ^2 2·667

whence we find D/σ 2·449 with $P < \cdot 02$, which is significant.

Adding these quantities to those obtained in 1 and 2 above gives

D 12·48 ; σ^2 32·456 ; D/σ 2·192 ; $P < \cdot 03$.

which is just decently significant but no more. The substantially null results obtained from the 80 sets of Expt. IV evidently vitiate an otherwise not unpromising method, but the final outcome is poor compared with that yielded by the more sensitive method used later.

APPENDIX IV

A. Copy of Instructions, etc., supplied to Mr Hindson for the Application of the Method of Palpable Hits

Guide to Scoring Hits

1. The general idea is to compare every drawing with each of the (50) originals. If any drawing plainly represents the same object or activity as that depicted in one of the originals, a 'hit' is recorded; if not, not.

2. Scoring of hits should be done on the simplest common-sense principles.

The primary question to be asked in respect of any drawing is "Does this drawing represent the same object or activity, or the same sort of object, etc., as one of the originals—or at least something extremely like it?" In other words "Does the drawing indicate that the percipient had 'X' prominently in mind at some time while he was doing the drawing ('X' being the subject of an original)?" If the answer to these is 'Yes', score a hit; if 'No', don't.

The second question, the answer to which may qualify the above, is "Does this drawing indicate that the percipient was thinking of the same kind of thing as was the agent when he produced the original?" Notes on what the agent actually had in mind when he drew the originals will be found in 'Notes on Originals'.

3. Pay no attention to the skill of drawing displayed. A badly drawn Horse, say, is still a horse, provided it is clearly nothing else.

4. Pay as much attention to the written remarks as to the drawings; a mention of an X is just as good as a drawing of an X. One or two 'scorers' have missed very palpable hits here; please be careful.

5. Eschew far-fetched resemblances, 'puzzle-picture' methods, and all but the plainest and most universal associations, like the plague. Be very chary even of these last.

6. Half hits (but no other fractions) may be given in cases of doubt.

7. The greatest difficulty will probably be found in connection with composite drawings containing several elements. Sometimes it is easy enough; for example, if a drawing shows prominently both a Tree and a Horse, a hit should be recorded on Tree and on Horse (Jennet). But one is often in doubt as to whether the X depicted was 'prominently' in the mind of the percipient at the time of making the drawing (Cf. first question in 2 above).

It will probably be convenient to recognise four grades of ' prominence ' as follows : 1. The X is the sole, or virtually sole, object depicted in the picture ; 2. It is ' co-equal ' with other objects ; 3. It is ' secondary ' but must none the less have required attention, or ' thinking of ', for it to have been drawn ; 4. It is purely ' incidental ', *i.e.* jotted in, so to say, as a kind of ' trimming '.

Give hits for the first three categories, but not for the 4th.

8. In general, a part of an object may be taken as equivalent to the whole. *E.g.* It is unlikely that anyone would draw a horse's head without having a whole horse more or less in mind ; by contrast, someone might well draw a hand without having a whole man in mind, so a certain amount of common sense must be used in such cases.

9. Some objects, such as Trees, Boats, Chairs, are very commonly drawn. In certain cases a hit must obviously be recorded, *e.g.* TREE, but if the correspondence is doubtful caution should be exercised and points sparingly given ; otherwise any real effect is liable to be masked or ' diluted ' by points needlessly given to objects drawn only because they happen to be very familiar.

10. On the other hand, some originals are very ' difficult ' and are seldom if ever drawn unmistakeably, *e.g.* Cross-stitch, Embattled Fess, Stop-cock. Perhaps it would be legitimate to relax the usual standard to some extent in such cases ; but this must be for the judgement of the marker.

11. General : Although a high standard should be maintained, so that, as a rule, there can be no reasonable doubt as to the correctness of the hits recorded (and only a half point given when there *is* doubt), it will be understood that we cannot rationally demand exact reproductions of the originals ; drawings ' as good as can be expected ' from a percipient who perhaps cannot draw well and may be supposed to have imperfectly cognised the original may be taken as acceptable.

In this connection it seems not unreasonable to suppose that whereas some subjects (*e.g.* Anchor, Buffalo, Frog, Hammer, Prawn, Violin, Tree) will ' get across ' in their entirety or not at all, others, which are built up of constituent parts—*e.g.* Embattled Fess (Shield *plus* Fess), Flag (plain flag *plus* cross), Royal Standard (plain flag *plus* lion rampant), Windmill (building *plus* sails), etc., might appear only partially ; that is to say, one of the constituents might appear without the other. It may be worth while bearing this in mind in connection with awarding half points.

(Signed) W.W.C.
(Dated) 2.viii.39

B. NOTES ON FIRST 50 ORIGINALS

The following notes are intended to indicate what was actually in the mind(s) of the agent(s) when the originals were drawn, and to draw the attention of scorers to certain doubtful points which have arisen. The latter are mostly dealt with in the form of questions so as to minimise the risk of unduly influencing an otherwise independent scorer.

1. ANCESTOR: The idea in the agent's mind was that of an *old* man, hence the beard and the staff. Should a point or half point be given to any 'man' drawn regardless of whether he has a beard, is leaning on a staff, or shows other signs of age?

2. ANCHOR: Straightforward; no comments.

3. ARROW: Straightforward; as an example (which has not yet been observed) of 'the plainest and most universal associations' it is suggested that it would be legitimate to give at least half a point to a drawing of a Bow, even if the Arrow were not drawn.

4. BALANCE: Fairly straightforward; but what do you propose to do about (if any) Steelyards, similar balanced mechanisms, see-saws, tight-rope walkers?

5. BAT: Drawn and thought of as BAT, though the word found in the dictionary was Flittermouse. What, if anything, will you give for a Mouse or mouse-like creature—birds, beetle, butterflies?

6. BEETLE: No comments.

7. BENCH: What about Chairs? Sofas?

8. BIRD: The dictionary word was 'Corn Bunting', but the original was thought of as BIRD, and the picture is not specifically of a Corn Bunting. The latter is a passerine bird and the picture agrees with this. How are you going to distinguish between this and DODO (*q.v.*) which was thought of as a 'duck-like' bird? Do you propose to give full or half hit for Eagles, Peacocks, Ostriches, Sea Gulls, etc., or only for birds of passerine appearance?

9. BOAT: The dictionary word was SHIP and was illustrated by a full-rigged sailing ship, but the agent thought this too difficult to draw and drew the fore-and-aft rigged sailing boat shown. Always thought of as BOAT rather than SHIP. What do you propose to do about Full-rigged ships, Steamers, Battleships, Boats with masts but no sails, Rowing Boats, Canoes, Racing eights?

10. BOOT: Dictionary word was SHOE, but agent drew a BOOT. What are you going to give to Shoes, Horseshoes, Feet?

11. BOTTLE: This was based on dictionary word VACUUM BOTTLE, which was rejected as too difficult. As indicated, the agent thought of a glass wine bottle. What will you give to Carafes, Medicine bottles, Rubber hot water bottles, Jars, Vases, etc., etc.?

12. Bracket: An unsatisfactory original, because something angular and structural can be found in very many drawings (*e.g.* chairs, houses, bridges, etc., etc.). Cf. paragraph 9 of Guide and keep the standard high to avoid dilution.

13. Brim: The word was Brim, understood in the sense of the rim of a vessel, etc., not a hat-brim. While drawing, attention was focussed on a Chalice or Goblet-shaped vessel as shown.

Do you propose to give full or half hits to all 'containers'—remembering that Bottle and Ewer (*q.v.*) must be distinguished, or to Goblet-shaped cups only? What about vases, bowls, plates, teacups, saucepans, eggcups, teapots, etc.?

14. Buffalo: It is generally agreed that all Cows, Bulls, etc., should count as hits on this original. What about Deer, Rhinoceri or animals with tusks resembling the B's horns?

15. Bulbs: Geissler potash bulbs were illustrated in the dictionary and copied. Agent unfamiliar with these. Probably chief idea in his mind was 'glass bulbs'. What about Electric light bulbs, Hourglasses, Bulbs of the horticultural variety?

16. Butterfly: No comment except that as an example (not yet observed) of 'extremely like' it would be considered correct to give a full hit on this for a Moth.

17. Castle: Battlements (castellations) were chief feature in agent's mind.

18. Cleopatra's Needle: No special comments. Common sense must be used in respect of drawings or mentions of Obelisks, Pyramids, Memorials, Monoliths, Church steeples, etc. Ask yourself whether these are in fact visually or functionally similar to C's Needle.

19. Cotton Aphid: No comments.

20. Cross-stitch: Cf. Guide 10. Consider whether it is likely that a percipient imperfectly cognising this original would produce a St Andrew's Cross, a Latin Cross, a mention of sewing or of embroidery. *In re* Crosses, cf. Flag.

21. Dodo: Thought of by the agent (wrongly as it happens) as a duck-like bird. Known to be flightless. Must be distinguished from Bird (Corn Bunting), *q.v.* Consider what, if anything, you will give for Ducks, geese, swans, etc., Passerine birds, Nondescript birds, Eagles, Gulls, Peacocks, Ostriches, etc.

22. Embattled Fess: An unfortunate original. Agent thought primarily of the Fess, not of the shield; but should anything be given for shields without fesses?

23. Ewer: In the agent's mind the distinguishing features were the handle and the constricted neck. Should full or half hits be

given for two-handled vases? For watering cans, teapots, etc., with handles but also spouts? For teacups, etc., with handles but no constriction? For saucepans, etc., with a different sort of handle? For vases, etc., with constriction but no handle?

Cf. BOTTLE and BRIM.

24. EXFOLIATE : The agent was thinking of Leaves, not Trees. Should anything be given for these, or for leaves attached to Flowers, Fruit, etc.?

25. FAN : No comments.

26. FISH : No special sort of fish was intended.

27. FLAG : It will be necessary to distinguish this from Royal STANDARD (44). The flag drawn is a black flag with a strongly marked White Latin Cross. Consider Black Flags (without crosses), Plain Flags, Striped Flags, Union Jacks ; Latin Crosses without flags, Other sorts of cross ; also pennants, burgees, etc.

28. FLEUR-DE-LYS : No comments.

29. FROG : No comments.

30. HAMMER : Presumably a Mallet would deserve a point, but what about axes and picks?

31. HAND : It is agreed that a hand is a hand regardless of whether it is right or left, open or closed ; also that nothing should be given to human figures merely because they may be presumed to have hands. Consider (*a*) figures with hands prominently outstretched or displayed, (*b*) hands without bodies holding things, (*c*) gloves.

32. HORSE (Jennet) : The dictionary word was JENNET, but the original was drawn and thought of as HORSE.

33. MOUSTACHE : An unlikely original for anyone to draw quite correctly. Should faces with prominent Moustaches be given a whole point, half a point, or nothing?

34. NET : The dictionary word was NET BLOTCH. This was rejected and the agent decided to illustrate NET. The man, beach and waves form a setting for the illustration ; the little fish are incidental.

35. PARNASSUS : The agent was more interested in the Temple than in the Mountain. Consider Greek-type temples (criteria, Pillars, Steps, Pediment) with and without Mountains, Mountains in conjunction with other types of building, Mountains without buildings.

36. PRAWN : Must be sharply distinguished from FISH. What about Lobsters, Crayfish, Crabs, etc.?

37. SATURN : Combination of Sphere and Rings, the last being most in agent's mind on account of difficulty of drawing ellipses freehand.

38. Scissors : No comments.

39. Shell : Definitely conchological ; artillery version not thought of. What about shells of somewhat different shapes—Snails, Oysters?

40. Shooting : See contemporary note on original.

41. Skull : No comments.

42. Spectacles : No comments, except that as examples (not yet observed) it would be proper to give a hit for pince-nez, but not for two tumblers (' a pair of glasses ').

43. Spinning Top : No comments.

44. Standard (Royal Standard) : the dictionary word was Standard, the agent thought of doing Royal Standard and was concerned to make it other than an ordinary sort of flag ; hence the rampant beast intended for a lion.

45. Stop-cock : No comments.

46. Violin : No comments.

47. Throne : The object at the top is supposed to represent a crown. Consider whether you will give full or half points to Easy chairs, Ordinary wooden chairs, Wooden chairs with arms, Chairs of definitely ceremonial appearance.

48. Tree : Trees arc drawn with very great frequency and in every degree of prominence, which makes hits on this original difficult to assess. Single trees, whether ' sole ', ' co-equal ' or secondary (Cf. Guide, 7) must clearly be given hits ; but it is not always easy to decide where ' secondary ' passes into ' incidental trimming '.

It is also for consideration whether Forests, Woods, Avenues, Rows of Trees, etc., indicate that the percipient had the idea of ' a tree ' in his mind ; also whether two, three, four . . . or how many trees should be given half or full hit.

49. Trident : The dictionary word was Spear and an illustration of ' Head of Fishing Spear ' was copied. Trident was introduced by the agent.

Consider pitchforks with two prongs, Garden forks with more than three prongs, Table forks, Rakes.

50. Windmill : No comments.

APPENDIX V

Two Notes on the Statistical Methods used

A. General Comments : by Dr J. O. Irwin and Mr Oliver Gatty.

The experimental precautions taken should successfully have eliminated all possibility of the percipients obtaining knowledge of the originals by ordinary sensory processes. Similarly, the random process by which the originals were chosen should have eliminated (1) the use of rational inference as a means of obtaining information about them, (2) effects due to the ideas and associations of experimenter and percipients running in similar cycles, (3) the possible effects of external circumstances causing the experimenter to draw and the percipients to think of the same idea.

But these experimental precautions could not prevent some experiments having a more *popular* set of originals than others, particularly because there were only ten originals in each experiment. Again, more percipients took part in some experiments than in others, and these might be expected to be more *active* than smaller groups.[1] A similar effect is to be expected if the generosity of marking varied from one experiment to another, and in fact Mr Hindson did mark Expt. I more generously than the others. But his ignorance of which drawings belonged to which experiment still qualifies him as a judge, even though he marked the drawings of the first experiment before those of the others.

The factors just discussed may be grouped together under the heads of ' varying mean popularity of originals ' and ' varying mean activity of percipients ' respectively. The statistical method used makes allowance for the effects of both these groups of factors.

There remain specific resemblances between the drawings of the percipients of a particular experiment and the originals used in a particular experiment (not necessarily the same).

Thus, for example, a Ship might be chosen as an original in some one experiment, and it might happen that the period of *some* experiment (not necessarily the same) might include that during which the *Graf Spee* was fought and scuttled ; or it might be supposed that the American percipients of one experiment (IVA, W.W.C.) were more prone to draw negroes than were other percipients, and that a Negro was chosen as an original in *some* experiment (not necessarily the same). The effect of this kind of thing would be artificially to increase the number of hits in the appropriate cell of the 5×5 Table [2], namely that corresponding to the experiment containing the original concerned and to the group of percipients affected. Such effects, due to chance, would tend to be eliminated as the number of originals used in each experiment was increased ; but we cannot say *a priori* how strong they would be likely to be,

[1] Activity is not necessarily proportional to the number of percipients, since some will have a greater tendency than others to return blank sheets, etc.

[2] *E.g.* the Table given at the top of Example I, or TABLE II. W.W.C.

and it is correspondingly impossible to say how large a number of originals would be necessary to eliminate them or whether ten is sufficient for the purpose. But, if they were to occur to any appreciable extent, they could be detected by inspection and their genuineness tested by appropriate tests of significance: further, it is important to note that, owing to the substantially random method adopted in choosing the originals, we should expect cells affected in this way to be distributed at random over the 5×5 table, and not merely along its principal diagonal.

The probability of a 'hit' being scored in the cell corresponding to row i and column j is taken to be $a_i b_j / N^2$, where a_i is the total number of hits in row i, b_j the total number of hits in column b, and N the grand total number of hits. From this it follows that the expected number of hits in this cell is $a_i b_j / N$. One may first examine, by means of a χ^2 test applied to the whole table, whether there are any significant departures from expectation at all, and, if there are, particular cells may then be examined. An approximate test is obtained by calculating the standard error of the number of hits in any cell as $\sqrt{Npq}$, where p is the proportion of the total number of hits expected to fall in it and q is $(1-p)$, and using this standard error in conjunction with a table of the normal deviate, to find the probability of the observed deviations or greater arising as the result of chance alone.

The actual statistical test employed was to decide whether the number of hits scored in the principal diagonal of the 5×5 table was significantly in excess of that expected. For this case the exact standard error has been calculated by Stevens, who treats the problem by considering two packs of N cards each [1]. In the first pack there are a_1 cards of a first type, a_2 of a second type . . . and a_n of a last type (n types altogether). In the second pack there are b_1 of the first type, b_2 of a second type . . . b_n of a last type (also n types altogether). The packs are supposed to be separately shuffled and dealt out. If the rth card of the first pack is found to be of the same type as the rth card of the second pack, Stevens calls this a 'hit' (he might have called it a 'success'). He then determines the mean (*i.e.* expectation) and variance of the number of hits. It is important to note that, if two cards which occur in the same (*e.g.* rth) place are called a *pair*, Stevens' result is independent of the order in which the pairs occur.

The analogy between the drawing experiments and Stevens' example may now be made clear. Any *hit* in the drawing experi-

[1] *Loc. cit.* Cf. p. 84.

ments (*i.e.* a worth-while resemblance between any drawing and any original, whether of the same experiment or not) corresponds to a *pair of cards* in Stevens' example. A '*winner*' in the drawing experiments (*i.e.* a 'hit' by a percipient on one of the originals at which he was aiming) corresponds to a *hit* in Stevens' example. The *originals* on which the hits were made, divisible into five types according to the experiment to which they belong, correspond to *one pack of cards*; the experimental *periods* during which they were made, also divisible into five types according to the experiment concerned, correspond to the *other pack of cards*. Corresponding to the fact that one card in each pair belongs to each pack is the fact that every hit is a hit on some original and is made during some experimental period. For mathematical details, Stevens' paper should be consulted.

It is evident from the foregoing that any strongly significant excess of hits in the principal diagonal of the 5×5 table is *prima facie* evidence of the percipients possessing knowledge of some sort about the originals.

If a significant excess of hits above expectation is found to occur in the principal diagonal, it is possible to recalculate the expectations in all the cells of the table on the hypothesis that a certain proportion of the drawings made in each experiment is in some way definitely directed on to the originals of that experiment while the rest are made at random. If, after this has been done, a χ^2 test applied to the whole table still shows significant departures from expectation (as is the case with the actual data here considered) this might be due to effects of 'specific resemblance' discussed in an earlier paragraph. Alternatively, it might be due to pre- or retro-cognitive effects of the same general nature as that responsible for the excess of hits in the principal diagonal, but capable of bridging the gap between one experiment and another.

The highly systematic character of the deviations from expectations in the diagonals of the 50×50 table may seem to suggest that the latter rather than (or perhaps in addition to) the former is the explanation in this case.

B. A Physical Analogy: by Professor C. D. Broad, Litt.D.

A flat board lies on a table. In the middle of it is a circle divided into n sectors (not necessarily equal), numbered $1, 2, \ldots j, \ldots n$.

A person is provided with a large number of equal small spheres. Of these, some are stamped with a 1, some with a 2, some with a j, and so on. These are thoroughly mixed up, and the person takes them in his hand and drops the whole handful on to the board.

Some roll off the circle altogether; the remainder come to rest at various places within it.

Suppose that a_1 come to rest in sector 1, a_2 in sector 2, a_j in sector j, and so on. Let the total number which come to rest within the circle be N. Then

$$a_1+a_2+\ldots a_j+\ldots a_n=N\text{ ; } i.e.\ S(a_j)=N.$$

Let b_1 of these be stamped with a 1, b_2 with a 2, and so on. Then

$$b_1+b_2+\ldots b_j+\ldots b_n=N\text{ ; } i.e.\ S(b_j)=N.$$

Of the N balls which come to rest within the circle a proportion a_j/N rest in sector j. Of the N balls which come to rest within the circle a proportion b_j/N are stamped with a j.

Therefore, if the two properties of "coming to rest in sector j" and "being stamped with a j" are mutually independent, the proportion of these N balls which have *both* these properties will be $a_j/N\times b_j/N$. Hence the proportion of these N balls which come to rest in sectors corresponding to the marks on them will be

$$a_1b_1/N^2+a_2b_2/N^2+\ldots a_jb_j/N^2+\ldots a_nb_n/N^2\text{ ; }\quad i.e.,\ S(a_jb_j/N^2).$$

Therefore, if the actual proportion should very greatly exceed this theoretical proportion $S(a_jb_j/N^2)$, it will be a sign that the two properties of "coming to rest in a certain sector" and "being stamped with the number corresponding to that sector" are *not* independent, but that there is a positive association between them.

Now the sector j corresponds to the originals in Experiment j. The balls stamped with a j correspond to the drawings made by the percipients in Experiment j. The N balls which come to rest within the circle correspond to the N marks assigned by the judge to drawings *from all the various experiments* in respect of *one or other* of the originals in those experiments. The a_j balls which come to rest in sector j correspond to the marks given by the judge to drawings which he considers resemble the *originals of experiment* j. The b_j balls which are stamped with a j correspond to those drawings *from experiment* j which the judge assigns to one or other of the originals of all the experiments.

Suppose that the two properties of "being assigned a mark in respect of one of the originals in a certain experiment" and "being a drawing made by a percipient in that experiment" are mutually independent. Then the proportion of the N drawings, to which marks are given by the judge in respect of one or other of the originals, which have *both* these properties will be $S(a_jb_j/N^2)$. Therefore, if the actual proportion should greatly exceed this theoretical proportion, it will be a sign that these two properties are *not* independent, but that there is a positive association between them.

FRESH LIGHT ON CARD-GUESSING—SOME NEW EFFECTS

By S. G. Soal

Introduction

Until the autumn of 1939 I still believed that it was practically impossible—at any rate in England—to find subjects who could demonstrate Extra-Sensory Cognition by guessing at the geometrical figures on Zener cards. This scepticism was not perhaps without its justification since during the past five years I had, without any apparent success, tested 160 persons and recorded 128,350 guesses. I drew attention to this record of persistent failure by articles in the Press and by lectures given to the British Psychological Society in London, to the Scottish branch in Glasgow, to the Society for Psychical Research and to the Ghost Club. At a meeting of the Ghost Club in March 1938 I read a paper on the snags and pitfalls which beset the path of the inexpert investigator. I pointed out that many of Dr Rhine's experiments had been inadequately reported and that some of his earlier successes might be accounted for by the guessers gaining clues from the backs of the cards. A little later in *S.P.R. Proceedings*, Part 154, pp. 86-96, I defended Rhine against the attacks of an American psychologist Professor Kellogg whose remarks revealed an ignorance of statistical method as amazing as it was lamentable.

In fairness I must add that though Dr Rhine's early work was open to criticism this no longer applies to many of the investigations which are now being reported in the *Journal of Parapsychology*. Indeed, the sources of error in the card-guessing techniques have now been thoroughly explored and there is no excuse if the experimenters of today make the same mistakes as the pioneers of five or ten years ago.

Last November my growing scepticism received a shock. The remarkable results obtained by Mr Whately Carington in experiments carried out under rigorous conditions and with methods and material differing from my own were brought to my notice. With remarkable pertinacity Mr Carington insisted that I should re-examine my experimental data. He suggested that I should compare each guess, not with the card for which it was originally intended, but with the immediately preceding and the immediately following card and count up the hits. For, according to Mr Carington, the faculty of extra-sensory cognition might not always succeed in hitting the object at which it was aimed. Just as a rifleman may show a personal bias which causes him persistently to strike the target at a point to the left or right of the bull's eye, so it might happen that the guesser at Zener cards all unwittingly was guessing correctly—not the card the experimenter was looking at—but a card which was one or two places earlier or later in the sequence. Carington even suggested that this " wobbly " type of perception might prove eventually to be more widespread than the exact type of cognition which so many experimenters have searched for in vain. And indeed, there is a vast amount of experience drawn from sittings with psychometrists and other psychical sensitives which goes to suggest that these subjects can seldom divine those thoughts which are in the focus of attention. They will refer to some trifling event known to the sitter, which happened last week or yesterday or a year ago, and make no mention at all of what is occupying their client's mind at the moment.

It was, however, in no very hopeful spirit that I began the task of searching my records for this " displacement " effect. And yet within a few weeks I had made two quite remarkable finds, which fully confirmed Carington's conjectures. From my records of the guesses of 160 persons I had discovered two whose results exhibited the kind of effect anticipated by Carington.

In Part I of this paper I shall describe the methods employed and make a general statistical analysis of the background of the experiment since it is a cardinal principle that any selected batch of results which appears significant must be estimated in its relation to the *total amount* of material which has been collected. Part I, therefore, may read like a dreary tale of negative results, and though it is really an essential preparation for the study of Mrs S. and Mr B. S. in Part II, the ordinary reader may skip it if he so prefers and pass on to the more important pages of Part II.

Acknowledgements

To mention by name the many scores of persons who have assisted me in the present investigation by acting either as percipients or witnesses would be clearly impossible even if it were desirable. Nevertheless I offer them all my hearty thanks for their co-operation and there are a lesser number of individuals whom I feel I must thank personally for the part they have played. Mr

Whately Carington is the prime instigator of all that this paper contains of value and, besides inspiring it, he has been ever ready to work out tiresome computations. For instance, he made an elaborate analysis of the χ^2 distribution for the Pure Clairvoyance experiments, and if I am not able to publish it in full, this is merely for reasons of space. To Dr R. H. Thouless I am indebted for much acute criticism and many valuable suggestions at different stages of the work. He also saw the experiments in progress in March 1935 and made suggestions which I adopted. My thanks are due to Professor Burt and Professor Flügel for their interest and for kindly putting the Psychological Laboratory at University College at my disposal. Professor E. S. Pearson gave me valuable help with the statistics of the subject, and devoted much time to the elucidation of knotty points. I must thank Mr Harry Price for the cards and other material and for the use of the quiet room at 13d Roland Gardens. Mr H. S. Collins assisted me as a witness for several months. Miss Rita Elliott has made a large number of very tedious " counts " extending over a year. These have all been checked by me and it was seldom that I discovered a mistake. Mrs Goldney very kindly organised the investigation of Mrs Eileen Garrett, one of Dr Rhine's subjects. (Cf. *Proceedings*, Part 154.) Mrs Johnstone, a member of the Society, and her daughter Miss V. Johnstone, undertook many journeys to Richmond to assist me in the investigation of Mrs S. described in Part II.

PART I

The General Background

1. *The Cards.*

The Zener cards used throughout this investigation were made under the direction of Mr Harry Price by a firm of playing-card manufacturers, and the backs were similar to those of the firm's ordinary playing cards. It will not be necessary to describe the backs of the cards in detail, since they were never exposed to the gaze of the subjects while the latter were making their guesses. The card to be guessed was either covered by a rectangle of white cardboard, or completely hidden by a screen, or, in a few cases, sealed in an opaque envelope.

On the faces of the cards the five symbols +, 0, Star, Rectangle and Wavy Lines were printed in thin red lines. From the outset I have rendered it impossible for the guessers to " learn " the cards from specks, etc., on their backs or to recognise them from impressions showing faintly through the backs. This was done by (i) never allowing the subjects to guess at the cards with their backs exposed, and (ii) by never using the same pack twice, for the same guesser, in an afternoon's work.

If one uses a single pack or two and shuffles the pack perfunctorily after each set of 25 guesses, there is a possibility that successive sets of 25 card symbols may be correlated and, though the effect of this on the expectation of successes may be only slight, I determined to avoid this possibility by compiling by the use of Mathematical Tables a *random* sequence of 1000 card symbols. This series was then split up into 40 consecutive blocks of 25 cards. The 40 sets of 25 were put into 40 envelopes which were numbered 1-40 and kept in two cardboard boxes, each containing 20 envelopes. Originally the 40 blocks were in the order of the random series, and the envelopes stood in their numerical order in the boxes. The first time after such a new random series had been compiled the percipients worked through the envelopes in the order in which they stood in the boxes. But when the thousand cards had been guessed the envelopes were shuffled in the boxes and before each pack was used the cards were themselves shuffled out of the subjects' sight. The greatest care was taken in clairvoyance experiments to prevent the subject from catching a glimpse of the bottom card of the pack.

At the beginning of each week's work the packs were taken out of the envelopes and the envelopes shuffled among themselves and the packs replaced so that the pack which was, say, in envelope 19 was now to be found in, say, number 12. This was to prevent subjects who came week after week from learning the composition of the packs in any of the envelopes.

A fresh random distribution of 1000 cards was compiled on *an average* after every 4000 guesses, and 31 such distributions were compiled from the tables during the five years' investigation. To be strictly accurate, the first random distribution by means of tables was made in January 1935 after about 1700 guesses had been recorded. The cards arrived at 13d Roland Gardens in November 1934 and were contained in 240 envelopes. Each envelope held five cards, one of each symbol, and 200 of these envelopes were emptied on to a large table, so that all the cards were face downwards. The cards were thoroughly mixed by Mr Price, Miss Beenhan and myself and then picked up into packs of 25 by the three of us, consecutive cards being chosen at random from different parts of the pile.

One distinct advantage of a random distribution is to be found in the fact that the binomial distribution can be applied to any large group of cards and guesses even if this is not composed of exact multiples of 25. This is of importance when, as in Part II, the groups may consist of 22, 23 or 24 cards.

This random sequence was compiled as follows. I had at my disposal exactly 1200 cards, there being 240 of each symbol. I first associated with each of the symbols +, 0, Star, Rectangle, Wave the respective numbers 1, 2, 3, 4, 5. I then provided myself with Chambers' *Seven-figure Mathematical Tables*, and read from them the *last* digits of the logarithms of the following numbers:

10078, 10178, 10278, . . . 99978.

The numbers chosen were thus taken at intervals of 100, so as to ensure that the last digits in the logarithms should be independent. If the digit happened to be one of the numbers 1 to 5 the digit was entered on the list, or more exactly the corresponding symbol was written. If the digit happened to be 0 or 6, 7, 8, 9, it was not entered. From this sequence I thus obtained a random series of about 450 cards. The process was then repeated with, say, the following numbers:

10043, 10143, 10243, . . . 99943,

and so on until a list of 1000 cards had been compiled. The actual cards were then chosen one by one according to the above list from the 1200 cards in my possession.

In the end there were of course not exactly 200 cards of each symbol, and up to January 1937 I made the mistake of adding or removing a few cards in such a way as to disturb the distribution as little as possible until I had *exactly* 200 cards of each symbol. This, however, is of small importance since few of the subjects did as many as 1000 guesses at a sitting, and when they arrived the following week, the packs of cards had been reshuffled in the 40 envelopes. After January 1937 I made random distributions without the above adjustment.

2. *The Subjects.*

In the summer of 1934 we circularised more than a hundred persons living in or around London who had taken part in my long-distance experiments of the years 1928-1929. Between 40 and 50 people responded, and ultimately some 23 of these came to 13d Roland Gardens to take part in the new experiments. Several of these persons claimed to have had psychic experiences of various kinds. The experiments began on 28 November 1934, and all the subjects who came to be tested were started in the (P.C.A.) technique.[1] Some dropped out after doing no more than 50 or 100 guesses but a good many completed 500 (or in some cases 450) (P.C.A.) guesses and then went on to do another 500 (or 550) using the (P.C.B.) technique. As the weeks went by other subjects were obtained, some through the kindness of Professor Burt at University College, London. The summer and autumn terms of 1935 were devoted chiefly to undifferentiated experiments in telepathy-cum-clairvoyance (U.T.). In November 1935 in the hope of providing a stimulus we offered valuable money prizes for persons who were able to score 12 or more hits in 25 guesses under controlled conditions. Though we gave publicity to these offers the response was very disappointing. Probably most people thought there was a "catch" in it, or that the prizes were offered for humanly impossible feats. Nevertheless it is worth noting that as a result of reading an article on the experiments in which the challenge to score more than twelve hits was displayed we secured the services of Mr B. S., one of our two principal subjects. Mr B. S. came to 13d Roland Gardens in the early part of 1936, not to win the prize, but because he felt confident he could accept the challenge.

In the autumn of 1936 I circularised about fifty spiritualist mediums whose names were in a book of reference just published. As a result one well-known automatic writer, one fairly well-known trance medium and four lesser known clairvoyantes came to be tested at Roland Gardens or elsewhere. All the results were negative. In November 1936 in the hope of finding fresh and younger subjects I removed the experiment to the Psychological Laboratory at University College. Here through the interest of Professors Burt and Flügel, my colleagues on the University of London Council for Psychical Investigation, I was accommodated in a cubicle, and the work in (U.T.) was continued with young students taking the courses in Psychology, while several of the post-graduate students rendered material assistance by acting as witnesses or agents. Here I was able to examine students of varied nationalities including over a dozen Indians, two Chinese, two Egyptians, one American, one Greek and several from the nearer parts of Europe. In the summer vacation of 1936 I tested four members of a Welsh family at their home in North Wales using the (P.C.B.) technique. One of these, a young man of 21, was a hypnotic subject in whom I was able to induce sensory anaesthesia to deep pricking with a needle. He was unsuccessful, however,

[1] The precise meanings of the terms (P.C.A.), (P.C.B.), and (U.T.) will be explained shortly (pp. 158-163).

when tested in the hypnotic state with 500 (P.C.B.) guesses, obtaining only 105 correct hits. Another professional hypnotic subject was tested with both (U.T.) and (P.C.B.) at University College. This subject, a man of about 45, showed very marked ability to recognise a playing card by minute specks on the back when the card was mixed with five others. He was not allowed to touch any of the cards, and the recognition was shown to be purely visual. In the normal state he obtained no more than chance results in experiments of this kind, while in the hypnotic state he succeeded in the experiment ten times out of ten. When, however, he was tested with the (P.C.B.) and (U.T.) techniques, he failed completely, obtaining only 105 correct hits in 500 (P.C.B.) experiments and 63 correct hits in 350 (U.T.) experiments.

One blind Indian subject was tested for (P.C.B.) at University College. As I lifted off each card he was allowed to lay his finger on its *edge* only. However, he scored only 94 successes in 500 guesses.

In the summer term of 1937 Mrs Garrett was investigated with 12,425 guesses. She is the only one of Dr Rhine's subjects I have been able to test, and the results have been already published. During the summer and autumn of the same year twelve other persons were tested by the "screened matching" (U.T.M.) technique. The spring and summer of 1938 were devoted mainly to further experiments in Pure Clairvoyance using the (P.C.B.) method only. In the autumn of the same year and winter of the next more University College students were tested for clairvoyance by the (P.C.S.) method, an opaque screen being placed between the guesser and the cards.

The Techniques Employed

1. *Pure Clairvoyance.*

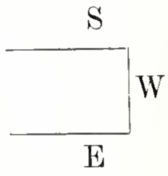
Five distinct methods were used which I shall denote by (P.C.A.), (P.C.B.), (P.C.S.), (P.C.D.), and (P.C.M.).

(i) *(P.C.A.) and (P.C.B.).*

The only essential difference between the (P.C.A.) and (P.C.B.) methods is that with the former the checking up was done after every five guesses with the guesser looking on, while in the latter the checking was not done until the 25 guesses were complete and the guesser was not allowed to watch the checking. If it is possible for card-guessers to improve by practice it is clearly necessary as in any other kind of skill that they should know exactly where they have made mistakes and so learn what introspective feeling is associated with a correct guess, and the (P.C.A.) method to some extent provided for this. No serious objection attaches to the method provided that a mathematically random series of card symbols is used. It would, for instance, be a fatal procedure if Dr Rhine's packs were used containing exactly five of each symbol, but with a random sequence, by noting what has already turned up one learns nothing about what is to come since the chance of each symbol appearing at any stage is always exactly one fifth.

It was my original intention to make each subject perform 500 tests by the (P.C.A.) method and then proceed to another 500 using the (P.C.B.) method. Until the year 1936 this plan was as a rule adhered to, but after that year I dispensed with the (P.C.A.) method altogether and allowed new percipients to start at once with (P.C.B.). In fact, I had found that the subjects did not improve as they went on with the (P.C.A.) method.

Three persons are, as a general rule, present at the experiments. They are the guesser or subject (S), the experimenter (E), (myself with few exceptions), and the witness (W). They are seated as shown in the figure, the experimenter and subject facing one another and about three feet apart. The witness sits at the end of the table as shown. The experimenter is the person who deals out the cards and generally supervises the procedure. The witness, who must be an intelligent and observant person, is present to keep a duplicate record of guesses and cards and to watch every detail of recording and procedure. The experimenter and witness are each provided with a scoring sheet foolscap size and designed to accommodate two sets of 25 guesses. For each set of 25 there are two columns, the one on the left headed (G) (guesses) and the one on the right headed (A) (actual cards). The two columns are divided into 25 rectangular cells. There are also spaces on the record sheet for Name of Subject, Date, Number of Pack, Time of first and last guesses for each 25, Remarks, Totals Correct, and a statement at the bottom of each sheet which reads: "*This independent record has been checked with the duplicate and found to agree.*"—*Signed* ——.



The experimenter is also provided with a clean white rectangle of cardboard, just a little larger than a Zener card, and cut from a postcard. As has been described on p. 155, the random series of 1000 cards is ready for use in 40 envelopes each containing 25 cards, the envelopes being in a determined order in two cardboard boxes on the table.

In the (P.C.A.) technique the experimenter takes the proper envelope from the box and, holding it below the level of the table, removes the pack of 25 cards which he shuffles (unless a new random distribution has just been made) beneath the table. He next rests the pack face downwards on the palm of his left hand and covers it with the rectangle of white card. He now raises the palm of his left hand until the white card just reaches the level of the table. With the thumb and forefinger of his right hand he *slides* off the top card on to the table, covered by the rectangle of white card. He then *immediately* lowers the pack again below the level of the table.

The guesser or subject (S) now calls aloud his guess for the covered card lying on the table. *Both* experimenter and witness record the guess in the appropriate cells of their scoring sheets in the (G) column. The experimenter then removes with his right hand the covering card, puts it over the pack which still rests on his left palm beneath the table, raises the pack and slides off the next card and so on. The "guessed" cards are placed in a pile, or sometimes in rows, with their faces downward, care being taken not to disturb the original order. In the (P.C.A.) technique, after five cards have been guessed, they are turned over by the witness and he and the experimenter record them in the A columns of their sheets. The subject takes note of his successes, watching the checking. When the column of 25 cards is complete—after 5 such checkings have taken place—the experimenter and witness tick off the correct hits and enter the totals, time of last guess, etc., at the foot of the column. They then compare scoring sheets card by card and guess by guess to see that they tally. In point of fact, errors are very rarely found, but they can usually be rectified by a reference to the pack whose cards still retain their original order. Both witness and experimenter sign the statement referred to above when the two sets of 25 are complete, each person signing on both sheets. The next envelope in order is then taken out of the box and the work proceeds.

In the (P.C.B.) technique the cards are not checked until the tale of 25 guesses are complete. The subject (S) then walks away to a remote part of the room, or sits behind the screen if one is on the table, and in his absence E and W check up and fill in the blank A columns of their sheets.

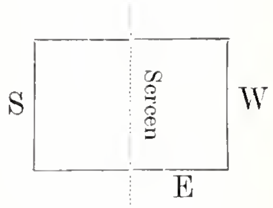
The *average* time for guessing 25 cards was nearly 4 minutes with (P.C.B.), but in rapid work it may be reduced to $1\frac{1}{2}$-2 minutes. Slow guessers took from 5-7 minutes. The subjects as a rule were allowed to choose their own tempo. Usually a subject guessed from 200-250 cards in an afternoon, but certain rapid guessers did 500 or more guesses in an afternoon. After the visit of Professor

Thouless to 13d Roland Gardens on March 28, 1935, I did not, except under special circumstances, allow any other persons than E, W and S to be in the room during the guessing, but as a rule two or three subjects were examined consecutively in the same afternoon, appointments being made for them to come at specified times.

At the end of each afternoon's work, the last witness placed the duplicate scoring sheets in an envelope, sealed it in my presence and posted it as soon as possible to Dr Joad. The other copies were retained by me.

I consider that the keeping of independent records by both witness and experimenter during the guessing practically eliminates errors arising from the mishearing of calls, from slips in entry, etc. The abbreviations used for recording are + (plus sign), O (circle), S (five pointed star), ▭ (rectangle), and W (Wavy Lines).

(ii) (*P.C.S.*). 26 persons took part in this technique between January 1938 and January 1939. None of these except Miss E. S. had taken part in any of the previous experiments. The subject (S) and the witness (W) sat at the ends of a deal table facing each other and at about 4 feet apart. Between them was an opaque light wooden screen measuring about 3′ broad by 2′ 6″ high. The screen rested on the table, there being no gap between the screen and table. The experimenter (E) sat on the same side of the screen as (W), but at the side of the table close to the screen as shown in the figure. The pack of cards rested face downwards on the table close to the screen on the same side as the witness. The experimenter, having shuffled the pack behind the screen, laid it face downwards on the table near the centre of the screen. He then enquired if the subject was ready, lifted off the top card and laid it face downwards on the table without looking at it. At the same time he called out " First guess " or " Next " as the case might be. The subject on the other side of the screen called aloud his guess which was recorded by both witness and experimenter. The subject was not allowed to move from his seat behind the screen during the checking which was done at the end of each 25 guesses. He was told, however, how many correct guesses he had made at the end of each 25 and given encouragement for a good score.



(iii) (*P.C.M.*). The only subjects who used this technique were Mrs Garrett and Miss E. S. It is the " screen and pointer " method fully described in my paper on Mrs Garrett in *S.P.R. Proceedings*, Part 154, p. 74. I shall therefore refer the reader to this report.

(iv) (*P.C.D.*). This is the " Down Through " method often used by Dr Rhine. I have only employed it with three subjects one of whom was " Marion " (Josef Kraus), the well-known vaudeville telepathist. The method was as follows. In the case of " Marion " the experimenter (myself), the witness (Miss Elliott or Mrs Goldney) and the guesser sat at a table, Marion being provided with a scoring sheet. I took one of the envelopes from the box and holding it under the table shuffled the cards and replaced them in the envelope. I then placed the envelope on the table with the flap closed and a rubber band round the envelope to keep it closed. While the cards were under the table and *before* they were shuffled, Marion was allowed to touch them once as he claimed that this was essential if he was to succeed. The envelope containing the cards was placed in front of Marion on the table flap downwards. He now wrote down on his scoring sheet with extreme rapidity a series of 25 card symbols. When he had finished I ran through the list asking him to say quite definitely what any carelessly drawn symbol was intended to represent. This was essential as Marion sometimes made his rectangles very like his circles. I was also particularly careful to see that Marion did not get a glance at the last card in the pack, since some records which he showed me of solo experiments proved that he had been scoring significantly by unwittingly getting a glimpse of the last card as he laid the envelope on the table, no rubber band having been used. After we were satisfied about the entries in the guess column we took the cards out of the envelope and checked up with Marion watching. Under these more rigorous conditions Marion failed to score above chance expectation, though he still continued to produce for our inspection highly significant scores that he had made while working alone. These amounted to tens of thousands of guesses, and the high scores were to be partly accounted for by the fact that Marion managed to glimpse the last card in the pack, probably without being aware of it.

As regards the other two D.T. subjects who only did 100 guesses each, the method in each case was as follows. The subject sat in a far corner of the room and called aloud a series of 25 symbols which were recorded by both experimenter and witness. I had previously shuffled a pack of cards under the table and replaced them in an envelope which rested on the table during the calling. No extra-chance results were obtained.

2. *Undifferentiated Extra-Sensory Perception.* [(U.T.) and (U.T.M.)]

Experiments in which the telepathic possibility was included, as well as the possibility of clairvoyance, were commenced at 13d Roland

Gardens shortly after Easter 1935. We have made no experiments in *Pure* Telepathy. I thought it best to try each subject with several different agents in turn, since success might conceivably depend upon some quality inherent in the subconscious mind of the transmitter no less than upon powers possessed by the percipient himself. The experiments were continued at University College between November 1936 and September 1937. In all the telepathy experiments the subject was separated from the agent and experimenter by a screen.

As in my paper on Mrs Garrett, I shall refer to the two techniques employed as (U.T.) and (U.T.M.). At 13d Roland Gardens the agent and subject who worked together were practically strangers to each other, and generally met at the laboratory for the first time. But in one case a mother and daughter played the parts of subject and agent, in another a father and daughter, in a third two brothers and in two other cases pairs of close friends. At University College the agent and subject were often friends or at least acquainted.

If any code was used to convey the card symbols to the subject on the other side of the screen, such a code would have to be (i) a "timing" code or (ii) an auditory code consisting of such sounds as coughs, scraping or tapping of feet, sounds of hand-movements on table or of pencil, blowing and so on. The possibility of (i) is ruled out by the fact that I dealt out the cards in my own time and gave the signal "Next" at my own pleasure. The agent, who was not allowed to speak during the guessing, sat close to me and a vertical board fixed beneath the table prevented the guesser from seeing his feet or legs or making contact with them. As a result of special tests we found that if this precaution is omitted, it is easily possible for the agent to use a code consisting of such movements as opening and closing the heels, crossing the toe of one boot over the other, all of which motions are so slight that they cannot be detected unless the experimenter is looking under the table.

As regards possibility (ii) (of auditory codes), whenever I heard the agent make any unusual noise I made a secret mark opposite the guess on my scoring sheet and waited to see if the noise were repeated when the same card-symbol was turned up again. I never, however, succeeded in detecting any auditory code.

I have taken no special precautions to obviate the possibility of the so-called "involuntary whispering" by the agent or normal leakage through involuntary changes in breathing. Had I discovered any subjects who scored positive results over any considerable period I should have tried the effect of distance on the results. But I think the very fact that 82 subjects who tried the telepathy experiments obtained no apparent success of any kind strongly indicates that if "involuntary whispering" ever helps persons to score beyond chance this effect must be an excessively rare one.

A certain subject, Mr H.—a Chinese student at University College, who had been credited by an investigator (who was testing the ability of students to interpret correctly subliminal auditory impressions) with the power of discriminating faintly whispered words that were below his normal threshold of recognition—submitted to 800 (U.T.M.) experiments but obtained only 171 correct hits as against the 160 expected by chance. After completing this series he did another 200 tests one day when he was fresh with Miss Elliott acting as agent. I asked Miss Elliott to whisper very faintly the name of each card while she looked at it. Mr H. who was sitting 4′ away from the agent—the normal distance—scored only 36 correct hits. I then let him do another 200 guesses with Miss Elliott now whispering repeatedly the name of each card far more loudly than before. He scored 47 successes, a result which is again without significance. The other conditions were exactly the same as in the (U.T.) experiments.

It will be obvious that the (U.T.) results scored by the two subjects Mrs S. and Mr B. S. in Part II could not reasonably be attributed to "involuntary whispering" or even to the use of a code between subject and agent.

(i) *The (U.T.) technique.*

The room at 13d Roland Gardens was first systematically examined for the possibility of reflecting surfaces that might enable the guesser to see the exposed card even while sitting behind the screen. As a result of a great many experiments carried out by Mr H. S. Collins and myself it was found that the only source of reflection that might give any assistance to a guesser was the sliding glass lid of a bookcase. At the commencement of each afternoon's work this was adjusted by sliding it up. In the cubicle at University College, I satisfied myself that when the screen was in use, there were no reflecting surfaces that would give the subject any assistance.

At Roland Gardens the subject and agent (who also acts as witness) were seated at each end of a table about 5′ apart and, resting on the centre of the table, was a plywood screen, measuring 3′ by 3′ and suspended by cords from the ceiling. There was no crevice between the bottom of the screen and the table. The experimenter (myself) sat on the same side of the screen as the agent but at the side of the table and close to the screen. The experimenter and the subject (unless the latter was in trance) were each provided with a scoring sheet identical with that used in the (P.C.) experiments. Beneath the table a vertical board was fixed which prevented the subject from seeing the agent's feet or legs. Thus every part of the agent's body was screened from the guesser's view.

The experimenter was also provided with five rubber stamps of similar make and weight together with a red or green ink-pad. The impressions produced by the stamps on the scoring sheet measured about $\frac{1}{2}''$ by $\frac{3}{8}''$.

To have used a pen or pencil for recording the actual card symbols would have been fatal, since a number of tests showed that if the experimenter recorded his card *before* the subject had made his guess, the latter was able to recognise the + by the lifting of the pencil, the rectangle ▭ by the time taken to draw it, and so on, and most of the symbols could be identified by the sound of the pencil strokes. It might of course be suggested that as the *rubber* parts of the five stamps were slightly different in size or shape, this might lead to auditory discrimination of the symbols. But this rather far-fetched hypothesis is refuted by the fact that 84 people did the experiment and all failed at such delicate discrimination both individually and in the mass.

At University College the same screen was used, being fixed in a vertical position by wooden guides attached to the wall, but the distance between guesser (S) and agent (W) was only about four feet. The same or precisely similar rubber stamps were used of which I had three sets.

The pack rested on the table near the centre of the screen face downwards. The experimenter closed his eyes, lifted off the top card and held it close to the screen its face towards the agent. With closed eyes the experimenter called out " First guess " or " Next " as the case might be. This was the signal to the subject that the agent had begun to look at the card. The experimenter then quickly opened his eyes and recorded the card-symbol in the A column of his scoring sheet by means of the appropriate rubber stamp. Meanwhile the subject behind the screen recorded his guess in pencil in the G column *without* calling it aloud. As a signal that he had made his guess the subject tapped twice on the table with his pencil. By this method the experimenter, who did not know what card he had lifted from the pack until he had opened his eyes, could give nothing away by the inflections of his voice. No words were spoken during the guessing except the words of the signal. The guessed cards were placed face downwards on the table in a pile, care being taken not to disturb the order. When the column of 25 guesses was complete the subject *without leaving his* *seat behind the screen* handed his scoring sheet, with the G column filled in to the experimenter. The latter *with the agent watching closely* copied the subject's G column on to his own scoring sheet and filled in the subject's empty A column from the record of his own scoring sheet. The agents were all specially instructed to watch every step of the checking and a comparison was made of the two sheets guess by guess. The agent and experimenter each signed both records, the successful guesses having ticks placed opposite them and the totals for each 25 being entered at the bottom of the column. The subject remained behind the screen during the checking. He was informed of his score after each 25 guesses and praised when he made seven or more correct hits.

Before starting work with a fresh subject or agent, we usually held a preliminary consultation as to the nature of the imagery to be employed by the agent. For instance, if the agent claimed to be a good visualiser, it would be agreed that he should glance at the exposed card, shut his eyes and imagine the symbol drawn in red paint on a white canvas or with chalk on a blackboard. In other cases it was decided that the agent should represent to himself the "Wave" by an image of waves breaking on a beach, the cross by a cluster of wooden crosses in a war cemetery, etc. In a few cases the agent employed either visual or auditory *verbal* imagery. But in every case it was firmly impressed on *the agent that during the guessing he must keep his lips firmly closed.*

When one kind of imagery failed we sometimes changed it for another. In certain cases it was agreed that the agent should glance once at the card, shut his eyes and dismiss it from his mind by thinking of something quite different.

In the case of eight of the subjects they were asked to grade each guess according to their introspective feeling of certainty or uncertainty, the four grades being A, B, C and D. After each guess the subject wrote opposite the guess one of these letters. They were understood to signify

A = Very certain the guess is right.
B = Only moderately certain.
C = Not very certain.
D = Feel it is a mere shot in the dark.

But an analysis of the scores of these eight subjects shows that neither individually nor in the mass are the guesses marked A and B any more successful than those marked C and D. Many subjects believed that introspection interfered with the spontaneity of their guessing and for this reason I did not care to burden the majority with this extra task.

Three subjects whose scores in (U.T.) had been hitherto unsuccessful were each dosed with caffeine citrate to see if any improvement would result. Each subject on three different days was made to swallow doses varying from 6-10 grains of the drug about twenty minutes before commencing work. No effect was noted in any case.

(ii) (*U.T.M.*). (*Screened-Matching Technique.*)

Thirteen subjects including Mrs Garrett were tested by this method in the summer and autumn of 1937. The technique is not a very rigorous one, but even so, no significant degree of success was scored by its use.

The subject sat behind a black metal screen measuring two feet broad by one foot 6 inches high which stood on the table. Between the bottom edge of the screen and the table was a gap $\frac{1}{2}$ inch high through which the subject could push a light metal pointer. The screen stood over the centre line of a row of five " target " cards bearing in order the symbols, +, O, Star, Rectangle, Wave and fixed faces upwards to the table by means of drawing pins. Between each pair of cards was a gap of about $\frac{1}{4}$ inch.

The agent and subject faced each other on opposite sides of the screen, while the experimenter sat at the side of the table close to the screen and on the same side as the agent. The experimenter held the pack of 25 cards close to the centre of the screen with the backs of the cards towards the agent's face. Having enquired if the subject was ready to begin the experimenter closed his eyes and lifted off the top card of the pack for the agent to visualise. With closed eyes the experimenter called out " First Guess " or " Next " as the case might be. Immediately the card was exposed the agent looked at it and then formed a visual image in his mind or sometimes a verbal image of the name of the card. The agent remained silent throughout. When the subject's pointer had come quite definitely to rest on one of the five " target " cards the experimenter who had opened his eyes immediately after giving the signal placed the exposed card face downwards opposite the " target " card chosen by the subject. At the end of 25 guesses the 25 cards thus found themselves arranged in five piles, all the cards being face downwards.

To prevent confusion by mixing, the five piles were planted at considerably wider intervals than were the " target " cards but in the same order as the latter. A count was then made of the successes under each symbol, and these were recorded on the scoring sheet by the experimenter thus :

$$5\begin{cases} + & 1 \\ \mathrm{O} & 0 \\ \mathrm{S} & 2 \\ \mathrm{R} & 1 \\ \mathrm{W} & 1 \end{cases}$$

The count was carefully checked by the agent. In these experiments the distance between subject and agent was about 4 feet.

The chief danger of this method lies in the possibility that the agent or experimenter watching the motion of the pointer along the row of cards may make some involuntary audible movement that will cause the guesser to stop the pointer opposite the correct card which of course is known to both agent and experimenter. The objection does not apply to the similar (P.C.M.) technique in which neither agent nor experimenter know the symbol on the exposed card.

An even more serious disadvantage is that this method does not readily permit of the experimenter obtaining an exact record of each card opposite its corresponding guess. It is therefore quite unsuited for the study of such " displacement " effects as those described in Part II, and it is a matter of some regret that I allowed Mrs S., one of the principal percipients, to waste as many as 925 trials on this technique.

Formulae and Methods

For the (P.C.A.) and (P.C.B.) experiments I have calculated the expectation of correct hits in three ways and the variance by two different methods.

Method (*i*). In a series of n guesses the expectation of hits is $\frac{1}{5}n$ and the variance is $\frac{4n}{25}$. These are the binomial formulae and are valid for such a distribution as I have employed, unless it happens to be a very improbable one.

Method (*ii*). The expectation and variance can be worked out for each 25 guesses by formulae due to Mr W. L. Stevens and the results summed for all groups of 25 to find the total expectation and total variance.

If a_1, a_2, a_3, a_4, a_5 are the actual numbers of cards of each symbol in a pack of 25, and g_1, g_2, g_3, g_4, g_5 the numbers of times respectively that these symbols are guessed by the subject, then the expectation of hits for the 25 guesses is $(\sum_{r=1}^{5} a_r g_r)/25$.

To find the variance we calculate the sums

$$E_1 = \sum_{r=1}^{5} a_r g_r.$$

$$E_2 = \sum_{r=1}^{5} a_r g_r (a_r + g_r),$$

and the *variance* can be shown to be

$$\frac{1}{24 \times 625}[625E_1 - 25E_2 + E_1^2].$$

This method, though exceedingly laborious when it had to be applied as in our case to 2003 sets of 25 and the results summed, is somewhat safer than Method (i) since it allows for the possibility of improbable distributions existing within the individual sets of 25.

Method (iii). The expectation of hits may be calculated by the formula

$$\frac{1}{N}\sum_{r=1}^{5} A_r G_r,$$

where N is the total number of guesses, A_1, A_2, . . . A_5, the total numbers of times each of the five card symbols occur, and G_1, G_2 . . . G_5, the total numbers of times these same symbols were guessed by the percipients.

Analysis of Data

(i) *Pure Clairvoyance.*

In all, 108 persons took part in the Pure Clairvoyance tests, and of these a few worked with more than one technique. I have paid special attention to the 50,075 guesses recorded by the (P.C.A.) and (P.C.B.) techniques because of a certain mass tendency which this batch shows to register scores slightly below chance expectation, and this is one of the effects Dr Rhine claims to have noted in his work in cases where physical or psychological conditions were unfavourable. It seemed therefore worth while to compare the estimates obtained from several statistical methods. On the whole 70,900 guesses recorded under all the techniques there is no *significant* tendency to score above or below chance. The results are given in Table 1.

TABLE 1

PURE CLAIRVOYANCE. ALL TECHNIQUES

By Method (i)

Guesses	*Hits*	*E*	*DEV.*	*S.D.*	*X*
70,900	14,020	14,180	−160	106·52	−1·50

In the above and following tables

E = expected number of correct hits, DEV. = deviation of hits from expected number. S.D. = Standard deviation. χ or X = DEV./S.D. Method (i) was used in Table 1.

TABLE 2

(P.C.A.) AND (P.C.B.)

By Method (i)

Technique	*Guesses*	*Hits*	*E*	*DEV.*	*S.D.*	*X*	*No. of Subjects*
P.C.A.	18,075	3,533	3,615	− 82	53·78	−1·52	40
P.C.B.	32,000	6,281	6,400	−119	71·56	−1·66	62
Totals	50,075	9,814	10,015	−201	89·51	2·24	81

We note a just significant "negative" tendency on the total (P.C.A.) and (P.C.B.) results.

If we calculate the expectation and variance for each set of 25 by Stevens' formulae and sum up for the 2003 sets of 25 we have Table 3.

TABLE 3

By Method (ii)

Technique	*E*	*S.D.*	*X*
P.C.A.	3,641·48	52·87	−2·05
P.C.B.	6,388·04	70·73	−1·51
Totals	10,029·52	88·31	−2·44

The variances obtained by Stevens' method [1] are almost without exception very slightly less than those given by the Binomial formula, and this confirms the fact that in my case the Binomial formula for S.D. is a safe formula to use. We see that by this method the negative effect is slightly enhanced, the odds against chance being now of the order 50 to 1 for the whole 50,075 guesses.

It is interesting to note that the total expectation given by Method (iii) agrees very closely with the value got by taking one fifth of the number of guesses.

The values of $A_1, A_2 \ldots A_5$ and G_1, G_2, G_5, are given in Table 4.

TABLE 4

NUMBERS OF TIMES OCCURRING

Symbols	+(1)	*O* (2)	S (3)	▭ (4)	*W* (5)	*Totals*
$A_1 \ldots A_5$	9,905	10,051	10,094	10,092	9,933	50,075
$G_1 \ldots G_5$	9,820	10,526	9,811	9,440	10,478	50,075

Whence the expectation by Method (iii) is 10013·8 as compared with $N/5=10{,}015$.

It will also be observed that Rectangles are definitely unpopular.

The negative effect may also be checked by counting how many of the 81 persons obtained total scores above expectation and the number with scores below expectation.

We have (Stevens' Scoring)

+Deviation	−Deviation	±0 Deviation
30 persons	50 persons	1 person

This gives $\chi=2{\cdot}24$, which agrees fairly well with Method (i).

Again, if we agree to call each day's work of each individual guesser an "occasion" we have in all 299 occasions for (P.C.A.) and (P.C.B.) work.

We find (Method (i))

+Deviation	−Deviation	±0 Deviation
111 occasions	151 occasions	37 occasions

Hence neglecting the 37 occasions with deviation ±0 we have $\chi=2{\cdot}47$ which gives odds of the order 50 to 1.

It is seen that the different methods agree in demonstrating a slight tendency to score below expectation on the (P.C.A.) and

[1] The *average* Variance by Stevens' method for 500 guesses is 77·865 compared with 80 obtained by the Binomial formula.

(P.C.B.) work, and it is worth while to give the Binomial Analysis by sets of 25 in Table 5. In this table the expected numbers [E] of sets of 25 with (0, 1), 2, 3, . . . 10, (11 or more) correct hits have been computed from the successive terms of the expansion of 2003 $[\frac{4}{5}+\frac{1}{5}]^{25}$, and these expectations may be compared with the corresponding actual numbers in column [A]. The highest score recorded for any set of 25 guesses in the whole investigation (comprising 5,134 sets of 25) was a solitary 13 in a (P.C.A.) experiment.

TABLE 5

(P.C.A.) AND (P.C.B.)

For 2003 sets of 25

	A	E	$X=A-E$	X^2/E
0, 1	73	54·86217	18·13783	5·996498
2	138	141·89252	− 3·89252	0·106783
3	286	271·96734	14·03266	0·724041
4	373	373·96010	− 0·96010	0·002465
5	387	392·66812	− 5·66812	0·081819
6	337	327·23011	9·76989	0·291693
7	220	222·03255	− 2·03255	0·018606
8	107	124·90708	−17·90708	2·567216
9	53	58·98835	− 5·98835	0·607923
10	23	23·59534	− 0·59534	0·015021
11, 12, etc.	6	11·10006	− 5·10006	2·343286
Totals	2,003	2,003·2		$\chi^2=12·7553$

With $n=10$ this gives $0·3>P>0·2$.

It is seen that though the value of χ^2 is not abnormal there is a certain excess of sets of 25 having the scores 0 and 1 which appears largely to account for the tendency to score below expectation.

A similar binomial analysis was made for sets of 5 guesses by means of the expansion of $[\frac{1}{5}+\frac{4}{5}]^5$, but the results throw no fresh light on the question.

The Major and Minor Sets ((P.C.A.) and (P.C.B.)).

A set of not less than 450 guesses done by the same subject in either (P.C.A.) or (P.C.B.) I have called a *Major* set, and a set of less than 450 guesses a *minor* set.

In the (P.C.A.) work there are 30 major sets, and of these three were sets of 550 guesses each, the remainder being sets of 500. In addition there are in (P.C.A.) 17 minor sets ranging from 50 to 400 guesses each. In the (P.C.B.) work there are 52 major sets, all these being sets of 500 guesses except for 4 sets of 450 and one of 600. There are also 29 minor sets ranging from 50 to 400 guesses.

For the whole 128 sets (P.C.A.) and (P.C.B.) we have $S(\chi^2) = 144{\cdot}385$ which with $n=128$ gives $P \simeq 0{\cdot}33$ which is not abnormal.

For the 81 (P.C.B.) sets $S(\chi^2)=106{\cdot}928$ which with $n=81$ gives $P \simeq {\cdot}055$, which again is not significant.

Mr Carington also made an analysis of the distribution of the 128 values of χ^2 according to Dr Fisher's method of 25% groups but the results were not abnormal. (All individuals were scored by Stevens' method.)

Exceptional Individual Scores.

The highest "positive" score was made by a young man at U.C., London, who, in a set of 500 (P.C.B.) guesses, obtained 128 correct hits—equivalent to $+3{\cdot}13$ times S.D. by Stevens' method. But when the same subject was tested with another (P.C.B.) set of 500 he scored only 102 successes, and I could not get into touch with him for further work.

The next highest score was a (P.C.B.) set of 500 with 125 correct hits, but in this case I was unable to arrange another test.

The lowest score was a (P.C.A.) set of 500 with only 77 correct hits, this being equivalent to $-2{\cdot}67 \times$ S.D.; but when the subject did another 500 guesses under similar conditions she obtained the normal score of 103 correct hits.

The Displacement Test.

In order to test further the random nature of the card distribution the 50,075 actual card symbols and the 50,075 guesses were written in two concentric endless rings and then the guesses were all shifted two places forward so that each guess was now opposite a card 2 places ahead of the card for which it was originally intended. Thus the experiment became a (-2) experiment. The "hits" were counted, and it was now found that the *negative* deviation had disappeared there being on the 50,075 guesses a small *positive* deviation of only $+16$ compared with an S.D. of 89·51 by Method (i). Similarly we find that the 128 Major and Minor sets mentioned above yield after the (-2) displacement exactly 59 sets with a positive deviation, 59 with negative deviations and 10 with zero deviation, a result in accordance with chance. This is an argument in favour of the genuine nature of the negative effect.

Other Techniques in Pure Clairvoyance.

In Table 6 I give briefly the particulars for the other clairvoyance experiments.

TABLE 6

By 1/5 N

Method (i)

Technique	*Guesses*	*Hits*	*E*	*DEV.*	*S.D.*	χ	*No. of Subjects*
P.C.S.	8,775	1,757	1,755	+ 2		<1	26
P.C.M.	7,400	1,495	1,480	+15		<1	2
D.T.	4,650	954	930	+24		<1	3
Totals	20,825	4,206	4,165	+41	57·5	<1	

The results on the techniques other than P.C.A. and P.C.B. are thus seen to be purely chance results, but with the exception of Miss E. S., a Greek lady, none of the above percipients had taken part in the P.C.A. or P.C.B. work.

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

84 persons took part in the undifferentiated experiments of whom 76 were tested by the (U.T.) technique and 13 by the (U.T.M.) technique, a few persons trying both methods.

The general results are contained in Table 7. They were all scored by Method (i).

TABLE 7

UNDIFFERENTIATED E.S.P.

Method (i)

Technique	*Guesses*	*Hits*	*E*	*DEV.*	*S.D.*	χ
(U.T.) -	44,100	8,838	8,820	+18	84	<1
(U.T.M.) -	13,350	2,703	2,670	+33	46	<1
Totals -	57,450	11,541	11,490	+51	96	<1

It is seen that all the deviations are slight positive deviations even less than the standard deviation. These results are a strong argument against the "involuntary whispering" theory or the theory that subjects are able to discriminate the sounds made by small pieces of rubber measuring $\frac{1}{2}''$ by $\frac{3}{16}''$. In fact, such theories appear to be the wildest nonsense when put to the test.

Individual Scores (U.T.).

There are 28 " Major " sets of 1000 guesses each and 57 " Minor " sets with numbers of guesses varying from 25 (once) to 800 (once).

For the 28 sets of 1000, $S(\chi^2)=28{\cdot}602$ which gives $P\simeq 0{\cdot}4$.

The only positive score of the slightest interest is the first thousand of Mrs S. whose work is described in Part II. The lowest score was obtained by Mr F., a student who had suffered from nervous breakdown. Mr F. got only 168 correct hits on his first 1000 guesses—equivalent to $-2{\cdot}53$ times S.D.—and two of his 40 scores for 25 guesses were zeros. However, on a second thousand his score rose to 193, a result which though still negative, has no significance.

I have also made a Binomial Analysis by sets of 25, but the results agree closely with expectation and the work is not worth setting out in detail.

(iii) *Singletons and Success Groups.*

In both (P.C.A.) and (P.C.B.) and for (U.T.) we have counted the numbers of singletons (*i.e.* isolated successes), " doubles " (*i.e.* runs of 2 successes), " triples ", etc., in order to see if there was any evidence for short bursts of intermittent extra-sensory cognition. The expected numbers of " singletons ", " doubles ", etc., have been computed from *a priori* expectation and the singletons have also been computed on the basis of the *actual* number of successful hits by a formula due to Mr W. L. Stevens. We have also applied Mr Stevens's method of " success groups ".

Thus if the n guesses are written in a continuous ring and $a=$number of successes, $b=$number of failures so that $a+b=n$, the expected number of singletons is given by

$$E=\frac{ab(b-1)}{(a+b-1)\,(a+b-2)},$$

with variance

$$V=E\left[\frac{(a-1)\,(b-1)\,(b-2)}{(a+b-3)\,(a+b-4)}+1-E\right]$$

Applying this to the (U.T.) results we find

$$E=5650{\cdot}77$$

$$V=3123{\cdot}18.$$

Whence $$S.D.=\sqrt{V}=55{\cdot}89.$$

Actually there is a deficiency of singletons

$$=104{\cdot}77$$

Whence $$\chi=\frac{-104{\cdot}77}{55{\cdot}89}=-1{\cdot}87,$$

which is not significant.

The numbers (A) of singletons, doubles, etc., for both the (P.C.A.) and (P.C.B.) work and the (U.T.) experiments are given in the table below, together with E, the *a priori* expectations to nearest whole number.

These *a priori* expectations of singletons, rows of 2, 3, 4, 5 successes denoted by E_1, E_2, E_3, E_4, E_5, were computed from the formulae :

$$E_1=\frac{16n+8}{125},\quad E_2=\frac{16n-8}{625},\quad E_3=\frac{16n-24}{3125},$$

$$E_4=\frac{16n-40}{15{,}625},\quad E_5=\frac{16n-56}{78{,}125}\ \text{etc,}$$

where n is the total number of guesses, and runs are continued from the end of one set of 25 to the beginning of the next with no break in the sequence.

TABLE 8

RUNS OF 1, 2, 3, 4, 5 CONSECUTIVE SUCCESSES

Technique		1	2	3	4	*5 or more*
P.C. (A.B.)	E	6,410	1,282	256	51	12·8
	A	6,379	1,244	251	41	6
(U.T.) -	E	5,645	1,129	226	45	11·3
	A	5,546	1,181	221	45	17

It is seen that in (U.T.) there is a slight positive excess in runs of 5 or more consecutive successes, but it is not significant.

For P.C.A. and P.C.B. we have

No. of Success Groups	=7921
Expectation	=7890·8
Deviation	=+30·2
S.D.	= 35·3

Hence there is no significant grouping of successful hits.

Similarly for (U.T.) we have

No. of Success Groups	=7010
Expectation	=7067
Deviation	= – 57
S.D.	= 33·65

Hence again there is no significant " crowding " of successful hits. [*Note* : In such a run as

$$s\,f\,f\,ss\,f\,sss\,f\,f\,ssss\,f\,s\,f$$

where s denotes a " success " and f a " failure " there are 5 groups of successes. If a run of guesses contains a successes and b failures so that $a+b$ is the total number of guesses, then the expected number E of " success groups " is computed from the formula

$$\mathrm{E}=\frac{a(b+1)}{a+b}$$

and the variance of this expression is equal to

$$\frac{\mathrm{E}\times b(a-1)}{(a+b)(a+b-1)}$$]

Counts were also made to discover whether (a) the *first* sets of 25 guessed at a sitting were more successful than the average, and (b) whether the first sets guessed by fresh percipients gave higher scores than the average. Similar counts were made for *first* sets of 5 guesses. The results were negative in both cases.

Conclusion to Part I.

In the case of one group of persons, a significant tendency to score slightly below chance expectation has been noted in the Pure Clairvoyance work which is absent in the telepathic experiments, but the degree of significance (odds of about 50 to 1) is not such as to inspire us with much confidence in the reality of this effect. If, indeed, this meagre result was all we had to show for the labour of five years we should have felt justified in regarding the outcome of the whole investigation as purely negative. Fortunately this is not the case.

PART II

Two Cases of Apparent Extra-Sensory Cognition

In May 1936 I was introduced by Mr Harry Price to a Mr D. A. S., a young consulting engineer who visited 13d Roland Gardens. He acted as subject for 1000 (U.T.) experiments with either Mrs Johnstone or her daughter acting as agent. Mr S., who met Mrs and Miss Johnstone for the first time at 13d Roland Gardens, scored only chance results, but told us that his wife believed herself to be the possessor of psychic gifts. She was unfortunately occupied in the afternoons and unable to visit Roland Gardens. Mr S., however, suggested that Mrs and Miss Johnstone and I should visit his home in an outer suburb and try some experiments there in the evening. This we did on 5th June 1936 at about 8.30 p.m. I had brought with me the packs of cards, scoring sheets and rubber stamps. We first rigged up a very efficient screen by suspending a thick doubled blanket from a line stretched across the room, so that the free ends of the blanket rested over the centre of a small table. Mr S. fixed a vertical drawing board beneath the table to prevent any contact of feet. I next arranged that Mr S. should sit in a line at right-angles to the screen through its centre, and about 12 feet away on the side opposite to the agents and the cards. Miss Johnstone and I sat at the table on one side of the screen with Mrs S. on the other side. Mrs Johnstone sat close behind her daughter. Having made these dispositions, we carefully examined the room for the possibility of reflections but found that no adjustments were necessary. The cards to be guessed were held by me one by one parallel to the screen and with their backs an inch or so away but without touching it.

The procedure then followed strictly that described under (U.T.) (p. 163) with Miss Johnstone acting as agent and witness. As we were late in starting, only four sets of 25 were guessed, the scores for these being 7, 8, 7, 8 and the average time for guessing 25 cards being about 4 minutes. No speaking was allowed while the guessing was in progress.

The result was encouraging, and we arranged to meet again in the same room at 8 p.m. on 17 June. The same persons were present and precisely similar arrangements were made with regard to seating, etc. The same screen also was used. This time Miss Johnstone was again the agent, except for one set of 25, during which her mother took her place at the request of Mrs S.

On this occasion six sets of 25 were guessed, giving the scores 7, 5, 9, 5, 6, 3,—a result which was not so promising.

At our next sitting on 24 June, Mr S. was absent, there being in the room only Mrs and Miss Johnstone, Mrs S. and myself. The scores were 5, 6, 2, 8, 4, 5—a chance result. Miss Johnstone was agent for the first three sets and Mrs Johnstone for the last three, Miss J. now sitting exactly where her mother had sat.

On 1st July Mr S. was again present, and during the experiments sat working at the far side of the room in a line at right angles to the screen and on the opposite side to the cards. The scores were 3, 3, 7, 9, 8, 7, 3, 9.

On 8th July Miss Johnstone and Mr S. were both absent, there being present only Mrs S., Mrs Johnstone and myself, Mrs J. acting as agent. The scores were 8, 9, 2, 8, 4, 3, 8, 4.

On 22 July Mrs J., Miss J., Mr S., Mrs S., and I were present. Mr S. sat in the same position relative to the screen as on 8 July, and Mrs J. sat close behind her daughter when the latter was acting as agent. Miss J. acted as agent for the first 6 sets of 25 and Mrs J. for the last 2 sets. The scores were 5, 6, 4, 8, 8, 5, 7, 5.

This concluded the first 1000 guesses with Mrs S. as subject for (U.T.).

None of the scores are sensational, but on the 1000 guesses she wins 238 successes which is just above $3{\cdot}0 \times$ S.D. As an *isolated* result the odds against this set being due to chance are about 370 to 1, but at that stage some 18 other persons had already completed sets of 1000 guesses in (U.T.) and in addition about 25,000 guesses at (P.C.) had also been recorded. When considered against this background, therefore, Mrs S.' result has little or no significance. We are not, however, on that account justified in ascribing it to chance, and subsequent developments suggest that the chance explanation is a very improbable one.

Mrs S. now began her second thousand (U.T.) guesses under similar conditions, but unfortunately the work was interrupted by the summer vacation and was not resumed till November. It soon became clear that chance scores only were being produced, and I felt I could not afford the time wasted on the journeys to Richmond. From now onwards only occasional sittings were held and the second thousand was not completed till June 4, 1937. The same agents, Miss and Mrs Johnstone, were employed throughout until the final 100 guesses which were witnessed by Miss Rita Elliott. On her second thousand Mrs S. scored only 209 successes, a purely chance result. Now and then hope flickered for an instant during this series, as for instance when on 28 May, 1937, the subject began a set of 25 with a run of 6 correct guesses, Mrs J. acting as agent.

In the summer of 1938 Mrs S. did 925 (U.T.M.) experiments, using the pointer and the same screen with the ½ inch gap which I had used at University College. She scored 191 hits, which is only 6 above chance expectation. After this I felt it useless to continue with the experiments.

Now as Mrs S. was the only (U.T.) subject who had shown any promise at all, I decided to re-examine her 2000 (U.T.) guesses when Mr Carington asked me to look for " displacement " effects.

I began by counting hits obtained by comparing each guess (i) with the card immediately preceding the actual card for which the guess was intended, and (ii) with the card immediately following the actual card for which the guess was intended. This will be clear from the examples given below.

	Actual Card		*Guess*
(i)	+		O
	W	↖	+ ←
(ii)	O		S ←
	S	↙	W

Example (i) shows a *post*-cognitive effect, for the successful hit is, as it were, delayed by one place, while example (ii) shows a *pre*-cognitive effect, for the percipient scores a successful hit one place before the card is actually lifted from the pack for the agent to look at. Perhaps a better word than " precognitive " would be " anticipatory " because " precognitive " is usually associated with the idea of seeing a future event which apparently is non-existent at the moment when it is cognised. But the 25 card images already exist objectively as a definite sequence in present time, and, for all we know, the guesser may gain his knowledge of the card one or more places ahead by exercising a faculty of *clairvoyance* in the *present*. If, however, it should ultimately be proved that the guesser's source of information is the mind of the person who looks at the cards, then the guesser is precognising a future mental state of this person which does not exist at the moment of guessing. It is therefore an open question whether or not *time* enters into these anticipatory effects ; the present data do not permit us to settle the question one way or the other. However, we can agree to call Example (ii) a pre-cognitive guess if we abstract from the term precognitive its purely temporal implications.

It is obvious that in a set of 25 guesses the maximum of possible (+1) and (−1) hits corresponding to Examples (i) and (ii) will be 24 of each, since the *first* guess cannot give rise to a "postcognitive" hit nor the last guess to a "precognitive" hit as the 25 guesses constitute a closed experiment.

Before I had finished scoring the first thousand guesses of Mrs S.'s for (+1) and (−1) effects I saw that I had made a remarkable discovery. I found there were 221 postcognitive successes as compared with an expectation of 192, and 225 pre-cognitive successes as against the same expectation of 192. These correspond to positive deviations equivalent to 2·34 and 2·72 times the standard deviation. But I asked myself: "Would these postcognitive and precognitive effects disappear in the *second* thousand guesses just as the success on the 'actual' card had petered out?" To my amazement the (−1) and (+1) effects on the second thousand continued unabated! There were in fact 232 precognitive successes and 221 postcognitive successes. These correspond to positive deviations which are equivalent respectively to 3·23 and 2·34 times the standard deviation.

So that on the whole 2000 guesses we have the following results:

TABLE 9

	Hits	*DEV.*	*S.D.*	X
On Actual Card -	447	+47	17·89	+2·627
Postcognitive (+1) -	442	+58	17·53	+3·309
Precognitive (−1) -	457	+73	17·53	+4·164

Thus on the three "central" counts (−1), (0), (+1) taken together we have 1,346 hits with a positive deviation from expectation of +178 which is equivalent to +5·822 times the standard deviation.

The chance of getting such a deviation on an *isolated* set of 2,000 guesses is less than 10^{-8}. But if we divide the whole of our 128,350 guesses into batches of 2000 we should have about 64 such batches. Very approximately, therefore, the chance of finding one or more batches with a deviation as high as 178 is about 64×10^{-8}, *i.e.* the odds are of the order of a million to one against the result being due to pure chance. Even if we multiply 10^{-8} by 160, the total number of percipients, the result is still highly significant.

The 80 " precognitive " scores were :

First Thousand : 8, 7, 6, 6, 10, 11, 8, 6, 5, 8, 4, 2, 6, 3, 1, 8, 3, 3, 7, 3, 8, 4, 1, 3, 11, 5, 10, 3, 7, 7, 7, 5, 7, 5, 9, 5, 4, 3, 3, 3.

Second Thousand : 2, 8, 9, 5, 1, 1, 8, 5, 11, 7, 6, 6, 10, 3, 4, 7, 6, 6, 6, 5, 6, 5, 9, 2, 4, 6, 7, 5, 8, 8, 5, 2, 2, 4, 10, 8, 7, 4, 7, 7.

The 80 " actual " card (0) scores were :

First Thousand : 7, 8, 7, 8, 7, 5, 9, 5, 6, 3, 5, 6, 2, 8, 4, 5, 3, 3, 7, 9, 8, 7, 3, 9, 8, 9, 2, 8, 4, 3, 8, 4, 5, 6, 4, 8, 8, 5, 7, 5.

Second Thousand : 5, 3, 5, 5, 2, 5, 5, 9, 7, 9, 4, 6, 6, 6, 3, 7, 4, 4, 5, 5, 4, 7, 6, 9, 2, 9, 2, 5, 4, 5, 5, 4, 7, 5, 6, 3, 6, 2, 5, 8.

The 80 " postcognitive " scores were :

First Thousand : 8, 7, 6, 3, 7, 7, 7, 6, 5, 5, 5, 5, 5, 2, 4, 5, 7, 4, 3, 4, 5, 8, 7, 5, 5, 5, 7, 9, 6, 6, 7, 4, 5, 7, 2, 6, 4, 8, 4, 6.

Second Thousand : 5, 3, 8, 7, 10, 7, 7, 5, 5, 7, 3, 4, 7, 3, 3, 7, 5, 4, 3, 6, 3, 5, 3, 9, 10, 9, 7, 9, 3, 4, 7, 4, 2, 1, 6, 6, 6, 10, 2, 6.

My next step was to compare each guess with the card that was situated two places after and two places before the actual card for which the guess was intended, and count the numbers of hits in each set of 25. It is clear that the *maximum* number of successes that could be scored on this plan is 23 (precognitive) and 23 (postcognitive) in every set of 25. It will be convenient to denote by (-2) the precognitive guesses and by $(+2)$ the postcognitive guesses when the displacement is two cards forward or backward. I then counted the hits on (-3) and $(+3)$ displacements and so on as far as (-8) and $(+8)$. The results are set out in Table 10.

It will be seen from this table that besides the three central values there is one other quite significant deviation—a *negative* one—at $(+2)$. This occurs also in the work of the other subject (Mr B. S.), and I shall try to probe its meaning later.

If the 17 values of χ are squared and added in Table 10 we find $S(\chi^2)=48{\cdot}73$, which with $n=17$ gives $(P<10^{-4})$.

TABLE 10

Mrs S.

DISPLACEMENTS UP TO (± 8) ON 80 SETS OF 25

		Hits	*E*	*S.D.*	χ
PRECOGNITIVE	-8	254	272	14·75	$-1·22$
	-7	299	288	15·18	$+0·72$
	-6	334	304	15·59	$+1·92$
	-5	344	320	16·00	$+1·50$
	-4	330	336	16·40	$-0·37$
	-3	358	352	16·78	$+0·36$
	-2	368	368	17·16	$\pm 0·00$
	-1	457	384	17·53	$+4·164$
	0	447	400	17·89	$+2·627$
POSTCOGNITIVE	$+1$	442	384	17·53	$+3·309$
	$+2$	315	368	17·16	$-3·089$
	$+3$	339	352	16·78	$-0·775$
	$+4$	340	336	16·40	$+0·26$
	$+5$	317	320	16·00	$-0·19$
	$+6$	293	304	15·59	$-0·71$
	$+7$	293	288	15·18	$+0·33$
	$+8$	282	272	14·75	$+0·68$

The Second Percipient Mr B. S.

On February 5, 1936, Mr B. S., a well-known London photographer, called at 13d Roland Gardens and asked if he might try some telepathic experiments with cards. He had read an article in the Sunday press dealing with my investigation, but hastened to add that the prizes mentioned for high scoring were of no interest to him. He had come, he said, " not to be tested ", but " to demonstrate to us the reality of telepathy ". He told us that often of an evening he had amazed his friends by " guessing through a pack of playing cards from top to bottom " and getting " most of them right ". That afternoon there happened to be present in the room Mr J. Aldred, whom I have known since the year 1922, and Mrs Crane who took part in the 1928-29 long distance tests. Mr Aldred had kindly come at my request to assist me as a witness for Mrs Crane who had come by appointment, but who kindly consented to wait till Mr B. S. had finished, since he had to leave early. I am reasonably certain that neither Mrs Crane nor Mr Aldred (who is a friend of mine) had ever met Mr B. S. before, and he appeared to be a stranger to everybody at Roland Gardens. We were not actually expecting him this afternoon though he had telephoned to say he would call one day that week. His manner was extremely assured and confident, and we were duly impressed. After I had explained the technique to him we began with (U.T.) experiments, using the 3′ by 3′ plywood screen which was ordinarily in position above the table. I placed Mrs Crane on the same side of the screen as Mr B. S. and directly behind him so that she could watch him. Mr Aldred acted as agent, with myself as experimenter. We sat in the usual positions described under (U.T.). I was especially on my mettle as I stood to lose a large sum of money should Mr B. S. score a 15. I therefore earnestly exhorted Mr Aldred to keep his lips firmly closed during the periods of guessing. In other respects the method did not deviate in any way from that described under the general technique for (U.T.). The percipient began by scoring a 10 and then a 7. We all thought this an excellent start, but Mr B. S. said the scores were " very poor " and asked if " this lady " meaning Mrs Crane, could take Mr Aldred's place as agent. Whereupon Mr Aldred rose and sat beside Mr B. S. on the other side of the screen while Mrs Crane took his place. Mr B. S. now scored 7, 6, 6, 3. After the last set he said it was useless for him to continue. He was best at this kind of thing in the evening especially after he had had a drink or two. He promised, however, to visit us again in a fortnight's time when he was less busy. Actually we did not see him again until the afternoon of March 27, 1936, when the only person available as a witness and agent was Mrs Dwyer who had previously assisted me in the same capacity on several occasions, and who had taken part in the 1928-29 experiments. Visual imagery was employed by Mrs Dwyer, and the scores were 4, 6, 6, 7, 3, 4, 4, 3, which is just slightly below chance expectation. No one entered the room during the series, there being present only Mr B. S., Mrs Dwyer and myself.

On April 3, 1936, Mr B. S. came again to 13d Roland Gardens and did 200 guesses with Mr H. Heckle acting as agent and witness. No one was in the room except the agent, the experimenter and Mr B. S. The scores were 8, 6, 5, 6, 4, 5, 6, 8, a result which is a little above chance expectation but not significantly so.

Mr B. S. now said that he would like to try the experiments in the evening at his own studio in the West End. After some telephoning I ultimately arranged for 7 p.m. on June 25, 1936. I asked him if Mr Aldred might act as agent and he agreed as I said I could not be sure of getting anyone else. Mr Aldred and I went together to the studio, and after chatting for a few minutes Mr B. S. said that before starting he must have a drink. We all three adjourned to a public house nearby where Mr Aldred and I had one lager each and Mr B. S. consumed a libation of brandy and soda. We then returned to the studio and rigged up a quite efficient screen, measuring about $3' \times 3'$, from several very large sheets of stiff cardboard pinned together with drawing pins. I had brought in my case one of the two boxes of cards and the rubber stamps and scoring sheets. A large sheet of stiff cardboard was also fastened in a vertical position beneath the table. The scores were again disappointing, being 6, 2, 2, 1, 3, 6, 6, 6, 5, 4. Mr B. S. attributed his failure this time to the noise of traffic which was certainly disturbing. He invited me to dine with him "some time in the autumn" and try some more tests in his private flat, but the plan did not materialise.

In all Mr B. S. did 800 guesses with 165 correct hits, which is apparently a chance result.

After having worked out the displacement effects of Mrs S.'s 2000 guesses, I examined for $(+1)$ and (-1) effects a good many of my (U.T.) records but did not go beyond (± 1). My natural interest in Mr B. S.'s personality led me to look into his record, and once again I made an astonishing discovery. Not only did I find very pronounced $(+1)$ and (-1) effects but the *negative* deviation on the $(+2)$ scores, which I had noted in the case of the other percipient Mrs S., were also very marked. I scored Mr B. S.' 32 sets of 25 as far as (± 8) displacements, and the results are set out in Table 11.

On the three central values (-1), (0), $(+1)$ it will be seen that we have 554 hits as compared with an expectation of 467·2. This denotes a positive excess of successful hits equivalent to $4{\cdot}49 \times$ S.D. with $P < 10^{-5}$ for an isolated set of 800 guesses. But the whole material could be divided into about 160 consecutive blocks of 800 guesses, and very approximately the chance of finding one or more blocks with the above excess of hits is of the order 1 in 600.

TABLE 11

Mr B. S.

DISPLACEMENTS UP TO ±8 FOR 32 SETS OF 25

		Hits	*E*	*S.D.*	χ
PRECOGNITIVE	−8	113	108·8	9·33	+0·45
	−7	116	115·2	9·60	+0·08
	−6	122	121·6	9·86	+0·04
	−5	131	128·0	10·12	+0·30
	−4	140	134·4	10·37	+0·54
	−3	144	140·8	10·61	+0·30
	−2	147	147·2	10·85	−0·02
	−1	194	153·6	11·08	+3·65
	0	165	160	11·31	+0·44
POSTCOGNITIVE	+1	195	153·6	11·08	+3·74
	+2	115	147·2	10·85	−2·97
	+3	119	140·8	10·61	−2·05
	+4	139	134·4	10·37	+0·44
	+5	121	128·0	10·12	−0·69
	+6	127	121·6	9·86	+0·55
	+7	124	115·2	9·60	+0·92
	+8	112	108·8	9·33	+0·34

For the 17 values of χ we find $S(\chi^2)=43{\cdot}14$, which with $n=17$ gives $P<0{\cdot}0005$.

It will be observed that there is on the whole no significant scoring on the actual card (0) but the few high scores with which the percipient began his guessing on February 5 at least suggest that he began by hitting the mark switched off to (±1) and never found it again. Mrs S., on the other hand, kept intermittently to the mark for a thousand guesses and then lost it almost completely.

It is instructive to see how the performance of Mr B. S. varied with the agent, although of course no satisfactory conclusions can be drawn as to whether any particular person favoured successful guessing.

The results appear in Table 12. It would appear that the only occasion on which Mr B. S. failed to obtain a + excess of (±1) guesses was on April 3, 1936, when Mr Heckle was the agent.

TABLE 12

(± 1) PERFORMANCES WITH THE DIFFERENT AGENTS

Agent 1. *Mr Aldred*

Dates	$\pm$ *Guesses combined*	± 1 *Hits*	*E*	χ
5/2/36	96	14	19·2	
25/6/36	480	126	96	
Totals	576	140	115·2	+2·6

Agent 2. *Mrs Crane*

Date	$\pm$ *Guesses combined*	(± 1) *Hits*	*E*	χ
5/2/36	192	51	38·4	+2·3

Agent 3. *Mrs Dwyer*

Date	$\pm$ *Guesses combined*	(± 1) *Hits*	*E*	χ
27/3/36	480	118	96	+2·5

Agent 4. *Mr Heckle*

Date	$\pm$ *Guesses combined*	(± 1) *Hits*	*E*	χ
3/4/36	480	80	96	−1·8

The Theory of Multiple Determination.

At this stage it seems advisable to try to form some picture of what is going on. We have evidently found two persons who score ± 1 successes much more easily than they score successes on the actual card that is being looked at by the agent. If they score significantly on the actual card it is only for a short time and then their aim becomes biassed so as to hit the card before or the card after. Presently, as is the case with both percipients, they fail to score significantly on the actual card itself but continue to score ± 1 hits. I do not think it likely that I have any more (U.T.) records which show the ± 1 span of cognition since I have (at least summarily) gone through all my (U.T.) records with a view to finding ± 1 cases. But it is conceivable that there may be persons with a (± 2) span of cognition.

Now it seems plausible to assume that in the cases of Mrs S. and Mr B. S. two images are present in the subconscious, the $+1$ image and the -1 image, both struggling to emerge into consciousness. Judging from the results, these appear to be of about equal dynamic energy, and when these two images are of *different* symbols one might assume it was about equally likely whether one or the other succeeded in pushing its way above the conscious threshold. But if the ± 1 images were of the *same* symbol, it might be reasonable to assume that one image reinforced the other, and that the percipient was more likely to score a correct hit in such a case.

I shall call a guess " multiply determined " if it belongs to one of the four following types :

Type I	*Type II*
Actual Card Sequence	*Actual Card Sequence*
+	+
0 ⟵	0
0	0 ⟵
S	S

Type III	*Type IV*
Actual Card Sequence	*Actual Card Sequence*
+	+
0 ⟵	+ ⟵
+	+

In each example the *arrow* points to the actual card for which the present guess is intended.

Thus in Type I we might suppose that the actual card image is possibly reinforced by the (-1) image, and in Type II by the $(+1)$ image, while in Type III it might be that the (± 1) images reinforce each other. Possibly in Type IV all three images might assist the guesser in getting the actual card correct.

Any guess which does not belong to one of the four types illustrated by the above examples we shall call " non-multiply-determined " (N.M.D.).

Now it is easy to see that multiple-determination of Types I, II and IV does not apparently assist the guesser in getting the *actual card* correct.

For instance, in the case of the *first thousand* guesses of Mrs S., we have

No. of guesses M.D. of Types I, II, IV		$=341.$
,,	successes on Actual Card	$=78.$

Expected No. of successes on Actual Card

$$=\frac{341}{1000}\times \text{Total No. of Successes}$$

$$=\frac{341\times 238}{1000}=81{\cdot}1.$$

The difference is so small that we need not trouble about examining the second thousand since it was only on her first thousand guesses that the subject scored significantly on the actual card.

Again in the case of Mr B. S., the number of M.D. guesses of Types I and II is 272 and the number of successes on the *actual card* is 64.

But the expected number of successes on the *actual card* on these 272 M.D. guesses is $\frac{165}{800}\times 272=56{\cdot}1.$

Here although there is a slight excess of successes on M.D. guesses it is not significant.

Multiple-determination of Type III is far more interesting.

For the 2,000 guesses of Mrs S. (equivalent to 3,840 (± 1) guesses combined) I have made out the following contingency table :

TABLE 13

Mrs S.

SHOWING EFFECT OF M.D. 3 ON ± 1 GUESSES

S = No. of Correct Hits, (either ± 1).
F = ,, Incorrect Hits, (whether ± 1).
M.D.3 = ,, Multiply-Determined Guesses, Type III.
N.MD.3 = ,, Guesses not Multiply-Determined of Type III.

	S	*F*	*Totals*
M.D.3 -	164	340	504
N.M.D.3 -	735	2,601	3,336
Totals -	899	2,941	3,840

For this table we find $\chi^2=26{\cdot}96$, so that the odds against the excess of (± 1) hits on M.D.3 guesses being due to chance are many millions to 1. It is clearly not worth while applying Yates's correction in such a case.

In the case of Mr B. S., the effect of M.D.3 on (± 1) successes is even more striking. The results are set out in the following contingency table:

TABLE 14

Mr B. S.

SHOWING EFFECT OF M.D.3 ON ± 1 GUESSES

	S	*F*	*Totals*
M.D.3 -	90	104	194
N.M.D.3 -	299	1,043	1,342
Totals -	389	1,147	1,536

giving $\chi^2=51{\cdot}11$, giving $\chi>7$

Here again the excess of correct hits on M.D.3 guesses is highly significant.

As the number of guesses of Type IV is necessarily small it did not seem worth while to examine this case separately.

It would seem that the $+1$ and -1 images are incapable of reinforcing the "actual card" image—perhaps because post and precognitive images being both in the unconscious are of a different quality from the image which has become the conscious property of the agent. On the other hand, the $+1$ and -1 images, being both outside the conscious field of the agent, are capable of acting in conjunction.

The Negative Deviation on ($+2$) *Guesses.*

In the case of both subjects we have noted very significant *negative* deviations on the ($+2$) guesses. It is difficult to suppose that these deviations—which amount to $3{\cdot}089\times$S.D. in the case of Mrs S. and $2{\cdot}97\times$S.D. in the case of Mr B. S.—are the work of chance. At first sight it seemed possible that the tendency to get the ($+2$) guess wrong might be a mere logical consequence of getting a large excess of ($+1$) guesses right. For it is a fact that the majority of guessers show a tendency to *change* from one symbol to another more frequently than happens in a purely random sequence of card-symbols. In other words, most people's records show a lesser number of "repeats" than occur in a random series. If therefore the guesser gets a (+1) guess *right* and changes his guess for the following card he automatically gets his (+2) guess *wrong*. But if this is the whole explanation of the matter we ought to find that those sets of 25 which contain a *large* number of (+1) successes contain a *small* number of (+2) successes. In other words, the (+1) and (+2) scores ought to show a significant *negative* correlation. But this does not appear to be the case. In fact, for the first 1,000 guesses of Mrs S. (40 sets of 25) there is a small *positive* correlation of 0·113 which is not significant, and in the case of Mr B. S., with 800 guesses (32 sets of 25) there is a small negative correlation of −0·287 between the (+1) and the (+2) scores which is again without significance.

The next question to be decided was: Does the guesser tend to change his guess more frequently immediately following a (+1) success than in the case immediately following a (+1) failure? To attempt to answer this I made out the following contingency tables for Mrs S. and Mr B. S. respectively.

TABLE 15

Mrs S.

1840 (+2) Guesses

	No. of (+1) Guesses correct	*No. of (+1) Guesses Incorrect*	*Totals*
Cases with next guess changed - - -	372	1,175	1,547
Cases with no change in next guess - -	54	239	293
Totals - -	426	1,414	1,840

After making Yates's correction we find $\chi^2=4{\cdot}058$ giving $\chi=2{\cdot}01$. Thus there is a *slight* but not marked tendency for the subject to change his guess after getting a (+1) success more frequently than after a (+1) failure.

But in the case of Mr B. S. there is apparently no such tendency, the changes of guess being proportionately distributed on (+1) correct and (+1) incorrect guesses.

TABLE 16

Mr B. S.

736 (+2) Guesses

	No. of (+1) *guesses correct*	*No. of* (+1) *guesses incorrect*	*Totals*
Cases with next guess changed - -	158	458	616
Cases with no change in next guess - -	32	88	120
Totals - -	190	546	736

This gives $\chi^2 = 0{\cdot}014$ (with Yates's correction) and the result is without significance.

In the case of Mr B. S., there are 115 (+2) successes, and an inspection of Table 16 shows that he obtained 32 (+2) hits which immediately follow (+1) successes. Hence on the 546 (+2) guesses which immediately follow (+1) failures we might expect, on the assumption that successes on these 546 guesses are due to chance alone, just 109·2 correct hits, whereas actually there are $115-32=83$ correct hits. This is a *deficiency* equivalent to $2{\cdot}8 \times$ S.D., and is certainly significant.

Evidently, therefore, with Mr B. S. there is a marked tendency to score *below* chance expectation on those (+2) guesses which follow *incorrect* (+1) guesses.

In fact, since he changes his guess 616 times in 736 guesses, we should, on the assumption that the (+2) successes on guesses which follow *correct* (+1) guesses are a mere consequence of the subject's *general tendency* to *change his guess*, expect that in the 190 cases which follow (+1) successes he would score

$$190 - \frac{616}{736} \times 190 = 31 \text{ correct hits.}$$

He actually scores 32. It will now be quite clear that, with Mr B. S., no psychological explanation is required to account for the scores which follow (+1) *successes*; it is the deficiency of correct hits which follow (+1) *failures* that requires elucidation. I would suggest the following hypothesis. When the subject scores a (+1) success with a certain card image, this image having found expression in consciousness ceases to worry the subject any longer, and his *next* guess is not influenced by it. In cases when the subject has called a *wrong* (+1) guess, the *correct* card image corresponding to this guess is, however, present in his subconscious mind and continues to worry him because it has not succeeded in emerging correctly above the threshold. In order to *exorcise* this disturbing image at the *next* guess, the subject pointedly ignores it by choosing one of the other four symbols. It was Dr Thouless who first suggested to me the idea of mental "exorcism" by thinking of something different, and in ordinary life we are constantly making use of this subconscious mechanism in order to rid ourselves of painful experiences.

Our hypothesis is, therefore, that subconsciously the guesser knows that he has the correct (+1) image in his mind, but knows also that he failed to get it over the threshold. Probably, therefore, at his next guess, he knows that this image is now "out of the running" and to get rid of it he chooses anything but this particular symbol. Hence the *tendency* to get the (+2) guess *wrong*.

But the theory does not perhaps apply quite so well to the case of Mrs S. In her case we should—on the assumption that the deficiency of (+2) hits following on (+1) successes is a logical consequence of *a general tendency* for the subject to change her guess—expect to find on the 426 cases following (+1) successes

$$426 - \frac{1547 \times 426}{1840} = 67{\cdot}8 \text{ correct hits,}$$

whereas there are actually 54.

This is a deficiency of 13·8 with S.D. = 7·55, so that there is still no reason to assume that (+2) failures following (+1) successes are due to anything beyond the general tendency of the subject to change her guess.

Again on the 1,414 (+2) guesses of Mrs S. which immediately follow (+1) *failures* we should, on the assumption of chance, expect to find 282·8 correct hits whereas there are actually 315 − 54 = 261. This is a deficit of 21·8 with S.D. = 15·04, but this deficit is only equivalent to 1·45 × S.D. compared with the corresponding deficit of 2·8 × S.D. in the case of Mr B. S.

In fact, in the case of Mrs S., the *total expectation*—allowing for deficit due to *general* tendency to change guess—is 282·8 + 67·8 = 350·6 correct (+2) hits on the 1,840 (+2) guesses, and the *actual* total of correct hits is 315 showing a deficit of 35·6 which is only just over 2 × S.D. In the case of Mrs S., therefore, the psychological hypothesis is not so necessary to account for the deficit as it is in the case of Mr B. S.

I need hardly point out that in a "random" series of n guesses the expected number of "changes of guess" is obviously $\frac{4}{5} \times n$, and hence that if the subject's tendency to change guess did not exceed $\frac{4}{5}n$, his $(+2)$ scores would not be affected normally by $(+1)$ successes.

Can the results be explained normally?

1. It does not seem reasonable to try to explain the ± 1 successes on the hypothesis of involuntary whispering, or sensory cues derived through noting changes of breathing, etc. In the case of Mrs S., for instance, the (-1) or effect of precognition is much the strongest of the three effects (0), (-1), $(+1)$. But, on the assumption of normal leakage, this is precisely the effect that ought to be the weakest! It is possible (though improbable) that the agent might whisper the name of the card he was looking at, and it is *conceivable* that this whisper might influence the trend of Mrs S.' thought without her being conscious of the stimulus. It is even conceivable that the stimulus might require some time for it to emerge in her consciousness and so produce a *post*-cognitive effect $(+1)$.

But how is the (-1) effect to be explained on this hypothesis? We should have to suppose that, through long experience with the 40 packs of cards, the agent Mrs J. or Miss J. had learned to recognise certain of the cards by noting small specks on their backs, and so had become aware of the denomination of the card which was resting face downwards on top of the pack ready to be lifted off for the next guess. We might suppose that the image was then transferred by involuntary whispering. But even if this far-fetched hypothesis had any truth in it, the guesser would surely win the bulk of his successes on the card whose face the agent was gazing at, and not on the card whose *back* only was visible to him.

2. Precisely similar objections apply to the hypothesis of an auditory or other code deliberately arranged between Mrs S. and one of the agents. How *could* such a code enable Mrs S. to obtain (-1) hits equivalent to 4·16 times the standard deviation? Moreover, how could the very definite effect of Type III multiple determination be accounted for on the hypothesis of a code?

3. Even if we assume (and extensive experiment shows this is *not* the case) that the slight differences in the shape of the rubber stamps can be discriminated by slight differences of the sound they cause on impact with the paper, this would not enable the guesser to get the card one place ahead correct.

4. If we make the quite absurd assumption that there was a small hole in the screen through which Mrs S. was able to peer, it could

scarcely assist her except to enable her to get the exposed card correct, and she failed to get this card correct after the first 1000 guesses.

5. Let us now consider possible objections of a statistical nature. In the first place it will be as well to observe that we do not claim to "prove" telepathy or precognition by means of statistics in the sense that one sets out to prove the validity of a theorem in mathematics.

These phenomena will be established in a scientific sense when we understand enough about them to be able to ensure their occurrence with reasonable frequency by experimental means. All that statistical methods can do is to tell us how likely it is that the effects we observe are fortuitous. In some cases the probability that the results are not due to chance may be so great as to amount to moral certitude, but, however careful the experimentation may have been, and however great the odds against the hypothesis of chance, we cannot hope to furnish the absolute proof of a mathematical proposition, or even the empirical proof of an experiment in physics which can be repeated at will. All we venture to claim is that the present investigation is a definite contribution to that rapidly growing mass of experimental evidence which is in favour of the reality of the phenomena of extra-sensory cognition. We believe that the odds against chance are sufficiently high and the experimental precautions sufficiently careful to merit the most serious attention.

Now it might be urged that the present investigation was not originally intended as an enquiry into precognitive or post-cognitive effects but was, in the first instance, a straightforward attempt to discover if there was any truth in Dr Rhine's claim that certain persons were able to divine correctly the figure on a card which was being looked at by an experimenter. Hence it might be argued that I ought to have confined myself to the task in hand and have left precognitive effects severely alone. Against this I would point out that Dr Rhine himself claimed that precognitive effects could be demonstrated by the use of Zener cards, and Mr G. N. M. Tyrrell claims to have obtained precognitive results by the use of a special apparatus which involved the choice by the subject of one of five operations. In my study of precognition I was testing a phenomenon for which there is a good deal of very impressive evidence both of a spontaneous and an experimental kind. I was not merely forming an irresponsible conjecture and then seeking statistical evidence with which to support it. The *a priori* evidence for precognition has been very ably analysed by Mr H. F. Saltmarsh in a long and important paper [*Proceedings*, Part 134, pp. 49-103], and I have yet to meet with any serious refutation of his conclusions, which are entirely favourable to the reality of precognition as a spontaneous mental phenomenon. I find invariably that the persons who maintain that there is no serious evidence for telepathy or precognition are those who have not taken the trouble to study the *Proceedings* of the Society. Until they have made this study they are not competent to air their negative opinions. I need hardly mention that *post*-cognitive effects of a spontaneous kind permeate the whole of the records of psychical research, or that Frederic Myers made a special study of them.

Now it is worth noting that my own technique—on the statistical side at least—is as well adapted for testing (± 1) effects as for testing successes on the actual card itself, and the investigation of such effects might well have formed a part of my original programme. In fact, as far back as 1935, I scored about 20,000 of the Pure Clairvoyance (P.C.B.) guesses for *post*-cognitive ($+1$) successes, on the suggestion of Professor Thouless, without, however, discovering any evidence of a deferred effect.

It cannot be argued that I applied statistical tests to a large number of features of my data and then chose the one feature, *i.e.* (± 1) effects, which happened to give high odds against the hypothesis of chance. Nothing, indeed, would be further from the truth. It was at Mr Carington's suggestion that I examined my records for displacement effects, and in selecting the records of Mrs S. and of Mr B. S. I was guided entirely by my belief that these two subjects were psychical sensitives. The first of all the records to be examined was that of Mrs S., and the successes in the three central positions yielded odds against chance of the order of a million to one even when estimated against the background of 128,350 guesses.

Some critic may question my right to select the three central values for separate statistical evaluation. My answer is: (*a*) Any sane person testing the results for precognitive and postcognitive effects would be naturally interested in testing the actual card, the immediately preceding card and the one immediately following, and (*b*) that with such material as the Zener cards, where the same five symbols occur over and over again, it is very difficult to imagine how large displacement effects could be detected, even if they occurred, since successes spread out over a number of displacements would be very diluted. It was entirely to be expected that if any displacement effect existed this would be most easily detected in positions close to the actual card.

But we have seen that in the case of both percipients, if the seventeen values of (χ^2) are summed corresponding to the displacements up to (± 8), the values of $S(\chi^2)$ obtained are so large that they lie right outside Fisher's tables. This in itself should be a sufficient answer to those who would question the propriety of choosing the three central values for special evaluation.

One could, of course, apply statistical tests to attempt to discover all kinds of bizarre relations among different features of the data, but if the effects sought for were *systematic* effects extending over 2000 guesses (as with Mrs S.), it would be a remarkable miracle indeed if the odds against chance turned out to be of the order of a million to one. And it would be an astounding combination of miracles if each of the cases which exhibited the first effect showed also a confirmatory effect with odds again of the order of millions to one. For this is what actually happened. The records of both Mrs S. and Mr B. S. showed that if the card to be guessed was sandwiched between two cards of the same denomination the (± 1) successes were enhanced to an extraordinary degree. In fact we find that Mr B. S. achieves 46% of (± 1) successes on the "sandwiched" cards and only 22% on the remainder. Mrs S. wins 33% on the "sandwiched" cards and 22% on the rest. These differences are not small discrepancies; they point to fundamental psychological factors at work.

Moreover I did *not* discover this effect by trying out statistically a large number of fantastic hypotheses until I succeeded in finding one which gave odds of millions to one against chance. If such had been my method I might have gone on testing hypotheses till the end of my days. *I discovered the effect by the perfectly natural and common-sense argument that if the* (± 1) *effects were genuine they ought to reinforce each other when the card lay between two cards of the same symbol.* In fact I actually suggested the probability of the effect to Dr Thouless one evening on a visit to his house and before I had made any counts whatever.

In conclusion, all the experiments have been witnessed and the records checked at the time by another intelligent person in addition to the experimenter. They have not been "hole in the corner affairs". Can the same be said for a great many of the orthodox investigations of experimental psychology?

If the conclusions of Mr Carington and myself put into question certain cherished dogmas of psychology such, for instance, as the dogma which states pompously that "nothing can enter the mind except by way of the five senses", then we say boldly, "So much the worse for the dogma!" Indeed, the truth is that experimental psychology is still so far from being an exact science that from its own house of glass it can scarcely afford to throw stones at psychical research. Orthodox experimental psychology today consists largely of a number of disconnected and relatively unimportant "researches"; it has no comprehensive theory by which to account for the phenomena of mental life. Psychical Research offers it something of real importance, and of real interest to humanity.

As for those narrow and limited specialists of science, who, devoid of philosophic outlook, label such careful investigations as those which Mr Carington and I have conducted with the generic name of "spooks", and pass on, we may safely leave them to the contempt of future generations.

REVIEWS

I

Infinite Traveller. By CHARLOTTE BACON. Williams & Norgate. 7s. 6d. net.

This is a book of considerable interest to those who cannot, or at all events do not, read the professional philosophers, but are anxious to think clearly about the nature and destiny of human life. Its interest for students of psychical research will be the stronger, the more they consider it a main object of the research to throw light upon the nature of personality. Three main motives for psychical research can be traced: scientific curiosity about events that seem to be outside the known, or "normal", pattern of cause and effect; desire for evidence bearing on the question of survival: and interest in the extended conception of personality which may be inferred from evidence of the paranormal. Mrs Bacon touches upon such evidence only as having relevance to a much wider discussion of the meaning of human life. But this wider discussion represents the framework within which all speculation upon the extension of human personality has to be conducted; and it is a good thing to have the general evidence so well marshalled as in this book for the proposition that man is more, and aims at more, than his science can yet describe to him.

Mrs Bacon maintains with considerable force that we are in an illogical position if we draw a hard and fast line between the reality of things perceived and the reality of qualities and values which are apprehended by other functions of the mind than perception in its narrower sense. "We must either take man-the-experiencer in his entirety or, determined to pick and choose among his news-collecting powers, we must decide to trust the reports of his calculating faculty alone." We need some such phrase as "calculating faculty", here, because of the often-forgotten fact that our sense-data are not directly given from the outer world. As Mrs Bacon puts it, "We do not know and cannot guess how a brain-impress becomes a thought. In short, we cannot detect any news in the act of *becoming* news."

The mind cannot apprehend the "news" brought in by the senses without apprehending qualities; it cannot apprehend qualities (nor, we could add, quantities) without making valuations; and it thus applies a discrimination of values which is an essential part of its apprehension of reality. But once we admit values into our scheme of reality (such as the truth of an impression received, or of a logical statement about it, or of a mathematical process for testing it), where is the sharp dividing line between the mind's report of things and its report of their meaning and significance? Actually, the line is drawn by science so as to include just those kinds of valuation which it can usefully employ for its own purposes. It is a problem for psychical research, as also for psychology, whether this convenient line is rightly drawn for us who attempt to estimate perceptions not hitherto within the range of formal science.

Mrs Bacon devotes several chapters to the demolition of the materialistic, or mechanistic, view of life. Such argument inevitably chases its own tail, since in a mechanistic world ideas can have no validity, and any theory is as meaningless as another, including the mechanistic theory. But the point emerges with increasing urgency that human experience, the very fact that there is such a thing as human experience, is left entirely unexplained in any world of which the mechanism alone is taken into account. And since human experience is our only source of evidence, the materialistic demonstration has the disadvantage of starting nowhere, as in practice it ends nowhere.

In any case, science has increasingly found that the study of material objects resolves itself into a study of energy and of the fields of influence by which energy is directed. In proceeding to show the highly potent directive power, and the constructive or destructive effect, of human values at their different levels of truth or falsity, Mrs Bacon is putting values in their proper place as subjects of scientific discourse. They are facts which direct the energies of humanity, for good or ill. They have produced manifest results in that structure of human life which is our real and intimate environment. And in this structure, Mrs Bacon argues that it is absurdly inconsistent to leave the properties of "spiritual man" out of account. For one thing, "no experience can be left in the air, unaccounted for": "either the mystic-saint's experience must be theoretically fitted in to biological life's Real, or our ideas of Reality must be theoretically enlarged beyond that scene of physical Reality which biological life occupies, surveys and knows."

For another thing, "If the potentialities of Man are to be ascertained the search must be conducted to the extremity of his achievements. Should a mountain's height be the object of inquiry, its lesser heights and its average height are ignored, for a mountain is exactly as high as its highest peak, however narrow and cloud-capped that peak may be." We are here in a region where the philosophy, however true and important in itself, may be thought to climb higher than psychical research has any business to follow. But it is of considerable value to us to be given a comprehensive view of the whole mountain, if we are to have a right and proportionate idea of the particular ridges and valleys which it is our business to explore. This book presents a comprehensive view of human faculty, actual and potential, which has a certain kinship with the outlook proclaimed by F. W. H. Myers, though Mrs Bacon's approach and treatment are widely different from those employed by the author of *Human Personality*. It is an outlook that keeps research in touch with a wider, philosophical frame of reference, and has its influence in keeping both the theory and the practice of research from neglect of the climatic conditions which fundamentally affect the quality of all psychic events.

K. R.

II

Mythology of the Soul, A Research into the Unconscious from Schizophrenic Dreams and Drawings: H. G. Baynes. Baillière Tindall and Cox, 1940: pp. xii+912; 32s. 6d.

Dr Baynes is well known as a leading member of the Jung school of psychology, and many of our members will recall the paper he read to the Society in 1936 on "The Ghost as a Psychic Phenomenon".[1]

In his first chapter he gives a brief account of the development of the psychological conception of mental disorders, as viewed from the standpoint of Jung, whose theories are probably not as well known in this country as those of Freud. Dr Baynes describes how both Jung and Freud, working independently, arrived by different methods—psychological experiment, particularly association tests, and clinical experience—at a more dynamic conception of the unconscious than had till then obtained. "Jung's conception of the autonomous complex became the syncretising basis which united the classical conceptions of the French school with those of Freud, the great pioneer of psycho-analysis."

For some time complete harmony prevailed between Zurich and Vienna. But by 1914, when Jung republished his *Content of the Psychoses*, from which Dr Baynes makes several interesting quotations, a fundamental divergence between the two systems had been revealed. In contrasting the "reductive analysis" of Freud with what he claims to be his own constructive methods, Jung says:

"The patient's unmistakable striving to express something by

[1] Reported in S.P.R. *Journal* for May 1937.

means of his delusion, Freud conceives retrospectively, as the satisfaction of his infantile wishes by means of fantasy. The constructive standpoint is different. Here the delusional system is neither infantile, nor, upon the whole, *eo ipso* pathological, but subjective, and hence it is justified within the scope of the subjective."

A significant feature of the Jung school is the emphasis laid on the "collective unconscious", which indeed seems to have been a principal cause of the split with Freud. The view is that the psychic constitution of man varies as little owing to differences in social environment as his body does owing to differences of climate, and that there are "primordial images" which have so established themselves in the psyche as to make it impossible to explain any individual case solely in terms of the patient's personal history. The primordial image "is the figurative expression in dream or fantasy of the living continuity of instinctual experience. It is also conceived as the source or container of that supra-personal afflux of energy, which, when released, either carries the individual towards his goal, or smashes him pitilessly on the rocks". Dr Baynes makes great use of "primordial images" in analysing his patients' fantasies, perhaps on occasion riding them too hard.

The chapters which follow set out in detail the cases of two schizophrenic patients, in whose treatment Dr Baynes relied largely on the analysis of drawings embodying their fantasies, a few of their dreams being also analysed. There are reproductions of a number of these drawings, both in black and white and colours, which add greatly to the interest of the book.

Jung and his school, as is well known, lay great emphasis on differentiation of psychological types, *e.g.* introvert and extravert. From a paper recently read by Jung to the Psychiatric Section of the Royal Society of Medicine, Dr Baynes quotes a passage in which schizophrenia is distinguished from hysteria and other neuroses:

"The fundamental difference which distinguishes hysteria and the other neuroses from schizophrenia consists in the maintenance of the potential unity of the personality in the former group. The general picture of an association-test of a schizophrenic may be very similar to the test of a neurotic, but a close examination reveals the fact that in a schizophrenic patient the connection between the ego and certain complexes is more or less completely lost."

There are two causative factors of schizophrenia, which seem to be mainly independent, the weakening of conscious control, and the activation of the unconscious with its atavistic content. Among the major consequences Jung mentions "a decrease in the responsibility and adequate reaction of the ego: realisation is interfered with thereby causing insufficient and inadequate emotional reactions ".

Both these symptoms figure, in varying degrees, in the two cases analysed by Dr Baynes. To all outward appearance there seemed little wrong with either patient. In each case the trouble started with shock in the patient's early years: in the first case, the death of the patient's mother followed a few years later by the suicide of his sister, in the second the scandalous conduct of the patient's stepfather. To the question "Were the patients cured?" Dr Baynes in the concluding section of his book replies "Cured of what?" "The schizoid individual is rarely, if ever, a disabled neurotic; neither does he suffer, as a general rule, from distressing bodily symptoms. He is a man who has been singled out by fate to accept solitariness as the fundamental condition of his existence." For such persons "a merely mechanical drainage of the unconscious by reductive analysis" is insufficient without the sense of renewal of life which is the essence of religion. Given a sufficiently extended definition of religion, support could be found for Dr Baynes's statement that, though the two patients were of very different psychological types, "in both cases the flow of unconscious events led straight to the religious problem."

The psychical researcher is frequently invited to consider "automatic", "psychic" or "inspirational" drawings produced when the control of the conscious mind is in abeyance. He will find it interesting to compare the numerous illustrations to this book with examples of "automatic" drawings already familiar to him. No claim is or could be made for the drawings in the book that they transcend the normal powers of draughtsmanship of the patients, since those drawn by the first patient, a doctor, touch the extreme limit of erudity, while the second patient was a draughtsman by profession; his designs are mostly, notwithstanding the frequently sinister character of the symbolism, agreeable to study.

The drawings of both series in the book give an appearance of purposiveness which is not always present in automatic drawings. They show none of that apparently meaningless repetition of patterns, especially squirls and squiggles, with which the student of "automatic" drawings is all too familiar. I do not know whether medical psychologists have attempted by the study of "automatic" drawings of automatists unknown to them to reconstruct the psychological state, and even the emotional history of the automatists. The results might be interesting, and would, if successful, give welcome support to the objective validity of their theories.

Much of the interest of the book derives from the many pleasant excursions which Dr Baynes makes into the domains of religion, mythology, symbolism in art and literature, primitive culture and world-politics. The psychological school to which he belongs does not regard religion as a tedious epiphenomenon on the juicier side of existence. Mythology, literature and art are often discussed by psychologists, but one cannot always read their comments, as Dr Baynes's may be read, without having one's teeth set on edge.

With great ingenuity he develops the view that many popular stories (*e.g.* the Book of Tobit, Parsifal, Dracula) whatever their authors' conscious intentions, do in fact symbolise the schizophrenic state and its treatment. What are usually considered the pseudo-sciences of alchemy and astrology were, in his view, largely concerned with the unconscious mind, so that when they were supplanted by orthodox chemistry and astronomy, psychology suffered a temporary set-back.

Dr Baynes follows Jung in attaching great value to what he calls the "concept of cosmic duality of Chinese culture, in which the dark, earthy feminine principle of Yin is opposed by the light, spiritual, masculine principle of Yang". (This conjunction of epithets is not the reviewer's.) The temporary predominance of either principle stimulates the other, till it in turn predominates, and a reverse process sets in. The two principles are roughly equivalent to reason and instinct, the latter having, according to Jung, a significance going beyond sexuality and self-preservation. To the disharmony between these two principles, in individuals and in nations, Dr Baynes would assign most of the evils of the modern, and particularly the western world.

Parallels to the Yin-Yang concept can, as Dr Baynes indicates, readily be found in Blake, notably in *The Marriage of Heaven and Hell*, where he asserts that "without contraries is no progression", and instances "what the religious call Good and Evil. Good is the passive that obeys Reason: Evil is the active springing from Energy". Dr Baynes, pushing a prophetic antinomianism to a point which might have alarmed Blake, says "A stable and reliable social order will be found eventually to rest on two reciprocal principles. The first is collective authority and discipline in all matters wherein conformity is indispensable, the second individual freedom of judgment in all matters which concern the life and health of the soul". It is not clear what he means by "reciprocal", but unless it is intended that the first principle should circumscribe the operation of the second, the suggestion appears to be that, provided we all observe the traffic-light rules, we can each do what is right in his own eyes without harm to the community.

It may seem out of place in a publication devoted to psychical research to labour a point of social ethics, but this and many other passages in Dr Baynes's book seem based on a view of the unconscious which has a very close bearing on psychical research. It appears to be implied that ethical differences, so far as they are not mere matters of traffic-light convention, are due to different degrees of harmony and dis-harmony between the conscious and unconscious minds of individuals. This is akin to the proposition, sometimes asserted by medical psychologists, that any form or degree of dissociation is pathological. That may possibly be true, but it is not a conclusion which would naturally be drawn from a study confined to those types of dissociation which come within the specific purview of psychical research.

The parallelism between secondary personalities of the Sally Beauchamp type and some mediumistic controls is well recognised, and where the secondary personality or control is of a tricky, untrustworthy type, the view attributed above to medical psychology would, so far as a layman can judge, fit the facts. Nor does this view appear to be contradicted by the fact that the primary personality (*e.g.* Miss Beauchamp herself) or the medium (*e.g.* Stainton Moses) is extremely conscientious, though both Miss Beauchamp and Stainton Moses were in some degree misfits, who found it difficult to develop normal reactions to life. The sort of case which the psychical researcher finds it difficult to fit into the picture is that of the medium, whose reactions to life are perfectly normal and show no sign of that apathy or desire to evade responsibility which Dr Baynes's two patients illustrate, with a trance personality on the same ethical level. The dissociation, as judged by such tests as amnesia, may be profound, much profounder than with Dr Baynes's patients, but the split is not on ethical or emotional lines at all, being apparently confined to a difference in mental content and, sometimes, in intellectual power.

It strikes the lay psychical researcher as curious that medical psychologists (Dr T. W. Mitchell is of course a notable exception) are so little interested in the psychology of mediums and automatists. It might have been supposed that they would have gladly seized the opportunity of widening their experience by the study of material of a kind different from that which comes to them in their ordinary practice. Students of psychical research, who fully realise the complexity of the problems they encounter, would welcome any assistance they could obtain from medical psychologists, especially from any giving proofs of such an open mind and so wide-spread interests as Dr Baynes has shown.

W. H. S.

OBITUARY

Mr Oliver Gatty

At the moment of going to press we learn with the deepest regret of the death at Cambridge of Mr Oliver Gatty as the result of an accident occurring while he was conducting an experiment connected with war work for the Government.

Mr Gatty had a brilliant career at Oxford, where he studied physics and became a Fellow of Balliol. But he soon developed an interest in biology, in which subject he did research work first at the Rothamsted Experimental Station, and later at Cambridge, where he had been living for several years.

His work in Cambridge, on the electrical conductivity of the frog's skin, gave full scope to his patience and ingenuity in experimental technique. Moreover the numerous observations required elaborate statistical treatment; and the knowledge of statistical theory and practice which he acquired for his biological work enabled him to give valuable help to the S.P.R.

He joined the Society in 1933 and became a Member of Council in 1934. He did much important research work for the Society, and also made generous gifts both of money and apparatus. He took part in the investigation of Rudi Schneider, on which he read the Society a paper, and of Mrs Leonard, and was a Member of the "Cambridge Committee", which has for some time past been investigating paranormal cognition. He was the joint author, with Dr Irwin, of a note (pp. 147–150) appended to Mr Carington's paper in this Part of *Proceedings*.

At the time of his death he was also engaged on a study of dowsing from the standpoint of the physicist, and was intending to give the Society a paper on this subject in the autumn.

Professor Broad writes:

It was a terrible blow to the Cambridge Committee, who have been collaborating with Mr Whately Carington for a long time past in his work on extra-sensory perception, to hear of Mr Gatty's tragic death. He had been a valued member of our Committee from the first: he had attended our last meeting on May 27th; and we had arranged to meet again on June 18th. His death, which was in every way comparable to that of a gallant member of the armed forces in the field of battle, is a severe loss, not only to science and to our Society, but also to those who knew him personally.

No one could know Gatty without liking him. To his great abilities he added a boyish vigour, enthusiasm, and readiness to explore even fantastic possibilities, for the fun of the game. He was always ready to spend time, labour, thought, and money on any problem of psychical research which seemed susceptible of experimental or statistical treatment. He was patient, kindly, and helpful in collaborating with others, and his cheerfulness and enthusiasm were infectious.

His colleagues on the Cambridge Committee can understand something of what his loss must mean to his wife and family and his intimate friends, and their sympathies go out to them in their sorrow—a sorrow which is mitigated by pride in his gallant death in the service of his country.

Mr Whately Carington sends the following note :

Oliver Gatty's tragic and untimely death is a serious loss to Psychical Research and a great personal blow. The first aspect needs no emphasis by me : as for the second, I shall miss him greatly both as a colleague and a friend. Few men were possessed of so great a measure of personal charm. and fewer still were so capable of consolidating immediate liking into lasting affection and regard. On the technical side I owe him much for his enthusiastic and helpful membership of the " Cambridge Committee ". His many interests and activities often prevented him from maintaining as close a contact with the work as we would both have wished ; but his encouragement was invaluable and he had a most unusual facility for grasping implications and for making suggestions of the most ingenious and stimulating character.

Many kinds of technical ability can be found elsewhere. but his especial gifts I feel to be irreplaceable.

PROCEEDINGS

OF THE

Society for Psychical Research

PART 163

OBITUARY

Sir Oliver Lodge, F.R.S.
Sir J. J. Thomson, O.M., F.R.S.

Since the issue of the last Part of *Proceedings* the Society has lost two scientists of the highest distinction, both of whom joined the Society in its very early days and were for many years actively associated with its work.

J. J. Thomson became a member in 1883, and served on the Council from 1887 to 1921 : on his retirement in that year he was elected a Vice-President. In 1918 he became Master of Trinity College, Cambridge, which from the days of our founders, Sidgwick, Myers and Gurney, has contributed to the Society many of its leading members, and during his Mastership the connection was strengthened by the institution of the Perrott Studentship.

It is, however, no disparagement of the great services which J. J. Thomson rendered our Society if Lodge's work in Psychical Research is treated at greater length. As he has himself recorded in *Past Years,* Lodge's first acquaintance with Psychical Research was through Edmund Gurney, who was then collecting and tabulating the material for *Phantasms of the Living*. The book struck him at the time " as a meaningless collection of ghost stories ", but he was impressed with the energy and seriousness that Gurney devoted to his task. Through Gurney he met Myers, and in January 1884, within two years of the foundation of the Society, he became a member.

His first active work is recorded the same year in Vol. II of *Proceedings,* in which there appears (page 189) a paper by him giving an account of some " Experiments in Thought Transference ".

This paper is a short one; the experiments took the form mainly of asking percipients to reproduce simple diagrams which had been excluded from their normal means of perception. Although these particular experiments were not amenable to statistical analysis, Lodge from the first recognised the important part that statistics could play in experimental psychical research, for in the same volume of *Proceedings* is to be found a note in the form of a letter to Gurney, in which he refers to "the valuable suggestion of M. Richet that feeble thought-reading powers or slight mental reverberations may be possibly detected in some persons by applying the laws of probability to a great number of guesses made by them on a limited series of objects"; he follows this up by suggesting various mathematical formulae which could conveniently be used.

Close association over a long period of years between Richet and Lodge ripened into the warmest friendship. International cooperation is no less necessary in psychical research than in other matters, and no less difficult to achieve, and the friendship between these two distinguished scientists resulted in much useful collaboration in our subject. It is of interest to note that while the French physiologist and the English physicist took much the same view of Psychical Research problems regarded in detail, they came to fundamentally different conclusions as to their ultimate significance, particularly as bearing on the question of human survival. The measure of their agreement and their difference appears from two short papers, "For and Against Survival," which they contributed to Vol. XXXIV of *Proceedings.*

It was perhaps natural that, while each of them was aware of the developments taking place in the branch of science in which the other had specialised, and indeed in scientific thought as a whole, he should have most clearly in his mind the developments taking place within his own province, so that to the physiologist the most significant development would appear to be the increased knowledge of the interconnection of mental and bodily processes, and the physicist would be inclined to stress the fact that matter seemed to be rapidly shedding many of the conceptions traditionally associated with it, physics, as Lodge put it, taking on the character of metaphysics.

It was through Richet that Lodge obtained his principal first-hand experience of physical phenomena. In 1894 as Richet's guest he attended sittings with Eusapia Paladino in the South of France, and formed a favourable opinion of her phenomena, all of which, however, he agreed could have been simulated, with or without previous preparation, if the control of her head and limbs was defective: in his view, however, the control was fully adequate. Hodgson made various criticisms of the control which were to some extent justified by a later series of sittings held at Cambridge in 1895, but Lodge, while admitting the fraud at Cambridge, considered that it could not account for the phenomena seen by him the year before, taken as a whole. In this opinion he was confirmed by the results of other sittings with the same medium, at which he was not himself present, notably by an incident witnessed by his son, Mr Brodie Lodge, at Richet's house in 1898, when a heavy table is reported to have been levitated in broad daylight out of doors.

An important landmark in Lodge's career was reached in the winter of 1889, when Mrs Piper, of whom favourable reports had been received by the Society from America, visited England. She was met on arrival by Lodge, who between November 1889 and January 1890 had 23 sittings with her, one at Cambridge, and the remainder at Liverpool. It is to be observed as an early indication of Lodge's ability as a sitter, subsequently shown on many other occasions, that the Liverpool sittings during this visit were much more successful than those held with other sitters either at Cambridge or in London. Lodge contributed to *Proceedings* (Vol. VI) a very full account of these sittings, which he prefaced with a formal report in which he said—"By introducing anonymous strangers and by catechising her myself in various ways, I have satisfied myself that much of the information she possesses in the trance state is not acquired by ordinary commonplace methods, but that she has some unusual means of acquiring information. The facts which she discloses are usually within the knowledge of some person present, though they are often entirely out of his conscious thought at the time. Occasionally facts have been narrated which have only been verified afterwards, and which are in good faith asserted never to have been known. . . . Concerning the particular means by which she acquires the different kinds of information, there is no sufficient evidence to make it safe to draw any conclusion. I can only say with certainty that it is by none of the ordinary methods known to Physical Science."

Mrs Piper's powers were much discussed, inside and outside the Society, during the succeeding years, more especially since, on her return to the United States, she was for a long time under the close investigation of Hodgson, whose well-known scepticism as to many of the phenomena of psychical research did not prevent him publishing, in 1898, a very long report on her (*Proceedings*, Vol. XIII), in which he gave emphatic support to the "Spirit

Hypothesis ". This paper gave rise to much debate, in which Lodge took a prominent part.

In 1900, the Society suffered a severe blow by the death of Henry Sidgwick, one of its founders, and its first President, and this was followed within a few months, in January 1901, by the death of another of the founders, Frederic Myers. Those who were closely associated with the Society's work at the time of this double disaster have declared that it was mainly the action of Oliver Lodge in accepting the Presidency that saved the Society from the apparently imminent danger of collapse. The acceptance of the Presidency at that time was a notable act of self-sacrifice as he had only the previous year become Principal of the newly founded University of Birmingham. He was re-elected President for the years 1902 and 1903, and the fact that the reins of the Society were during these years in the hands of a man of outstanding distinction in science, combined with Lodge's own personal gifts of tact and geniality, prevailed to set the Society once again on a firm basis, and indeed to strengthen it by the attraction of a large number of members who became actively interested in its work.

In his Presidential Address, during his second year of office in 1902, he said, " If any one cares to hear what sort of conviction has been borne in upon my own mind as a scientific man, in some 20 years familiarity with those questions which concern us, I am very willing to reply as frankly as I can. First, then, I am for all present purposes convinced of the persistence of human existence beyond bodily death ; and though I am unable to justify that belief in a full and complete manner, yet it is a belief which has been produced by scientific evidence, that is, it is based upon facts and experience, though I might find it impossible to explain categorically how the facts have produced that conviction."

In view of certain misconceptions which are still current it may not be superfluous to emphasise that as early as 1902, when he was not under any particular emotional stress, he declared himself without qualification a believer in survival, the belief being based not upon emotion but on " facts and experiences ". That Lodge only adopted this belief after the death of his son Raymond in the last war is, of course, an absurdity which has never found acceptance among our members.

It was, however, not only the evidence obtained through Mrs Piper which led Lodge to this position. He had also had successful sittings with that very remarkable medium, Mrs Thompson, of whose phenomena, which particularly impressed Myers, regrettably few records survive. He did not make up his mind as to the significance of the Piper and Thompson evidence without giving full weight to the complications caused by telepathy, our ignorance of the method by which it works, and our uncertainty as to the limits that can reasonably be attributed to its operation.

In the " Cross-Correspondences " which developed shortly after Myers's death, Lodge, like many other close students of the problem, thought he saw a prospect of excluding the operation of telepathy between living minds, and from the very beginning he devoted himself to a study of the complicated evidence resulting.

Mention has already been made of the heavy bereavement he sustained in the last war, through the death in action of his son Raymond. With great courage he placed before the public the evidence he received, before and after his son's death, which seemed to him both to foretell that event—the " Faunus " incident is one of the most striking single incidents in the whole of Psychical Research—and also to prove that his son's personality had survived death, and could and did communicate with those he loved on earth. The frankness of this book provoked in some quarters, as was to be expected, hostility and even ridicule, which Lodge unflinchingly met and answered in a later edition.

It was also during the last war that Lodge, whose remarkable success with mediums was due less to luck than to his own personal gifts of sympathy, made contact with Mrs Leonard. Not the least of his many services to us was to bring the Society into touch with her.

When the Myers Memorial Lectureship was founded the appointment of Oliver Lodge to deliver the Inaugural Lecture in 1929 was, in view of his long and intimate friendship with Myers, and his own outstanding position in Psychical Research, inevitable, as was his election as Joint President of Honour with Mrs Sidgwick on the occasion of the Society's Jubilee in 1932.

In the many books with which he appealed to a wide audience, Lodge set out with great lucidity his own philosophy, which found nothing incompatible between the conceptions of science, as they had developed during his own lifetime, and those of Psychical Research. On Lodge's contribution to science, and its connection with Psychical Research, Lord Rayleigh writes as follows :

Sir Oliver Lodge has latterly been known to the general public even more as a protagonist of Psychical Research than as a pioneer in physical science. Few scientific workers can expect that what they do in their prime will long remain in full view. If it is in the main stream of progress it is almost certain to be overgrown by the work of others who have improved upon it and completed it ; and if not in the main stream of progress, it is liable to be pushed out of sight by developments which are more in fashion. It may be suspected that Lodge is no exception to this rule : and it is fitting that his scientific work should be recalled to memory. Only a few salient points can receive notice, and they will be treated in a general way. Those who want details will not be at a loss to find them elsewhere.

Coming then to Lodge's early electrical researches, he began with the investigation of lightning conductors, and this led him to set up stationary waves of potential difference along parallel oppositely electrified wires, and he identified the position of nodes and loops. Waves of electric force necessarily existed in the intervening space. The velocity of these waves was determined from the measured wavelength and calculated frequency, and was shown to agree approximately with the velocity of light. All this was concurrent with the work of Hertz on free electric waves, and goes far to establish the same general conclusion. Hertz says, in the preface to his book on electric waves :

" In the same year that I carried out the above research Professor Oliver Lodge carried out a series of experiments on the discharge of small condensers which led him on to the observation of oscillation and waves in wires. . . . There can scarcely be any doubt that if I had not anticipated him he would also have succeeded in observing waves in air, and thus proving the propagation in time of electric forces."

The detector of electric waves used in the early wireless telegraphy was Lodge's " coherer ". As first used by him this took the form of two slightly oxidised brass knobs, in contact. When electric waves fell on this arrangement it was found that electrical contact was established between the knobs, which could readily be detected by means of a battery and galvanometer. The tube of iron filings which was used later was due to Branly, and Lodge, with characteristic fairmindedness, always emphasised the importance of Branly's contribution.

Everyone is familiar nowadays with " tuning in " to the wireless. This really has no necessary connection with transmission over long distances. It should be emphasised that Lodge was the first person to put electrical circuits into and out of resonance. He did this in the experiment called the " Syntonic Leyden jars ". Hertz a little earlier had spoken about resonance, but there was no real resonance in his experiments, because his sender did not give out persistent oscillations, and only so can resonance come into evidence. You can have resonance to the note of a tuning fork but not to the crack of a whip.

Lodge later applied electrical resonance to the first system of *tuned* wireless telegraphy, of which he was the pioneer. This was a spark system, which only gave limited trains of oscillations, like a tuning fork intermittently struck. The modern system uses a continuous train of oscillations, like an organ pipe continuously blown, which of course lends itself to more accurate tuning: but Lodge made the essential step.

It is not always realised that the famous principle of relativity has its origin not in a metaphysical conception about the nature of space and time, but in an induction from experience. Nineteenth century physics had built up the conception that space was filled with a medium of a quasi-material nature (called ether) through which light was propagated. The earlier, and simpler, view was that this medium carried transverse waves of elastic displacement, like an elastic solid. But when the idea of waves of elastic displacement was superseded by Maxwell's conception of waves of electrical displacement, the idea of an ether was not abandoned. If there is a medium, it looked as if we ought to detect our motion through it, as for example when the earth on which we are situated moves round the sun at a speed of some 19 miles a second. There should be, in effect, a wind of ether of 19 miles a second blowing through our laboratories, and it ought to be possible to bring it into evidence by a variety of methods. But the result is always negative. The wind will not come into evidence. Some people thought that this proved that the ether was carried along locally by the earth, as water is to some extent dragged along by a ship. This view leads us into difficulties in other directions; for example, it is not easily reconciled with known facts about the aberration of the stars. But Lodge's best contribution to this question was in the direction not of speculation. He determined to try the difficult experiment of determining whether in fact the ether was carried along by a moving body, using not the earth, but a pair of rapidly rotating steel discs. The question was whether light would travel faster with the discs than against them, when it was sent round the circuit by successive reflexions: and the answer which Lodge arrived at was that it did not. If the ether moved, it was with less than $\frac{1}{500}$th of the velocity of the disc. This experiment then suggested strongly that the ether was not carried along with the earth: and in fact it was one of the contributory causes which led most physicists to abandon the attempt to interpret the optical phenomena of moving bodies in terms of ether, and to range themselves with the view of Einstein, who recommended the abandonment of this method of approach as unfruitful, and substituted the approach by the principle of relativity which asserts *ab initio* that any experiment designed to detect uniform motion relative to the ether is doomed to failure.

Lodge himself, however, oddly enough, could never wholeheartedly accept this standpoint. He always wished to retain the earlier and more matter-of-fact point of view, insisting on an ether with definite quasi-material properties. It was in fact curious to contrast his rather materialistic point of view about ether with his anti-materialistic point of view about other things, and notably about the subjects with which our Society is mainly concerned.

Lodge attempted to relieve the difficulties attending the conception of an afterlife without a material basis by pointing out that the processes of the physical world are largely occurring in what may be called at pleasure *ether* or simply *space*. For example, take the transmission of light from the sun to a terrestrial observer who may be watching solar phenomena. The sun is material, and sends out light. The observer's eye is material, and receives it. But the transmission takes 8 minutes, and is going on in the intervening space. Nothing can be found out about what is going on without introducing a material receiver, such as the eye, a photographic plate, or a thermopile. His point was that the material receiver gets its inspiration from what goes on in the space outside, and he suggested that the material human organism plays a similar part of receiving and making apparent what is going on in a non-material, *i.e.* spiritual world. No doubt the attempt to analyse what is going on in the empty space of physics has proved very difficult, and opinion about it has shifted. But, as it seems to the present writer, Lodge did not attempt to define in detail the processes going on in the spiritual world either ; and the rather vague analogy which he suggested will stand equally well whether we attempt to define the properties of the intervening space in terms of an " ether " or whether, as is now more customary, we avoid the word. He did not in fact appeal to any supposed property of the ether, such as, for example, the very high density which he at one time attributed to it, in his suggested analogy.

The above seems to the writer to express the gist of Lodge's views. But they have been quite freely restated above, and it is possible that he would not have accepted the restatement which is here offered.

With the advent of broadcasting, Lodge found himself in touch, very close touch, with an audience even larger than his books had reached. The appeal of his thought was reinforced by the charm of his voice and manner. Few who heard it are likely to forget the emotion in his voice when he closed his contribution to a series of talks which members of the Society had given over the wireless in 1934, with these words: "I am grateful to the authorities of the B.B.C. for allowing me to express my mature convictions unhindered in what may possibly prove my last talk to you. If it should happen that my work down here is done, or nearly done, let me take an affectionate farewell."

But for the Society the day of farewell was still far distant. Until the date of his death he remained a member of the Council of the S.P.R. and also of the Committee of Reference and Publication, and except when prevented by ill-health was very active in both capacities.

Lord Balfour writes :

To know Lodge was to love him : to know him better was to love him better. I have some right to speak, for my acquaintance with him, begun more than forty years ago, ripened into close friendship from the time when we became associated together in the investigation of Mrs Willett's mediumship.

In what did this quality of loveableness consist? Perhaps little or nothing would be gained by trying to resolve it into its elements. Spiritually, and physically, everything about him was beautiful. But in the forefront I should be inclined to place a certain grand simplicity, adorable in a man so richly endowed by nature, and so successful in achievement.

Lodge's share in the practical research work of the Society was mainly, if not wholly concerned with his sittings with mediums, notably with Mrs Piper, and Mrs Leonard. The simplicity, of which I have spoken, was undoubtedly an asset to him as a sitter, by creating that atmosphere of sympathy which seems to be essential if the best results are to be obtained. It would not have been surprising if at the same time it had tended to detract from the cautious attitude which the investigation of mediumship calls for. But I do not think this happened in Lodge's case. He was fully alive to the pitfalls which beset the inquiry, and was a most conscientious recorder. If certain passages in his records were calculated to raise a smile, or even to excite ridicule, this is not to be ascribed to guileless naïveté but to a scrupulous integrity which forbade any tampering with the records. What I most admire in him was his intellectual honesty, and his courage in standing fast by what he believed to be truth. Lodge was the one eminent man of science who wholeheartedly accepted the evidence for survival and the possibility of communication with the departed. He devoted his remarkable gift of exposition to missionary work on behalf of his convictions, and his scientific reputation probably suffered in consequence. If so, that is all the more reason why members of the S.P.R. should honour his memory. If the convictions which he so valiantly championed continue to make headway, as I believe they will, great will be his vindication. In any case, his name must always rank in the annals of the Society with those of Myers, Gurney, Mrs Verrall and the Sidgwicks.

Miss Nea Walker, who for several years had special opportunities of seeing Oliver Lodge at work and in his home life, has sent us the following memories :

I have been asked to write a few personal reminiscences of Sir Oliver Lodge. In trying to do this I have found it impossible to give the kind of picture of him which I should like to have, were I the reader, without appearing in it myself. And I therefore ask forgiveness in advance for the inevitable amount of this intrusion.

Sir Oliver chose me during the summer of 1915 to replace another woman secretary who was getting married, and my work was to begin in October of that year. I well remember the, to me, terrifying interview before my appointment, when he asked me whether I " spoke French, German, Italian, and Spanish? " I could only claim the first two. Then,—" What about Greek and Latin? " The Greek alphabet, and a very shaky acquaintance with Latin, were all I could muster. " Humph,—well, how about Physics and Mathematics? " These were *nil.* " What do you know about psychical research? " I could not possibly say that I had only heard of " Sir Oliver Lodge and his Spooks ". So there was an embarrassed pause. Then,—" Ever heard of Myers, Gurney, Sidgwick? " I had not. " Dear, dear, they are household words here. Do you object to psychical research? " I could not say,— I knew nothing about it. (Sigh) :—" Well, let's go and have tea ", and I was led from the summerhouse at Mariemont, through the lofty book-lined study, to the dining-room with its long table seating ten or fifteen people comfortably, and fairly regularly. This tea-time was to be, for the next five years, a very familiar and pleasant scene : Lady Lodge's chair was at the end of the table. Sir Oliver sat on her left ; any guests near them ; then as many of " The Family " as might be in ; and near the doors leading to the study and the hall respectively, always room for Mr Briscoe (Sir Oliver's University Secretary) and me. Mr Briscoe did not always achieve tea ; but I found myself very ready when the five o'clock gong went. And, though one sat, as it were, " below the salt ", that being conveniently near the door by which one had entered, Sir Oliver had a charming way, if a distinguished guest were present, of remarking,—"That is Miss Nea Walker, she knows your wife",—or some equivalent description, mostly based upon a very flimsy link. Often one would much rather not have been noticed in one's distant corner, but the kindly wish to put at ease was none the less appreciated. And, frequently, the interesting conversation tempted me to linger listening when I ought to have been back at work in my room. Thought for employees did not end with tea, for we all had a glass of hot milk at eleven; and Sir Oliver would often, during the 1914-18 war, ask me if I were "sure I was getting enough to eat at home?" He knew that I lived on my salary, and I wonder now if he was remembering his own early hardships.

Very occasionally, if tired or worried, he might answer irritably at tea-time about some trifle, and immediately there would be an expression in his eyes so like that of a St. Bernard who has been rude to some smaller friend. I cannot think that he ever let the sun go down upon his wrath in things large or small. His attitude towards anything frail was always gentle, and it was an unforgettable picture to see him carry a baby grandchild firmly cradled in the crook of one arm—the baby so obviously feeling comfortable and safe that it did not cry.

Mr Briscoe worked in Sir Oliver's Study, but I had a room near the front door to which Sir Oliver came when he had time for what was the lighter part of his work. I often did not see him of a morning; he would come in after tea and dictate for a couple of hours; or, I should find a note on my table after lunch saying,—"N. W., come to the summerhouse". These entries and summonses used, for the first six months or so, to cause me to shake at the knees each time: it must have been due to his great height and big voice, for I later found that there was nothing to fear, only much to love.

I sometimes, in early days, incurred his temporary wrath by not understanding that two minutes after he had left me, he had switched his mind on to another subject, and that, if approached with a supplementary inquiry about the previous one, he did not know what I was talking about and, by the time I had begun to remind him, was quite put off his track in the new compartment of his brain to which he had switched over. Exit a crestfallen secretary leaving an irritated chief. This ability to work in different and, as it were, watertight compartments of the brain must have contributed to his ability to undertake such an enormous amount of varied work.

I had been nervously expecting to take up my work with Sir Oliver on the first of October 1915, but was alarmed by a telegram asking me to come on the afternoon of September 29th to Mariemont. On arrival I was asked to " go to the summerhouse with my shorthand notebook ", and there, without further preliminaries, Sir Oliver began to dictate. It proved to be a report of his first anonymous sitting with Mrs Leonard which had taken place on the 27th. I was bewildered. I had never done any work like it, and went home wondering whether I had been dreaming or was " going mad ". The next day I tried to transcribe my notes, and left the effort on Sir Oliver's desk, to find it the following morning on mine, scored and marked and corrected in every direction. I thought,—" That's the end of this post for me! " and waited for the wrath to come. But, to my amazement, when I shakily apologised for having evidently made a hopeless muddle of it, Sir Oliver said,—" Oh, but I made the muddle of your typing—it's only that I don't yet know myself how I want it set out." So we settled down to work out the method, and the rest of that winter went mostly in preparing the book " Raymond ". It was at this time that I learnt always to give prominence to the weaknesses of evidence laboriously gathered, and to record at once every tiny item whether it seemed relevant or not. Also to regard oneself as suspect, and, whenever feasible, to get outside testimony to the truth of one's statements.

Then came the publication of " Raymond ", and the terrific correspondence which resulted, from opponents, from sympathisers, and from people bereaved by the war. All this correspondence fell to my lot, except what Sir Oliver sent on to Mr J. Arthur Hill, after always answering the first letter himself. And both J. A. H. and I had as much work as we could manage : the files in my room grew and grew.

Sir Oliver was working hard in all kinds of other directions, at the University, and on research connected with the war ; often in summer when I arrived at nine o'clock, he would say proudly that *he* had been " doing mathematics " in the summerhouse since seven. And I know that his day often ended with reading aloud to The Family for a couple of hours after an eight o'clock dinner. But he always made time to answer, almost by return, the bereaved people who wrote to him. Sometimes he sat in my room, sometimes, even in winter, in the summerhouse, clad in a large fawnish check Inverness coat with a hood, though he always wore a cap, or a panama hat. Frequently he nursed one or other of the Mariemont Persian cats. I remember his saying one day that he wondered whether the prevailing method of destroying domestic animals by using chloroform was as kind as we thought, their noses being so sensitive—" Perhaps one day we shall find it isn't." I think that the best modern veterinary practice has justified his suspicion ; they certainly seem to like to inject morphia first.

At these times he allowed himself to smoke—always a cigarette in a long holder. One could never forget his hands with their long sensitive fingers—rather like the Dürer etching, only more powerful.

The correspondence was so large that he occasionally became confused among the correspondents, and started his letter on the wrong track for some one. I had learnt not to interrupt, but he would often stop and say,—" Am I telling the truth ? " which allowed me to disentangle the particular individual for him out of a composite picture he had evolved for himself. But this was rare, and only the slightest reminder was necessary.

When he had been up to London for S.P.R. Meetings he would immediately after his return dictate a full account to J. A. H., or to Lady Lodge, if she were away from home. And sometimes he would stop and tell me tales of the old days at the S.P.R., elucidating a reference which he knew I should not understand. Thus, quite quickly, Myers, Gurney, Sidgwick, Richet, Mrs Verrall, Eusapia, Mrs Piper, and many other names became household words to me also ; and I now feel astonishment when I realise that, as to me in 1915, they still have no meaning for the many. At these times I learnt how deep was his affection for Mr Myers ; how much he felt Mrs Verrall's death and admired her courage ; and how he regarded, almost with awe, Professor Sidgwick's " shining integrity ".

Sir Oliver's desk left much to be desired from a secretarial point of view, as also from the point of view of a faithful parlourmaid whose duty it was to dust the Study. There Sir Oliver worked " in geological strata ", and it was a miracle how he found what he required. But nothing important went astray.

His handwriting was well described by Lady Grey as " the spider out of the ink ". He wrote extremely fast, being able to keep pace when taking longhand notes at sittings with mediums, recording at the same time anything he might say himself—a feat, as any shorthand-writer who has worked with mediums will know. It looked illegible but was not really so when one was accustomed to it ; and I often found that, when he himself was puzzled by a word, I could read it by turning it upside down.

He always walked to and from the University, and, in addition, used to try and get some exercise without going far afield ; this most conveniently took the form of sawing wood, and Lady Lodge was, I fancy, often anxious lest some decorative tree in the Mariemont garden fell a victim to his need.

In 1919 came Sir Oliver's retirement from the Principalship of Birmingham University, and his Lecture Tour in the United States where Lady Lodge accompanied him. There they made many friends, one of whom at least was closely in touch with him to the end. I had commitments in Birmingham then, and could not leave to settle with him in Wiltshire when he decided to go there. So, rather than let me go back to what he called " the *status quo ante* ", by accepting another clerical post in Birmingham, he decided to carry on his work with me by post. A sudden illness of mine in 1921 led to the " temporary " appointment of a resident secretary at Normanton, Miss Helen Alvey, who was with him when he died on his wedding day, August 22, 1940. And my work from then on evolved into dealing with his bereaved correspondents, and, while trying to help them, carrying out experiments which might be useful in psychical research. This work he generously financed, though it helped him no whit. I often wonder whether those whom he helped had the faintest idea of his generosity to them, for he was never a rich man. I remember being troubled frequently because what I was doing was not helping *him* ; but his answer invariably was : " If you are doing psychical research work of any sort, you are doing my work." And he always allowed one to express one's own point of view, whether it agreed with his or not. He had a knack of acquiring helpers whom he was loth to drop, fortunately for them, even after they had ceased to function in the way he had at first intended.

A last example of kindness occurs to me. On his return from his American Lecture Tour, and before it was decided that I was to continue working with him, I found on my desk an envelope inscribed in his own handwriting :

" N. W.

I have been giving a little present to each of my daughters. So I hope you will include yourself in that category for the present.

OLIVER LODGE."

Inside was a cheque for twenty-five pounds! I still keep that envelope, and have never had cause to feel that I was outside that " category ", even though I saw much less of him in the subsequent years.

There are memory pictures of him at Normanton too, where I am glad to have stayed. But, though I have a vivid picture of a real home in lovely surroundings, and of evenings spent listening to his inimitable play-reading, there are others better fitted to describe that part of his life and work which still went on. One of those closely associated in my mind in the picture of his declining years is his chauffeur-valet Walker. I have rarely seen anything as beautiful as the way Sir Oliver depended upon Walker, and the unobtrusiveness of any necessary help Walker gave. The friendship, and mutual respect and affection, were so obvious. One can most safely judge a man or woman by their treatment of subordinates, and I think that no one who worked for Sir Oliver Lodge could fail to love him for that alone. His body is being buried at Wilsford as I write this small account: I chose the time, thinking it would perhaps bring back the kind of memories in which his other admirers might like to share.

N. W.

22 *August* 1940.

PSYCHOPATHOLOGICAL ASPECTS OF TELEPATHY

By Hans Ehrenwald, M.D. (Prague).

So-called supernormal phenomena—if they are discussed seriously at all—are a foreign substance in the body scientific. Academic science even to-day emphatically denies their existence or even their possibility. Yet the investigations of the last decades—not only in the line of psychical research but also of clinical psychiatry and deep-psychology—have gathered a great deal of evidence for the occasional manifestation of extraordinary phenomena in certain " border-line " cases, in hysterical patients and in persons in mediumistic trance. To cite exceptional performances during sleep and dream, in the psycho-analytical situation, in states of split personality, etc., has become well-nigh a commonplace.

Not long ago, however, the rationalistic trend of psychopathology was reluctant to acknowledge even the reliability of observations of such kinds. Henry Maudsley, one of the pioneers of English psychiatry, insisted on ascribing all these reports to defective observation and fallacious interpretation, or even to delusive self-deception on the part of the observers. The same objection has been put forward ever since over and over again—and we must admit, often with good reason—by numerous authors. The criticism of Millais Culpin,[1] about twenty years ago, made at least one concession. He pointed to the rôle of Janet's mental dissociation in causing the alleged supernormal phenomena. This state facilitates, in his view, an incidental hyper-sensitiveness of the sensory functions. He refers to the well-known observations of Binet, A. Hurst, etc., on hyperaesthesia of hearing and of touch, amounting to 16-50 times the sharpness of that of the normal individual. Supernormal phenomena, particularly telepathic performances, are based, on this view, mainly on uncritical interpretation of hyper-functions of the sense organs, such as muscle reading, listening to " unconscious whispering ", etc.

Yet these hyper-functions, however striking they may appear, do not suffice to explain the occurrence of real " supernormal " phenomena, in cases where we can accept their existence as estab-

[1] Millais Culpin, *Spiritualism and the New Psychology.* London, 1920.

lished. A quasi-physiological working hypothesis of this kind seems to be, nevertheless, an expedient starting point for the approach attempted here. It seems to be advisable to make a particular instance of this group of hyper-functions the object of our closer consideration. Experimental psychology has repeatedly dealt with the extraordinary faculty of an accurate time-appreciation during sleep and hypnosis. Such observations have been reported by Bernheim, Forel and more recently by Milne Bramwell, T. W. Mitchell, L. D. Boring, E. N. Brush and others. The present writer's experiments in the Prague Psychiatric Clinics from the year 1923[1] may also be mentioned here. These attempted to verify the observations in question by means of experiments of unconscious time-estimation during hypnosis. In these tests the subjects were found to be able to perceive times vaguely from some minutes up to several hours with considerable exactness, the error scarcely exceeding from two to five per cent. Moreover certain individuals were found to commit errors of a constant over- or under-estimation of the times tested in the form of a constant quotient. In this way they resulted in giving a significant demonstration of the exactness of this group of "unconscious" performances, such as could never be achieved by the waking consciousness. There are moreover a number of clinical observations which indicate the preservation of similar faculties, even where there is a marked impairment of corresponding conscious mental functions. For instance, a further series of experiments with hypnosis has ascertained the function of an unconscious, "primitive" time-sense in patients with Korsakoff's disease, although the complete lack of temporal orientation is one of the main features of this syndrome. Experiences in certain patients with various aphasic and kindred disorders caused by more or less localised bilateral cerebral lesions gave similar results.[2]

Thus in these cases hypnosis or more or less pronounced organic lesion has led to what might here also be called a "dissociation" of mental function, and a hyperactivity of a particular faculty, unknown in the normal individual in the waking state, was attained in these conditions too. We can even say that this hyper-function seems to be particularly marked in the condition of a reduced mental level of an "abbaissement général du niveau mental" as Janet puts it. The time-estimation of modern man represents a deteriorated atavistic function, and it seems to be just the relapse to a primitive functional level during sleep, hypnosis or an organic

[1] "Experiments on Unconscious Time-Appreciation". *Archiv. f. d. Ges. Psychologie.* 1923.

[2] "The Problem of 'Time-Sense'" (*Klinische Wochenschrift*). 1931.

lesion of the brain which leads to the re-emergence of a forfeited faculty, usually inaccessible to the waking state. We know that analogous faculties seem to be preserved in certain reactions of the infant, and in the habits of social insects, particularly bees. The striking performances of spatial orientation in migrant birds are proverbial, and Professor Culpin refers to analogous powers of orientation in savages as an example of hyperaesthesia in the sense mentioned above.

All these observations deserve more than a mere accidental interest. They show that the fundamental biological principle of compensation and over-compensation holds true also in regard to the mental activities of man. It is undoubtedly a lasting merit of Alfred Adler, the founder of Individual Psychology, to have made this principle the chief tenet of his doctrine, even if it has not been entirely a matter of recent psychological discovery. Homer, the blind seer, Demosthenes, the stammering orator, have exemplified the same principle among the ancients, and the case of Helen Keller, and some facts revealed by modern sense-physiology, as for instance the marked refinement of the cutaneous sensibility in congenital blindness, etc., all point in the same direction.

A corresponding observation of the present writer,[1] concerning the conditional improvement of the faculty of " reading " by the skin in a case of word-blindness due to an arterio-sclerotic lesion of the brain, gives a further instance of these correlations. Any temporary impairment of the patient's residual reading ability (it extended to a few meaningless letters only) was linked with a temporarily marked improvement of his " skin-reading ", and vice-versa any improvement of his visual functions led to a transient reduction of his " skin-reading " ability. In this case I realised that these alternative processes were a sign of a counterbalancing transmission of central energies from one kind of mental activity to another, the impairment of the first appearing to set free a certain amount of energy, thus improving the second, and the improvement of the second consuming an adequate amount of energy, thus impairing the first.

The observations made in this case are significant for our problem mainly as a further example of the suggested compensatory correlations in the field of the pathology of the brain. But they reveal at the same time a remarkable counterpart of another observation, leading far beyond the sphere of any merely biological compensatory mechanism, into the sphere of alleged supernormal phenomena.

[1] " Observations in a Case of Word-blindness " (*Zschr. f. d. Ges. Neur. u. Psychiatrie*). 1930.

The ease was examined by the well-known Professor of Forensic Medicine in Riga, F. von Neureiter,[1] and his record is based on thorough observation both medical and psychological. As regards the thoroughness and comprehensiveness of these preliminary examinations the ease seems to be unique of its kind and therefore particularly apt for the present purpose. The experimenter seems to have observed every precautionary measure desirable against any possible deception or self-deception. Moreover, it is to be emphasised that the subject was more or less an imbecile child so that any fraudulent intention, any possibility of collusion eould be ruled out at the outset. A great part of the examinations took place in the absence of the child's relatives in Professor Neureiter's institute in Riga, and their authenticity was vouched for by several members of the Riga University.

As far as I know his small booklet of 56 pages has remained little known to the English public and the great theoretical interest of the facts revealed has been disregarded both by orthodox seience and psyehical research. Thus a short epitomé in these pages of Professor Neureiter's account seems to be justifiable. It was in his capacity as a medical authority that Dr F. Kleinberger, a medical practitioner in Trapene, Lithuania, appealed to him in a letter, part of which may be reproduced here in my own translation.

" . . . As I assume that you are interested in things of that kind, I should like to report to you a case of outstanding interest, as far as I can see. . . . It is the case of a nine-year-old girl. Both parents are alive and healthy, they are smallholders. The mother, whom I met, makes a thoroughly intelligent impression. No mental disorders or other abnormalities in the rest of the family or in the ascendants, as far as I can understand. There are two more children, an elder and a younger one, both normal. The child in question was normally born, and developed, according to her mother, in a normal way. She played with other children normally, etc., she was rather lively and sociable. The only thing whieh struck those who knew her was that she remained backward in speaking. When 7-8 years of age she expressed herself in the manner of a child of 2. At the age of 8 she eventually managed to speak so far as to be able to attend an elementary school. Initially, as long as she had to learn only disjointed letters, she was able to follow the tuition. Later on, however, when reading syllables was to be taken up, the teacher was startled by her complete incapacity to read even the simplest context. That was the ease when she was sitting in the classroom, on her form. But if the teacher were standing near the girl, reading

[1] *Wissen um Fremdes Wissen*, etc. Gotha, 1935.

gently or only in *thought* to himself, the child " read " without mistake, a text which she had never learned to read before, or even any text required, and in any foreign language, though she knew only Lithuanian."

Dr Kleinberg adds that although the child's performances in arithmetic were similarly poor, they became equally amazing when the calculation in question was made by the teacher (or by her mother) in *thought*. The latter also complained that she was unable to hide anything from the child—Ilga knew at once where the object concerned was hidden. Dr Kleinberg, also, had performed a few tests with Ilga which fully confirmed the information obtained.

Professor Neureiter, after some hesitation, consented to see the child in Riga. The first session took place in his consulting room with Ilga's mother acting as agent. Ilga was sitting with her back turned towards her mother or behind closed doors in a neighbouring room. These preliminary observations confirmed the previous reports in every respect. Yet it was only the vivid interest of Professor Neureiter's colleagues at the University of Riga and an encouraging letter from Professor Driesch of Leipzig that induced him to take up the investigations in a more systematic way. The time available for that purpose was somewhat short. Ilga's home village was rather far away from Riga and her mother could not spare more than two days' absence from her home. The second session took place next morning, on May 22nd, 1935, with several members of the University present, the third session on the same evening with still more witnesses participating. To begin with, the results were disappointing. The tests proved after a few lucky hits, a complete failure, leading eventually to Ilga's starting to cry bitterly. Yet after the child had had a little rest and a meal, the atmosphere became somewhat more cheerful, and Ilga succeeded in producing several of her usual performances. She did still better in the third session, after Professor Neureiter had ceased to carry out the tests in presence of all the invited witnesses, and instead allowed only a couple of them to participate at any one time.

All these observations, however, did not fully satisfy Professor Neureiter. He therefore decided to see Ilga in her home village. Here he resumed his investigations with his colleague, Professor Amsler, the pharmacologist of the University of Riga, and Dr Kleinberger present. The observations gathered on that occasion were so convincing that he could no longer refrain from acknowledging that the phenomena were genuine. Prof. Neureiter points to the following arguments in favour of this opinion: (1) The subjeet was a feeble-minded ehild whose redueed mental eapaeity neeessarily ruled out the possibility of using some intrieate code, that is, the possibility of eollusion. On the other hand, the faets to be established required no intrieate means of observation. They eould be elieited and eheeked in the most simple way. (2) The possibility of deeeption eould be ruled out by plaeing the agent and the percipient with their baeks turned to one another, when sitting in the same room, or separated by a curtain. In some of the later experiments they were sitting in two separate rooms. (3) Particular eare was taken to notiee and avoid the slightest sign of involuntary whispering on the part of the agent. Lip reading was exeluded beforehand by the measures mentioned above ; Professor Neureiter and his substitutes, when acting as senders, took speeial eare to avoid giving away elues of any sueh kind. (4) Professor Neureiter eonsiders that the faet that any attempt at intentional and deliberate transmission of " thoughts " failed eompletely was a further argument in favour of the genuineness of the phenomena. The best results were to be obtained when the agents refrained from any sueh intentional behaviour, *e.g.* when the ehild's attention was diverted from the test by her being absorbed in play.

The number of tests performed altogether amounted to 42, all of which have been thoroughly reeorded. A few are reprodueed below.

Test 9. One of the partieipants, Professor Brüekmann, writes this ealculation on a sheet of paper :

$$4.4+5.5=41$$

and hands it over to Ilga's mother who was playing the part of agent. She turns to Professor Neureiter stating that she did not understand the task. Professor Neureiter is just about to explain to her that the points mean signs of multiplieation, when the child, unexpeetedly, utters the number 41. Her mother, obviously, following Professor Neureiter's explanations, was just eonsidering the result of the ealeulation and the transmission took plaee in that very moment.

Test 12. Professor Amsler writes in the adjoining room a list of words and figures on a sheet of paper. Then he enters the room where, behind a curtain, the mother is situated, while the rest of those present stay with the child in front of the curtain. Ilga, without being asked to do so and without interrupting her play, reproduces the whole list without the slightest mistake.

Task :	ger, til, tli,		Solution :	ger, til, tli,
	123, 213, 312.			123, 213, 312.

Her voice differs on these occasions from her normal way of speaking. She pronounces each syllable more distinctly, in a somewhat exaggerated manner. In another test she reproduces the figure 42 as 12. From the attached reproduction of Professor Neureiter's handwriting the cause of this mistake is clearly to be seen : her mother mistook the 1 for a 4, thus revealing unmistakably her exclusive rôle as an agent in the instance in question.

We have already emphasised that in the beginning experiments with persons, other than her mother, acting as agents, proved failures. Yet the following test gives a very instructive example of such an initial failure and subsequent success.

Test 38. " I (Professor Neureiter) take the place of the agent and try to transmit to the child the figures 9 and 2 ; then a sentence, printed in the child's Lithuanian primer : *Mate gaja uz kleti.* I concentrate with exertion upon my task, sharply accentuating every syllable in thought . . . yet in spite of all effort the percipient remains silent. I am just about to close the book discontentedly and to break up the experiment when my look is caught by the word *Brute* (=bride) in the context of the Lithuanian poem, the first words of which I was trying in vain to transmit. And at that very moment the child, situated in the adjoining room, uttered the word. Thus this was the way by which the reception happened to work perfectly, although—or better because—I refrained from volitional sending. At the moment when I perceived the word *Brute* I wondered why it was used in a modern Lithuanian first spelling book. It was taken from the German *Braut*, thus it was a foreign word, and, moreover, obsolete. There was instead, the genuine Lithuanian *Ligava* available. Considering all that, I did not, in any case, think any longer of my original task. . . ."

Professor Neureiter adds that this whole chain of reasoning did not take place in any formulated way of thinking. Accordingly only the word that gave rise to his reflections became transmitted to the child. From that he draws the conclusion that it is not the mental contents pure and simple which are liable to telepathic transmission in his case but exclusively *words* formed in the " inner language " of the agent. Yet this activity of these formulated thoughts is obviously dependent on their remaining in the periphery of consciousness,—in what W. James has called the *fringe* of the mind. The more the agent's attention is attracted by any other mental contents, the better are the conditions for their acting upon the mind of the percipient.

This is illustrated also by the excellent success of the experiments performed with Ilga's brother, Victor, a boy 6 years of age, as an agent. He transmitted to his sister simple words read and the names of pictures shown to him without her committing the slightest mistake. In Professor Neureiter's view this success is to be attributed to the fully candid, unintentional attitude of the boy towards his task, in contradistinction to the often spasmodic efforts of the adult.

Ilga's restriction to reproducing only the literal wording of thoughts to be transmitted is shown also by the following test. She is playing with a picture book, while her mother, in the adjoining room is told to " send " her the order to put the book away. At the same moment the child produces the sentence. Yet she keeps on playing, without the slightest attention to the order received. Another significant clue was given by the following

Test 20. Dr Kleinberger hides his watch under one of the many cushions in the room. Ilga stays in the meantime with her mother in the adjoining room. Whilst the mother is informed of the hiding place the child enters the room saying at once : " *The watch is under the cushion !* " Yet she is far from being able to get hold of it at the first attempt. She finds it only after having inspected one cushion after the other.

However, the most striking of her performances were in " reading ". She managed to read any test offered to her mother, be it in Lithuanian, German, French, English or Latin. The mistakes she committed corresponded in every respect with those appropriate to her mother's degree of education. For instance she spelled numbers she came across in the French context in Lithuanian language, she pronounced the French words phonetically in the Lithuanian way, etc.

Professor Neureiter did not restrict himself to these and similar experimental findings. He undertook a thorough physical and psychological examination both of the child and her parents and enquiry into the family history. Here he gathered that a brother of Ilga's mother was reported to be feeble-minded. Yet both parents were found normal in every respect. The father's attitude towards Ilga's extraordinary faculties was rather reserved, whilst her mother—more impulsive and ambitious by nature—appeared to appreciate them somewhat more. Ilga's brother Victor is a well-developed, intelligent boy, his sister Velta, 14 years, somewhat less gifted, pale and thin. She is backward particularly in arithmetic and looks embarrassed towards strangers.

Ilga, 10 years and 4 months at the time of examination, had a normal upbringing ; no childish ailments, no fits were reported. Yet she was backward in the control of her sphincters. Her weakest point, however, was in speaking ; she was hardly to be understood even by her family up to her eighth year of age. Then she showed a marked improvement and was able to attend school from autumn 1934 onwards. There she learnt writing fairly well, as can be seen from a specimen of her handwriting in *fig.* 1. Yet she remained definitely incapable of learning to read more than a few meaningless letters, as already intimated in Dr Kleinberg's letter. Her " supernormal " abilities became conspicuous about that time, yet on retrospection her mother recalled that as long as two or three years ago she became aware that she was hardly able to hide objects from her daughter.

Ilga's photograph as reproduced here (*fig.* 2) shows a child of peasant-like appearance, with a comparatively big skull and somewhat dull facial expression. Her physical condition, particularly the central nervous system, was found completely normal, as were the ocular fundi, her sight, hearing, etc. Her speech is described as somewhat unwieldy, the phonation of the consonants *r* and *s* deficient. Her manual skill and dexterity are fully adequate ; she is lively and sociable, good tempered and complaisant. She answers questions readily, yet in short and primitive sentences. She is hardly likely to take the initiative in conversation or otherwise, her movements are rather clumsy.

Two of Professor Neureiter's collaborators, psychological experts, accomplished the child's examination by testing her by means of the Binet-Bobertag and Bühler-Hetzer methods. These tests resulted in an IQ of 0.42. Her best performances were in various bodily actions, in dealing with materials ; the lowest were in speaking, which was found to correspond to a mental age of a child of between 3 and 4 years. (Neurologists would call her mode of speaking a " developmental aphasia of agrammatical type ").

These findings led Professor Neureiter to diagnose Ilga K. as a case of feeble-mindedness of considerable degree.

So much for his records of the case, the importance of which need not be further emphasised. It is one of the rare thoroughly investigated and well confirmed cases of telepathy, examined under favourable circumstances by a trustworthy and critical observer. Yet there may be added a few more remarks which might possibly accentuate still more the interest of the case.

In a paper [1] dealing with Neureiter's publication from a medico-psychological point of view I ventured to show that the classification of Ilga's case as a common form of feeble-mindedness needs some

[1] " Brain-pathological Remarks on a Case of ' Supernormal Phenomena ' " (*Zeitschrift f. d. Gesamte Neurologie u. Psychiatrie*). 1937.

PLATE I

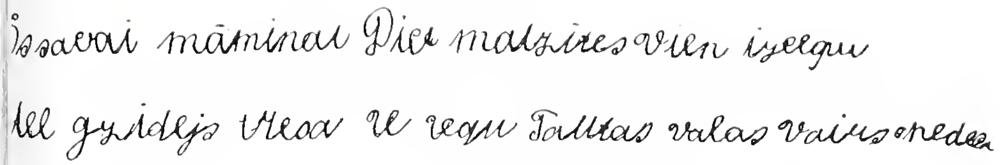


FIG. 1
Specimen of Ilga's handwriting. The Lithuanian text is worded: Es savai māminai Diy' maizītes vieu izcepu; Vēl gribēju trešo cept, Tautas valas vairs nedeva.
Neureiter: *Knowledge about Foreign Knowledge.*

FIG. 2
Ilga K.

correction. In such persons all intellectual capacities are usually found to be equally defective—although it is well known that one or another mental function may happen to be found developed to an extraordinary degree. In Ilga's case, in contrast, there was present a more or less circumscribed intellectual defect, with no marked impairment of the rest of her mental functions. Her complete inability to read, in contrast with her preserved faculty of writing, completely dominated her picture—at least from the neurological point of view.

Professor Neureiter's report points particularly to her striking inability to compose disjointed letters which she succeeded in deciphering into syllables and words. She was not even able to read her own handwriting. On the other hand this handwriting is fairly good, at least as far as the flow of writing and the forming of letters is concerned. It is at any rate little worse than might be expected from her age and the extent of her schooling. On closer scrutiny, however, there are to be found tendencies to distort and reverse letters, which remind the expert of mistakes characteristic of the writing of patients with lesions of their brains in the left parietal lobe. Also what we have gathered from Professor Neureiter's report about her speech (her slight aphasic disorders) points to the same direction.

These clues, taken together, lead to the classification of the described disorder as a case of so-called *congenital word-blindness*. That is a particular neurological syndrome, as first described by Déjérine, and specially by Pringle-Morgan, Hinshelwood, and others. Patients of this group, although their faculty of visual recognition is scarcely impaired, show just the very defect found in the child. Both acquired and congenital forms of word-blindness have become known, and Ilga K. obviously represents nothing else than a case of the latter group. Thus we must realise that the case which, by the routine way of testing had appeared a simple instance of imbecility, is in fact to be regarded as a more or less circumscribed intellectual, that is to say, cerebral defect of congenital origin.

This suggestion, however, serves to open up a new aspect of Professor Neureiter's observations. The development of Ilga's "supernormal" faculties—her performances in telepathic *reading*—in conjunction with her overriding intellectual defect, seems to be more than mere coincidence.

This diagnostic classification of Professor Neureiter's case attracts attention to the striking fact that here again the child's most outstanding "supernormal" performances seemed to correspond with just that order of functions which proved to be impaired by her overriding congenital defect. On the one hand a more or less circumscribed minus-function was present, and, on the other, a particular " supernormal " function which can, by analogy to the previous cases, be best realised as the product of some unusual compensatory tendency of the affected organism. Even the apparent contradictions to this suggestion seem to point in the same direction. For instance—besides her predominant word-blindness—there were observed some less conspicuous aphasic disturbances, as mentioned above. Accordingly the child's " supernormal " performances in the wider domain of " thought-reading ", in the finding out of hidden objects without the mediation of the written word, remained less conspicuous. Her dexterity in this respect seemed to be dependent on how far the tasks were expressly formulated in the experimenter's inner language, so to speak. Thus the suggested correspondence between the minus-function on the one hand, and the tendency for its compensation in some " supernormal " way on the other, seems to be indicated here as well, although to a slighter degree, in as much as the child's speech-disorders were less pronounced in comparison with her prevailing difficulty in reading.

So far this case is yet another illustration of the general biological law of a compensatory tendency in any defective organic structure or mental function, as claimed particularly by Individual Psychology. There is, however, a very great discrepancy, if not a gulf, between the previous cases and the one under discussion. In the case of dreams, of hypnosis and related states, there are to be observed effects which come within the scope of recognised medical and psychopathological experience, even when they happen to exceed the usual ability of the subject's faculties. In the case of Ilga K. the problem is different. There is no doubt that the child's obvious mental insufficiency, her circumscribed intellectual defect, falls within the field of medical psychology. The compensatory reaction claimed for it leads on the other hand into a world where there is a complete lack of evidence for any comprehensible organic basis for the alleged supernormal abilities. This difficulty, it is true, cannot be eliminated by the present account. Yet that does not absolve the scientist from his obligation to deal with it similarly to other problems met with in the course of his experience.

But even in this incomplete stage of understanding, new light seems to be thrown upon certain conditions required for the origin of the child's " supernormal " manifestations. One essential condition seems to be the presence of a more or less circumscribed mental defect, combined with some more general lowering of mental activity. On the other hand there seems to be required the presence of what we might call the reactive powers of the organism, which could hardly be defined more exactly without recourse to philosophical speculation. The comparison of the case of alexia due to cerebral sclerosis with the case of congenital origin illustrates the problem in a very instructive manner. In the first instance the compensatory mechanism involved an alternative weakening or strengthening of either tactile or optical central activities. In the second example also, this alternative interaction seems to involve a source of energy made accessible by the given cerebral defect. Yet these sources are to be located, obviously, in deeper strata of the subject's mental activity. It is a matter of convenience to consider the unconscious as being the matrix of these energies. Further speculations about their mode of action seem to be premature for the time being. The main point is that such a conception seems to facilitate the first approach to the problem of alleged telepathic phenomena from a " normal ", that is to say, a biological point of view throughout. At the same time it will prove an expedient working hypothesis for the approach to further psychopathological problems, as will be shown later on.

However, such a working hypothesis of the origin of telepathic phenomena enables us to understand more of their conditions only so far as the part of the percipient is concerned. They throw no light upon the conditions on the part of the agent—for example, Professor Neureiter as the experimenter in the case in question. There is, however, one important remark to be made at this point in regard to his observations. At the beginning of his experiments he had great initial difficulties in verifying the phenomena. Eager to facilitate and provoke them by his own voluntary effort, he sought to concentrate on the context before him, but without success. The child remained dumb. Meanwhile, the experimenter's attention became diverted by a certain obsolete word in the passage he was reading. At that very moment the child, unexpectedly, uttered the word. We know, as a matter of fact, by a number of similar experiences that the optimal condition for producing the reactions concerned seems to be a relaxed passive mental attitude, abandoning as far as possible any purposive intention.

On the look-out for further clues so far as the psychological conditions on the part of the agent are concerned, we are confronted with a mass of similar observations, particularly in the formidable bibliography of psychical literature. It may, however, be permissible to disregard this material in this article. The present considerations propose to deal mainly with the medico-psychological aspects of telepathic phenomena. Thus it seems to be advisable to restrict substantial implications to experience in the field of medical and psychological research, contributed by medical and medico-psychological authors.

There are two remarkable observations of Sigmund Freud [1] which may be referred to here. The first case he reports is of a lady, 43 years of age, undergoing treatment for different neurotic disorders. Her psycho-analysis revealed a strong fixation to her father, which prevented her finding happiness in her marriage. She was longing for children and hoped to reawaken her love for her husband by the roundabout way of identifying him, as the father of her children, with her own father-image. As she understood that she had to abandon any hope of having children in consequence of her husband's disease, she became more deeply entangled in her neurosis. She was 27 years old when she turned to a "fortune-teller" in Paris who, taking her for unmarried, prophesied that she would have two children at the age of 32. But by the time her psycho-analysis began, this prophecy had proved manifestly wrong: the patient had remained childless, and there was less hope than before of her having children. Yet psycho-analysis resulted, nevertheless, in a surprising revelation of a certain correspondence to reality in the fortune-teller's statement. The patient's mother was just 32 years old when, within the shortest possible time, she gave birth to two children, having almost resigned hope of having children at her age. It was clear that the patient in her phantasy took the place of her mother, and the fortune-teller, by introducing into the "fortune" told to her a circumstance relating to her mother's life, hit upon the daughter's most ardent secret desire. The wish to have children governed her unconscious life for years and was certainly the original cause of her falling ill just at that time. Thus the fortune-teller's prophecy had proved wrong, it is true, but the striking fact is that it disclosed precisely and in a fully adequate manner a distinct unconscious idea in the patient's mind. It is certain that the fortune-teller had no normal means of understanding the significance to the daughter of either the reference to two children or to the age of 32.

The second observation of Professor Freud refers again to a fortune-teller's prophecy. This patient, a young man, fell ill at the time of his beloved sister's wedding. Psycho-analysis revealed here a strong fixation to his sister. Nevertheless, he encouraged the marriage in spite of the disapproval of their parents. Now, after having undergone a first analytic treatment, the young man con-

[1] *Traum und Occultismus. Neue Folge der Vorlesungen über Psycho-analyse.* 1933.

sulted a lady astrologer who "prophesied" her client's future by means of a horoscope, starting from birth-data. The young man gave her the birth-data of his brother-in-law, who it was foretold would die the next July or August from poisoning by raw fish or oysters.

Now, his brother-in-law, certainly, did not die at the predicted time. Yet he had fallen ill in the course of the past summer, before the patient consulted the astrologer, and the cause of the illness was in fact poisoning from raw fish and oysters. Analysis made it obvious that Freud's patient had not at that time overcome his unconscious hatred of the rival who had married his beloved sister. Hence his repressed wish that he might have died of the poisoning. According to Freud's conjecture he might have thought: "The inclination to eat such things is a persistent one. . . . Why should it not occur again. . . . ?" As there was no possibility of the astrologer's knowing about this insignificant detail from the life history of the brother-in-law, Freud was compelled to suggest that this intelligence was to be attributed to some "supernormal" telepathic means of communication with the inquirer's unconscious. In this way only could the fortune-teller be supposed to have revealed to her client a certain item in his brother-in-law's biography, related in a particular way to his now repressed wishes and expectations. Thus her statements, though having no value as prophecy, succeeded in revealing certain elements of his unconscious mental life which stood in close connection to the precipitating cause of his neurosis.

There is no reason to call into question the account of such an authoritative and critical investigator as Freud. There are only two possible courses, either to deny the facts—or to attempt their explanation on the basis of our available knowledge. Freud himself has considered in his cases the possibility of a particular effectiveness of repressed wishes and impulses. This activity, in his view, is wont to be emphasised by their particular stage of repression, by their obvious tendency to emerge from the unconscious into the stage of pre-consciousness.

This hypothesis, though it ignores the conditions on the part of the fortune-teller—that is, the percipient—elucidates them as they are to be realised on the part of the agent (that is of his client) in a very informative way. And it may be added that such a hypothesis leads to certain suggestions as to the problem of extra-sensory perception in general, such as are found in para-psychological and psychical literature. Similarly Helen Deutsch[1] in a paper on

[1] *Imago.* 1926.

"Occult Manifestations during Psycho-analysis", states that particularly emotionally stressed processes within her own consciousness appeared to have been "guessed" in a telepathic way by her patients. These experiences, in her view, seem to confirm Freud's hypothesis. Analogous observations have been published by Hollós, and by Dorothy Burlingham in connection with child-analyses, and others.

Freud himself has pointed to the happy conditions for such occurrences with children as subjects. An observation of this kind was made by the present writer in his own household. His wife was occupied with house-work, whilst their four and a half year old daughter Barbara was absorbed in play by her side. A few days previously his wife had answered a letter received from the U.S.A. and she was thinking about her friends over there, Clarence and his wife Matilda. Clarence is her half-cousin, with whom she had spent a happy time in America a few years before her wedding. Clarence married at about the same time as she did and the letter from America announced the birth of his second child. There was a photograph of three ladies attached to the letter, one of them being Matilda. As she did not know her personally, she wondered which of them was Clarence's wife. Just as she was thinking about this photograph, she was struck by the unexpected and apparently completely unmotivated ejaculation of the name Matilda by the child. It may be added that Barbara used to speak German at that time and that she scarcely had any opportunity of meeting with the name in any other version than "Tildi" (the name of one of her aunts). The child, asked the other day by the writer, whether she remembered having said the name replied, "Yes . . . I was having a game with mummy." But on further inquiries and reflexion she added that she remembered having heard Matilda as the name of a cat which she had seen with friends a few months ago. No further associations were to be had from her.

It is clear that this observation, again, does not exclude the possibility of a coincidence. Yet it is obvious that the odds for such an event are enormously small, particularly when we consider that from Barbara's point of view the English form of the name was unusual.

So much is known of the psychological conditions on the part of the child. They revealed no sensible reason for her unexpectedly uttering the word. It may be added here that Barbara has never shown any abnormal signs; the general mental age of a child of four or five years is the only justification for ascribing to her also a condition of "minus-function" similar to that claimed for the percipients in the foregoing examples. How far her actual mental state at the moment of transmission affected it cannot be decided here.

But her mother gave more information, saying: "No . . . I do not know Matilda. . . . I only know that they have been married for about the same time as we have. They have a child of about Barbara's age . . . and now they have got another . . . these happy Americans. . . . They can afford such a luxury. . . ." It would be redundant to continue the chain of her associations, which led to an idea whose strong emotional stress need not be particularly explained. It is sufficient to intimate that she has had to resign hope of having another child for the time being. It is clear that the frustration of this desire was likely to strengthen the emotional emphasis of her associations linked to the name of Matilda. This particular emotional emphasis is obviously to be regarded as the activating factor for the telepathic transmission of the name from mother to child.

We see that the postulate of a certain emotional stress being responsible for the activity of ideas or impulses liable to telepathic transmission holds true for the present case as well. It clearly shows that the activating forces are to be located in the unconscious or pre-conscious of the agent, as in the preceding instances.

On the other hand there are some observations of Dr William Brown [1] during the course of psychotherapy which show that it is just the attitude, as already emphasised, of a certain mental relaxation and passivity which seems to be the psychical state required for the bringing about of these phenomena. He reports the case of a patient to whom, during a session of treatment by suggestion, "the vivid picture of a page of a scientific journal appeared, with two columns in the form of a letter, signed at the bottom". On reflection Professor Brown realised that when glancing at a copy of *Nature* his eye had been caught by one of the letters printed. He handed the copy of the journal to his patient, who looked through it and found "the identical letter". Professor Brown felt compelled to agree with his patient in regarding this observation as a case of telepathy. He also reports a further analogous case where, in the course of a discussion of a Greek quotation, something like a telepathic transmission of a distorted word occurred, belonging to a jocular recollection of his school years, though there was no conscious intention of communicating it to the friend with whom he was talking.

Comparing these reports with the above-mentioned self-observation of Professor Neureiter—*viz.* the instance when he became an

[1] *Psychology and Psychotherapy.* 1934.

involuntary " agent " at the very moment when he was meditating about the meaning of an obsolete word—there seems to be an unmistakable similarity with Dr Brown's description of his own mental state during his observations. This mental state implies only seemingly a certain contradiction to the conditions suggested above as responsible for telepathic manifestations, that is a state of a certain incomplete repression or emotional emphasis of the ideas or impulses concerned. It is obvious that there is one feature, common to both apparently contradictory mental conditions, to be taken into consideration: it is the abandonment from the content of conscious ideas which makes them liable to extra-sensory communication. This condition given, they seem to be particularly susceptible of activity in some way or another, that is, of operating upon the mind of another person with a corresponding sensitiveness. Thus the required condition for telepathic manifestations on the part of the agent could be appropriately described as a lack of actual conscious control over certain thoughts or impulses—whether due to their particular emotional stress, linked up with their repression, or to an incidental state of passivity and absent-mindedness, the closer determinants of which would obviously be accessible only to deep-psychological analysis.

Considered together, both groups of sufficiently confirmed arguments for telepathic manifestations result in the following set of conditions: (1) a more or less circumscribed minus-function on the part of the percipient, linked up (2) with an adequately preserved faculty for compensatory reaction of his organism. Where these two conditions are sufficiently pronounced, no " supernormal " faculties seem to be required on the part of the agent. (As a model case of this kind we may quote the relation between Professor Neureiter and his subject, or Professor Brown and his patient.) (3) In the case of an absent or scarcely discernible minus-function on the part of the percipient, a more or less emphasised plus-function on the part of the agent seems to be necessary. That is an emotionally stressed idea or impulse as in Freud's two cases—or at least an incidental lack of conscious control of certain ideas, with their resulting unconscious activation.

The main instances given, however, represent extreme cases, seldom to be met with in practice. In the majority of spontaneous or experimental telepathic manifestations all three groups of conditions may be involved with the alternative of either one or another group of conditions prevailing. Accordingly there may exist a particular coincidence of conditions, present in both the percipient and the agent. This coincidence may represent the most favourable constellation for producing the phenomena, though, of course, it is only rarely to be found in practice. Usually we may have only one or the other of the suggested conditions, with, consequently, only an occasional occurrence of the phenomena. Further, there may exist a minus-function, yet without an appropriate reactive power of the organism—as in the case of the patient with cerebral sclerosis—moreover with no appropriate " agent " available. Finally there may arise the condition of an adequate mental or bodily reactive power and general fitness, yet without the postulated minus-function—as in the normal individual. Considering the obvious rareness of a simultaneous coincidence of all these conditions, the relative rareness of sufficiently well-attested " genuine " observations of this kind needs no further explanation. The same reflexions explain the difficulties of deliberately producing them, *e.g.* under laboratory conditions.

These considerations, it is true, suggest a reading only of the psychological conditions of the phenomena. They yield no information about the corresponding physiological processes. Yet this question seems to be in no way more transcendental than the question of the bodily correlates of " normal " psychical phenomena. But this question falls rather within the province of philosophy, and thus there is reason to deal with it in the present connection. From a merely descriptive, psychological point of view it simply confronts us with a particular issue of psycho-physical correlations, *viz.* with the fact of an immediate " extra-sensory " communication between two individuals or " minds "—as telepathy was originally defined by F. W. H. Myers. The scientist, once convinced of the integrity of the evidence, is simply compelled to acknowledge this group of experiences, to eliminate any quasi-scientific flavour as far as possible and to try to reconcile it with his previous knowledge.

Yet scarcely have the difficulties of a first scientific approach been overcome, when a further and no less considerable obstacle seems to arise. Both common sense and scientific thinking obstinately refuse to acknowledge facts of this kind. A. Carrel, in his book *Man the Unknown*, is right when he points to the fundamental tendency of our minds to reject the things that do not fit into the frame of the scientific and philosophical beliefs of our time, so that scientists are willing to declare that things which cannot be explained by current theories do not exist at all. This attitude, well known in various fields of research, is common to both the general public and the research-worker and seems to be particularly emphasised in the field of so-called supernormal phenomena. It has certainly been the most serious obstacle up to the present to their systematic exploration, and, it may be admitted here, an initial scepticism and distrust have for a long time hampered the present author in drawing the appropriate conclusions from his own occasional experiences in this direction both in everyday life and in the field of psychotherapy. But he can refer to no less an authority than Sigmund Freud, whose reluctance in acknowledging and dealing seriously with his two observations, quoted above, lasted as long as ten years " for fear of our scientific world-view being menaced by them ", as he confessed. This fear may be understood by another statement, reported of a no less distinguished scientist, the physicist Helmholtz: " Neither the testimony of all the Fellows of the Royal Society nor even the evidence of my own senses could lead me to believe in the transmission of thoughts from one person to another independently of the recognised channels of sensation. . . ." And Helmholtz has his counterpart in the late Lord Kelvin, who attributed all such accounts "to bad observation chiefly, mixed up . . . with the effects of wilful imposture."

In one of his latest papers the well-known German psychiatrist, W. Weygandt[1] has advocated a similar obstinate attitude. He cites Neureiter's case in the bibliography of his article, but anxiously avoids dealing with it in its context—or is his omission only to be attributed to a significant faulty action of the well-known kind? Psycho-analysis has coined the term *scotomisation* for this attitude. Scotomisation is the bringing about of something like a " blind spot " in the mind's eye which facilitates the disregarding and repression of undesired mental contents by the unconscious.

Yet even the psycho-analyst H. Hitschmann[2] in his paper *Telepathy and Psychoanalysis* declared some fifteen years ago that it is " unpleasant " to see the facts of physics and physiology go by the board through the evidence for telepathy, which as a matter of fact he uncompromisingly rejected. He pointed out how deeply the disposition to primitive ideas is rooted in the human mind, and suggests that the narcissistic tendency to ascribe to oneself " supernormal " faculties corresponds to the infantile craving to possess omnipotence of thought, a faculty of acting at distance, and the like, as found alike in the child, the savage, and the insane. Thus all alleged observations in this line are, in his view, purely mental subjective ones, products of man's wishful thinking, of weakness of intellect, which makes him cling to the mystical interpretation of his

[1] " Der Occultismus, seine Grundlagen u. Gefahren " (*Zeitschr. f. d. Gesamte Neurologie u. Psychiatrie*). 1939.

[2] *The International Journal of Psycho-analysis.* 1924.

experiences. Similar was and is the standpoint of Th. Reik and other psycho-analysts.

Such views for that matter are predominant in the majority of the educated public right up to the present; thus it is not astonishing that any closer dealing with it has remained a rather suspicious enterprise—as it was in Maudsley's time—its explorer running the risk of having not only his scientific reputation but also his mental integrity called in question. We have seen that even the expert may be misled by the same error he is used to revealing by deep-psychology. There is no doubt that such an attitude is far from being merely rational. Here reason seems to be fighting for her own sake with forces drawn from deeper levels of mental activity, thus jeopardising her primordial aims.

We have emphasised that a certain impairment of the purely rational attitude of consciousness, a certain "minus-function" of mind, has been found to be one of the essential conditions for the origin of telepathic phenomena. Accordingly any realisation in practice of these conditions is diametrically opposed to man's elementary intellectual striving. It implies a relapse to the stage of animism and magic, a regression to a superseded, primitive level of our thinking and reasoning, with the interdicted return of and return to the repressed, in the sense of Freud.

Thus we have good reason for admitting the justification of the modern man's uncompromising rationalistic and over-rationalistic claims. There is no doubt that the preservation of his present stage of mentality, of his recent level of consciousness, so hardly achieved in the course of his individual and racial development, is indispensable to his position in nature and even to his mental normality. Any relapse from this stage means a re-emerging of his prehistorical past, a menace to all the moral and intellectual achievements of the present. That is the reason why we find both human nature and the attitude of community equipped with powerful protective measures against such an event. It is the natural trend of the individual's mental constitution that works against the occurrence of "supernormal" phenomena. Hence their outlawed position, so to speak, their comparative rareness, their frequent association with twilight states between normality and mental illness, or even insanity—as I propose to show elsewhere. On the other hand it is with a fully purposive resolution that human society opposes any acceptance of their possible occurrence. Society protects itself in case of need by allowing them to be explained away by its scientists, or simply by means of a social act of scotomisation, when facing this special field of experience.

Thus we become aware that any psychopathological reading of telepathic phenomena has to reckon not only with the revealed facts but also with the ambiguous reactions of their observers to the facts revealed. Only after clearing away our deeply rooted prejudices—after getting rid of our scientific scotoma—can we begin their truly rational psychological exploration, unhampered by unconscious mental reservations and inhibitions. It is clear, however, that no less care is to be taken to avoid the temptation—a very real one, as deep-psychology has revealed—of slipping into the opposite extreme, no less dangerous for any scientific progress.

There is no doubt that such a resolute yet cautious approach as suggested here would enable psychopathology to submit the problem of the alleged supernormal phenomena to a new critical consideration and, possibly, to revise to a certain extent its attitude to them. The importance of such an issue for a further advance of the frontiers of psychology and of our present scientific outlook in general need not be further emphasised.

THE ISOLATION OF THE PERCIPIENT IN TESTS FOR EXTRA-SENSORY PERCEPTION

By Geoffrey Redmayne

Salute Parnassus! Let the climber find
True solitude upon the lonely peak,
And from his point of isolation seek
The ultimate horizons of the mind.

Introduction

In the recognised tests for extra-sensory perception, such as the tests employed by the Duke Parapsychology Laboratory or the ingenious mechanical apparatus devised by Mr G. N. M. Tyrrell, it is necessary that there should be an operator. In the case of tests for precognition and for clairvoyance it is for consideration whether the results obtained by the percipient may not be inadvertently influenced by that operator—even in cases where the latter may have no normal means of acquiring knowledge which might be of use to the percipient. If a test for clairvoyance is in progress, the operator may himself acquire the requisite knowledge by clairvoyance (or by normal means) and then transmit that knowledge telepathically to the percipient, so that it is difficult to claim that the ultimate result is indicative of " pure clairvoyance " ; similar considerations apply in the case of tests for precognition, except that the acquisition of knowledge by normal means is excluded. Again, the operator may disturb the course of an experiment by the telepathic transmission of subconscious preferences—quite independently of whether he has any knowledge of value to the percipient.

In order that the percipient may be isolated from the possibility of such undue influence, it is suggested that the normal duties of the operator should be undertaken as part of the functions of a form of automaton, *the actions of which, within the desired limits, are designed to be incalculable by normal means.*

If such an automaton is to be effective it must be capable of selecting numbers or symbols at intervals in a truly random sequence, and recording all calls and successes made by the percipient. Furthermore, as it is to usurp the functions of an operator, it must hold, as it were, a "watching brief" for the operator and give an indication if, on any occasion, the chance factor should be altered by inadmissible action on the part of the percipient. Apparatus designed with the above requirements in view, and suitable for tests for clairvoyance and precognition, is here described.

Specification of the Mechanism

The machine, details of which are shown in the accompanying photograph and diagram, has the following characteristics :

1. A motor-driven rotary selector, adapted to select one of five circuits, has five vanes mounted at equal intervals on the periphery. This selector can be locked in one of five positions by a magnetically operated brake which will make positive contact with one of the five vanes.

2. Means for ensuring that the selector will be stopped in an "incalculable" position. This is achieved by the following methods which are supplementary to one another in the case of tests for clairvoyance ; in tests for precognition, the second method only is used.

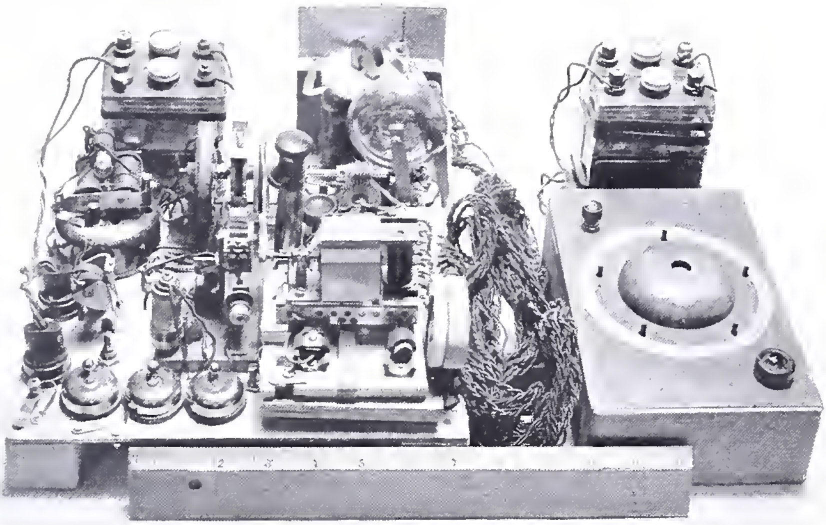
(*a*) *Variation of the time interval.*

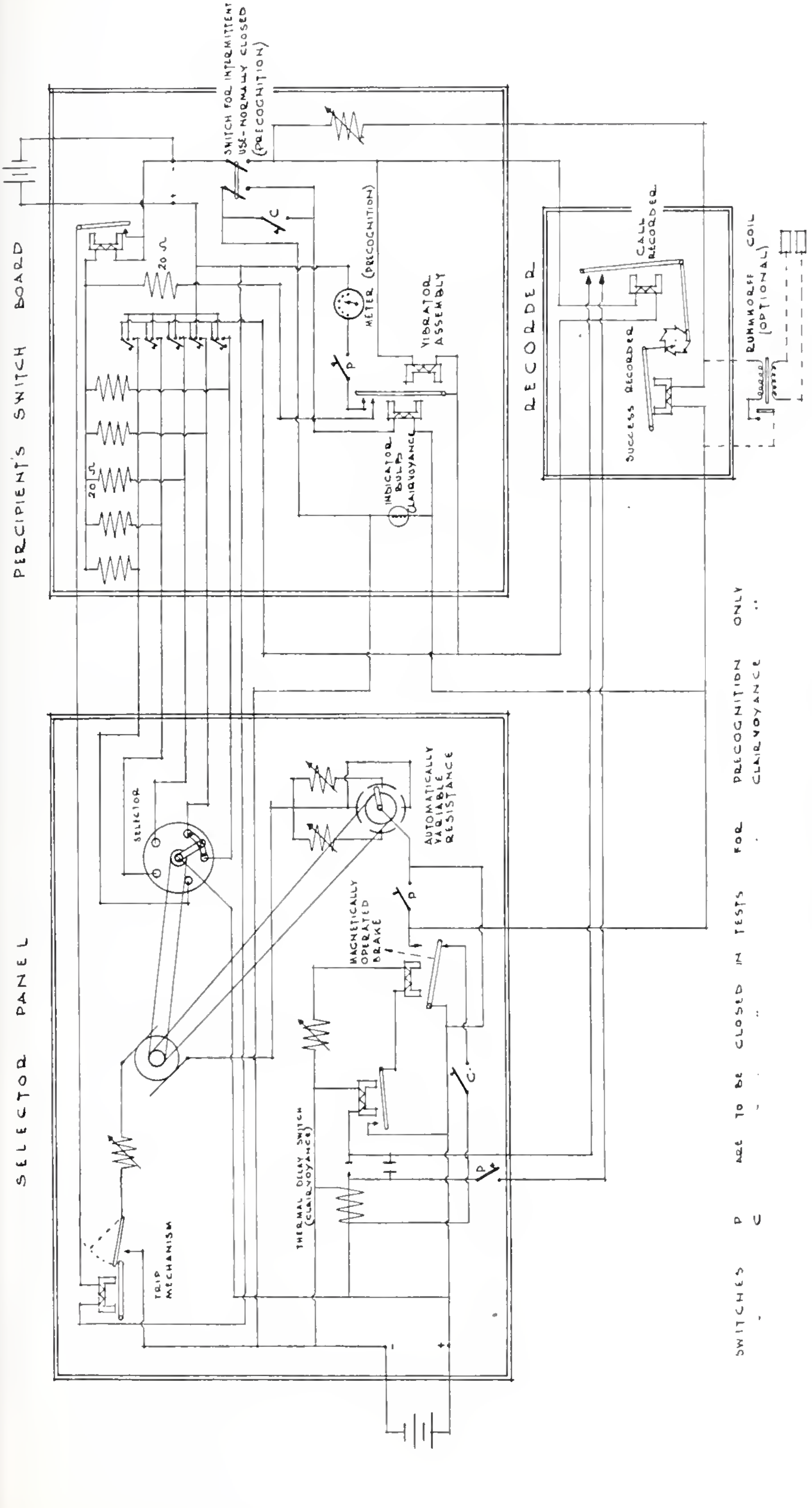
A thermal delay switch is so arranged that it will cause the brake to go "on", and then "off", at uncertain intervals. The switch is of the type used for safeguarding smoothing condensers from the peak voltages which arise when power is applied to the anodes of indirectly-heated valves before the latter are sufficiently heated to have cathode emission. The normal delay of such switches is generally over 30 seconds, but the model chosen can be adjusted so that the delay is one of approximately five seconds for each interval. In practice it is found that the switch has an erratic action when adapted for the present purposes, and on this account it is thought to be mechanically suitable for controlling the time interval. While some percipients will not object to the length of interval, others will consider it to be unduly long, and it is realised that there is likely to be adverse criticism on this score. Consequently it would be advantageous if arrangements could be made for the time interval to be varied to suit the percipient, and this is a matter which will receive consideration.

(*b*) *Variation of the speed of the selector.*

Variation in speed is attained by the use of an automatically variable resistance in the circuit of the electric motor which drives the selector. A rotary switch blade is continuously driven by the

PLATE II





CIRCUIT DIAGRAM FOR E.S.P. APPARATUS

electric motor at a speed of approximately 4 r.p.m., and the switch blade, in the course of each revolution, makes four changes in the resistance of the motor circuit : consequently the selector is frequently in a state of either acceleration or deceleration.

(Originally an entirely different method of varying the speed of the selector was employed, the time intervals being constant. Use was made of the variation in resistance set up between iron filings when they are mechanically agitated while retained in a strong magnetic field. This variation in resistance affected the operation of a supplementary magnetically operated brake which was capable of retarding, but not stopping, the selector. For some reason, probably faulty design, the device caused the selector to choose numbers in a sequence which, on analysis, did not prove to be truly random ; it was only with great reluctance that the device was finally abandoned, as it was unusually picturesque in its action and, from the theoretical standpoint, had much to commend it.)

3. A switch box upon which are mounted five press switches for the use of the percipient, and

(*a*) for use in tests for clairvoyance, an indicating bulb which shows when the machine is ready to receive a call, and

(*b*) for use in tests for precognition, a plunger which the percipient must press after making each call, and a meter which will inform him whether he has manipulated the plunger correctly.

The switch box contains suitable devices for the prevention of inadmissible calls ; these are described later.

4. An automatic recorder which indicates each call and registers each success on a circular paper disc ; aural indication is given whenever a success is scored. Each disc is designed to register 50 calls.

Procedure in Tests for Clairvoyance

The percipient scores a success if, when the selector has chosen a circuit, he presses a switch which is connected in that circuit.

A switch may be pressed at any time when the bulb on the switch box is glowing. The bulb will be " on " for about five seconds and then go " off " for about five seconds while the selector is choosing another circuit.

Procedure in Tests for Precognition

A simple system of switches enables the circuit to be altered so as to be suitable for testing precognitive faculties. In this case the selector is rotating continuously except for a momentary period after the percipient has chosen a circuit. Almost immediately after a switch has been pressed down to its fullest extent an automatic switch on the call recorder will close the magnetic brake circuit and the selector will suddenly stop ; if the percipient had chosen a circuit which the selector was about to choose, a success will be recorded. When a call has been made, a pointer will appear on the dial of the meter ; this is a warning that the plunger on the switch box must be pressed for approximately two seconds in order to allow the selector to rotate a sufficient number of times before another call is made. If the plunger is operated correctly the pointer will no longer be in evidence when the plunger is released, but if the plunger is released too soon the pointer will appear again.

Prevention of Inadmissible Calls

The most complicated part of the apparatus is that which is concerned with the prevention of inadmissible calls. It would seem that there are three types of erroneous calls against which provision must be made :

(*a*) The percipient presses two or more switches at the same time, and thus increases his chance of a successful score.

(*b*) In tests for clairvoyance, the percipient makes more than one call during the period when the selector is stationary.

(*c*) In tests for precognition, the percipient makes calls with such rapidity that the selector does not rotate sufficiently between each call.

In order to ensure that the percipient cannot score a success if he presses two or more switches at the same time, use is made of certain trip-mechanism which is capable of preventing any further rotation of the selector. All the switches are connected through a relay to the trip-mechanism so that one switch cannot alone operate the trip-mechanism, but so that two or more switches can operate it. This has been arranged by coupling all the press switches to a relay through 20 ohm resistances, the relay being adjusted so that it will not operate with 20 ohms resistance in circuit, but will operate with 10 ohms resistance in circuit. Thus, if one switch is pressed, the resistance in the relay circuit will be 20 ohms and the relay will not be affected; but, if two or more switches are pressed at the same time, the resistance will be lowered to 10 ohms or less and the relay will immediately affect the trip-mechanism on the selector panel, with the result that the selector will not be able to rotate again until the trip-mechanism has been re-set.

The trip-mechanism will also be affected if, in tests for precognition, the percipient should attempt to make calls at intervals of less than about two seconds. The trip-mechanism relay is in circuit with a device which is similar in many respects to the vibrator assembly of a full-wave vibrating reed rectifier, but the vibrations of the reed are utilised for an entirely different purpose. Details of the device are as follows:

An iron reed is suspended between two electro-magnets. One of the magnets is in parallel with the call recorder and so is energised only at the time during which the percipient presses a switch; the other and less powerful magnet is continuously energised, except at the time when the percipient presses the plunger on the switch box. The latter magnet cannot attract the reed unless the reed swings over to it on being released by the more powerful magnet. When the plunger on the switch box is pressed, the less powerful magnet will release the reed, but the vibrations of the reed will cause it to come frequently within the previous sphere of influence of the magnet for a period which is dependent on the fundamental tuning of the reed. It will be evident, then, that should the plunger be released before the vibrations of the reed have died down the reed will again be attracted to the magnet. If the percipient should attempt to make a call at a time when the reed is attracted to the magnet, the trip-mechanism on the selector panel will operate because the resistance in the relay circuit will have been lowered to 10 ohms (*i.e.* one of the 20 ohm switch resistances, in parallel with the 20 ohm resistance in circuit with the vibrator). The meter on the switch box will inform the percipient whether or not he may safely make a call.

The same device is also used in tests for clairvoyance for the purpose of preventing the percipient from making more than one call during the period when the selector is stationary. In such tests the less powerful magnet is connected to an automatic switch on the magnetically operated brake so that the reed is automatically released whenever the selector starts to rotate; consequently the vibrations of the reed do not serve any useful purpose, and the percipient is not concerned either with the plunger or with the meter on the switch box.

If, in tests for clairvoyance, the percipient should press a switch while the selector is rotating, the trip-mechanism will terminate the experiment. This, while being desirable, was not intended to be a feature of the design, and the reason for it did not immediately become apparent. Further study, however, made it clear that the machine had not acquired some of the characteristics of a monster of Frankenstein.

It is not thought that a percipient could overcome the limitations imposed by the above-mentioned devices, as steps have been taken to ensure that the percipient cannot usefully interfere with the vibrator assembly by magnetic or other means. In the absence of any contra-indication, therefore, it will be assumed that adequate precautions have been taken.

It will be evident that if the apparatus, with the exception of the switch box, is placed beyond the reach of the percipient, he can be left entirely alone while he makes each series of 50 calls. The objection may be raised, however, that the percipient is not completely isolated because it is still desirable that there should be an operator to exercise a general supervision over the apparatus and change the record disc after each series of 50 calls. The only supervision required, however, is that of the engineer in charge of machinery whose duties are limited to maintenance; he is not concerned with the quality of the work which is performed with the aid of that machinery, nor is he indeed cognisant of it during each series of calls. It is claimed, therefore, that the percipient is no longer subject to undue influence and that, from this point of view, he is virtually, in the words of the Ancient Mariner,

> "Alone, alone, all, all alone,
> Alone on a wide, wide sea!"

Analysis of Alleged Random Numbers

The Research Officer has very kindly made an analysis of a series of circuits which were chosen by the selector at a time when the apparatus was adjusted for the testing of the clairvoyant faculty; for this analysis (and for the photograph) my thanks are due to the Society. The report is as follows:

"Tests for Randomness applied to 1000 Numbers produced by the Apparatus when working on the Principle of Thermal Delay

The frequencies of the 5 digits are 177, 211, 222, 190 and 200, the expectation being 200. The value of χ^2 is found to be 5·67, with

4 degrees of freedom. This corresponds to a value of P of more than 0·2, which is perfectly consistent with the hypothesis that the numbers are random.

If we consider the series to be made up of 1000 pairs of digits, the first pair being the 1st and 2nd digits, the second pair being the 2nd and 3rd digits, and the 1000th pair being the 1000th and 1st digits, we can count the frequencies of the 25 possible pairs and enter them in the cells of a table, as follows :

FREQUENCIES OF THE 25 PAIRS

		Second digit of pair					Total
		1	2	3	4	5	
First digit of pair	1	31	45	47	25	29	177
	2	39	46	43	45	38	211
	3	35	43	45	43	56	222
	4	36	41	43	30	40	190
	5	36	36	44	47	37	200
Total		177	211	222	190	200	1000

The expectation for each cell is $1000/25=40$. If m is the expected number, and $m+x$ is the observed number, then

$$\chi^2 = \sum \frac{x^2}{m}.$$

Cell	$m+x$	x	x^2	$\frac{x^2}{m}$
11	31	−9	81	2·025
12	45	5	25	·625
13	47	7	49	1·225
14	25	−15	225	5·625
15	29	−11	121	3·025
21	39	−1	1	·025
22	46	6	36	·9
23	43	3	9	·225
24	45	5	25	·625
25	38	−2	4	·1
31	35	−5	25	·625
32	43	3	9	·225
33	45	5	25	·625
34	43	3	9	·225
35	56	16	256	6·4
41	36	−4	16	·4
42	41	1	1	·025
43	43	3	9	·225
44	30	−10	100	2·5
45	40	0	0	0
51	36	−4	16	·4
52	36	−4	16	·4
53	44	4	16	·4
54	47	7	49	1·225
55	37	−3	9	·225
	1000	0	$\chi^2 = 28{\cdot}300$	

As there are various relations between the numbers of the pairs, the degrees of freedom are reduced to 20. The value of P is about 0·1, which, though a trifle low, is not inconsistent with the hypothesis of randomness."

Portability of the Apparatus

Care has been taken to make the apparatus as compact as possible in order that it may be readily portable. The whole apparatus, including accumulators, can be fitted into an attaché case measuring $17\frac{1}{2}'' \times 10'' \times 5\frac{1}{2}''$, but normally a case with special fittings measuring $20\frac{1}{2}'' \times 12'' \times 6\frac{1}{2}''$ is used. Weight, including larger case and accumulators, is 16 lbs.

Provision for Stimulus

The recorder is provided with terminals to which may be attached a Ruhmkorff Coil for the purpose of giving the percipient a stimulus automatically whenever he scores a success. If the percipient is attempting to make a high score, then the coil should be adjusted so that the percipient feels no more than a tingling sensation in the hands. If the attempt is for a low score, then the shock may be increased, but care has to be taken not to make the shock so great that fear is produced, otherwise the paranormal faculty may be inhibited. The coil will probably give most assistance in cases where it is desired to obtain a significantly low score.

Proposed Developments

It is thought that it would materially add to the value of the apparatus if it were to be provided with certain adaptors which could be plugged into the selector panel. Progress has already been made with designs for ancillary devices which give visual and olfactory indications of the circuits chosen by the selector.

In the case of visual indication, it is considered that it might be helpful to a percipient if, before making a call, he were able to concentrate upon the actual surface upon which a symbol is about to appear. This is to be arranged by optical means—five miniature epidiascopes adapted to throw Zener symbols, in random sequence, upon the back of a translucent screen.

Olfactory indication presents certain difficulties, chief amongst which is the design of suitable stopcocks which can be operated by the selector through relays. An attempt is being made to adapt an Ether-" gas-oxygen " apparatus (as used for general anæsthesia) so that various aromas are delivered to the percipient through the usual facepiece. Compressed air is to be used both as a vehicle for the aromas and as a scavenger during the intervals. For the purpose of loading the vehicle it is proposed to make use of a special device which has been primarily designed for the effective control of the chloroform content of anæsthetizing gases, but which will be particularly suitable for the present purposes. Details cannot yet be published, as the device in its application to anæsthesia may eventually form the subject-matter of Letters Patent.

It would seem that both the above adaptors will be more suitable for experiments in precognition than in pure clairvoyance. Details of suitable recorder and associated circuits have not yet been worked out, but these are matters which must wait upon the completion of the adaptors.

There may be those who think that the proposed adaptors are an unnecessary complication and that a solution to the problem of the paranormal faculty will be found along more conventional lines. But it may well be that the means whereby we eventually attain this particular Northwest Passage would not readily be accepted as a reasonable basis for experiment by the orthodox opinion of the present day.

ON THE INTERPRETATION OF THE DATA OF CERTAIN EXPERIMENTS IN PARANORMAL COGNITION

By W. L. Stevens.

Whately Carington reports in *Nature* (and later in the *S.P.R. Proceedings*) that, in a series of experiments conducted at Cambridge, "percipients scored significantly more resemblances on originals of experiments in which they were working, as compared with the originals of the experiments in which they were not working, than would be expected on the null hypothesis that there is only a chance connection between the originals used and the drawings produced ($P<0{\cdot}0001$)."

There is no reasonable doubt that the null hypothesis which Whately Carington chooses to test (and of which the above is not a very accurate description) has been successfully disproved, but it would be unfortunate if his readers were left under any misapprehension as to the exact nature of the evidence which this supplies for telepathy or clairvoyance.

The work consisted of a series of five separate experiments, in each of which drawings were "exposed" consecutively while subjects (who generally each participated in only one experiment) drew "what they liked". Their drawings were then scored against all the originals, and the 1,209 resemblances recognised by the judges can be distributed in a five-by-five contingency table according to the numbers of the experiments, in which the original was exposed, and in which the drawing was made. Table I of Whately Carington's *Proceedings* paper reads :

On the Originals of Expt.	Hits by Drawings made in Experiment I	II	III	IV	V	
I	81	10	9	53	33	186
II	125	26	11	76	44	282
III	61	15	9	66	52	203
IV	43	11	7	58	33	152
V	109	24	14	133	106	386
	419	86	50	386	268	1,209

Taking as the null hypothesis that each "hit" enters this table independently, subject only to the restrictions imposed by the fixed marginal totals, Whately Carington tests the total of entries in the leading diagonal (Stevens, 1938 or 1939) and shows that it exceeds expectation by an amount which is highly significant. Disproof of a null hypothesis does not, however, automatically establish the truth of any other hypothesis which the experimenter might have had in mind when he did the work, and so we must examine more closely the evidence which this supplies for telepathy.

Stated more fully, the null hypothesis is that the probability that a hit, made in any experiment, is made on an original of any particular experiment, is independent, not only of the experiment in which the drawing was made, but also of any other hits scored.

This null hypothesis might be wrong for a variety of reasons. One obvious *possibility* is that subjects may tend to have favourite drawing topics, or, to put it another way, that the things I draw may tend to resemble each other more than they resemble the things you draw. To take a very extreme example; suppose a subject draws a horse on the ten consecutive nights of his experiment. Then according to whether a drawing of a horse was or was not exposed, he scores zero or ten, and in the latter case—this is the essential point—all ten hits must fall *en bloc* into the one appropriate cell of the contingency table. This contradicts the null hypothesis that his ten hits distribute themselves, in the appropriate column of the table, independently of each other. Of course, nothing so extreme as this happened, nor indeed need one suppose that there was excessive duplication of drawings. The definition of "resemblance" being that used by the judge who scored the drawings, then the null hypothesis would be invalidated if two drawings made by the same person are more likely to "resemble" each other than are two drawings made by different persons.

To test whether the contingency table shows deviations from independence, we can follow the usual procedure. For the whole table, χ^2 of sixteen degrees of freedom, is 41·7, which is less probable than one in a thousand. The hypothesis of independence can reasonably be held to be disproved.

This leaves open the question of what explanation, if any, we are to give. I have already suggested one, without in any sense claiming that this is the true explanation (for I have not seen the original drawings). Whately Carington would say it was due to telepathy. Others might maintain that it was the work of the devil. The point is that, so far, it is purely a matter of personal taste which explanation we choose to adopt, if any, since their effects on the criterion we have used are indistinguishable.

It is however possible to construct other tests to discriminate between effects of the kind I have suggested and telepathy. If on the telepathy hypothesis we thought it likely that the data should deviate from independence in a particular way, and if in fact we could show that the data did deviate in this way *and not in other ways*, then we should be in possession of good evidence for telepathy. Now it is a reasonable supposition that a subject picks up telepathic ideas of the drawings of the experiment in which he participates, and not of the drawings of the four experiments in which he does not participate. Expectations in the cells of the leading diagonal of the contingency table would thus be enhanced, and it is therefore pertinent to test whether the total of the entries in the leading diagonal significantly exceed expectation, on the null hypothesis. Whatever his reasons might have been, this is what Whately Carington does, and he finds a highly significant excess.

This does not complete the evidence, for such a significant excess might well be due to effects of the kind I have suggested. Of course, my tentative explanation would not imply increased expectations on the leading diagonal; it would however mean that the diagonal total has a bigger variance than that deduced from the null hypothesis, so that " significant " results in either direction (in this case it is the right direction for telepathy) would arise more frequently than indicated by their theoretical significance levels. To complete the evidence it is therefore necessary to go further, and show that not only does the contingency table deviate from independence in the particular way which would be ascribable to telepathy, but also that, if we accept the telepathic hypothesis as a new null hypothesis, the data become such as could reasonably have arisen, *i.e.* that they no longer show deviations, which require explanation in terms other than telepathy.

This can be tested. Telepathy is to account for the excess on the leading diagonal. Let us therefore obliterate from the data the 280 hits in the leading diagonal, and test whether the remaining data are independent; *i.e.* let us distribute the remaining 929 hits over the remaining twenty cells so that, if the expectations in any four of the twenty cells lying at the corners of a rectangle are

$$\begin{matrix} m_1 & \dots & m_2 \\ \dots & \dots & \dots \\ m_3 & \dots & m_4 \end{matrix}$$

then $$m_1 m_4 = m_2 m_3.$$

When this has been done χ^2, calculated in the usual way from the discrepancies between data and expectations, yields 24·20 for eleven degrees of freedom, which is practically on the one per cent. significance level.

While not a very startling level of significance, it should be enough to make any scientist very shy of accepting the telepathy hypothesis. Of course, if one takes the view that any explanation other than telepathy is rather fantastic, one could tolerate such an improbable result. Otherwise one is forced to the conclusion that telepathy does not adequately explain the peculiarities of the data, and that therefore there must be some "natural" explanation of the kind I suggest. At this point, I must again stress that my criticism does not rest on the one alternative explanation which I have put forward by way of illustration, but on the demonstrated fact that the null hypothesis is contradicted even in regions of the contingency table, where telepathy cannot be, *or at least has not yet been*, invoked as an explanation. There is therefore another explanation, and one is obliged to seek it out. This explanation, whatever it may be, invalidates the null hypothesis, and therefore invalidates the test of significance for the leading diagonal total.

Whately Carington may indeed reply that he had no right to assume that telepathy should give rise only to the particular effect he was looking for, when he tested the diagonal total. This is fair enough, but it means abandoning the diagonal total test, and basing conclusions on the result of the comprehensive test of independence of the contingency table. The mode of operation of telepathy or clairvoyance, needed to account for the observed deviations from independence, would then indeed be very curious, though that is beside the point, for the preference for such an explanation over possible alternatives would then, in any case, be arbitrary.

In conclusion, I must say that I believe that if I had been permitted to examine the original material, I should have been able to discover the natural explanation of the phenomenon.

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REPLY TO MR STEVENS'S CRITICISM

BY WHATELY CARINGTON

IT would seem that the malign enchantment of which the late Dr Franklin Prince wrote so feelingly is still potent in our subject. How else are we to account for the fact that a statistician of Mr Stevens's distinction writes 'null hypothesis' four times running when he is not referring to a null hypothesis at all but to an assumption underlying the method whereby the null hypothesis is tested? This is, I fear, not a matter for argument, but merely for re-affirmation: the null hypothesis IS that there is only a chance connection between the originals used and the drawings produced, and the improbability of this being true is all that the test of significance tells us. The test itself does depend on the kind of independence Mr Stevens discusses; but this is not part of the hypothesis, for there might be this independence and yet the effects not due to chance, or a measure of dependence and yet no more than chance at work.

In developing the argument Mr Stevens goes on to enunciate at least one demonstrably false proposition. He says (para. 5), "the things I draw may resemble each other more than they resemble the things you draw," and later "... the null hypothesis would be invalidated if two drawings made by the same person are more likely to 'resemble' each other than are two drawings made by different persons". Even allowing for the fact that by 'null hypothesis' Mr Stevens means "methodological assumption", this proposition is simply not true. If you make ten drawings, A, B ... I, J very skilfully in red ink and the cubist style, while I make ten different drawings Q, R ... Y, Z very badly in green ink and the vorticist style, there are at least three respects in which each of yours is more like the others of yours than it is like any of mine, and vice versa. But, since, *ex hypothesi*, all 20 drawings are different, the hits scored (if any) will all be on different originals and will therefore be independent in Mr Stevens's sense. As a matter of fact, Mr Stevens does not define what he means by "dependent" and "independent" in this context; but I take it that "dependence" involves some factor tending to cause a percipient who has been credited with one hit on a given original to be further credited with another hit on the same original. The point is of some importance, for whereas there are more ways than one in which this might occur (obsession, practical joking, forgetfulness on the part of the percipient, or aberrations on the part of the judge) the result would always be the same, namely replication of hits on the same original from a single set of drawings. Thus the somewhat damaging suggestion in the opening sentences of paragraph 5, to the effect that though Mr Stevens has only pointed out one source of error there may be others left undetected, is erroneous.

In short, the perfectly good point that Mr Stevens is trying to make is that we should not allow any one set of drawings to score more than one hit on the same original. If we do, then the result is mathematically indistinguishable from a plurality of percipients independently scoring one hit each ; and this, in whichever direction it operated, would be misleading.

This is 100 per cent. sound criticism, and I wish I had thought of it myself. Fortunately, the remedy is simple—" dereplication ", if I may coin a word—and the same in all cases regardless of the source of the error, while the effect, as might be expected, is small. In the case of the " Hindson All Entries " data quoted by Mr Stevens, elimination of replicate hits reduces the value of D/σ only from 4·166 to 3·803, leaving P a trifle less than 1 in 7,000, which is still highly significant. In fact, this point, though perfectly valid and a most acceptable contribution to technique, is in the nature of a correction or fine adjustment and in no case invalidatory.

This leaves the position substantially unchanged, and we must now turn to Mr Stevens's much more interesting and highly ingenious attack from the angle of interpretation. This is a minor masterpiece of sophistry which both deserves and demands careful analysis.

It is not in dispute that even after the data have been duly dereplicated the 5×5 table yields a highly significant result ; and I should be the first to agree with Mr Stevens in saying that this does not *per se* " prove " the occurrence of paranormal cognition. Natural phenomena, indeed, are never " proved " to occur in the sense that mathematical propositions may be proved to be true ; they become established through familiarity, with greater or lesser rapidity according to circumstances, through the devising of repeatable experiments and an increasing understanding of the laws which govern them.

But Mr Stevens is not content with anything like this ; he produces a superficially most plausible argument to show that the facts are actually to a significant extent incompatible with the " telepathic " hypothesis. He is quite right. They *are* significantly incompatible with " telepathy " IF, but only if, we accept his own definition of " telepathy ". Stripped of its obscuring embroideries the argument runs, " If telepathy occurs at all it must be of a particular type, namely a substantially ' now or never ' type, such that a hit will be scored by its means only on one of the originals used in the experiment in which the percipient was engaged and not on one of any other experiment. This would inflate the frequencies of the leading diagonal only. But other frequencies are also significantly inflated. Therefore it is not this type of telepathy that occurs. Therefore no telepathy occurs."

The falsity of this logic, unless it be conceded that paranormal cognition really *must* be of the now or never type if it is to occur at all, is manifest as soon as the argument is stated in this form; but Mr Stevens is naturally not so crude as this. The trick is mainly turned in the third sentence of paragraph eight, where he says " . . . it is a reasonable supposition that a subject picks up telepathic ideas of the drawings of the experiment in which he participates, and not of the drawings of the four experiments in which he does not participate ". Of course the supposition is reasonable enough, and if it read " . . . is *more likely* to pick up . . . etc.", it would be not only reasonable but true. But a reasonable supposition is one thing and an established fact is quite another. If it were known that paranormal cognition was exclusively of the now or never type postulated by Mr Stevens, his inference that what is here observed is due to something other than paranormal cognition would be entirely justifiable; but as it is the conclusion is no more than a piece of indifferently rationalised apriorism.

Mr Stevens himself seems to be a trifle uneasy, for in his concluding paragraph he admits that there is no necessity for assuming that paranormal cognition must give rise only to the particular effect I was looking for when testing the leading diagonal. So far so good; but I think he is wrong in saying that I *must* abandon the diagonal test and rely on the test of the whole table. I don't much mind, for this—as he points out—also gives a highly significant result, though I should have thought that in so far as one happens to be interested in the diagonal, the diagonal would be the thing to test. But it is giving the whole argument away in a sad anticlimax to complain that in that case " the mode of operation of the telepathy or clairvoyance . . . would indeed be very curious ". We more or less know that before we start, for otherwise there would be little mystery by this time about the phenomenon: while there seems no coercive reason why it should take a form corresponding to Mr Stevens's preconceptions. On the other hand, I do not think that anyone familiar with the qualitative literature of the subject will be particularly surprised to find a measure of precognitive and retrocognitive effects emerging.

What would have been curious, to the point of incredibility, would have been to find a paranormal effect giving a significant overall result for the whole Table, with the high and low cells randomly dotted about the place or alternating like the squares of a chess board. What we actually obtain, as explained in my section on Displacement, is a straightforward effect with a diminishing probability of occurrence before and after the event. If this "spread" and diminishment had been spatial instead of temporal, it would probably have been hailed as strong confirmatory evidence. To object to what has actually been found suggests a somewhat naïve, non-relativistic attitude with something of what we might call an Oedipus complex with respect to Father Time.

Mr Stevens's concluding sentence (added after the above was written) seems a somewhat unworthy red herring. The method and procedure adopted have been described in great detail: if Mr Stevens can find a flaw in either, well and good; if not, then differences of opinion as to particular judgements made by Mr Hindson (which is all, so far as I can see, that scrutiny of the actual drawings could lead to) are altogether irrelevant, as I have been at the utmost pains to show. However, if Mr Stevens can indicate what kind of systematic error he expects to find, compatible with the reported facts, capable of invalidating the conclusions reached, and such as could be made by an uninformed judge, he is very welcome (as, indeed, I have already assured him) to examine the whole or any part of the drawings or originals. Failing this, his point remains no more than an unsupported expression of opinion.

W. W. C.

REVIEW

Extra-Sensory Perception after Sixty Years. By J. B. RHINE, J. G. PRATT, C. E. STUART and B. M. SMITH, with J. A. GREENWOOD. Pp. xiv + 463, with 30 Tables, 21 Appendices, 6 Figures, 2 Graphs. Henry Holt & Co., New York, 1940. $2.75.

With the doubtful exceptions of one or two of the early classics, which are scarcely comparable, this is unquestionably the most important book yet published in the field of Psychical Research and Parapsychology. If it were not for the fact that these earlier works to some extent prepared the ground, I would go further and say that it is the *only* important book of the kind yet published. Certainly I know of no other which answers with anything like the same completeness, so far as they can be answered, the manifold objections, some reasonable but mostly not, which have been advanced against accepting ESP as a fact in nature. Only one stronghold of the critics is left unchallenged and unreduced, and the strength of this is wholly illusory, as I shall indicate below. On the other hand, a good deal of space is wasted in pricking bubbles which ought never to have been blown, but the blame for this lies more with the would-be critics of ESP than with the authors.

The book is divided into four Parts: I The Question of the Occurrence of ESP; II The Criticism and the Evidence; III The Nature of ESP; IV The Present Situation. The first two and last two of these naturally fall together, the former pair being the more immediately important, and the latter (for many readers) the more interesting. I shall concern myself mainly with the first two.

The principal items here are a discussion of the mathematical methods used, a survey of all quantitative work published between 1882 and 1939, a list of no less than thirty-five "counter-hypotheses" or possible objections, a more detailed discussion of six "test" cases believed to meet all these objections, some account of the general course of criticism, and the results of submitting the crucial chapters to seven of the more eminent or more prominent critics.

Nothing is to be gained by recapitulating mathematical controversies here. The methods used are now generally accepted, and should never have been challenged except on such points of detail as the preferability of the "matching" to the "binomial" hypothesis, which is of importance only in a few borderline cases and for exact work of informative as opposed to demonstrative character. Most of the objections raised, *e.g.*, Kellogg's contention that the observed instead of the theoretical variance should be used, seem to have arisen from a failure to understand the nature and purpose of tests of significance.

The collection of previously published material has been done with very great care and completeness. There are 145 items listed in Table 29 of Appendix 17, and many of these are classified under various headings in other Tables in the Text. I should be surprised if anything worth mentioning had been omitted, and this feature alone will make the book of great value to students as a work of reference. Similarly, the list of counter-hypotheses ranges from the foolish suggestion that the results reported may be due to chance, through various not unreasonable criticism, to the preposterous notion that ESP is contrary to science and impossible anyway. Again, I think there can be no doubt that every worth-while objection has been included together with many that are worthless.

All this testifies eloquently to the zeal, sincerity and thoroughness of the authors, but I am not sure that they have adopted the best policy in the presenting of their case. It does not seem to me unequivocally clear from the treatment that they have fully grasped the great principle that ten leaky buckets will not hold water longer than one leaky bucket and that this is true even if the leaks are in different places. Although the great number of experiments reported as favourable to ESP may constitute a strong *prima facie* case for further investigation, rigid demonstration cannot be advanced by claiming that experiment A excludes counter-hypothesis X, and that experiment B excludes counter-hypothesis Y, if Y is not excluded by A or X by B. It would, of course, be very strange if it just so happened that whenever an experiment was water-tight in one respect it was leaky in another; and this might be made the basis of a special argument of some cogency. In the absence of this, I should have preferred to see a few of these early cases (*e.g.*, Usher and Burt, Groningen, Estabrooks) picked out and treated in more detail as virtually water-tight, and the others marshalled as no more than a part of a general attack on the apriorist position.

It seems to me that the authors, in common with many others including myself, have all too long and all too tamely submitted to the dogma that ESP is "inherently" or "antecedently" improbable. I doubt whether this dogma is more than a fear-promoted bluff, and it is certainly high time that someone led a spirited onslaught against it. We might well begin by asking those who propound it to give us an estimate of the probability of the occurrence of ESP and to tell us how they arrived at it: I think it would be found that they are unable to do so and would quickly take refuge in evasive ambiguities or *ad hominem* arguments. If the dogma means that observations made antecedent to a given experiment or situation indicate that the probability of ESP occurring in that experiment or situation is small, the question is begged; for any such proposition can only summarise accumulated experience regarding the relative frequencies of occurrence of ESP in situations of that type and in others; and this implies the, at least, occasional occurrence of ESP. But if the dogma is intended to assert that ESP is inherently impossible, then it is not concerned with probability at all, except in that purely formal sense in which it may be said that the probability of anyone drawing a circular square is zero, and the word should not be introduced. In this case it is for the critics to show, as they have never attempted to do, that the alleged phenomena of ESP either involve a contradiction in terms or are formally incompatible with established facts of physical science. But physical science and parapsychology operate in fields which, though presumably coterminous, are certainly not coextensive, just as is true of the fields of magnetism and mechanics. Physical science has abstracted from the totality of possible observables those only which are measurable in terms of the gram, the centimetre and the second; and in thus abstracting it has automatically restricted itself to observables of that kind, and its "laws" consist of statements of relations between these. It is difficult to see how such statements can possibly be formally incompatible with statements about states of mind and cognitive relations of which the subject-matter is largely or wholly different. Finally, if critics mean that ESP is contrary to experience, they can only justify their allegation by tacitly defining ESP in advance in such a way as to make it so, for example as "a faculty which would enable one to read all the books in a library simultaneously and from a distance"; but this is too base and too feeble to be worth powder and shot.

The digression seems justified because failure to realise the extreme weakness of the apriorist position is liable to make people think it necessary to produce very high levels of significance in order to outweigh the (actually non-existent) "antecedent improbability" so gratuitously postulated by their opponents. This distracts attention from the only function of tests of significance, namely to tell us whether the effects observed are likely to be due to chance deviations from the null hypothesis tested.

With the apriorist and mathematical objections out of the way, the only alternatives left worth mentioning are those involving the perception of sensory cues; for hypotheses about rational inference and clerical errors will not bear examination, while that of extensive and collusory fraud has yet to be responsibly suggested.

Now there was a time when many of us were gravely perturbed at the discovery that the standard ESP cards, as sold to the public and used in most of the reported experiments, could be read from the back in suitable lights, and this "alarm and despondency" was justified by the fact that many of the early experiments of this kind were not described in sufficient detail to assure us that this possibility had been recognised and eliminated.[1] But such misgivings are now altogether out of date, as Dr Lemmon very fairly insists (p. 222), for most recent work has been done with the cards so screened that the percipient cannot see them at all. Certainly nothing of the kind can have arisen in any of the six selected cases cited.

These cases are all of very high quality, particularly—to my own mind—the Pratt-Woodruff, Warner, and Pierce-Pratt experiments, which I should regard as to all intents and purposes flawless; but I think it a pity that full experimental details from the original reports were not repeated in the text. Not every reader will have easy access to the originals, while the form in which they are dealt with makes it very difficult to reconstruct just what happened.

The principal fault of the whole work lies in the direction of protesting too much—of trying to pile every scrap of evidence and every shred of argument into the scale, instead of cutting out as much rubbish as possible and relying on the high quality of what remains. Not all critics are worth answering, and to thunder in pursuit of every paltry red herring drawn across the trail does not really add to the strength of the case. In Churchillian phrase, "Why should we stop to hurl a stone at every cur that yaps?"

Turning to the second half of the book, dealing with the nature of ESP, future developments, and cognate matters, I have space to comment on only one point. Foster, developing a suggestion due to Dr Thouless, has proposed the use of a quantity called the ESP Quotient, which is the percentage of hits most probably made by ESP alone apart from those due to chance; he also proposes to use

[1] It seems only fair to Dr Rhine and his colleagues to mention here the circumstances described on p. 193, where it is explained that "Careful specifications and warnings to the printers resulted in adequate proof sheets. The warping" (*i.e.*, the shrinkage which makes reading from the back possible) "apparently occurred after the cards were cut and stored". This would appear to exonerate those concerned from the otherwise not unreasonable charge of incompetence.

this to investigate the kind of psychological process involved in certain ESP situations. In particular : In the technique known as Open Matching the subject is required to match a pack of ESP cards against five " key " cards (one of each type) which are exposed face upward in front of him ; in Blind Matching, the key cards are face down. Thus in the second technique the percipient has to cognise by ESP both the key card and the cards to be matched as compared with the latter only, in the first. Quoting from p. 316 : " The simplest supposition would be that when confronted with the task in Blind Matching (*a*) two cards are to be matched ; (*b*) one card is perceived ; (*c*) the other card is perceived ; (*d*) the two are identified as similar. This is termed by Foster a *circumferential function,* implying a going around the long way. Contrasted to this is the *diametric function* . . . , which assumes that the perceptual act cuts across from (*a*) to (*d*) . . . making a single act of the perception of likeness." It is then shown that, in the first case, the fraction representing the ESP Quotient would have to be squared in order to calculate the expectation of successes due to ESP, while in the second it would not. This is then used as the basis of an attempt to ascertain which hypothesis is correct. It is found (p. 317) that " . . . on the whole the results clearly fall *between* the two extremes expected on the basis of the two hypotheses considered. In no series did [the relevant figures] fall to the level supposed by the hypothesis that the elements are separately perceived (circumferential function) and the relations established by a separate act." But surely Blind Matching is precisely the same as Open Matching, from this point of view, *once the percipient has made up his mind which key card is which* ; if so, the circumferential process would only have a chance of operating, if at all, during some early period of indecision and could scarcely pull the figures down to the required level. To test the point properly it would, I think, be necessary to ask the subject to indicate which pairs of cards were similar in two face-down packs, or something very like this. But the idea is promising and might well repay further research. Incidentally, it is interesting to note that the various quotients reported, though ranging from as high as 17·7 to as low as 1·3, appear to average at much the same value (round about 6) as the analogous figures derivable from my own experiments with drawings ; but this is very provisional and not to be taken too seriously.

A few miscellaneous points : The book is well produced, excellently printed, and provided with a Glossary, two good Indexes and an admirable list of 361 References ; but its utility and convenience would be enormously increased by the addition of a list of Tables, preferably showing the nature of each.

The Appendices, which are mainly mathematical, present a variety of useful formulae, methods and Tables; but they also contain a few slips. In Appendix 3, it is implied that the best estimate, s^2, of the variance of N quantities is obtained by dividing the sum of the squares of their differences from their mean by N; the divisor should be N – 1, otherwise the variance of a single quantity would be computed as zero, whereas it is indeterminate for lack of sufficient data. Table 25 of App. 4 has three trivial errors in the last column; the figures 9·82, 4·06, 4·06 should read 9·80, 4·05, 4·05, and the total should be 35·80 instead of 35·84. Appendix 7 is perfectly sound as it stands, but would it not be simpler to compute χ^2 with one degree of freedom from a 2×2 table of hits and misses under the two conditions? In Appendix 9 (Stevens's Matching formulae) the extreme case suggested of 25 circles presented and 25 circles called would lead to expected and observed numbers of hits both equal to 25, and thence to $D/s = 0/0$; so that P is not 1 as stated but indeterminate.

To sum up: The book, though not wholly free from defects, mainly tactical and of no real importance, is a product of which its authors may well be proud. It does not "prove" the occurrence of ESP as the books of Euclid prove propositions about triangles; but natural phenomena are insusceptible of that kind of proof. It does not produce evidence overwhelmingly outweighing the antecedent improbabilities of the apriorists; and that is because there are no such improbabilities to outweigh. But it does state a case enormously stronger than that on which murderers are usually convicted and hanged. All reasonable people will feel, I think, that ESP is now well rooted and likely to grow apace. Doubtless a few diehards will still hold out; but who cares about a few diehards more or less—provided they die?

W. W. C.

PROCEEDINGS

OF THE

Society for Psychical Research

PART 164

HENRI BERGSON

Although M. Bergson died only a few months ago, he will be remembered as the philosopher of the period 1890–1914. All his main contributions to Philosophy were made during those years. (It is true that he published a full-length work, *Les deux sources de la morale et de la religion*, as recently as 1932, but it is no more than an interesting afterthought.) Those were also the years of his greatest influence and reputation. That influence extended far beyond the narrow circle of professional philosophers. His doctrine of "lived duration", as opposed to the conceptual and spatialised time of Physics, did much to determine the psychological method of Proust's novels; and the very peculiar literary style adopted by that writer—so different from Bergson's own—was intended, apparently, to reproduce the flowing character of the Bergsonian stream of consciousness, in which every part interpenetrates every other. But Bergson's thought had more important repercussions than this. His sharp antithesis between intelligence on the one hand, instinct and intuition on the other; his doctrine that intelligence is only a practical tool (*homo sapiens*, he says, should rather be described as *homo faber*); whereas instinct and intuition, which is instinct become reflective, put us into touch with ultimate reality; his vitalistic metaphysics, which made the "life-force", *l'élan vital*, more fundamental than either matter or consciousness as we commonly understand them: all these have played their part in shaping those irrationalist or anti-rationalist movements of our own time which have exalted action, and even violence, as an end in itself, and have overthrown, perhaps for ever, the old bourgeois-liberal civilisation of the European continent. (It is fortunate perhaps that the English-speaking peoples do not take Philosophy so seriously.) On the other hand, he also undermined the prestige of Materialistic Positivism, and so paved the way for that revival of Intellectual Catholicism which is so important a feature of contemporary France. It is not for nothing that his disciple, M. Jacques Chevalier, holds office in the Vichy government, and was for a time its Minister of Education.

But while these effects of Bergson's thought have been showing themselves upon the stage of world-history, his reputation among professional philosophers has steadily declined ; and outside his own country, the works which astonished the Edwardian age are nowadays rarely opened, and still more rarely discussed. He is classed with " the metaphysicians ", and metaphysics of any sort is now very much under the weather. It seems to me, however, that this decline is quite unjustified. There are fashions in Philosophy, as in other things, and some day he will come into his own again. We must admit, I think, that the astonishing brilliance of his style—and few, even among French writers, have shown a greater stylistic mastery—has on occasions betrayed him. It has enabled him, sometimes, to distract the reader's attention with a chain of dazzling images where solid argument is called for, and to " get away with " the substitute, where a more humdrum writer would never have succeeded. This applies particularly to the more speculative parts of *L'Evolution Créatrice*, which both for good and for ill is the most metaphysical of all his writings. It is also true that in the epistemological parts of his work he insists upon using a peculiarly clumsy and ambiguous technical terminology (inherited from the French philosophers of the nineteenth century), which often obscures his meaning ; though all the time he is trying to break through the muddles and false problems which that very terminology had engendered. It may well be true, again, that he failed to understand the mathematical theory of continuity, as Lord Russell and others have argued, and that the antithesis which he sets up between the " spatialised time " of Physics and the " real duration " characteristic of life and consciousness is vitiated to some extent by this failure. But when all these defects have been duly taken into account, it will be found, I believe, that a very solid residuum remains. Some day the problems which he posed, and the startlingly original solutions he suggested for them, will have to be considered again. The relation of life and consciousness to the physical universe, the main theme of all his major works, is not a topic which the human mind can permanently neglect.

It follows from what I have said that the time for a balanced estimate of Bergson's thought has not yet come. But it does not follow that in the meanwhile we had better refrain from reading him. Certainly we in this Society should not refrain from doing so. It is clear that the main themes of his philosophy lie very near to the sphere of our special interest. Indeed the theme of one of his most important works, *Matière et Mémoire*,[1] lies right in the centre. For its main purpose is to propound a new and revolutionary theory of the relation between mind and brain. We are therefore not surprised to find that some of the most interesting passages in his Presidential Address to the Society in 1913 [2] are concerned with restating the main conclusions of that book, in such a way as to bring out their bearings upon the problems of Psychical Research. I will now try to summarise his theory of the relation of mind and brain, illustrating my summary by quotations from the Presidential Address.

Bergson's fundamental idea is that the brain is the organ of *action*. This is its primary function, in relation to which all its activities have to be understood. In perception, for instance, its function is to select a certain part of the external world as the object of our consciousness, shutting out all the rest; and this selection is made for rigorously practical ends. The normal healthy man perceives so much of the world as he can act upon, and he perceives it as a possible subject-matter for action; he imposes upon its " real extensity " a framework of homogeneous and infinitely divisible space, so that every object, and even every living being, appears to him as something in principle decomposable into separate parts, which he may re-combine into new forms to suit his practical ends. The mechanistic interpretation of Nature is simply an extension of this instinctive view of *homo faber*, and so itself has a biological basis. Perception, then, is primarily a practical not a theoretical function; it exists for the sake of action, not for the sake of knowledge. Nevertheless, Bergson holds that the range of *unconscious* perception is probably far wider than this: each of us perceives " virtually " far more than he perceives actually. When our " attention to life " lapses for any reason, and the barrier imposed by the brain is weakened, some of those " virtual " perceptions may be able to pass the threshold of consciousness, and telepathy or clairvoyance may occur.[3]

His view about memory is similar. He begins by distinguishing

[1] First edition 1896. There is an English translation, *Matter and Memory*, published by Allen and Unwin, with a special introduction by Bergson himself.

[2] *Proceedings*, Vol. XXVI (1912–13), pp. 462–479.

[3] Cf. *Proceedings*, Vol. XXVI, p. 475. English translation, Vol. XXVII, pp. 157-175.

habit-memory or rote-memory from memory proper. Habit-memory is certainly a function of the brain. But, equally certainly, it is not in the strict sense memory at all.

When I repeat a stanza of Tennyson by rote, I am not *recalling* anything in my past (for example, the first occasion when I read that particular stanza); I am simply performing an habitual action. On the other hand, memory proper, the retaining and recollecting of past experiences, is not dependent on the brain at all. It is a purely psychical function; and it is probable that the whole of our past is unconsciously retained, though only a small part of it can normally be recalled. We must not suppose that cerebral traces, corresponding somehow to past experiences, are physically stored up somewhere in our heads. So far is the brain from being the organ of memory, that its function is rather to prevent us from recalling too much. Just as it prevents us from perceiving what would be biologically useless to us, so here; it allows us to recall only that particular bit of our past which is relevant to our present practical situation, and shuts out all the rest from our consciousness. At least, all the rest is shut out from the consciousness of the normal healthy man. But if our bodily mechanism is enfeebled or deranged, we shall find that all sorts of *useless* memories will come flocking in. Indeed, according to Bergson, this is precisely what happens to all of us in dreams, when our practical activities are in abeyance. It is from this point of view that he approaches certain disorders of memory, notably aphasia and word-blindness. Since such disorders can be correlated with precisely localised injuries to the brain, they have been thought to provide direct evidence for the cerebral trace theory of memory. Bergson explains the facts differently. It is not that the memories themselves have been destroyed. It is merely that the connection between memory and action has been cut at a certain point, and this prevents certain of our memories from reaching consciousness. The damage to the brain prevents a certain class of *actions* from occurring; and then the particular memories which would have been relevant to those actions cannot rise above the threshold, though they are still retained in the unconscious.

The part played by the brain in thinking is explained on similar lines. According to scientific orthodoxy, every act of thought has its corresponding brain-state; and if a sufficiently accomplished physiologist could inspect the physico-chemical processes which are going on in my brain while I am thinking, he would be able to infer every detail of my train of thought. Bergson's view is very different. He holds that the role of the brain in thinking is only to produce movements and bodily attitudes which "act out" what the mind thinks—" qui *jouent* ce que l'esprit *pense* "[1] The brain in this connection is " an organ of pantomime "—in the etymological sense of the word " pantomime " (perhaps " dumb-show " would be a better rendering). The imaginary physiologist who looked into my brain could infer very little about my train of thought. He could only discover so much of it as is expressible in gestures, postures, and bodily movements, " what the psychical state contains in the way of action, either actually in process of accomplishment or merely nascent ".[2] He would be like a man who observes the comings and goings of the players on the stage, without being able to hear a word they say : or like a deaf man at an orchestra who can only see the gesticulations of the conductor. " Accordingly an examination of the interior of the brain would reveal nothing of mental processes strictly so called. The sole business of the brain, apart from its sensory functions, is to *mime* the mental life." [3] On the other hand this mimetic function is of the utmost biological importance. Without a brain to ensure that our thoughts are " mimed " by bodily dumb-show, we could not " insert ourselves in reality " : we could not respond to or cope with our physical environment. In short, it is this " organ of pantomime " which makes *intelligent action* possible. " Though consciousness is not a function of the brain, at any rate the brain keeps our consciousness fixed upon the world in which we live ; it is the organ of attention to life." And again : " To orientate our thought towards action, to induce it to prepare the act which circumstances require : that is the task for which our brain is made." [4]

But, as has already been made clear, the very fact that the brain is the organ of action also makes it an organ of limitation. By keeping our nose firmly fixed upon the grindstone, it prevents us from noticing a number of interesting but " useless " things. The vast panorama of our past is shut out from our view, except for that little corner which is relevant to our action at the moment ; and for all we can tell, a whole world of extra-sensory perceptions is likewise prevented from rising above the threshold of consciousness, though present to the mind in a " virtual " or unconscious state. It is therefore not at all surprising if some of those extra-sensory perceptions should find their way into our consciousness, when " attention to life " is impaired or deliberately suspended.

Such is Bergson's theory of the relation of mind and brain, in very brief outline. It seems clear that if this theory be true, we

[1] *Proceedings,* Vol. XXVI, p. 472. Bergson's italics.
[2] *Ibid.,* p. 472. [3] *Ibid.,* p. 473. My italics. [4] *Ibid.,* p. 473.

must conclude that survival is more likely than not. In the Presidential Address Bergson explicitly says so;[1] though he does not discuss the question in *Matière et Mémoire* itself. For according to his theory, there is by no means a one-one correspondence between brain processes and mental processes; the mental life of the embodied human personality is far wider than its cerebral life. Memory—that is, memory proper as distinct from rote-memory—has no cerebral correlate at all; nor has extra-sensory perception; while thought only has a cerebral correlate in so far as it is expressible in " pantomimic " actions. Accordingly " the burden of proof will fall upon the man who denies survival, rather than upon the man who affirms it ".[2]

What *sort* of survival Bergson would offer us is another question, upon which he does not particularise. I suppose it would be an extreme form of what he calls " life on the plane of dream ",[3] with all the memories of our past perpetually spread out before our consciousness, diversified perhaps by large doses of extra-sensory perception.

H. H. PRICE.

[1] *Proceedings*, Vol. XXVI, p. 476.

[2] Same page.

[3] Cf. *Matière et Mémoire*, ch. 3.

EXPERIMENTS ON THE PARANORMAL COGNITION OF DRAWINGS

By Whately Carington, M.A., M.Sc.
(Perrott Student in Psychical Research, 1940)

A. Experiment VI: 'Known' versus 'Unknown' Originals

ABSTRACT : Fifty originals, randomly selected from a larger list, were drawn by a third party and enclosed in envelopes. From these fifty envelopes ten were taken at random for use in the experiment. These ten, divided into two groups of five each, were put up as targets for the percipients, first in their closed (' unknown ') state and later after they had been opened and copied by the experimenter. The drawings received were suitably randomised and were scored by the experimenter against the ten working originals. They were later scored against the whole fifty originals from which these ten had been taken by an independent judge. Neither scoring shows any appreciable advantage for the known as compared with the unknown condition.

1. *Introductory:* So soon as we have satisfied ourselves to a reasonable degree of assurance, such as I venture to believe has been afforded by the experiments already described,[1] that some kind of paranormal cognition occurs at all, a number of questions immediately arise as to the nature and *modus operandi* of the phenomenon.

One of the most important of these is whether, or to what extent, knowledge of the original (or, more generally, of the object to be cognised) in the mind of the experimenter, or of some person connected with the experiment, is essential to the process involved. Somewhat roughly and colloquially speaking, this is equivalent to the question of whether the phenomenon is ' telepathic ' or ' clairvoyant ' in character, or perhaps a mixture of both ; but I think it would be injudicious to commit ourselves at the present stage to

[1] *Proc.* S.P.R., Part 162, Vol. XLVI, June 1940. This paper may be referred to as PNC.D., I, for short. The group of experiments numbered I to IV B described therein will henceforward be known as Experiments I to V respectively, unless otherwise indicated.

using these words as more than temporary conveniences of locution. It is not a question of deciding into which of two known and established categories a newly observed phenomenon falls, as one might seek to classify an unfamiliar zoological specimen. Categories can only be established after the examination of a large number of specimens, not before, whereas at present we are barely sure that there are any specimens to be examined at all. Thus, while it is legitimate enough to reflect that paranormal cognition may be of two types, in one of which there is interaction of two or more minds (telepathy), and in the other between a mind or minds and a material object (clairvoyance), it would be dangerous to allow ourselves to suppose that the fact of a given example of the phenomenon appearing to be of the one type or the other necessarily carries with it any implications beyond the bare fact stated.

The foregoing, however, in no wise diminishes the importance of the point at issue, and I decided at an early stage to make some attempt to throw light upon it so soon as I had completed the preliminary experiments reported in my first paper.

I may say at once that the results of this experiment are not in themselves conclusive, though I think we shall be able to form a fairly well founded opinion when they have been considered in the context of the other work. For the purposes of the present discussion, however, I shall confine myself to the internal evidence which the experiment was designed to yield.

2. *General Technique:* Except that some of the originals[1] used were in closed envelopes and were unknown to me at the time, the technique employed was substantially identical, in its general features, with that of Experiments I to V. That is to say, the originals were, in every case, put up in my study at or before 7.0 p.m. and were taken down at about 9.30 a.m. the next morning. The same precautions as regards curtaining, etc., were taken, and the chance of any percipient obtaining knowledge of any original by sensory means may be regarded as altogether negligible.

The drawing books issued to percipients were precisely similar to those used in the earlier experiments, so far as their pages were concerned. Apart from dates, etc., the only change worth mentioning in the instructions printed on the covers consisted in the addition of two items, viz. :

> (i) " Do not draw vague scenes, elaborate interiors or geometrical diagrams."

[1] For terminology throughout, unless otherwise indicated, see my first paper referred to above.

(ii) "If your drawing is at all ambiguous, please say what it is meant to be."

The object of these additions was to avoid ambiguities and to increase the proportion of drawings which could be scored with reasonable confidence. I have the very strong impression, though it is scarcely practicable to demonstrate its correctness rigidly, that the desired effect was produced.

3. *Preparation of the Originals :* The object of the experiment, as already implied, was to ascertain whether knowledge of the original in the mind of the experimenter (myself) was necessary or advantageous to the successful functioning of the percipients. It was therefore necessary that I should be ignorant of at least some of the originals during at least part of the experiment. Accordingly, I clearly could not prepare them all myself, and there seemed nothing to be gained, but rather the contrary, in preparing some myself and enlisting the services of someone else for the others.

I also felt it undesirable that originals which had been used in the first group of experiments should be used again, and this precluded me from giving some third party a free hand in selecting subjects or illustrations at random from a dictionary, as I myself had done in the first instance.

Moreover, I had in mind the possibility that knowledge of originals in the mind of *anyone* connected with the experiment, even at one remove, so to speak, might have some influence on the results ; and I was accordingly anxious, in order to minimise this, (*a*) that whoever drew the originals should not know what had been used in the earlier experiments, and (*b*) that he or she should not know just which particular originals were being used in this.

To meet these requirements I adopted the following plan :

I made out a list of 216 possible subjects for illustration, arranged in six blocks of six columns and six rows, avoiding subjects which had already been used or others very like or closely associated with them, but otherwise selecting in a more or less haphazard manner.[1] This list I sent to my friend Mrs Aletta Lewis, who is a professional artist and had most kindly consented to help in the matter, and asked her to select and illustrate *fifty* of the subjects mentioned. The selection was done by throwing three dice fifty times and taking,

[1] It may be worth noting that it is not nearly so easy as most people seem to suppose to find a really large number of readily illustrable, clearly distinguishable and widely familiar subjects for originals. I went through the whole of a small pocket dictionary fairly systematically and then had to do an appreciable amount of thinking before I found enough words for my purpose which fulfilled all requirements.

in each case, the word appearing in the block, column and row of the list indicated by the numbers so obtained, using any suitable convention as to which should be associated with which die. I do not now think that this was any better, but probably somewhat worse, than the simpler plan of writing some large number of suitable words on separate slips of paper, enclosing these in a like number of envelopes, shuffling the envelopes and instructing the artist to take out fifty at random.[1] However, it served the purpose for which it was intended, namely that of securing a substantially random sample from the initial list.

These fifty words were illustrated by Mrs Lewis, on sheets taken from surplus drawing books, in precisely the same manner (though naturally with far greater technical skill) as had been those selected from the dictionary by me for use in the first five experiments. That is to say, they were illustrated by simple line drawings executed in Indian ink on drawing-book sheets, as described in my first paper, and each had the name of the represented object written on it in Roman letters about $\frac{1}{4}''$ to $\frac{3}{8}''$ high.

Each of these potential originals was placed by Mrs Lewis between the leaves of an ordinary double sheet of foolscap paper, and enclosed in a 14″ by 9″ envelope. It may be noted here that the foolscap sheet and the envelope were not, between them, opaque in any strict sense of the term. That is to say, it would have been easy enough, as I knew from previous experiment, to determine the nature of the enclosed drawing by holding the envelope against a strong light. But I had, of course, no temptation in this direction, for to have done so would have been to risk vitiating the whole experiment by giving me knowledge of originals which it was essential for me not to have. The sheet of foolscap was only an additional, and in practice a superfluous, precaution against my inadvertently acquiring knowledge of the originals despite the protection of the envelopes.

These fifty envelopes were sent by Mrs Lewis direct to Dr E. J. Dingwall, to whom I must again express my indebtedness, and were thoroughly shuffled by us jointly, after which *ten* were drawn at random, numbered 1 to 10, and used as originals in this experiment. As explained in my first paper, these ten were later returned to the remaining forty, after they had been used, and the whole fifty were employed as pseudo-originals for the Control Marking carried out by Mr Saltmarsh.[2]

The result of all this was that I myself knew nothing whatever,

[1] I adopted this latter procedure in Expt. VII, *q.v.*

[2] *Loc. cit.* p. 112.

at this stage, about the selected ten, except that they must be *some* ten from my list of 216 possibilities. Mrs Lewis knew, in one sense, even less; for, although she was aware of the general character of the work on which I was engaged, she did not know the precise nature of the experiment in which her drawings were to be employed, or just when it would take place, or how many of them would be used. And even if she had known all these things, she could not have known which ten originals would be chosen from the fifty she had prepared. Thus the possibility of normal leakage from this source may be wholly disregarded, especially as Mrs Lewis did not know at all what percipients would be taking part. As regards the possibility of paranormal influence from her mind, it is to be noted that any effect which this might have would necessarily be spread over the whole fifty originals she had prepared and not concentrated on those actually used, still less on those used in any particular period of the experiment. Note also, with a view to possible developments to be discussed at a later stage, that she could hardly be regarded as having strongly *associated* the originals with the experiment, because she had no direct experience of the latter, but only the sketchiest conversational account from me of the kind of work I had been doing.

The ten originals actually selected proved to be, in alphabetical order: Book, Bow (tie, not archery), Chopper (drawn more as a hatchet), Cigar, Coat (no wearer shown), House (conventional), Lamp (old-fashioned oil table lamp with glass chimney, no shade), Pelican, Ruler (student's type footrule), Teapot.

I may say here that the forty envelopes not used, together with the fifty originals of the earlier experiments, and such of the ten selected envelopes of this experiment as had not been opened, were removed from my study and placed during the period of the experiment at the bottom of a wardrobe trunk in an adjoining room. After any one of the ten selected envelopes had been opened and its contained original used, the latter was locked up in the steel box referred to in my first paper.

4. *Arrangement of the Experiment:* It is not nearly so easy as might be imagined to devise a satisfactory experiment of this type. On the face of it, the obvious plan would be to put up the ten originals first in their envelopes (*i.e.* with their nature unknown to the experimenter) and then again, perhaps in a different order, after they had been opened by him. If we obtained a significantly higher proportion of hits under the second conditions than under the first, we should tend to conclude that the experimenter's knowledge of the originals favoured the process of cognition. But such

a plan has certain serious defects. In the first place, percipients might become bored or fatigued in the course of the work, and this would tend to mask the kind of effect just mentioned; or they might, oppositely, improve with practice, which would tend to enhance or even spuriously generate it. Apart from this, it is at least possible that many would hesitate to draw the same thing twice; and this, assuming that clairvoyance were operative, would tend to favour the 'closed' condition as compared with the 'open'. Moreover, a test consisting of twenty consecutive trials is longer than I should care to ask most percipients to undertake, and longer, I fancy, than most would complete even if they consented to start. This objection also applies to any scheme for alternating the ten closed and unknown originals with ten known originals selected in the same way as those of the earlier experiments. Besides, the two series would be bound to have different popularities, and this would give one of them an advantage over the other of which it would be impossible to determine the magnitude.

There is also the point that once an original is known it cannot, for the same experimenter, be made 'unknown' again, and this fact introduces certain restrictions of its own.

Finally, but in some ways most important of all, it is imperative that drawings compared from the point of view of whether the originals at which they were aimed were open or closed (*i.e.* known or unknown) should be of equal lateness in the series drawn by the percipients. To illustrate simply the point involved, let us suppose that we put up five originals, first in closed envelopes (unknown) and then after they have been opened (known) on ten successive evenings, and ask a group of percipients to attempt to 'reproduce' them. Suppose also that these five drawings represented Astrolabe, Okapi, Xylophone, Platypus and Bistoury. I think it will be generally agreed that, assuming chance alone to be operative, these objects, if drawn at all, would probably be drawn late in the series rather than early. If so, examination of the drawings would falsely suggest that the known condition was advantageous; and an opposite effect might be produced if the originals were, let us say, Tree, House, Table, Chair and Boat. The point, however, is not that very common or 'popular' objects are necessarily drawn early in a series of ten, or *vice versa*, but that it is quite unsafe to assume that objects are equally likely to be drawn in any position. On the contrary, we may be fairly sure that of any two originals one will tend to be drawn on the average earlier than the other; and that the same will be true of any two groups of five originals such as were used, as will be seen below, in this experiment.

To meet these various considerations I decided that it would be necessary to use ten originals divided into two groups, to divide my percipients also into two categories, to arrange for each category to aim at both groups of originals and for each to aim at one group closed and at the other open. The two groups of five originals are referred to for convenience as X and Y, and the two categories of percipients as A and B. It should be noted that although, as in the earlier experiments, percipients were necessarily grouped, for the most part, in the geographical sense, nearly all these geographical groups included percipients of both categories, and that there was no systematic arrangement as to which should be A and which B. Thus each category was just as fair a sample as the other and there is no reason at all to suppose that either possessed or displayed proclivities which the other lacked.

I am much indebted to Dr R. H. Thouless for help in deciding on the final arrangement, which was as follows :

Period	Occasions	Percipients	Originals	Condition
1	1 to 5	A	1 to 5, *alias* X	Closed
2	6 ,, 10	B	1 ,, 5 ,, X	Open
3	11 ,, 15	B	6 ,, 10 ,, Y	Closed
4	16 ,, 20	A	6 ,, 10 ,, Y	Open

The originals actually used on the various evenings of the experiment are given below :

Period	Date	Original	Condition
1	Nov. 14, 1939	Cigar	Closed
	15	Book	,,
	16	Bow	,,
	17	Ruler	,,
	18	Lamp	,,
2	19	Book	Open
	20	Bow	,,
	21	Ruler	,,
	22	Cigar	,,
	23	Lamp	,,
3	24	House	Closed
	25	Chopper	,,
	26	Pelican	,,
	27	Teapot	,,
	28	Coat	,,
4	29	House	Open
	30	Pelican	,,
	Dec. 1	Teapot	,,
	2	Coat	,,
	3	Chopper	,,

It will be seen from the foregoing that this arrangement involved a twenty-day experiment for the experimenter, but only ten for each percipient. Percipients in category A, however, had to do five evenings' work, then stop for ten evenings, and then do the remaining five. This was an inevitable inconvenience and I am much indebted to them for the high proportion of cases in which they remembered to do so.

5. *Procedure:* There is little to add to what has already been said, in this paper or my first, on this subject. As explained, the closed envelopes or opened originals, as the case might be, were pinned up on my study bookcase at or before 7.0 p.m. each evening, and were taken down again at or after 9.30 a.m. the next morning. At the end of the first and third periods, slips of paper were suitably pasted over the numbers on the envelopes which had been used, and they were shuffled out of my sight by my wife and then renumbered. Thus the originals for periods 2 and 4 were, as will be seen from the list given above, the same as for periods 1 and 3 respectively, but were used in a different order.

To make sure that the experimenters paid due attention to the originals, each was traced in Indian ink on a fresh sheet of paper as soon as it was opened and immediately before it was put up. As before, my wife and I took turns in doing this, I starting. The tracing and the artist's original were put up together, one behind the other.

6. *Percipients:* As the experiment was intended to throw light on the *difference* between two conditions, I felt it desirable to secure the participation of the largest possible number of percipients. I accordingly sent out a grand total of 484 books, of which 247 were A's and 237 B's. Of these, however, only 212 were returned more or less completely filled up. In addition, Mr J. J. Poortman, to whom it was impossible to send books in time owing to war restrictions, very kindly organised a group of thirty-four percipients from the Dutch S.P.R., who worked on sheets of ordinary paper under the same instructions as were issued to the other percipients. I am very much obliged to Mr Poortman for his continued assistance in difficult circumstances.

Thus a total of 246 complete or partially complete sets was received, of which 134 were A's and 112 B's. I should like to take this opportunity of thanking all those concerned for the trouble they took in enlisting percipients and distributing and collecting the books, particularly Dr Thouless, Professor Norrish and Mr C. A. Mace at Cambridge, Dr Mary Collins at Edinburgh, Dr Thorburn at Cardiff, Dr Vernon at Glasgow, Mr Rex Knight at Aberdeen, Mrs Chance at Aberystwyth, Mr Gibson at Duke University, and Miss Wellman of the American Society of Psychical Research.

7. *Randomisation and Scoring:* The percipients' books were all sent, in the first instance, to Professor Broad, who very kindly consented to randomise them in the same way that he had done for Expt. IV A & IV B. In this case, however, the randomisation was confined to the books themselves and was not extended to the individual pages of each book.

The books so randomised, with covers detached and each bearing two identifying letters as before, were next passed to me. I then scored all drawings against the ten originals used in just the same sort of way as Mr Hindson had scored the drawings of the first group of experiments against the fifty originals used in them. I adopted the same plan of assigning 1, $\frac{1}{2}$ or 0 to each drawing; and in view of the success of Mr Hindson's half-point markings [1] I tried to work on a somewhat more generous scale than I had used in my earlier tentative attempts.

It is important to note that the randomisation made it impossible for me to distinguish between A and B sets; and that consequently I could not, even if I had wished, favour open as compared with closed originals, for the former came early in B sets but late in A sets, and *vice versa*.

The whole of the material (246 sets) was later scored again, but this time against all the fifty originals prepared by Mrs Lewis, by my friend Mr Fraser Nicol of Edinburgh, to whom I am very greatly indebted for carrying out this laborious piece of work. In this case he not only did not know which sets of drawings were A's and which were B's, because of the randomisation, but was equally ignorant as to which of the fifty originals had been used in the experiment.

This second scoring not only afforded a check on my own work, but, because it used all fifty originals instead of only ten, was capable of greater sensitivity, and thus gave a greater chance of detecting any effect which might be present. Neither scoring, however, yielded anything approaching a significant result.

8. *Results:* In order to meet the point raised by Mr Stevens in *Proc. S.P.R.*, Part 163, *q.v.*, to the effect that it is illegitimate to give more than one point to any percipient in respect of the same original, both Mr Fraser Nicol's markings and my own were carefully 'dereplicated' before computation. That is to say, wherever more than one entry had been made for the same original in marking a single set, I retained only the highest; or, if there were more than one candidate for retention, decided by a random procedure.

[1] *Loc. cit.* pp. 94–5.

Since all the results are null, I do not think it worth while to give the figures for the ten originals *seriatim* or to separate the full from the half points. Table I below accordingly shows only the incidence of *entries* (1 and ½ treated alike) on the two groups of originals, X and Y, during the four periods, as allocated by Mr Fraser Nicol and myself.

Table I

	W.W.C.			Mr Fraser Nicol				
Period	X	Y	X+Y	X	Y	X+Y	Others	Total
1	53	56	109	27	40	67	118	135
2	36	43	79	18	27	45	94	139
3	77	33	110	36	18	54	80	134
4	74	50	124	46	30	76	88	164
Total	240	182	422	127	115	242	380	622

As might be expected, there are appreciable discrepancies between our figures, even when we allow for the fact that I was deliberately marking on a more generous scale than I encouraged him to do. None the less, certain important features agree well; in particular, note that both scorings show much higher proportions of hits on X, as compared with Y, originals in periods 3 and 4 than in periods 1 and 2. The figures (%X) are

Period :	1	2	3	4
W.W.C. :	48·6	45·6	70·0	59·7
J.F.N. :	40·3	40·0	66·7	60·6

When we reflect that the X group of originals was completely known throughout periods 3 and 4, quite unknown in period 1, and only progressively ascertained during period 2, these figures (which are easily shown to be significant) strongly suggest that knowledge is advantageous.

I must freely admit that, at one time, I allowed myself to be misled to this effect, and was confident that the experiment had detected a significant difference between the two conditions of 'unknown' and 'known'. Fortunately, however, the considerations detailed on page 282 above occurred to me before it was too late; and further consideration of the data convinced me that this effect may be fully explained by supposing that the X originals (Book, Bow, Cigar, Lamp, Ruler) happen to be of a kind which people mostly draw later in a series of ten than they do the Y originals (Chopper, Coat, House, Pelican, Teapot), supposing that they draw any of these at all.[1]

[1] Since writing the above, I have had occasion to score the whole of the material of Expts. I to V against all originals used up to date, including those

In order to make a valid test we must compare periods of like lateness, such as 1 and 2 or 3 and 4. Thus, in period 1, both X and Y originals are wholly unknown, but in period 2 the X originals progressively become known while the Y's do not; similarly, the X's are fully known throughout periods 3 and 4, but the Y's become known only in the last. And the periods are comparable, because 1 and 2 both consist of drawings 1 to 5, for A and B percipients respectively; while 3 and 4 both consist of drawings 6 to 10 for B and A percipients respectively. Consequently, if there is a real advantage accruing to the more known condition, there should be a higher proportion of X's in period 2 than in period 1, and of Y's in period 4 than in period 3. We may examine whether this is the case by means of the familiar "2×2 table", using χ^2; but when we do so we find a null result in every case. As a matter of fact, the expected proportions are (quite insignificantly) reversed for periods 1 and 2, as reference to the percentages given above will show. This is found whichever figures we use; but in each case also it is outweighed by a small positive effect in periods 3 and 4.

Thus, on balance, the evidence is slightly in favour of the known as compared with the unknown condition; but no result comes anywhere near significance,[1] so there is nothing to be gained by giving the relevant calculations here.

We may therefore conclude, without going into further details, that these data afford no worthwhile grounds for supposing that contemporary knowledge of the original by the experimenter enhances the percipient's prospect of success.

9. *Interim Discussion:* The null result reported above does not, of course, settle the point at issue one way or the other, for it is susceptible of more interpretations than one. In the first place, it

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here discussed. Taking the hits on X and Y originals for Early (1 to 5) and Late (6 to 10) positions in a 2×2 Table, I find

	Early	Late	Total
X Originals - -	46	37	83
Y ,, - -	61	31	92
Total - -	107	68	175

There is clearly a tendency for hits on Y originals to be made relatively early. This is not significant, for the corrected value of X^2 is only 1·74 with P about ·19. On the other hand there are only 175 entries here, compared with my total of 422 in Table I above. If the tendency showed itself in Expt. VI with the same strength as in I to V, the effect would be significant. This cannot be claimed as establishing the truth of the suspicion mentioned; but it is certainly sufficient to warrant our rejecting the *prima facie* indications of Table I.

[1] The best, derived from my own figures, gives P no better than ·13.

is possible that the experiment simply failed to 'work', that is to say, that no paranormal cognition occurred under either condition; in the second, it might well be that there actually was some difference between the two conditions, but that the experiment was insufficiently sensitive to demonstrate it. Only alter eliminating these possibilities would we be entitled to conclude that unknown originals are as easily cognised as known.

If we had obtained a significant positive result in favour of the known originals, there would, I think, have been no doubt about the conclusion that knowledge was at least definitely advantageous, though we should not have proved that it was indispensable. As it is, we cannot interpret the result with confidence until we have compared the experiment as a whole with the preceding experiments, so as to ascertain whether the percipients engaged, or either category of them, scored relatively more hits on the ten originals used than on the earlier originals, and *mutatis mutandis*. To do this satisfactorily, we need a more detailed and more delicate method of marking than can practicably be obtained from uninformed judges using the roughly graded scale of 1, $\frac{1}{2}$ or 0. I accordingly propose to postpone further discussion of this experiment until after I have completed an improved scoring of Experiments I to VII.

The question of sensitivity, however, deserves a note here. The total number of drawings involved is approximately equal to the number obtained during the first five experiments; so that it would be not unreasonable to expect a significant result if there were a real difference between the two conditions. On the other hand, intervals ranging from five days to about as many weeks were allowed to elapse between constituent experiments of the first group, whereas the twenty 'exposures' of this experiment were made on consecutive evenings. If precognitive and retrocognitive effects be real, as the results described in my first paper suggest, this is likely to lead to a confusion between the two conditions having nothing to do with their intrinsic merits. That is to say, percipients of the first period might score precognitively on originals known only in the second; or percipients of the third might score precognitively on the originals of the fourth or retrocognitively on those of the second. The first two of these effects would tend to obscure any difference between the two conditions that there might be; the third would tend, *per se*, spuriously to enhance it, but it must be remembered that, from the arrangement of the experiment, the X originals are known to the experimenter during period 3, quite apart from any true retrocognition on the part of the percipients. Thus factors of this kind, if operative, would tend on balance to obscure the effect we are looking for, and I am inclined to attribute the inconclusive result mainly to this cause. Certainly, if I had realised then as clearly as I do now the extreme importance of *time* as a factor in the process, I should have tried to arrange for intervals of at least two weeks between the various experimental periods.

As it is, comparisons between the numbers of hits scored on X and Y originals in periods 1 and 2, or in 3 and 4, do not properly correspond to comparisons between hits scored in different experiments separated by an adequate time interval; the correspondence is rather with comparisons between the first and second halves of the same experiment. If we pool the data for Experiments I to V, we have, using the Hindson All Entries figures as in PNC.D.I,

On the Originals of of Occasions	Hits by the Drawings of Occasions 1–5	6–10	Total
1–5	89	68	157
6–10	59	64	123
Total	148	132	280

This gives χ^2 as 1·77 with P as large as ·19. It is accordingly quite insignificant, though we know that the experiments were eminently successful when compared with each other as wholes.

B. Experiment VII: First 'Inter-University' Experiment

ABSTRACT : Each of five 'primary' Experimenters prepared and displayed ten randomly selected Originals on ten successive nights, at Glasgow, Edinburgh, Oxford, Bristol and Reading. Attempts to reproduce these were made by five associated groups of percipients and also by five other groups organised by 'secondary' Experimenters at Leeds, Cardiff, Birmingham, London and Cambridge.

Taken as a whole, the experiment appears to have been intrinsically successful, but the percipients of particular groups did not succeed in scoring relatively more hits on the originals intended for their groups than did the percipients of other groups. That is to say, something in the nature of 'cross-influence' or the equivalent seems to have been operative in an important degree.

Some further evidence of precognitive and retrocognitive effects was also found.

A tentative theory of paranormal cognition is advanced.

1. *Introductory:* In the present state of our knowledge of Paranormal Cognition there are two main lines along which we must try to progress. In the first place we must devise and perform experiments of a specific or *ad hoc* character with a view to throwing light on the nature of the process involved and the conditions of its occurrence; in the second, we must develop a repeatable technique and do our best to induce independent experimenters, particularly psychologists, to apply it. Experiment VI, which I have just described, is a not very successful example of the first class, while Experiment VII, discussed below, was mainly intended as a preliminary first step in the second direction.

I have long felt strongly that this whole question of repeatability, and of actual repetition by others, is of the very greatest importance. In the long run, a subject can no more be firmly established on the work of one or two individual experimenters, however fortunate or gifted they may be, than on the performances of one or two percipients of no more than transitory brilliance. I was accordingly very anxious to arrange as soon as possible for experiments to be carried out, on the same general lines as my first five, in which everything of major importance should be done by some person or persons other than myself and in such a way that no amount of bias, or even malfeasance, on my part could affect the issue. In this way I hoped to obtain independent confirmation of my results, to spread interest in the subject, and to provide for the work a somewhat wider basis than could be afforded by the activities of a single individual.

There were, however, and still are, considerable difficulties in the way of realising any such project. I had virtually no doubt at the time I first envisaged the plan, and have less now, that anyone who could arrange to conduct five 'ten evening' experiments, spaced over as many months and employing about fifty percipients each, would obtain results of the same kind as I had found, but such a programme involves something very like full time work, as I well know, and is far more than one could possibly expect any professional psychologist, already well occupied with his ordinary duties, to undertake. Besides, it was by no means certain that there would be many, if any, to be found who would consider the subject sufficiently advanced to warrant the expenditure of so much time and energy, even if they could spare them.

It was accordingly clear that the only hope was to arrange something on co-operative lines, so that the work could be shared among several experimenters, each of whom would make some not prohibitively laborious contribution, and in such a way that by combining these contributions a significant result might be obtained.

Even so, and assuming that the requisite co-operation could be secured, I was faced with the difficult question or whether to aim at a succession of (say) five experiments well separated in time, more or less as my own five had been, or whether to permit experimenters to suit their own convenience, with the consequent likelihood of some considerable overlap between the periods of the constituent experiments. I had, of course, no doubt at all that the first plan would be *preferable*—partly because it would reproduce more accurately the procedure of which I hoped to confirm the results, and partly because I was very much alive to the possibility that some kind of cross-influence between experimenters or percipients or both might tend to obscure the effects sought. Against this, I had considerable doubts as to whether such a plan would be *practicable*, at least within the limits of time I judged it reasonable to allot to it. Most, if not all, of those whom I proposed to approach were subject to the limitations of University terms, particularly as regards the availability of percipients; consequently, it would probably have been difficult to arrange for more than one separate experiment in each term time, and this would have meant spreading the work over more than a year. Moreover, though I have no wish to be alarmist or despondent, even in retrospect, there was a not wholly negligible risk that if the proceedings were unduly protracted it might prove impossible to conclude them without external interference.

I therefore decided to arrange, if I could, for five independent experiments to be carried out by five different experimenters, but to put up with whatever complications might be caused by such overlap in time as might be found unavoidable. Apart from the use of a plurality of experimenters instead of one, this would correspond very fairly closely to the structure of my first group, except that the constituent experiments would be mainly separated in space and not in time. I reflected that, if my misgivings as to cross-influences were unfounded, I ought to be able, by arranging for all drawings to be scored 'blind' against all originals as before, to obtain a significant positive result; if they were not, then it should be possible to demonstrate the fact by scoring the whole of the drawings of the first five experiments and of the five sections of this against the hundred originals used in both, and the resulting evidence of cross-influence (if any were found) could hardly fail to be of great interest.

I have little doubt that this decision was correct in the circumstances prevailing at the time; and I certainly do not regret it, even though I consider that it prevented me from obtaining the straightforward confirmation of my results which I had in some degree allowed myself to hope for. Prophecy is always rash, and I should be sorry to commit myself at the present stage; but I am strongly inclined to suspect that the outcome of this experiment, which strongly suggests something equivalent to cross-influence, will be found to mark a milestone in our understanding of what is going on at least comparable to the realisation of the fact of displacement forced on our notice by Experiment II.[1]

2. *The Notion of Cross-influence:* I think this question of possible cross-influence deserves a short digressive section to itself before I go on to describe the actual experiment.

The first five experiments gave us good grounds for supposing that paranormal cognition occurs, but they told us nothing as to how the percipient contrives to cognise the right drawing out of all the millions in the world, or the right element of mental content out of the many thousands which must have passed daily through the experimenters' minds; or even how those particular minds were selected out of the mass of humanity. I do not suppose that I had wittingly so much as set eyes on more than forty or fifty of the 250 who took part, and was not on terms approaching intimacy with more than perhaps ten or a dozen: similarly, it is unlikely that greater numbers than these knew where Fitzwilliam Road was or had ever passed along it. Thus it seems out of the question to

[1] Cf. PNC.D. I, p. 54.

postulate any direct personal rapport; on the other hand, the supposition that adequate psychological linkage with, for example, the percipients of Duke University was provided by my correspondence with Dr Rhine and his personal contact with them appears an extremely tenuous and unconvincing hypothesis.

It accordingly seemed at least possible that, if two or more experimenters were working at once, percipients might 'pick up' impressions from any or all of them indiscriminately: alternatively, percipients might 'relay' impressions to each other by what one might term a sort of 'lateral' telepathy; or both these processes might operate together. It was on account of considerations such as these, which were fully justified by the event, that I should much have preferred a succession of experiments separated in time to a group of which the constituent members overlapped.

3. *Organisation of the Experiment:* In the hope of securing the requisite co-operation, I approached a number of psychologists, philosophers, and others of like qualifications and interests, in the principal universities of the British Isles. The response was most gratifying. Three or four, not unnaturally in the circumstances, pleaded pressure of work and war-time conditions, and begged to be excused; but I had no great difficulty in finding ten, in ten different Universities, who were willing to cooperate in greater or less degree according to their opportunities. I should like here to express my most cordial gratitude for their help and my warm appreciation of the way in which, at what must often have been very considerable inconvenience to themselves, they supported me in this exploratory enterprise into unfamiliar and debateable territory; and my feelings are enhanced by the fact that nearly all those concerned, and one or two newcomers also, have consented to continue the work with further experiments now in progress.

It will readily be understood that it was not until I knew, at any rate approximately, the number of co-operators available that I could finally decide on the form the experiment should take. The general plan, of course, was that each of, say, five or six experimenters should conduct an independent 'ten evening' experiment, on the lines of my own Expts. I, III, IV & V, with his own group of percipients; and that the drawings of all percipients, suitably randomised, should be scored 'blind' against all originals in the same way that Mr Hindson scored the drawings of the first five experiments against the fifty originals used in them. The somewhat qualified hope (for the reasons given above it would be incorrect to say 'expectation') was that when these scores were arranged in a suitable 5×5 (or 6×6) Table, they would show that the percipients used by each experimenter had scored, to a significant extent, relatively more hits on the originals drawn by him than on those drawn by the other experimenters. There was, however, the difficulty that, if too many experimenters took part in this way, the number of originals might easily become unmanageable from the scorer's point of view—even though I had been so fortunate as again to secure the help of Mr. Hindson in this connection; moreover, not all of those who expressed their willingness to cooperate were able to undertake the full duties of a ten-evening experimenter.

I therefore decided to organise the experiment in two ranks, as it were, with five 'primary' experimenters each doing a full ten-evening experiment on his own and with his own group of percipients, and five 'secondary' experimenters who had the less onerous task of enlisting the services of percipients, distributing and collecting books, etc., without drawing any originals themselves. Each secondary experimenter and group of percipients was associated (randomly) with one of the primary experimenters, and the percipients were asked to 'aim at' the drawings produced by him; they were not told that other experimenters and groups were working at the same time. My idea was that, if knowledge of or personal contact with the experimenter played any part in the process, the secondary groups might reasonably be expected to do less well than the primary, or even to yield a null result while the primary groups yielded a significant one, and that some effect of this kind might throw useful light on what was taking place.

The locations and names of the experimenters, their identifying letters, and the numbers of percipients taking part (neglecting two or three who returned nothing but blank sheets) are given in Table I below:

Table I

Identifying Letter	Place	Experimenter	No. of Percipients
		Primary Experimenters	
A	Glasgow	Dr P. E. Vernon	15
B	Edinburgh	Dr Mary Collins	24
C	Oxford	Mr and Mrs W. Kneale	20
D	Bristol	Mr M. H. Carré	21
E	Reading	Dr Hilda Oldham	33
		Total	113

Secondary Experimenters

a	Leeds	Dr Ll. Wynn Jones	20
b	Cardiff	Dr J. M. Thorburn	8
c	Birmingham	Miss M. Hammond	25
d	London	Mr R. J. Bartlett	38
e	Cambridge	Dr C. A. Mace	23
		Total	114

In addition to the foregoing, Leeds contributed sixteen 'False Starters' (see section 12 below), and there were two sets forwarded by Mr Bartlett which arrived after the sets from other percipients had been sent for scoring.

It will, I hope, be understood that the percipients of the Leeds, Cardiff, Birmingham, London and Cambridge groups were instructed to try to reproduce the originals prepared by the experimenters at Glasgow, Edinburgh, Oxford, Bristol and Reading respectively.

4. *Percipients' Books:* The books sent out for distribution to the percipients were identically similar in their main features to those used in earlier experiments. The name and address of the experimenter whose originals the percipient was desired to reproduce was printed on the cover of the book in each case; for example, the books sent to both Edinburgh and Cardiff had the words

In your case the arrangements will be made by

Dr. Collins,
Psychological Department,
The University,
Edinburgh.

and similar wording, *mutatis mutandis*, appeared on the books sent to other groups.

The instruction introduced in Expt. VI, viz., "Do not draw vague scenes, elaborate interiors, or purely geometrical diagrams" was retained with the qualification "unless your impressions are particularly vivid."

In an attempt to help the percipients, the following words were inserted, "Do not strain after obtaining 'occult' impressions unless you feel you must. Probably the best plan is to think of the experiment in a general way, to reject images of which you clearly recognise the source, and then draw the first thing that comes." The part of this from "to reject" to "the source" was almost certainly an error of judgment. Instead of merely eliminating certain kinds of trite and irrelevant drawings, as I had hoped, it seems to have embarrassed the percipients and to have led in some cases to earnest introspectionists reporting that they could not think of anything of which the source was not recognisable.

Since there was no question of 'matching' in this experiment, the ten sheets of each book were numbered, for convenience of later reference, in the top right-hand corners instead of beyond the perforations to the left as in previous books.

5. *Selection of Originals :* I was anxious that each primary experimenter should himself carry out as much of the essential procedure as possible, and that I myself should have no means of knowing what originals were being displayed where and by whom. On the other hand, I thought it might lead to confusion if originals which had been used in the first six experiments were to be used again.

I accordingly prepared a list of 150 words, mainly but not exclusively taken from the list of 216 which had been sent to Mrs Lewis for Expt. VI (*vide supra*) and not containing any of the fifty which had been illustrated by her. I wrote each of these words on a separate slip of paper, occasionally with a brief indication of the kind of illustration I desired, enclosed each of these slips in a separate envelope, and finally shuffled the lot. These 150 envelopes were then sent to Professor H. H. Price, our President, who very kindly reshuffled them and, in due course as notified by me, sent a random selection of thirty to each of the five primary experimenters. Each of these experimenters was told to draw one envelope at random from the thirty supplied, on each evening of his experiment, and to illustrate the word therein as his original for that evening. Experimenters were told that they might reject the word if they wished, *e.g.*, if they thought it too difficult to illustrate ; but that, if they did so, they were to replace the slip in the envelope, mark the latter 'rejected' with the date of rejection, and return it with the other envelopes and slips when the experiment was over ; there were, however, only two or three instances of this being done.

6. *Display of Originals, etc. :* Originals were actually selected, prepared and 'displayed' by Primary Experimenters as shown in Table 2 below :

TABLE II

Date, May, 1940	GLASGOW	EDINBURGH	OXFORD	BRISTOL	READING
8		Corkscrew			
9		Fireplace			
10	Caterpillar	Box			
11	Braces	Chimney			
12	Toothbrush	Camera			
13	Screwdriver	Easel			
14	Crown	Knife			
15	Tent	Motor Horn	Lily	Unicorn	
16	Cock	Pincers	Key	Cup	
17	Golf Club	Sheep	Rake	Star	
18	Ladder		Thermos	Volcano	
19	Spring		Catapult	Wringer	
20			Carrot	Stool	
21			Comb	Funnel	Harp
22			Pillar Box	Pickaxe	Perambulator
23			Ace of Clubs	Elephant	Spanner
24			Church	Safety Pin	Bowl
25					Tumbler
26					Telephone
27					Pansy
28					Locomotive
29					Thermometer
30					Watering Can

It will be seen that there was a very heavy overlap. All fifty originals were drawn and displayed in the course of no more than twenty-three evenings. On three evenings there were four originals displayed at once, on six there were three, on six there were two, while only on eight had a single original the field to itself.

The only points worth noting about the foregoing list of originals are the following : Braces—trousers, not ship ; Cock—rooster, not tap ; Golf Club—instrument, not house ; Spring—mechanical (helical), not vernal ; Box—specified to be with lid open ; Chimney—as in factory ; Knife—table, not pocket ; Motor Horn—old-fashioned kind with bulb ; Rake—garden, not profligate ; Cup—specified to be without saucer, see Section 12 below ; Star—conventional five-pointed ; Stool—three-legged, as for milking, not foot ; Funnel—as in chemistry, not steamboats.

All experimenters had of course been most straitly charged, in the Memorandum of Instructions circulated to them, to take the same rigorous precautions about safeguarding the originals from all possibility of being seen by the percipients, or unauthorised persons, as I had taken in my first five experiments. I have no doubt at all that all concerned fully realised the importance of this, and conscientiously carried out the relevant instructions.

7. *Randomisation of Drawings, etc.:* Each experimenter collected the completed books from his group of percipients and sent them to Professor Price at Oxford, who randomised them in the same way that Professor Broad had done with the books of Expts. IV, V and VI, before sending them on to the scorer. As in the case of Expt. VI, randomisation was confined to the books as wholes and was not extended to individual sheets. Each book was given a pair of identifying letters, which were entered on the outer cover and on the first page; the cover was then torn off and kept by Professor Price, together with the key to the identifying letters, until after the scoring had been done. I am very much indebted to Professor Price for the trouble he took in this matter and for dealing, as mentioned above, with the shuffling and distribution of the envelopes for selection of originals.

Primary experimenters sent their originals to Dr R. H. Thouless, who kindly arranged them in alphabetical order and forwarded them to Mr Hindson for use in scoring.

It will be noticed that this process of randomisation, though similar in effect, was somewhat different in plan from that adopted for the first five experiments. Arrangement in alphabetical order would not conceal similarities in style or mode of production between the originals drawn by any given experimenter; thus, though the scorer would have no idea as to which originals were produced by which experimenter, he might easily be able to form an opinion to the effect that certain originals belonged together, and this might conceivably have led to the operation of some measure of bias. It was therefore necessary to rely primarily on the randomisation of the books themselves; the sending of the originals *via* Dr Thouless was mainly in order to ensure that I myself had nothing to do with the matter.

8. *Hindson Scoring:* As already intimated, I was so fortunate as again to secure the services of Mr M. T. Hindson as scorer, and I must again express my most cordial thanks for his good offices in the work.

I asked him to apply precisely the same principles as before, but with the additional refinement of using three-quarter and quarter points as well as full and half points. I hoped that this more finely graduated mode of scoring might throw light on the relation (if any) between the strength of resemblance of a drawing to an original and the likelihood of the former being a 'winner'; that is to say, I thought it possible that the full points might give a stronger result than the three-quarters, the three-quarters than the halves, and the halves than the quarters, or something like this: actually, however, all categories gave null results, so that my hopes in this connection were disappointed.

I have discussed the whole question of scoring so carefully in my former paper that I need not go into it again here, except to remind readers that the essential condition of uninformedness on the part of the judge was as well fulfilled in this case as in the earlier.

9. *Results*, (*a*) *By Inspection*: It needed no more than a relatively hasty skimming through the drawings received, by anyone familiar with those of the first five experiments, to create a strong impression that this experiment had been strikingly successful in the sense that the percipients had scored a notable number of hits on the originals used in it as compared with the percipients of the earlier experiments. The following examples show the kind of thing I mean. The figure preceding the name of the original indicates the number of hits scored by the 227 percipients of this experiment while that in brackets gives the number scored by the 251 percipients of Expts. I to V. Thus, there were unmistakably 5 Toothbrushes (0), 3 Corkscrews (0), 1 Safety Pin (0), 3 Cameras (0), 5 Pillar Boxes (0), 12 Elephants (1), 16 Locomotives (7), 8 Watering Cans (2), and so forth: these are the high lights only, and the list is not exhaustive.[1] This sort of thing is legitimately impressive to the experienced eye, but it is not to be considered as rigid evidence, and I shall return below to this question of the success of the experiment as a whole.

10. *Results*, *Continued*, (*b*) *Hindson Scoring*: So soon as I had received the drawings and scoring sheets from Mr Hindson, and the key to the identifying letters from Professor Price, I set to work to prepare 5×5 Tables, corresponding to Table I of my first paper, *q.v.*, showing the hits scored by the percipients of Groups A, B, C, etc., and a, b, c, etc., on the originals displayed by the experimenters of A, B, C, etc. As in Experiment VI, and henceforward unless otherwise stated, I used 'dereplicated' or 'nett' figures; that is to say, no set of drawings was allowed to score more than one hit on any single original.[2]

Examination of these tables made it abundantly clear that, how-

[1] It will be understood that these figures were the result of a relatively superficial inspection of the two sets of material; they are given by way of justification of the impression received, not as final or authoritative results.

[2] Cf. *Proc.* S.P.R., XLVI, p. 262.

ever successful the percipients as a whole had been in scoring hits on the originals as a whole, they had signally failed to discriminate between those displayed by the experimenter with whom they were associated and those displayed by others.

In these circumstances there is nothing to be gained by giving elaborate numerical details. In Table III below I accordingly present only 'skeleton' figures for the primary, secondary and combined groups, using the Hindson 'All Entries' data in which full, three-quarter, half and quarter points are reckoned as of equal value.[1]

TABLE III

		Primary Groups						Secondary Groups					
		Hits by the drawings of						Hits by the drawings of					
		A	B	C	D	E	Total	a	b	c	d	e	Total
	A	8					76	17					75
On the	B		13				109		9				113
Originals	C			14			94			21			96
of	D				18		118				22		84
	E					44	122					26	124
Total		69	93	92	82	183	519	96	27	109	159	101	492

Primary Groups: O is 97; E is 107·960; D is −10·960; D/σ is −1·209; P about ·23

Secondary Groups: O is 95; E is 94·705; D is 0·295; D/σ is 0·035; P about ·97

Combined Groups

	Aa	Bb	Cc	Dd	Ee	Total
A	25					151
B		22				222
C			35			190
D				40		202
E					70	246
Total	165	120	201	241	284	1011

O is 192; E is 206·025

D is −14·025; D/σ is −1·104

It will be seen that, for the sake of clarity, I have given only the figures for the marginal totals and the leading diagonal in each case. Otherwise the arrangement is the same as for my Table I of my first paper, and the results are worked by the same procedure as

[1] Cf. PNC.D. I, pp. 94 and 134.

was illustrated in Example 1 on page 132 thereof. As before, O stands for the observed number of hits scored by the percipients on the originals at which they were supposed to be aiming, *i.e.*, the number in the leading diagonal; E is the expected number of such hits obtained from the marginal totals; D is O – E; 'σ' is the standard error calculated by the method just referred to.

It is evident that the only points worthy of mention are the negative deviation shown by the primary groups and the somewhat unnaturally close agreement of the Observed with the Expected number of hits in the case of the secondary groups. Both effects, however, may safely be neglected here. The first is perhaps the more curious, because we should naturally expect some slight positive result, however much 'cross influence' might be going on; but it is quite trivial. The second effect is intrinsically significant; on the other hand, it is, so to put it, the best of two, so that the value of (1 – P), which is what interests us here, must be doubled, leaving us with a probability just below the recognised level of significance.

The results make it clear that there is *no* tendency for the percipients of a given group, whether primary or secondary, to score relatively more hits on the originals displayed by the associated experimenter than on those displayed by others. In other words, so far as this, the principal overt objective of the experiment is concerned, the results are completely null.

11. *Results, Continued, (c) Manning-Sanders Scoring:* The conclusion stated above renders it a matter of no small importance to ascertain as definitely as possible whether the remarkable successes recorded in Section 9 above were fortuitous, and the whole of the percipients' drawings no more than chance-determined, or whether they were genuine and the failure of the groups to 'separate out' consequently attributable to the overlap of the experiments or to what I have referred to as 'cross influence'. If the former alternative is true, then something has gone sadly and seriously wrong with the technique; if the latter, then we are likely to find ourselves led to conclusions of very great interest indeed.

The only way to do this is to compare the drawings of this experiment, taken together, with those of some other experiment or experiments, in respect of the hits scored on their own and each other's originals—in much the same way that we did when we were first enquiring as to whether there was any paranormal effect at all.

The obvious material to use is that of the first five experiments, for it would be scarcely practicable to organise another complete experiment of the type of VII specially as a 'control'.

To do so involves finding a fresh unbiassed (*i.e.*, uninformed) judge, which is much more easily said than done, and I consider myself extremely fortunate in the circumstances to have been able to induce Mr G. Manning-Sanders, who is well known as a writer and was formerly an instructor in the Royal College of Art, to undertake the formidable task of scoring the 4,000-odd drawings of Expts. I to V and VII against the 100 originals used in them. I should like here to express my very great indebtedness to him for this most timely assistance.

The method of scoring was the 1, $\frac{1}{2}$ or 0 point system used by Mr Hindson in scoring the first five experiments; but, as before, I have found it simpler, and sufficiently effective, to treat all entries as of equal value. The 100 originals were arranged in alphabetical order (more for convenience of reference than for the sake of randomisation), while the drawings bore no indication of the experiment to which they belonged and were presented in an arbitrary order as suitably code-lettered volumes each containing on the average some fifteen to twenty sets. Thus the scorer had no possible means of knowing which drawings had been aimed at which originals, and the necessary condition of 'blindness' was fulfilled.

Mr Manning-Sanders will not, I know, resent my saying that his relevant forte is in the appreciation of form rather than in the niceties of clerical meticulosity; moreover, external circumstances prevented him from devoting as much time to the task as we could both have wished. It is not surprising, therefore, that his work shows a considerable number of omissions, with the result that his scoring is considerably 'weaker', so to say, than Mr Hindson's. Thus, for Expts. I to V, Mr Hindson recorded a total of 1,069 nett (*i.e.* 'dereplicated') entries, and these led to a value of D/σ of 3·803 with P about 1 in 7,000; Mr Manning-Sanders records only 310, with D/σ 2·575 and P ·01. This is definitely significant, though the debilitating effect of the omissions is very marked. The quality (100 D/N) of Mr Manning-Sanders' result is somewhat higher than that of Mr Hindson's, the values being 5·61 and 4·30 in the two cases respectively, which suggests that Mr Manning-Sanders has somewhat hastily skimmed the cream off the milk by picking only the more obvious hits.[1] This reduction in significance is, of course, merely the result of relatively bad—*i.e.* hurried—scoring; it does not imply that the data are really less significant than Mr Hindson's results indicated.

Mr Manning-Sanders' full results for Expts. I to V and VII are shown in Table IV:

[1] Compare the figures for the 'undoubted' hits shown in the last column of Table 5 of my previous paper.

TABLE IV

Hits by the drawings of

		I	II	III	IV	V	VII	Total
	I	10						77
	II		44					72
On the	III			4				142
Originals	IV				28			103
of	V					40		237
	VII						248	431
	Total	72	29	34	216	142	569	1061

O is 334 ; E is 295·29 ;
— D is 38·71 ; D/σ is 3·022 ;
P less than ·01

This result is significant ; but the contribution made by Expt. VII is relatively small, though positive, so that we are not entitled to say much more on these grounds than that the figures are consonant with the supposition that the experiment is of the same family as its predecessors. This supposition appears reasonable on general grounds and is strongly supported by the evidence from inspection. I accordingly propose to adopt it for the purposes of discussion in Section 13 below ; but it must be regarded as no more than provisional pending the application of more thorough methods of scoring.

12. *Various Points of Interest :* The first of these is that of the Leeds False Starters. It so happened that, through an error on the part of a temporary typing assistant, the covering letter of instructions was not sent to Professor Price with the 150 envelopes for distribution to the experimenters. The result was that Dr Vernon, at Glasgow, did not receive his envelopes when he should have and therefore could not start on 1st May as had been planned. I did not know this, and therefore could not warn Leeds. Consequently, the Leeds percipients started trying to reproduce non-existent Glasgow originals, and made some five or six attempts each before I discovered what had happened and asked Dr Wynn Jones to stop them. They were afterwards so good as to restart and finish the course.

The ' false start ' drawings, however, were of remarkable interest. They contained two Crowns, one Toothbrush, one Church Steeple, one Ladder, one Easel, two Bowls, one Hen (good enough for Cock), one Watering Can, one Camera, one Elephant, one Key, one Rake and one Knife. All these from sixteen percipients producing sixty-one pages of drawings. These were included *incognito* in the material submitted to Mr Manning-Sanders for scoring. Unfortunately, only twelve of the above-mentioned hits were noted; but, even so, we find (using Expts. I and II only as a basis of comparison)[1]:

TABLE VI

		Hits by the drawings of		
		I & II	L.F.S.	Total
On the	I & II	28	2	30
Originals of	VII	30	12	42
	Total	58	14	72

Using the exact method[2] we find P slightly less than ·02.

I have no desire to press this result unduly on those who suffer from a congenital antipathy to precognition, but it is certainly interesting and, in its own way, as elegant a piece of evidence in favour of the phenomenon as any I have come across—and all the better rather than the worse, I think, for its *imprévu* character.

Second: As already noted, I specified (just to make it more interesting) that the Cup should be saucerless. A good number of saucerless cups were drawn, though I am not yet sure whether there were relatively more of these than in earlier experiments. But one percipient drew a cup and saucer, crossed it out, and then drew a saucerless Cup; another drew a saucerless Cup, and added a note saying "wanted to draw a saucer, *but this was inhibited*". (My italics.)

Third: The Stool was represented as a round-topped, three-legged type; one percipient drew, very carefully, a rectangular stool, and *left out one of the four legs.* This may, of course, have been an accident, despite the evident care of the rest of the drawing; but it is pleasantly suggestive of the 'psychopathology of error', and suggests that processes with which we are familiar in everyday life are operative in this field also.

13. *Discussion; Tentative Theory of Paranormal Cognition:* I am all in favour of caution of the right kind and in the proper place;

[1] I use Expts. I & II only because, if there is a real precognitive effect such as we are looking for, it is likely that the drawings of IV & V will show an unduly high proportion of hits on the originals of VII, and this would tend to mask the Leeds precognitions, whereas, if there is none, Expts. I & II are just as good a basis for comparison as any other data. The procedure may seem a trifle arbitrary, but I think it is legitimate in the circumstances, and the whole matter is raised only as a point of interest.

[2] Fisher, *Statistical Methods for Research Workers*, 21·02.

but we so urgently need some kind of a unifying theory in this subject that I feel it would be mere pusillanimity, and not true caution at all, to refrain from describing the conclusions to which these results have led me, just because I may be obliged to revise them later. The true caution will lie in preserving the necessary readiness to do so if ever the evidence requires it.

Accepting, then, for the purposes of discussion, the supposition that paranormal cognition occurred in Expt. VII, as it did in Expts. I to V, if perhaps not to the same degree, its outstanding feature is clearly the complete failure of the percipients to distinguish between the originals produced by their ' own ' experimenter and those produced by others. Just how strange or how natural this will appear to my readers I cannot tell, since it will depend on their preconceptions, with which I am not familiar. But it certainly does not seem particularly surprising to me, for I can find no good reason for postulating any kind of ' rapport ', or any but the most tenuous ' psychological linkage ', between the great majority of the percipients of the first five experiments and myself; and at best, it seems to me, such terms as ' rapport ' and ' psychological linkage ' are little more than disguises of ignorance. Thus it would be unreasonable to expect that any rapport or linkage there might be between individual experimenters and the associated percipients would noticeably facilitate the process involved. On the contrary, I think that, if the various groups of percipients *had* succeeded in, so to say, identifying their ' own ' sets of originals, we should have had the greatest possible difficulty in explaining how they did so.

As it is, there seem to me to be three main types of theory worth considering. I will deal with these briefly in order of implausibility.

First, there is our old enemy the Radiation Hypothesis, the inadequacy of which I—in common with many other writers—have discussed elsewhere.[1] So far as the present series of experiments is concerned I should rule it out of court on the ground that the most spatially remote group of percipients participating were outstandingly successful [2]—a fact which it would be extremely difficult to reconcile with any radiative theory whatever.

Second, we might do something with a kind of Corpuscular Theory, in which some hypothetical entity, to be called ' an Idea ', conceived of as in some manner emanating from the mind of the experimenter might be picked up and assimilated by that of the

[1] For the latest contribution, *cf.* Professor Price's excellent paper " Some Philosophical Questions about Telepathy and Clairvoyance " in *Philosophy*, Oct. 1940.

[2] PNC.D., I, 61–2.

percipient. I should not reject such a theory merely because it sounded a trifle fantastic—for what could be more fantastic than the present-day irreducibles of physics?—and I have no objection to postulating strange hypothetical entities provided we are careful to define them solely in terms of the observations made upon them. Indeed, at one period, I flirted seriously with such conceptions as these, and thought it possible (as indeed I still do) that, given care and a little judicious juggling with Space and Time—and perhaps Number—one might construct from them a not wholly implausible theory, which might even make some useful contribution to orthodox psychology. But I finally discarded them—mainly because I shrink from multiplying irreducibles beyond necessity—in favour of the simpler if perhaps more drastic view which I shall now try to explain.

The third type of theory is of a kind which I shall call, temporarily and for the sake of brevity, the One Mind type. I am quite well aware of the shortcomings of such a name for it, and particularly of the fact that, for all purposes of ordinary discussion, we are dealing with an undoubted plurality of what are usually termed ' minds '. But the essence of any theory of this type is the supposition that, *so far as these phenomena are concerned*, or, if you prefer it, *at the psychological level on which they occur*, things happen as if not *many* minds were involved, but *only one.*

There is nothing startlingly new in this, for plenty of writers, both psychologists and philosophers, have put forward the notion of a common Subconscious or Unconscious, in one form or another. The novel and possibly revolutionary suggestion I have to make is that we should try the effect of taking such proposals seriously and as being literally true within their sphere of relevance, instead of treating them as mere exhibitions of intellectual virtuosity and passing on our way as if they had never been advanced.

Let us do this, and let us further, subject always to the reservation italicised above, examine the suggestion that the phenomenon of paranormal cognition occurring in the joint mind made up of the experimenters' and the percipients' ' sub-minds ' is identical with that familiar phenomenon of the single mind known as The Association of Ideas—to use a slightly antiquated but still serviceable phrase.

I do not propose to enter upon a detailed logical analysis of what this phrase means or ought to mean ; and so far as the term Idea is concerned it will be sufficient for the present purpose to say that by it I refer to the image or images which occupy or approach the field of consciousness when we think of the entity in question—for example, when we think of Boat we probably have an image of some particular (or may be imaginary) boat, while images of waves, beaches, oars, sails, anchors and what not are 'nearer' (*i.e.* more likely to appear in) our field of consciousness than at most other times.

The term Association, on the other hand, demands somewhat more careful discussion. The process which interests us here consists of not less than two parts; first an 'associative' event, in which two ideas, of X and of Y, become as it were linked in our minds by virtue of juxtaposition in experience or otherwise; second a 'revocative' event in which the re-presentation of the object X, say, or the recurrence of the idea of X, recalls or tends to recall the idea of Y. There may, of course, be many associative events, and many more than two ideas may be associated; but the above sufficiently describes, I think, the main outline of what we are talking about. It is important to note, however, that the words 'recalls or tends to recall' must refer to an increase in a probability. We may put this into something like formal shape as follows: Let p_1 be the probability of the idea of Y succeeding, within a period t, the idea of X (or the presentation of the object X) in the mind concerned, *before* the occurrence of the associative event; let p_2 be the precisely corresponding probability *after* the associative event. Then X and Y may properly be described as having been associated by the associative event if p_2 is greater than p_1.

Now, if we consider our experiments in paranormal cognition, we shall see that there is a point to point correspondence between the essential events constituting them and those we have just described, except that the associative and revocative events take place in what are commonly termed 'different' minds; but this difference is just what we have decided to ignore for the purposes of this hypothesis.

Substituting 'Experiment in paranormal cognition' for X and 'any original' for Y, we clearly have a perfectly good associative event whenever an experimenter selects and draws an original; that is to say, the original (or subject matter thereof) and 'the experiment' or experimental situation are juxtaposed in his experience in just the kind of way we know to be favourable for forming an association between the two corresponding ideas. Further, whenever a percipient sits down to try to 'reproduce' an original, we have the elements of a revocative event; that is to say, he is confronted with the experimental situation, with 'the idea of the experiment' in fact, and since his mind is assumed to be identical with that of the experimenter, at the level and for the purposes concerned, it follows from our definition of association that the probability of the idea of Y occurring to him is greater than if the associative event had not taken place.

In other words: *Once we have made the fundamental assumption about the essential unity of the different minds at the relevant level, we need add nothing to what we already know about single minds in order to account for the main phenomenon observed.*

I am not concerned to defend this theory very elaborately, though I personally happen to believe that it is as nearly correct as no matter in its main outline. The question of whether I am 'right' or not, whatever that may mean, is of comparatively small importance; what *is* important is whether the theory enables us to form a tolerably comprehensible picture of what is going on with the introduction of the minimum of additional unknowns, and whether the various features of the phenomenon, so far as they are at present ascertained, fit into that same picture with reasonable neatness and without undue strain.

So far as the second of these points is concerned, I think it is easy to see that the theory covers the main facts very comfortably. The associative act of the experimenter takes place in one part, as it were, of the joint mind, the revocative act of the percipient in another. Each successive preparation and display of an original associates a fresh 'Y' with the experiment ('X'); thus, though it is natural enough from what we know of single-mind psychology that the latest Y's should, on the whole, come most easily to the percipients, it is also not at all surprising that the earlier should also occur though in diminishing frequency; and this accounts very pleasantly for the retrocognitive side of the displacement curve.[1] Further, since we are dealing essentially with *one* mind at this level and not with a plurality, the failure of the percipients of Expt. VII to discriminate between their own and other originals follows almost as a matter of course, for *ex hypothesi* there is nothing except the above-mentioned effect of succession to distinguish them, and the separation in time is small.

As regards the first point, I do not think anyone will complain of the theory on the ground of undue complexity; for I have introduced no anatomically invisible transmitters or receivers, no subconsciously operated tuning appliances, and no automatic decoding devices such as would be necessary on any radiation theory. On the contrary, the objection is more likely to be that, in my desire for simplicity, I have taken too drastic a short cut; but, after all,

[1] It will be interesting to see whether this side of the displacement curve corresponds reasonably well, *mutatis mutandis* if need be, with the curve of oblivescence for the single mind.

we must do something fairly drastic unless we are prepared to reject the facts altogether. Moreover, to say that, at the level concerned, all minds (or all relevant minds) are one, is very much the same thing as to say that at that level all minds are in free telepathic communication; and this is just about as drastic—no more and no less—as Newton's supposition that every particle of matter in the universe attracts every other particle, which was the basis of his theory of gravitation.

I can hardly hope that my theory will enjoy such general acceptance and so honourable a career as his; but at any rate the parallel suggests a good historical precedent for swallowing the camel rather than straining at the gnat.

C. Ten-Grade Scoring of Experiments I to V

ABSTRACT : The Drawings of Expts. I to V were scored on a scale from 10 to 1 against (a) the 50 Originals drawn and used for these experiments, (b) the 50 drawn for Expt. VI, of which only ten were actually used, (c) the 50 drawn and used by the five experimenters of Expt. VII, and (d) 163 words (names of possible originals) which might have been selected for illustration in Expts. VI and VII.

Examination of the results for the 50 Originals of Expts. I to V indicates that markings below three may advantageously be discarded if the most informative data be required.

1. *General: Objects of the Work.* Readers of my last paper will recollect that it was necessary to arrange for the drawings of Expts. I to V and of VII to be scored together against all the relevant originals before we could be sure that Expt. VII was intrinsically successful; even then, the comparative casualness of the scoring led to a result much less emphatic than might have been obtained with more thorough methods. Somewhat similarly, we cannot be sure of properly interpreting Expt. VI until it also has been scored as a whole against other material. A comprehensive set of markings covering all work done up to the present time (about 7,000 drawings) is accordingly highly desirable, and might well prove valuable as a standard of comparison for future experiments.

But, as will be readily understood, the difficulty of finding scorers able and willing to undertake tasks of this magnitude is considerable, and necessarily increases as material accumulates. Moreover, it is easy to see that anyone who has scored any given batch of drawings is *ipso facto* disqualified, so far as 'uninformedness' and consequent absence of bias is concerned, from scoring any other collection of which the first batch forms a part; and this absence of bias is the only reason, of course, for employing an external scorer at all. Thus it is clear that the policy of inducing some kindly outsider to score all drawings against all originals every time a fresh experiment is performed or whenever additional information is wanted, is not one which can in practice be pursued indefinitely.

Fortunately, however, there is no particular reason why it should be. Absence of bias is important in the early stages of the work, when the crucial question of whether the phenomena occur at all is still in doubt; but once this has been settled there is, in general, no greater need for adopting such methods than in any other class of scientific work—and I know of none in which ignorance and inexperience are regarded as desirable qualities.

I do not mean by the foregoing to belittle the great and vital service rendered by Mr Hindson in connection with the first five experiments, or to suggest that the possibility of bias will never again be important, or to contend that my first five experiments have necessarily established the occurrence of paranormal cognition in all minds beyond any peradventure. The point is rather that, if and in so far as they fail to carry conviction, it cannot be on account of bias in the assessment; and that, having eliminated bias in the case in which it would be most likely to produce a serious effect, there is no special need to do so in respect of routine matters where it could hardly be supposed operative at all. In the second part (B) of this paper, for example, I propose to examine the question of whether such factors as Sex, Age, Imagery and Confidence exert any appreciable influence on the degree of success achieved; but such questions, though of some general interest, are insignificant compared with that of whether the phenomena occur at all, and not of a kind to prompt bias in either direction in any ordinary mind. And the same applies to the determination of the quantitative features of the process, such as the rate at which the probability of a non-chance hit being scored falls off with lapse of time after the exposure of an original.

For all purposes such as these, which are matters of *information* rather than of *test*, into which bias could hardly enter, we need the highest quality of data that experience and thoroughness can produce. Above all, we need data scored as nearly as possible on a consistent plan throughout the work, and this requirement at once rules out any idea of having part of the material scored by one judge and part by another.

In the circumstances there was clearly no practicable alternative to doing the work myself; nor, indeed, any reason why I should not do so. On the contrary, although I have tried to make the process as objective as possible, cases of doubt inevitably arise from time to time in which a relatively intimate acquaintance with the material and the course of the experiments is likely to promote a judicious decision.

Superficially, of course, it might be urged that almost anyone's opinion as to the resemblance between a drawing and an original is as good as mine, or anyone else's; but I do not think this is altogether true. Other things being substantially equal, it seems not unreasonable to suppose that prolonged first-hand study of the material is likely to facilitate the formation of an approximately correct theory as to the nature of the process involved. And any policy of scoring that may be adopted necessarily rests logically (if it be logical at all) on the nature of the theory adopted, just as two different opinions will necessarily represent two different theories even though they may not be specifically formulated. Thus the man who would credit a drawing of two tumblers with a full hit on Spectacles, on the ground that it represents A Pair of Glasses,[1] has obviously adopted a different theoretical approach from one who would prefer two adjacent circles because of their likeness of Shape. I myself would reject both, for neither seems to me to afford good reason for supposing that the percipient was thinking of Spectacles at the time of making the drawing.

It will be necessary to discuss the policy and principles of scoring in some detail below; but I think it will be convenient to make first a short digression dealing with the more mechanical aspects of classification and marking.

2. *Classification of 'hits', etc.* In the interests of completeness and with a view to various investigations which I hope to undertake in due course, I decided to make a substantially exhaustive catalogue of all objects, etc., represented in the drawings and to extend the scoring to cover 'hits' not merely on the originals which had actually been used up to date but also on those which, if I may put it so, might have been used but were not. For example, 50 originals, randomly selected from a list of 216 words, were drawn

[1] Acknowledgments are due to Dr E. J. Dingwall for this ingenious suggestion.

by Mrs Lewis for the purposes of Expt. VI, but only ten of these were actually used ; and I wished to be in a position to enquire whether the 40 drawn but not used, which were to some extent associated with the experiment in Mrs Lewis' mind and to a presumably lesser degree in my own, had relatively more hits scored on them by the percipients of this experiment than by those of others. Similar considerations applied to the 166 words listed by me but not selected for illustration by Mrs Lewis, and to the words enclosed by me in envelopes and sent to the five experimenters of Expt. VII but not drawn by them.

In addition to drawings corresponding more or less closely to these originals and words, totalling 313, I had naturally to deal also with a considerable number representing clearly identifiable objects, etc., of other kinds. Finally, there were those geometrical figures and indefinite scrawls, vague lines, and the like, which some percipients seem to delight in producing in face of the most explicit discouragement.

Consideration finally led to the institution of fifteen classes, as given below :

No.	Known as	Constitution	Number of Originals, etc.
1	1	Originals drawn and used in Experiment I -	10
2	2	,, ,, ,, ,, ,, ,, II -	10
3	3	,, ,, ,, ,, ,, ,, III -	10
4	4	,, ,, ,, ,, ,, ,, IV -	10
5	5	,, ,, ,, ,, ,, ,, V -	10
6	6	,, ,, ,, ,, ,, ,, VI -	10
7	6*	Originals drawn for Expt. VI but not used -	40
8	7	Originals drawn and used in Expt. VII, not appearing in the list of 216 words prepared for Expt. VI ; *i.e.* 'fresh' for VII - -	18
9	7a	Originals drawn and used in Expt. VII which did appear in the list of 216 prepared for Expt. VI ; *i.e.* 'potential' for VI - -	32
10	10	Words potential for VI but not for VII ; *i.e.* words appearing in the aforesaid list of 216, but not in the 150 envelopes distributed in Expt. VII - - - - - - - -	63
11	11	Words potential for VII but not for VI ; these, like Class 7, were 'fresh' so far as Expt. VII was concerned, but the envelopes were not selected by the experimenters - - - -	29
12	12	Words which were potential for both VI and VII (*i.e.* appeared both in the 216 list and the 150 envelopes) but were not used in either	71

No.	Known as	Constitution	Number of Originals, etc.
13	M	Miscellaneous Objects, etc., drawn by percipients but not corresponding to any of the above - - - - - - - -	—
14	G	Geometrical diagrams and figures - - - -	—
15	I	Indeterminate shapes and vague scrawls, etc.	—
		Total number of Actual or Potential Originals	313

In case this is not clear, the following points may be noted : The first five classes are self-explanatory. Of the 216 words drawn up in the first instance for the purposes of Expt. VI, 50 were selected and illustrated by Mrs Lewis, ten being actually used (Class 6) and 40 not (Class 6*) : Of the remaining 166, I discarded, so to say, 63 (Class 10), but retained 103 for possible use in Expt. VII ; all these went into the envelopes, but only 32 were actually used by the experimenters (Class 7a) while the remaining 71 rank as potential for VI and VII but not used in either (Class 12) : In preparing the material for Expt. VII, I used 47 fresh words in addition to the 103 taken over from the initial list of Expt. VI ; of these, 18 were used by the experimenters (Class 7) and 29 were not (Class 11).

Thus all varieties of actual and potential originals are suitably segregated into separate classes, so that, if ever we wish, we can easily study the relative frequencies of hits made on them by different categories of percipients. This may be of interest if we have occasion and opportunity to study the effect of varying degrees of association between originals and the experimental situation in the Experimenter's or other minds. Only the first five classes will be discussed here.

This paper contains little but a tedious account of a tedious piece of work, so I will pass lightly over the mechanical details ; all the same, it will be desirable to make the essentials of the procedure clear.

For each set of drawings by any percipient I used a Catalogue Sheet. These sheets were printed on foolscap paper and each was provided with suitable headings for recording the percipient's name, address, age, sex and so forth. In addition, vertical columns were provided for recording the ordinal number of the drawing in the set, the degree of Imagery and Confidence reported by the percipient, the nature of the object, etc., depicted, and the name of the original or potential original which it was judged to resemble. These were followed by fifteen others, headed to correspond with the fifteen classes just described, and two more for recording the total number of entries and the score in any line. Thus, if the third drawing of a set represented unmistakably a fore-and-aft rigged sailing boat, the percipient denying both Imagery and Confidence, the record would read :

					Classes						
No.	I	C	Item	Original	1	2	3	4	5	6	etc.
3	0	0	BOAT	BOAT	.	.	.	.	10	.	.

denoting that drawing number 3 (no Imagery, no Confidence) depicting a Boat has been given full marks (10 out of 10) in respect of the Original BOAT, which is a member of Class 5.

There is nothing novel or exciting here. The only point that matters is that I *specifically wrote down* in the 'Item' column the name of each object depicted, or of every worthwhile constituent of the drawing if the drawing was composite. It would be an exaggeration—but quite a small one—to say that I listed every blade of grass or squiggle of smoke jotted in as trimmings to sylvan or urban scenes ; and I did not, of course, list component parts of wholes (eyes, nose, mouth, hands, feet ; doors, windows, chimney pots, etc.) unless there were some special reason of emphasis or the like for doing so. Apart from the most incidental trivialities, I doubt whether anything at all escaped listing, and I am quite sure that nothing of consequence has as yet slipped through the net.

This procedure is somewhat laborious, but it has the advantage of making it almost impossible to miss straightforward hits, even when so large a number as 313 possible originals has to be considered. Moreover, up to a point, the procedure is highly objective. The drawing represents an 'X' ; we write down 'X' in the appropriate column and, if in doubt, look at the list of 313 possibilities to see whether it contains an 'X' ; if it does, then an entry of some kind must be made in the proper place, and the only question is that of how many points are to be given. Of course, one has to be on one's guard. For example, a percipient might draw a saurian reptile and label it Alligator ; reference to the list would show no Alligator, whereas there is a Crocodile (Class 11) to which it would be unreasonable not to assign marks. However, I do not think I have missed many points of this kind, and am virtually certain (which is much more important) that if I missed them at all I did so quite indiscriminately and not in any preferential manner.

The main features of the entries on these Catalogue sheets were also entered on Index cards which had been suitably printed to receive them. These were arranged by Subjects (*i.e.* names of actual or potential originals) such as Hand, Boat, Tree, etc., so that particulars of all the drawings depicting these are assembled together by experiments under the appropriate heads. From these cards it is easy to read off the number of Boats, say, drawn by the percipients of Expts. I, II, III, etc., or the total score allotted to them, or the number of entries reported as Confident, and so forth ; otherwise, of course, it would be necessary to search through the whole of the Catalogue sheets every time one wanted information of this character, and this would be quite impracticable. The system of dual entry also provided means for applying extremely error-proof checks on the figures.

3. *Policy and Principles of Scoring.* Among other reasons which prompted the undertaking of the work was the desire to apply a more finely graded system of marking than that of giving only 1, $\frac{1}{2}$ or 0 points, as had been done by Mr Hindson, Mr Saltmarsh, Mr Fraser Nicol and Mr Manning-Sanders in the cases in which they had severally cooperated. It will be remembered that I tried the effect of introducing quarter and three-quarter points in Mr Hindson's scoring of Expt. VII ; but it so happened that all the results were null, so that there was no opportunity of studying the differences between the various grades of markings.

There is, of course, no *a priori* reason for adopting any particular number of grades ; for example, the grades α, β, γ are frequently used for certain purposes, and are sometimes elaborated by the use of plus and minus signs. But the decimal system is so familiar as to be preferable to any other in the absence of strong positive reasons against it, and I accordingly decided to adopt it for this purpose. My only misgiving in doing so was to the effect that it might prove too finely graded for convenient use ; but I reflected that it would always be easy enough to convert the results into, say, a five-grade scale, by pooling the 10's with the 9's, the 8's with the 7's, and so on, whereas, if one started with too coarse a gradation, one would have to work through the whole of the material again if one wanted anything finer. Actually, I experienced little difficult in using it.

The important point to note in this connection is that the process is not strictly one of measurement but rather of ranking or grading. I do not think that one can measure resemblance, in the sense that one can measure length or weight, very much better than one can measure beauty or tactfulness ; but one can *grade* resemblances in the sense of being able to say that drawing A is more like original X than is drawing B, and B than C, and so forth. In general, if we had an original and ten drawings, we could arrange the drawings in order of merit, each in the series having less resemblance to the original than the one preceding it but more than the one following ;

but this would not necessarily imply that the best was ten times as good as the worst.

The analogy of temperature is useful here. We could very likely arrange ten jugs of water in the order of their temperatures by sense alone, but no one would contend that the hottest was ten times as hot as the coolest; and even if one measured their temperatures with, say, a Fahrenheit thermometer and found them to be equally spaced at ten-degree intervals from 150° to 60°, it would still be incorrect to say that the hottest is $2\frac{1}{2}$ times as hot as the coolest, because the Fahrenheit scale is not based on absolute zero.

In the case of resemblances we are a good deal better off, for it is easy to define the upper and lower limits of our scale in the shape of a 'substantially perfect' resemblance in the first case and 'no resemblance at all' in the second. If between these two limits we could insert nine drawings at equal intervals of resemblance to the original, and could match all subsequent candidates to one or other of these, *qua* resemblance, we should be achieving something practically indistinguishable from true measurement. This, unfortunately, is impracticable; but what I have said should serve to indicate the kind of process to which the scorer must try to approximate mentally as he does his work. In so far as he succeeds, the 'scores' will approximate to true measurements, but it should not be forgotten that they are primarily indications of rank and not of quantity. Thus, when we speak of giving 10 points (or 'a full mark', or the equivalent) to a drawing, we mean that we regard it as being of the highest grade in its resemblance to the original; when we give 7 points (or 7/10 of a full mark), we imply that we can imagine at least three degrees of resemblance intermediate between it and a 'substantially perfect' resemblance and seven between it and 'no resemblance at all'; if we give only one point (or 1/10 of a full mark) the suggestion is that the drawing could hardly be less like the original and still retain any claim to resemblance whatever.

This all sounds rather elaborate and formidable, but I imagine that something of the kind, even if scarcely formulated, must be at the back of the mind of everyone who undertakes any kind of task involving the grading or ranking of material. The real trouble starts when we begin to consider what quality is to be the basis of our judgments, and how much weight is to be allowed to each if we decide on more than one. In grading fruit, for example, one might, I suppose, base one's judgments on size, colour, firmness, ripeness, or smell, or perhaps on other qualities, while in practice one would presumably use some scarcely formulated combination of all these. Somewhat similarly, in judging resemblances, there are various considerations which might be taken into account, each of which, as I have observed above, depends for its validity on some kind of a theory as to what is likely to occur if paranormal cognition be a fact in nature.

Thus, if we regard Form as all important, we are tacitly assuming that the percipient in some sense *sees* and *copies* the original; if we reckon a drawing of a Bow (archery) as a high grade hit on Bow (ribbon), we suggest that it may be the *word* that is in some manner 'transmitted'; if we pay attention to relationships of the 'Two Tumblers for Spectacles' variety, we clearly entertain the possibility of subconscious *punning*; and if we were to recognise Bow (archery) as a hit on Arrow we should be regarding *association* as likely to play an important part. Other possibilities could no doubt be thought of, while various combinations and elaborations of those just mentioned would be tolerably plausible.

I can perhaps best lead up to the principles which I myself adopted by saying that, to the best of my ability, I paid negligible attention to any of the foregoing. This is not to say that I deny, or even seriously doubt, the occasional occurrence of any of the processes concerned, except the 'seeing' mentioned in the first; on the contrary, I should not be surprised to learn that all of them, and perhaps others also, play a part from time to time. But at present I know of no reason whatever for supposing that they do so more than occasionally and to a secondary extent; on the contrary, the fundamental principle of scoring hitherto—which has led to highly significant positive results—has been "Stick to the obvious; avoid the recondite; give one mark for a palpable hit, nothing for a miss . . ." [1] or "If any drawing plainly represents the same object or activity as that depicted in one of the originals, a 'hit' is recorded; if not not." [2] It may very well be, of course, that processes of the kind I have mentioned above come into operation much more often and more importantly than I at present suspect; but successes based on instructions of the kind just quoted do nothing to encourage us to rely on relationships of the more fanciful types for routine and informative purposes.

Reliance on Form is, of course, entirely a matter of degree, for it is evident that, in general, it is by its form alone that we recognise what a drawing is intended to represent; exceptions occur when the drawing is unrecognisably bad or non-existent and the percipient writes something like "This is intended to be an X" or "Thought of an X, but could not draw it"—X standing for any object. But it sometimes happens that the marks made on the

[1] PNC.D. I, p. 81. [2] *Ibid.*, Appendix IV, p. 142.

paper, though not unmistakably an attempt to represent anything particular, do suggest something ; for example, two equal triangles apex to apex and with bases vertical might suggest either a Butterfly or a Bow (ribbon). I have been extremely chary about paying any attention to this sort of thing at all, and have certainly never looked out for it. I have given a very few low grade (1 or 2) markings in cases where the resemblance forced itself on me—probably not more than six or eight altogether, if so many ; but that is all.

To pure *homonyms* I have paid no attention whatsoever ; that is to say, I have never given anything to Bow (archery) for Bow (ribbon), or in similar cases. The same applies to *punning*, of which no instance has attracted my notice.

The only case I can remember in which I succumbed to temptation in the matter of association was a drawing of Cross-bones which I graded as 4 for Skull ; but there may have been a very few others which escape my present recollection.

There have, of course, been a number of borderline cases in respect of which a severe critic might attempt to convict me of misstatement in what I have just said. For example, when I drew the Spinning Top of Expt. II, I was trying to illustrate the word Spinning which has various applications, so I thought it right to give one or two points to such things as the word Swirl (almost a synonym of Spin) or a drawing of a Spinning-wheel, which would have done just as well as a Top to illustrate the word Spinning.[1] No doubt a few other instances of the same kind of thing could be found ; but my statement to the effect that I have used such relationships to a negligible extent is, I believe, substantially correct, and I have added these explanatory qualifications partly as a precaution and partly on the chance that someone else may wish to score drawings, for comparative purposes, on lines as close to my own as possible.

Turning to the more positive aspects of the matter, the fundamental principle I tried to bear in mind for my guidance throughout may be formulated somewhat as follows : Given that this drawing has a *prima facie* claim to be given points for a certain original, what degree of plain and straightforward evidence does it afford for supposing that the percipient at the time of making it had the same

[1] It is of some interest to record that the percipient of Expt. II who drew a substantially perfect picture of the Spinning Top also produced, in the course of the same experiment, the only example of a Spinning-wheel hitherto noted. This, superimposed on a Flag, curiously enough, was not so good as the Top, but scarcely mistakable. In practice, it is stricken out of the record as a ' replicate ', as explained below.

sort of 'idea' in his mind, or was thinking of the same kind of thing, as the experimenter when he drew the original? This, or something very like it, should be graven on the heart of every scorer, for I can think of no other which both goes to the root of the matter and can be relied upon in all circumstances.

The words "plain and straightforward" are important; without them, the door is left open for the use of every kind of fanciful relationship that the whim or prejudice of the scorer may suggest; none the less, I think it reasonable to enter the proviso that a striking degree of what I may term indirect evidence, as in the examples given above, may be regarded as equivalent to a small degree of "plain and straightforward" evidence. Even so, such indirect evidence, involving relationships of the kind discussed above, should be very sparingly used pending *ad hoc* research as to their genuineness.

At first sight, this rule seems very easy enough to apply, and so it is—up to a point—in the case of single-item drawings, though even here complications soon arise. Let me illustrate from the example of the original Horse (Expt. II).[1] I drew as best I could a Horse standing in side elevation and facing to the left. Now, confining ourselves to single-item drawings only, are we to give full marks (highest grade) to all drawings of a horse, regardless of whether the animal is represented as in side view or facing the observer, standing or galloping, etc.? I think that if we were concerned with an 'all or none' system of marking, or with the 1, $\frac{1}{2}$ or 0 method used by Mr Hindson, the answer should unquestionably be 'Yes', on the ground that "A man's a man for a' that". But when we have plenty of grades to play with, I think not; and my general rule has been never to give 10 points (highest grade) unless there was no reasonable respect (other than skill in draughtsmanship, in which we are not interested) in which the drawing could be improved upon. I found it useful always to bear in mind the possibility that I might come upon another drawing later on which would be *more* like the original (point-to-point correspondence apart) than that which I was considering. Thus I would not give 10 points to a galloping horse, because I could give no more to one that was standing, though the latter would be more like the original than the former. In general, I deducted one point only, or maybe two, for each difference of this kind between the original and the drawing, so that a single-item drawing, recognisably representing the same

[1] As a matter of accuracy, the word actually turned up in the dictionary was Jennet, there defined as "Small Spanish Horse", but the Small Spanish part was ignored.

object as an original, would usually score not less than 7 points.[1] Just what differences should be recognised as warranting deduction, and whether two points should be deducted instead of one, are necessarily matters for personal judgment in particular cases; but there is little room for serious error.

Another 'dimension' of difference, if I may borrow a term, usually quite independent of what I have just discussed, is that of the *kind* of object represented. For example, the Tree drawn as an original in Expt. V was a somewhat 'cabbagy' sort of thing, of more or less deciduous appearance; whereas percipients drew quite a number of palm trees, fir trees, Christmas trees in pots, and other varieties. Deductions were made for differences of this sort on much the same lines as those just discussed.

Another dimension of difference is found in the varying *numbers* of objects represented. A single Tree is drawn as original: What are we go give for Two Trees? For Three Trees, or Four? For Avenues, Woods, Groves, Spinneys, etc.? The proper answer is, I think, much as before; that is to say, deduct points for each step, so to say, by which the drawing differs from the original; but in this case I should cut the points pretty rapidly so as soon to reach vanishing point, for I find it hard to suppose that an impression which starts as a single deciduous Tree would end up as a forest of conifers.

This is closely akin to the very trying matter of composite drawings. Broadly speaking, I treated these as follows: If the object in question (a Tree, say) was Primary in the drawing, *i.e.* manifestly the principal item of interest and attended only by incidental trimmings (*e.g.* grass jotted in in the foreground, a cloud indicated in the background, etc.), it was treated as if it were a single-item drawing; if it appeared as Co-equal with some other object, it would be docked of a point or two on this account; if it was definitely Secondary, another point or so would be deducted; and if it were clearly no more than Incidental, it would only be given one or two points 'for charity'.

This brings us to the lower end of the scale, where the same sort of plan was adopted as at the higher end, but naturally inverted. That is to say, just as I did not give 10 points if I could reasonably expect a stronger claimant, so here I tried not to give so low as one point if I could imagine a worse claimant still having some sort of a claim.

[1] Perhaps I ought to say explicitly here, in case it is not already clear, that the phrases "give (or score) 7 points", etc., are precisely synonymous with "place (or be placed in) the seventh grade", etc.—the tenth grade being the highest.

The foregoing probably sounds complicated enough, and I could easily make it worse by citing the complications peculiar to certain particular originals; but in practice the task is not really so bad as it sounds, for it really reduces to applying the ruling principle about "plain straightforward evidence" with the aid of what I can only call common sense and no more than a minimal modicum of imagination. The 'rules', if I may term them so, about deducting points and the like, cannot be applied in a too rigidly mechanical fashion; but if one keeps them in the back of one's mind for guidance in cases of doubt, and modifies them with one or two more—such as tending to be a trifle more generous with unpopular originals than with popular and a trifle more severe with hits which one knows to be 'winners' than with others, both of which seem sensible principles—one soon finds that one is seldom in doubt as to what points to give.

I need hardly say that I do not regard this as an exhaustive or particularly satisfying exposition of the subject, still less as a comprehensive manual for the guidance of would-be scorers; and I should be sorry to have to stand up to hostile cross-examination as to just why I made every decision. But I felt it necessary to give some indication of the general principles underlying my efforts, of the kind of problems which demand solution in work of this kind, and of the kind of way in which I have tried to deal with them; otherwise my readers might think that I had been guided by nothing but an injudicious mixture of wish-thinking and guesswork.

It is perhaps just worth mentioning that, almost from the start, I had in my mind the idea that the four top grades of 10, 9, 8 and 7 should comprise all the really first-class hits, and that the 1's and 2's should form a kind of rubbish bin for charity and duty points—that is to say, for drawings which one regarded as thoroughly bad candidates (*e.g.* the forests and palm groves) but which could not be discarded altogether with a clear conscience. In view of the results, it may be desirable to repeat that this notion began to take shape almost as soon as I started work and certainly long before I had any notion what the results would be like.

4. *Results :* (*a*) *Introductory.* Before I discuss the results obtained I think we should spend a moment in considering why it is worth while working out results at all; or why, alternatively, we should not just treat the grade numbers as decimals of a hit, tot up the scores, and leave it at that. After all, it might well be suggested, we already know that these five experiments have led to a highly significant positive result when scored under 'test' conditions, and whatever we find now can neither add to nor detract from it.

This is true enough; but we are not here concerned, as we were previously, with the question of whether paranormal cognition occurs at all. Our purpose here is to extract from the raw material the set of data best calculated to throw light on various points relevant to the manner and laws of its occurrence and to serve as a tolerably reliable standard for future reference. If there were any infallible method for distinguishing genuinely paracognitive hits from those due to chance, we should naturally (for many purposes, at any rate) pick out the former and study them intensively. But at present there is not, so the best we can do is to select, as objectively as possible, the data containing the highest proportion of genuine hits and the lowest of irrelevant and merely diluent material.

One reservation must be made here, namely that it might happen that the body of data containing the highest *proportion* of hits was too small, absolutely, to yield significant results in the kind of cases we shall be considering; that is to say, we must effect the best compromise we can between quality and quantity.

Now it should be evident that if we had adopted criteria for scoring other than those which I have tried to describe we might have collected a far larger proportion of irrelevant 'hits' than we are actually likely to have done; for the recognition of sufficiently far-fetched relationships would enable us to claim a 'resemblance' of some kind between almost anything and anything else. The question is whether we have carried generosity too far, and whether, in order to obtain the best material for our purpose, we may profitably eliminate some of the lower grades of hits.

I shall deal with this very shortly, but before doing so one brief digression must be made.

(*b*) *Dereplication.* It was pointed out (in effect) by Mr W. L. Stevens [1] that the method use for testing significance depends for its validity on the assumption that the entries in the 5×5 (or similar) table used are independently distributed, and that this is untrue if any percipient is allowed to score more than a single hit on any given original; consequently, surplus or 'replicate' hits must be eliminated before computation of the data.

This might be done by simply not entering replicate hits on the catalogue sheets at all, *i.e.* by making up our minds by inspection which of a plurality of Trees, say, was the best reproduction of the original and entering that only; but I thought it better to enter every such hit, with its appropriate marking, as if the others were not there, and then strike out those which were not wanted.

[1] *Proc.* S.P.R., XLVI, Nov. 1940, p. 257.

The plan adopted was as follows: If one of the hits was graded higher than the others, I retained this and struck out the remainder; if there were two of equal value, I tossed up as to which to keep; if there had been three, I should have kept the middle one; if four, I should have drawn lots; and so forth.[1]

The figures dealt with in the following section, and henceforward unless expressly stated to the contrary, have all been dereplicated in this way.[2]

In this connection the following point of interest arises. The tendency to draw the same thing more than once might, of course, arise from many causes. It might, for example, represent the symbolic fulfilment of some pent up wish; or the object depicted might merely be one of great normal interest to the percipient, which he could not 'get out of his mind'. But it might also be in some measure a sign of a genuine paracognitive impression. If so, we should expect to find replicated hits (counted, of course, only once each) to show a better result than non-replicated. The fact that they form a significantly positive 5×5 table, when collected and tested in the usual way, does not help us; for we may reasonably expect *any* large enough sample drawn from significant material to be itself significant. But when we examine the proportion of genuine hits (see below, and Appendix I) we find it is so high as 16·09% for the replicated hits, but only 8·80% for the non-replicated—that is to say, almost twice as great. The difference, however, is not significant, for P is almost exactly ·10. But the suggestion that the tendency to repeat is an index of genuineness is fairly strong and very well worth bearing in mind.

(*c*) *Results for different Grades.* We start by collecting all the hits graded 10 (highest grade) into a 5×5 table and testing them for significance in precisely the same manner as described in my first paper;[3] then we do the same for all the hits graded 9; the same for all the 8's; and so on down to the 1's. Next we take the 10's and 9's together; then 10's, 9's and 8's; then 10's, 9's, 8's and 7's; and continue the process, adding in the data for the next lower grade at each step, till all entries are included. The first set of results is shown in the left-hand half of Table I below under 'Separately', and the second set in the right-hand half under 'Cumulatively'.

[1] It would be improper systematically to retain the first, say, or the last, for this might affect any investigation we might wish to make of Displacement within the experiment.

[2] Also in my preceding paper dealing with Expts. VI and VII, as there noted.

[3] PNC.D, I, pp. 90–94 and 132.

After the Grade number, in the extreme left-hand column, each half of the Table shows: N, the number of hits of that grade; D/σ, which has its usual meaning, D being the difference between the observed and expected number of hits in the leading diagonal—*i.e.* on originals of the experiment in which the percipient was working—and σ the standard error; P, the probability of such a difference or a greater arising as the result of chance alone; Q 100D/N, being a measure of ' quality ' introduced in my first paper,[1] and G, the percentage of ' genuine ' hits—*i.e.* hits due to paranormal cognition rather than to chance. The method of obtaining this quantity is given in Appendix I.

TABLE I

RESULTS OF TEN-GRADE SCORING

Main Results

	Separately					Cumulatively				
Grade	N	D/σ	P	Q	G	N	D/σ	P	Q	G
10	103	1·38	·17	5·06	7·47	103	1·38	·17	5·06	7·47
9	83	1·39	·17	5·75	9·94	186	1·91	·06	5·28	7·50
8	89	2·50	·02	10·34	14·77	275	3·05	·01	6·89	9·75
7	106	3·50	·001	12·82	17·69	381	4·33	10^{-4}	8·39	11·58
6	73	·06	·95	·26	·35	454	3·96	10^{-4}	7·01	9·73
5	95	1·39	·17	5·68	7·77	549	4·41	10^{-5}	7·09	9·75
4	84	·95	·34	4·05	5·79	633	4·37	10^{-4}	5·08	9·21
3	146	1·67	·09	5·56	7·65	779	4·82	10^{-5}	6·69	9·22
2	109	·50	·68	1·83	2·67	888	4·71	10^{-5}	6·13	8·41
1	88	−·25	·81	−1·01	−1·36	976	4·49	10^{-5}	5·58	7·63

Supplementary Results

	N	D/σ	P	Q	G
Replications of hits graded > 2 -	73	2·84	·01	11·78	16·09
Score, 10 to 1 - - - - -	522·4	3·89	10^{-4}	6·54	9·77
Grades 10 to 3, omitting 6 - -	706	5·86	10^{-8}	7·35	10·16
Grades 6, 5, 4 and 3 taken together	398	2·39	·02	4·69	6·40

This Table is by no means devoid of interest. It is evident by inspection that the cream of the milk is to be found, as we should expect, in the highest grades—particularly in 8 and 7, which is rather more surprising. Grade 6 shows a terrible slump right down to chance values; 5, 4, and 3 do nearly as well on the average as 10 and 9, by whatever standard they are judged, while my ' rubbish bin ' of 2 and 1 plainly contains nothing but rubbish.

[2] *Loc. cit.*, p. 97.

Superficially, I suppose the most striking feature is the violent contrast between grades 7 and 6. I decline to believe that this remarkable superiority of grade 7 is due to a superstitious veneration for the magic number on my part ; even if it were, one would expect grade 8 to be depleted about as badly as grade 6, which it is not. I have no doubt at all that it is due to the fact mentioned at the end of section 3 above, namely that I had the idea in mind that grade 7 should represent the lower limit for first-class hits ; this is likely to result in my grading as 7 instead of as 6 a certain number of hits (apparently about 16) which I regarded as unmistakable, but which might otherwise have suffered a sufficient deduction of points to bring them down to grade 6. I am quite sure that I did not tend to reduce hits to grade 7 which ought to have been graded 10, 9 or 8. If this be the case, it is odd that there should be such a marked rise in the values of D/σ, Q and G from 10 to 7 ; and doubly so when we reflect that, on general grounds, we would rather expect a falling off.

Clearly we must not attach much importance to this ; but it does look rather as if the most perfect resemblances contained a lower proportion of genuine hits than the slightly less perfect. Very tentatively, I suggest the following explanation, purely as a matter of interest, and on the strict understanding that it is not a matter of demonstrating an effect but of picking up such crumbs of information as the facts seem to suggest.

In many cases the originals drawn were highly conventionalised. If the percipient, for reasons other than that of receiving a genuine impression, thinks " Let's draw an X ", he is likely, I suggest, to draw an ' impersonal ', *i.e.* a conventional X ; but if he does receive a genuine impression, it seems not unreasonable to think of it coming up, so to say, through his subconscious and being subjected to a certain amount of modification or embroidery on the way ; and this might well involve just the kind of differences from the original which would lead me to deduct a few points from the maximum grading of 10. The point is interesting, but should not be taken too seriously.

Reverting to Table I, I think there can be no doubt as to what policy we should adopt in the matter of selecting material for future use. Ignoring, for the moment, the anomalous values for grade 6, we see that the cumulative figures reach maximum significance with the inclusion of grade 3 hits. Grades 2 and 1 are virtually worthless, so we shall certainly discard these as merely tending to obscure any effect we may be looking for. That is to say, when studying particular points of interest, such as sex and age differences, we will use only those hits which have been graded 3 or higher. As a matter of fact, I have no doubt that it would be perfectly legitimate to discard grade 6 also; for its worthlessness has been empirically discovered by a process which is entirely independent of the points we shall wish to investigate. This would leave us with the extremely high quality data referred to in the Supplementary Results as 'Grades 10 to 3, omitting 6'. However, I prefer to keep to windward of criticism, so I propose to retain them. Moreover, as shown in the lower part of the Table, Grades 6 to 3 show a decently significant result, so that the 5's, 4's and 3's must be making quite a useful contribution, and it would be a pity to discard this for no special reason.

The only reasonable alternative would be to use the top four grades only; but these, though of very high quality (G=11·58) are rather small in number—only about half as many as for 10 to 3—so that we should run the risk of missing points of importance through lack of sufficient material to bring out significance.

At this point it might not unreasonably be asked why, having taken all the trouble to separate out the hits into 10 different grades (or 11, counting complete misses) I should now propose to lump eight of them together and treat them all as of equal value. It might seem more sensible to utilise the grading by reckoning the 10's as full hits, the 9's as 0·9 of a hit, the 8's as 0·8, and so on. This may be regarded as equivalent to treating them all as full hits and then weighting them according to their grading. The results of doing this are given in the Supplementary part of the Table opposite 'Score'.

I think the answer is twofold. First: In the present state of the subject we are inevitably more interested in the fact of a hit occurring than in its quality; and I had little or nothing to guide me in forming an opinion as to where on my mental scale of resemblances (if I may use so formal a term to refer to the various considerations discussed earlier in this paper) the frequency of genuine hits was likely to drop to a negligible proportion of the total. Thus it would have been quite impracticable to fix some arbitrary level of resemblance in my mind and then give a full mark to every resemblance above it and nothing to all below; for if I had fixed the level too high I should have wasted valuable material, but if too low there would have been an equally pernicious dilution. The matter was one which had to be dealt with empirically by grading all plausibly possible resemblances as intelligently as I could and seeing whereabouts the critical level did in fact occur. Second: The policy to be pursued will depend on the nature of the problem under investigation. If we are interested in the simple question of, say, whether women are more successful than men, and define a success as the scoring of a hit on one of the originals in the experiment in which the percipient is working, then it would be a mere gratuitous sacrifice of sensitivity not to treat all hits as of equal value. We might also be interested to know whether women score more 'good' hits than men, defining 'good' as 'graded higher than 6'; here we should still treat all hits as of equal value. But in other circumstances it might be desirable to weight the hits according to their merits; then we might use the score down to 3's inclusive, or preferably, I am inclined to think, we might weight the hits according to the probability of their being genuine, *i.e.* proportionally to G. For both the last two purposes, prior grading of the hits is indispensable.

This seems as convenient a point as any to insert two notes almost by way of apology to the reader: (1) In dealing with the non-random distribution of instances of replication, I actually used in the calculation only hits which had been graded higher than 2; but I did not mention this because I had not then shown the reason for discarding lower gradings: (2) Throughout, I have written as if the phrases 'genuine impression' and 'a hit due to paranormal cognition, on one of the originals of the experiment in which the percipient is working' were synonymous; but this is not true unless we choose to make it so by definition, for the percipient might, by genuine paranormal cognition experience a precognitive or retrocognitive impression of an original in some other experiment; this, however, does not affect any issue discussed here.

(*d*) *Comparison with Hindson Results:* There is very little to be discussed under this heading. If we compare results for my gradings from 10 to 3 with Mr Hindson's dereplicated 'All Entries' figures, which seems to be the most appropriate comparison, we find that I have attained an appreciably higher level of significance with the use of appreciably fewer entries. The figures are $D/\sigma = 3{\cdot}803$ and $4{\cdot}82$, $N = 1071$ and 779, for Mr Hindson and myself respectively. That is to say, my procedure appears to have picked out a higher proportion of genuine hits than his; and this is confirmed by the values of G, which are 6·093% for him and 9·217% for me. Since we do not know the variance of G, we cannot test this conclusion directly; but it is easy enough to show that the 10-grade procedure has picked out a higher proportion of 'winners' than Mr Hindson; for in a 2×2 table we have

	Winners	*Losers*	*Total*
Ten-grade, 10 – 3	220	559	779
Hindson All entries	251	820	1071
Total	471	1379	1850

which gives 2·289 for the corrected value of χ^2, with P less than ·03.

Now, apart from the fact that he was only using the grades 1 and $\frac{1}{2}$, while I worked from 10 to 1 (or to 3 for the figures now considered), there seems to have been only one considerable difference in our methods, as judged by examination of a good number of his markings and confirmed in conversation. This is that he paid much more attention than I did to what I may call 'mere form'; in other words, he was much more likely than I to give a mark to a drawing which was more or less the *shape* of an 'X' as opposed to one plainly representing an 'X'. If this be true, as I have little doubt that it is, then we have here a fairly strong suggestion that mere resemblances of shape are not, in general, likely to be genuine hits, or at any rate a good deal less likely than are unmistakable representations. This, for what it is worth, would seem to tell against the view that any process analogous to *seeing* is involved; if one were, we should rather expect that the general form of the original would sometimes be imperfectly 'seen' and reproduced, without being recognised; and, if such quasi-seeing were the rule rather than the exception, resemblances of mere shape should, I think, tend to enhance and not to diminish the effect. The indication is feeble, though worth bearing in mind; but it seems to be supported by the rise in the value in G from grade 10 to grade 7. If quasi-seeing were involved, then surely the highest proportion of genuine hits would fall to those having the clearest 'quasi-eyesight', and these would presumably produce the most faithful reproductions; but this is contrary to the facts.

5. *Summary and Conclusion.* The perpetration of so dull a paper requires some apology. I can only plead necessity. It is not a practical proposition to go on indefinitely invoking the aid of external scorers, however benevolent; nor is it at all certain that even the most careful instructions would always overcome their prepossessions or imbue them with a reasonable attitude.

It was therefore necessary to undertake a comprehensive and systematic scoring of the whole of the available material, partly in order to provide the best possible data for the investigation of particular points of interest, partly for the purposes of future reference.

I did not feel justified in taking the line 'These are my scores;

I have had great experience of these drawings ; therefore these scores are perfect ', or in just dumping them on the reader without comment. I was therefore forced to explain as carefully as I could, and as fully as the limitations of time and space allow, the basic principles which guided me, the collateral considerations which contributed to judgments, and the kind of way in which a scale of grading was formed.

Examination of the results for the 10 grades used shows (*a*) that there is a rise in quality from the 10th to the 7th grade, (*b*) that hits graded less than 2 may profitably be discarded. Data consisting of hits graded 10 to 3, inclusive, will accordingly be used for future work unless otherwise stated.

Only three small points of direct technical interest have been incidentally encountered. 1. There is a certain suggestion that some degree of individual modification accompanies genuine impressions. 2. The ten-grade figures show a higher proportion of genuine hits than the Hindson figures ; the latter rely more on resemblances of mere shape than do the former ; this indicates that quasi-seeing of the original is not likely to play a major part in the process. 3. There is some indication that the tendency to repeat a drawing is a sign of genuineness of impression.

It is hoped to extend the work to Expts. VI and VII.

APPENDIX I

It seems to be often insufficiently realised that not every 'winning' hit, or successful guess, in experiments in this subject is necessarily due to paranormal cognition. This is fairly easily realised in the case of guessing Zener cards, for example; for it is easy to see that chance alone will tend to give an average of one success in every five trials, or five in the standard pack of 25 cards. But, if the percipient gets ten right out of 25, it is not correct to conclude, as might appear at first sight, that five of these ten are due to paranormal cognition. The most probable number is given by the relation $m+(25-m)/5=n$, where m is the number paranormally cognised and n is the number of correct guesses, as was first shown (I believe) by Dr Thouless in his review of Rhine's early work.[1]

Somewhat similarly in the case of drawings we know that people may very well produce drawings more or less closely resembling originals for all kinds of extraneous reasons; and it is incorrect to suppose that the number due to paranormal cognition is given by the excess of observed over expected hits falling in the leading diagonal.

And it should never be forgotten, in this or similar work, that we have no means of telling (at present) *which* of the resemblances observed are 'genuine' and which are fortuitous.

In any table of the kind we have been using, an estimate (on certain assumptions) of the mean proportion of 'winning' hits due to paranormal cognition as opposed to chance may be obtained as follows.

Using a 3×3 table as an example, let us suppose that in each experiment the percipients obtain a certain proportion g of winning hits (*i.e.* hits on the originals of the experiment in which they are working) by virtue of paranormal cognition; let the remaining, chance-determined, hits be distributed between the originals of the three experiments in proportions of which the mean values are k, m and n for Expts. I, II and III respectively. Let a_1, a_2, a_3 be the total numbers of hits scored *by* the percipients of the three experiments, and let b_1, b_2, b_3 be the total numbers of hits scored *on* the originals of the three experiments. This conforms to the previous notation in which the a's and b's are respectively the column and row totals of the table.

Then the number of hits, due to paranormal cognition, made by the percipients of Expt. I on the originals of Expt. I will be a_1g by the above definitions of a and g. This leaves a remainder of $a_1(1-g)$

[1] *Proc. S.P.R.*, Vol. XLIII, Part 139, p. 32.

hits due to chance, and these must be divided between the three experiments in the proportions k, m, n as postulated above. This gives $a_1k(1-g)$, $a_1m(1-g)$ and $a_1n(1-g)$ for the number of chance-determined hits, on the originals of the three experiments respectively, by the percipients of Expt. I.

Similar considerations apply to Expt. II, writing a_2 for a_1 throughout and remembering that the hits due to paranormal cognition must be placed in the appropriate cell; and similarly again for Expt. III.

Then the expected frequencies for the cells of the 3×3 table, on the assumptions made, will be

On the Originals of	Hits by the drawings of I	II	III
I	$a_1g+ka_1(1-g)$	$ka_2(1-g)$	$ka_3(1-g)$
II	$ma_1(1-g)$	$a_2g+ma_2(1-g)$	$ma_3(1-g)$
III	$na_1(1-g)$	$na_2(1-g)$	$a_3g+na_3(1-g)$

It is simple enough to verify that the columns add up correctly to a_1, a_2 and a_3; but the rows must also add up to make the marginal total b_1, b_2 and b_3. And since $a_1+a_2+a_3=\mathrm{N}$, the grand total number of hits, we find

$$\begin{aligned} a_1g+kN(1-g)&=b_1,\\ a_2g+mN(1-g)&=b_2,\\ \text{and}\quad a_3g+nN(1-g)&=b_3, \end{aligned}$$

whence

$$k=(b_1-a_1g)/N(1-g);\quad m=(b_2-a_2g)/N(1-g);\quad n=(b_3-a_3g)/N(1-g).$$

Substituting these values for k, m and n, the expected frequeneies beeome

	I	II	III
I	$a_1b_1/N+a_1g\left(1-\frac{a_1}{N}\right)$	$a_2b_1/N-a_2a_1g/N$	$a_3b_1/N-a_3a_1g/N$
II	$a_1b_2/N-a_1a_2g/N$	$a_2b_2/N+a_2g\left(1-\frac{a_2}{N}\right)$	$a_3b_2/N-a_3a_2g/N$
III	$a_1b_3/N-a_1a_3g/N$	$a_2b_3/N-a_2a_3g/N$	$a_3b_3/N+a_3g\left(1-\frac{a_3}{N}\right)$

and here again the columns and rows will be found to total correctly.

But the first term in each case is easily seen to be the frequency expected on the null hypothesis that there is no paranormal cognition and the distribution of hits due to chance alone. The second terms accordingly represent the changes in the expectations due to adopting the hypothesis that cognition occurs to the extent assumed.

To find g, then, we sum the second terms of the revised expectations in the cells of the leading diagonal, and equate to the difference, D, between the observed number of hits in the leading diagonal in any particular case and the number expected on the null hypothesis. For three experiments this gives us

$$g\left\{a_1\left(1-\frac{a_1}{N}\right)+a_2\left(1-\frac{a_2}{N}\right)+a_3\left(1-\frac{a_3}{N}\right)\right\}=\mathrm{D}$$

or in general $g \,.\, S\left\{a_r\left(1-\frac{a_r}{N}\right)\right\}=D$

which easily reduces to the more convenient form

$$g=\frac{ND}{S\{a_r(N-a_r)\}}.$$

The percentage of genuine hits, *i.e.* the quantity G used in the text, is $100g$.

It should be noted that the foregoing method is based on the assumption that the incidence of hits in cells other than those of the leading diagonals is determined by chance alone; that is to say, the facts of "displacement" discussed in my first paper are ignored.

The quantity G, therefore, though an interesting index to the quality of the data from which it is derived, should be regarded rather as an indication of the extent to which the proportion of paranormally determined hits in the leading diagonal is superior to that in other parts of the table than as a determination of the absolute value of this proportion.

D. Association of Paracognitive Ability with Sex, Age, Imagery and Confidence

ABSTRACT : The figures obtained in the course of C above are used to investigate the question of whether success in the paranormal cognition of drawings is associated with differences of Sex, Age, Imagery or Confidence. No evidence of significant association is found.

1. *General :* The questions of whether Women are more successful at paranormal cognition than Men, Young persons better than Older, those who form visual Images than those who do not, or those who have Confidence in their drawings than those who lack it, are of some general psychological interest. Personally, I do not regard them as of primary importance from the point of view of enlarging our knowledge of the phenomenon as such. If we were to find, for instance, that Women were markedly superior to Men, this would add, I feel, more to our knowledge of Women than to our knowledge of paranormal cognition ; for I do not see what inference regarding the nature of the phenomenon could safely be drawn from the fact.

On the other hand, if marked differences between any or all of these categories were to be found, the discovery might be of considerable practical value. One of our great difficulties in pursuing the subject further is the fact that the effect is very small in the case of ordinary people ; another that we can never be sure that any particular hit is due to paranormal cognition and not to chance, even when we have ample grounds for supposing that a good proportion of the observed hits are not chance-determined. If we knew that some particular quality favoured success, we could choose our percipients accordingly and might reasonably expect to reduce these difficulties by securing a higher proportion of genuine hits.

In any event, if only as a matter of routine, points such as these ought to receive some measure of attention before we pass on to more interesting problems.

In what follows, I have, in effect, taken the proportion of ' winners ' (*i.e.*, hits on originals used in the experiment in which the percipient was working) as the measure of ' ability '. This is arbitrary, for we might equally well take the proportion of ' direct hits ' (*i.e.*, hits made on the original used on the same occasion as that on which the drawing was made) as our criterion, or indeed hits falling within any chosen range of occasions ; and it may well be that at some future stage it will be worth while to enquire whether variations in the different sorts of ability thus definable are associated with factors in which we are interested. But this would take us too far at present, while the criterion adopted conforms with the general method of the first five experiments.

The figures used are those decided on in the earlier part of this paper, namely all hits (treated as of equal value) to which, in my own scoring there reported, I assigned a grade not lower than three.

On general principles, a note of caution should be sounded here. I happen to have taken particulars (in most cases) of four factors which it seemed likely, on general grounds, might be associated with success; but I might have noted innumerable others, from weight or eye colour to knowledge of French or liking for oysters. If we were to test a large number of such factors for association with success, we should expect to find, on the average, that one in twenty attained the ·05 level of significance, one in a hundred the ·01 level, and so on, even if there were nothing but chance at work. Consequently, if we find apparently real associations among those we actually do test, we must be on our guard against the possibility of their being due to chance effects of this kind. I shall return to this point again at a later stage.

2. *Methods:* At first sight, the obvious plan would appear to be to form a simple 2×2 table showing the numbers of Winning and Losing hits scored by Women and Men respectively, say, and calculate χ^2 in the usual way. But this will not do, because it might well happen that there were more Women than Men (or vice versa) in an experiment of which the originals happened to be particularly popular, *i.e.* more likely to be drawn by chance alone; and, if this were so, it would tend unduly to favour in the final result whichever sex was preponderant in that experiment; and similarly for other factors.

Alternatively, we might form a separate 2×2 table for each experiment. This would overcome the trouble just mentioned, but would have two disadvantages; first, if Women did better than Men in some experiments but worse in others, the result would be awkward to combine; second, if there were no appreciable difference between experiments, in this respect, we should be wasting sensitivity in allowing for it.

A more promising scheme would be to work separate calculations for Women and Men, following the procedure of my first paper in each case, and to compare the differences between the observed and expected numbers of Winners in the two cases. This will tell us correctly whether Women do better than Men, Old persons than

Young, etc.; but not whether the superiority is significant. The reason is that Stevens' formula for the variance of the difference is computed on the assumption that the null hypothesis is correct, and that there is no paranormal cognition, whereas we have every reason to believe this to be false, and we do not know what it is when there is a real effect.

The only proper procedure is to analyse the variance in the recognised way. This is a matter of three factors in each case, namely, differences between Experiments, E, differences of 'rightness', R, *i.e.*, whether the hit is a winner or loser, and differences due to the factor, F, with which we are dealing, such as Sex, Age, etc. There are also the interactions between these; that is to say, there will be a higher proportion of winners in some experiments than in others, represented by the contribution of ER, or the interaction of experiment with rightness; there will be a higher proportion of hits (regardless of type) by Women, say, in some experiments, represented by the interaction EF of experiment and sex; and there will be some difference between the proportion of winners scored by women and by men, when everything else has been allowed for, and this, in which we are primarily interested, is represented by the interaction FR of sex and rightness. Finally, there is the second order interaction ERF of all three factors, which constitutes 'error' for our purpose.

These are the simplest analyses possible which are capable of throwing light on the points at issue, and I shall give the figures in each case below. But it is worth noting that a much more elaborate investigation could, in principle, be undertaken if it were thought desirable. We could, indeed, form a six-dimensional table embodying cross-classifications under Experiment, Rightness, Sex, Age, Imagery and Confidence, and investigate not merely the first order interactions mentioned above, but those of higher orders also, such as, say, Sex-cum-Imagery-cum-Rightness. But apart from the fact that this would be extremely laborious, and would involve discarding Expt. II altogether, because no data for Imagery or Confidence were collected, it would hardly be worth while undertaking unless the first order interactions showed significant results, which they do not.

3. *Sex:* Among the 251 sets of drawings dealt with in the first five experiments, 120 were from Women, 106 from Men, while in 25 cases the sex of the percipient was unknown. These were omitted altogether, and this accounts for the discrepancy between the total number of hits recorded here and that given in the first part of the

paper. The sexes were distributed between the five experiments as follows:

Experiment	I	II	III	IV	V	Total
Women	24	7	4	51	34	126
Men	13	13	7	45	28	106
Unknown				9	16	25
Total	37	20	11	105	78	251

Women scored a grand total of 130 Winners to 285 Losers, and Men 76 to 236. This is 3·46 hits per woman, with 31·3% winners, and 2·94 hits per man, with 24·4% winners; but these figures are of no real interest, for the reasons given above.

The full data are given below as an example:

		Women	Men	Total
Expt. I	Win	29	10	39
	Lose	72	29	101
	Total	101	39	140
Expt. II	Win	6	17	23
	Lose	16	41	57
	Total	22	58	80
Expt. III	Win	3	7	10
	Lose	11	18	29
	Total	14	25	39
Expt. IV	Win	34	24	58
	Lose	116	107	223
	Total	150	131	281
Expt. V	Win	58	18	76
	Lose	70	41	111
	Total	128	59	187
	TOTAL	415	312	727

The variance analyses as follows:

Source of Variance	Sum of Squares	Degrees of Freedom	Mean Square
Between Expts. E	8,936·30	4	2,234·075
,, Sexes, S	530·45	1	530·45
,, W & L, R	4,961·25	1	4,961·25
Interaction, ES	2,065·30	4	516·325
,, ER	3,491·50	4	872·875
,, SR	1·25	1	1·25
,, ESR	224·50	4	56·125
Total	20,210·55	19	

The Mean Square for SR, the only quantity in which we are interested is smaller than that for Error. It follows that the data afford no grounds for supposing that sex makes any difference to paracognitive ability.

4. *Age:* To simplify matters, I divided percipients into three age groups, viz. less than 25, from 25 to 44 inclusive, and over 44. This is arbitrary, but it gives a good contrast between the extremes, which is what we want, and happens to fit the peculiarities of the data in a convenient way. In particular, I have no exact information as to the ages of the percipients of Expt. III (members of a Workers Educational Association class) or of the Duke University Group in Expt. IV. Almost certainly, however, all of these belong to the central age group, which, for our present purpose may conveniently be omitted altogether, particularly as its mean age is unlikely to be far from the general mean. The distribution of percipients between age groups and experiments is given below:

Experiment	I	II	III	IV	V	Total
Group A, under 25	26	20		9	22	77
,, B, 25 to 44	4		11	48	28	91
,, C, over 44	7			45	21	73
Unknown				3	7	10
Total	37	20	11	105	78	251

Group A has 3·03 hits per percipient, with 34·8% Winners.
,, B ,, 2·61 ,, ,, ,, 31·6% ,,
,, C ,, 2·02 ,, ,, ,, 40·1% ,,

There is a prima facie suggestion that older persons do slightly better than younger, but it is quite unreliable for the same reasons as before. Analysis of the variance gives

Source of Variance	Sum of Squares	Degrees of Freedom	Mean Square
Between Experiments, E	298·50	3	99·50
,, Age Groups, A	462·25	1	462·25
,, W & L, R	625·00	1	625·00
Interaction, EA	3,310·25	3	1,103·42
,, ER	738·50	3	246·17
,, AR	110·25	1	110·25
,, EAR	686·25	3	228·75
Total	6,231·00	15	

As before, the Mean Square for the interaction in which we are interested (AR) is less than that for Error. The data accordingly afford no grounds for supposing that there is any association of ability with Age.

5. *Imagery:* In all experiments except the second percipients were asked to note on each page of their drawing books whether they experienced good visual imagery; the phrase used was "... a clear picture 'in your Mind's eye'". Most did so, and I classified the hits under the four headings of Clear, Doubtful, None and Unrecorded. The distribution of the hits scored by percipients of the various experiments, omitting hits by the percipients and on the originals of Expt. II, was as follows:

Experiment	I	III	IV	V	Total
Clear	64	15	88	104	271
Doubtful	20	6	39	33	98
None	22	10	68	25	125
Unrecorded	6	1	54	25	86
Total	112	32	249	187	580

The proportions of Winners in the four classes in order are 38·7%, 27·6%, 28·8% and 33·8% respectively. Drawings for which clear visual imagery is reported appear to be slightly more successful than those for which it is doubtful or lacking.

Analysis of the variance gives

Source of Variance	Sum of Squares	Degrees of Freedom	Mean Square
Between Experiments, E	1,805·00	3	601·67
,, Clear & None, I	1,332·25	1	1,332·25
,, W & L, R	812·25	1	812·25
Interaction, EI	248·75	3	82·92
,, ER	298·75	3	99·58
,, IR	4·00	1	4·00
,, EIR	182·00	3	60·67
Total	4,683·00	15	

The central class (Doubtful) has been omitted since it cannot affect the issue, while the fourth (Unrecorded) is clearly irrelevant. Again the Mean Square for the component in which we are interested (IR) is smaller than that for Error, so that the data afford no reason for concluding that the presence of visual Imagery favours success.

6. *Confidence:* As in the case of Imagery, all percipients except those of Expt. II were asked to record whether they felt confidence in their attempt or not. The hits made by the percipients of the

various experiments were distributed with respect to Confidence as shown below :

Experiment	I	III	IV	V	Total
Good	19	10	51	59	139
Doubtful	1		10	7	18
None	82	21	148	98	349
Unrecorded	10	1	40	23	74
Total	112	32	249	187	580

The proportions of Winners in the four groups respectively are 41·0%, 22·2%, 30·9% and 37·9%, suggesting that confident shots were somewhat more successful than others.

Omitting the doubtful group as before, analysis of the variance gives :

Source of Variance	Sum of Squares	Degrees of Freedom	Mean Square
Between Experiments, E	3,969·00	3	1,323·00
,, Good & None, C	2,756·25	1	2,756·25
,, W & L, R	1,560·25	1	1,560·25
Interaction, EC	998·75	3	332·92
,, ER	2,132·75	3	710·92
,, CR	729·00	1	729·00
,, ECR	782·00	3	260·67
Total	12,928,00	15	

In this case, the Mean Square for CR, in which we are interested, is larger than that for Error ; but the effect is far from significant for the Variance Ratio is only 2·80, with P very little less than ·2 and a long way from ·05. The data accordingly furnish no grounds for concluding that there is any appreciable association between success and Confidence.

7. *Discussion :* The position at which we finally arrive is, so to say, doubly null and correspondingly unsatisfactory. We have failed to detect evidence of significant superiority in respect of any of the factors examined ; but I do not think that we are entitled to conclude with assurance that, individually or collectively, they make *no* difference, for our failure might be due either to insufficiency of data or insensitivity of method. Moreover, owing to the somewhat complicated method of investigation forced upon us by data being drawn from several different experiments, it is impracticable even to estimate how great a difference the method would have detected in any given case ; thus we cannot, strictly, even say that the difference, if any, must be very small. On the other hand, I think we may reasonably conclude, speaking provisionally and with all

due reservation, that the differences are unlikely to be very large or very important.

From the purely practical point of view we should probably do well (since any evidence is a better guide than none) to select percipients from among Elderly Confident Image-forming Women rather than from Imageless and Diffident Young Men, if we had an unlimited choice; but there is certainly no warrant for excluding from our experiments any type of percipient so far investigated.

The question of whether we shall regard the provisional nullity of these results as disappointing or otherwise will depend on personal temperament and on the views we hold regarding the kind of way in which paranormal cognition ought to behave. Personally, I do not find them so, because my own impression, for what it is worth, is that paranormal cognition—or at any rate that variety of it with which we are concerned here—is an extremely deep-seated phenomenon, lying at the very foundations of human nature, so that I do not find it surprising that differences of the kind we have been examining here have a negligible effect on its operation. Or we may put it the other way round, if preferred, and provisionally conclude from these null results that, since these differences have no appreciable effect, it must be a very deep-seated process.

Apart from this, the apparent lack of association between success and Visual Imagery deserves, I think, a special word of comment. I find it almost impossible to suppose that such association should be lacking if anything at all resembling *seeing* were involved—if, that is to say, the process were in any reasonable sense *clairvoyant*. So far as it goes, then, I am inclined to note this fact as evidence against the 'clairvoyant' and therefore in favour of the 'telepathic' or 'purely mental' view of the phenomena.

E. Two Proposed Refinements in Technique

1. *Selection of Originals:* It will be remembered that, in all the first five experiments except the second, the words selected for illustration as originals were determined by taking a number at random from mathematical tables, turning up the corresponding page in a dictionary and illustrating the "first reasonably drawable word found on or after that page". In the second experiment, numbered and shuffled cards were randomly inserted between the leaves of a dictionary and a similar procedure then followed. In Expt. VI, I compiled a kind of artificial dictionary from which words were

selected by throwing dice ; and in Expt. VII I wrote suitable words on slips of paper which were placed in separate envelopes and twice shuffled before being picked out by the experimenters.

For the sake of saving trouble and similar reasons I also prescribed a dictionary method, using cards, for use in the independently conducted experiments now being carried out by various experimenters [1] ; but it has two disadvantages and I propose to change it, so far as practicable, in future work.

In the first place, it allows a certain small latitude to the experimenter as regards deciding whether a word is 'reasonably drawable' or not ; and this might, theoretically speaking, lead to more or less topical words being chosen in preference to others. I regard this possibility as in the highest degree academic and there is certainly no evidence for its ever having occurred ; on the contrary, percipients' drawings show a surprisingly small number of bombs, guns, aeroplanes, fire-engines and the like such as one might expect to find at the present time. And I do not think anyone is likely to suggest, in the case of my own five experiments, that Cows and Bottles, Horses, Spectacles, Fans, Scissors, Jugs, Trees or Boats, etc., were more topical in the periods in which they were used than in others. Still, even such remote possibilities may as well be eliminated.

Much more serious from the practical point of view is the fact that meticulously conscientious experimenters are apt to avail themselves *too little* of the latitude conveyed by the words " reasonably drawable " or the equivalent, and so to attempt the illustration of unsuitable words. Experience tends to confirm the view provisionally expressed on page 47 of my first paper to the effect that the originals should represent reasonably familiar objects and should be unambiguous. Too close an adherence to the rules of the dictionary procedure might result in the use of originals representing, say, Orrery, Autoclave, Arybalus or the like, which few percipients would recognise or be able to name even if they saw them ; or again, we might have two or more illustrations of virtual synonyms, such as Bull, Ox, Steer, Bullock, etc., and this also would handicap the technique. This kind of thing has already shown itself in the course of the independently conducted experiments referred to above.

[1] As an informal note of interest in passing, I may say that these independent experiments are shaping very well. Of seven so far received, of slightly different types, which have been scored 'blind' either by the percipients themselves, my wife or myself, all have given positive results. One or two of these strike me as having been somewhat lucky, and I do not expect this rate of success to continue ; but unless something goes seriously wrong, it looks as if we have a reasonable prospect of developing a strictly repeatable experiment in due course.

From every point of view, therefore, the plan of using selected words randomised by shuffling in envelopes before use, is to be preferred wherever circumstances permit. It may even prove desirable, for routine experiments, to adopt a standard list of, say 100 or 200 words from which the number required would be randomly selected for each experiment or part thereof.

2. *Randomisation of Drawings :* In that section of my first paper which was devoted to 'Anticipation of Criticism' I was at great pains to demonstrate (pp. 124–5) that "If there is no real effect, a judge who does not know the answer cannot generate a spurious one by any kind of wishful or misguided marking", and I concluded that ". . . it is literally impossible for any degree of fancifulness, of prejudice . . . or of eccentricity on the part of the judge to generate anything but a chance effect if there is no non-chance relationship between the drawings and the originals."

The argument is sound, and the conclusion correct so long as we mean that it is impossible for the judge to adopt a policy which is *certain* to tend towards a spurious positive result. On the other hand, I have recently discovered that there is a kind of mis-scoring which could tend to produce significant positive results spuriously too often.

In principle, this is akin to the 'replication' effect to which Mr Stevens drew attention,[1] and I am again indebted to him for his share in a stimulating correspondence, but for which I should probably not have pursued the question further.

The gist of Mr Stevens' criticism then was that if a percipient draws the same object (resembling some original) more than once in the course of an experiment, and each drawing is credited with a hit on the original it resembles, then the effect, mathematically speaking, is the same as if more than one percipient had independently scored a hit; and this is not true. Consequently, the assumptions on which the mathematical treatment is based are invalidated, and it can be shown that the result will be to reduce the variance relatively to the difference between the observed and expected numbers of winners, and thus to magnify the apparent significance of whatever result, whether positive or negative, may be obtained. This is dealt with by striking out the replicate hits.

The point to which I wish to draw attention here is that, if the judge wrongfully (*i.e.*, without there being a real resemblance) credits a drawing by percipient B with a hit on original X *because* percipient A, working in the same experiment, has scored a hit on original X, then the same situation will arise ; that is to say, it will

[1] *Proc.* S.P.R., Part 163, Nov. 1940, pp. 256 *sqq.*

appear as if more percipients have independently scored hits on X than have actually done so, and the effect will be the same as that described above. It is, of course, not easy to see how such a tendency could arise, unless we postulate a quite remarkable degree of iniquity or feeble-mindedness on the part of the judge ; but to make the point clear I will illustrate with a somewhat fantastic example.

Perhaps the most striking hit in my first five experiments was that by a percipient of Expt. II on the original Spinning Top used in that experiment. Now suppose the judge had been so impressed by this as immediately to form the conclusion that Spinning Top *must* be one of the originals at which the percipients of that experiment were aiming ; he might then go on to back his opinion (in the hope of generating a positive result) by crediting with a hit on Spinning Top every drawing of Expt. II which could conceivably be thought even remotely to resemble it (or, indeed. others that could not). This would have the desired effect, for it would mean the crediting of the percipients of Expt. II with a number of undeserved hits in the right place, and if it were done on a big enough scale it would ensure that the result was not only positive but significant.

If on the other hand he were to put his money, so to say, on the equally unmistakeable if less well-drawn Anchor, which one of these percipients drew. he would produce or tend to produce a *negative* result, for the Anchor was an original of Expt. I, not of II. And, in general, any such attempt will produce a negative rather than a positive result four times out of five, if there is no real effect—*i.e.*, if a striking resemblance is no true indication that the original is one of those aimed at by the percipient. In fact we have the apparent paradox that a good way of faking a positive result is to do something which you know will produce the required effect only once on the average in every five attempts ! The solution is, of course, that the procedure in question gives you a one in five chance of pulling off, what may appear to be a thousand to one (say) shot ; but it is hardly likely to commend itself in practice to anyone wishing to fabricate spurious results.

The remedy would be to randomise the sets of drawings as well as the originals ; for, in this case. even if the hypothetically unjust judge were right in his assumption that percipient A's drawing of original X was a winning hit, he would not know which were A's co-percipients and so could not apply his assumed knowledge ; and *a fortiori* the effect could not arise unwittingly.

Again, this possibility appears to me to be more of academic than of practical interest, though I think it would be wise to randomise drawings as well as originals before scoring whenever practicable.

As regards my first five experiments, the supposition in question is certainly not applicable at all to the 314 'unmistakeables' for which data are given in the last column of Table 5 (p. 99 of my first paper) unless either Mr Hindson or I, or both, is to be regarded as grossly misrepresenting facts or as pathologically stupid ; and I am confident that the same applies to the hits placed in the top four grades of my own scoring described in III A above. On the other hand, I think it would be unreasonable to deny altogether the possibility that, where less palpable hits are concerned, the judge might be in some minor degree influenced by what has gone before ; in particular, it seems to me, some original might be kept near the forefront of his mind, so to speak, by the fact that a good hit had just been scored on it ; so that, though he would not mark an obvious Tree for Fleur-de-Lys, say, or vice versa, he might be fractionally swayed in favour of one or the other if it were a matter of marking a vague scrawl which might equally well be either. Of course, under the instructions issued, the judge has no business to mark such far from palpable hits at all, and the evidence indicates that Mr Hindson did not in fact do so ; for, even after the cream has been skimmed from the milk by subtracting the 314 unmistakeables, the mean arbitrated figures, in which every item represents at least two independent opinions given under different conditions, still show a significant result.

But although one may argue on these lines, it is not really of importance or of interest to do so, unless we consider, as I do not, that the occurrence of paranormal cognition must forever stand or fall by these experiments alone. My own view is that, while they provide extremely strong immediate evidence in favour of such occurrence and a great deal of useful information, their more important long term value is as experience in the light of which we can devise a sound repeatable technique for routine use by others. On this view, the discovery of minor imperfections such as this, which cannot plausibly be held to invalidate the main results, is a source of satisfaction rather than otherwise.

My most grateful acknowledgments are due to Professor C. D. Broad, Mr E. G. Chambers, the late Mr Oliver Gatty, Dr J. O. Irwin, and Dr R. H. Thouless for much valuable help, encouragement and advice at various stages of the work described in this and the preceding paper.

PROCEEDINGS OF THE SOCIETY FOR PSYCHICAL RESEARCH

PART 166

THE PRESENT POSITION OF EXPERIMENTAL RESEARCH INTO TELEPATHY AND RELATED PHENOMENA

By Robert H. Thouless, M.A., Ph.D.

Presidential Address delivered at a General Meeting of the Society on 1st May 1942.

May I begin by saying how very deeply I feel the honour which this society has done me in electing me as its President. It is impossible to consider the long line of distinguished past presidents of the Society for Psychical Research without a reasonable fear that any address I may make will appear a ragged intruder in such a distinguished series. I think it safest not to attempt to compete with those whose addresses have made important general contributions to thought about psychical phenomena, but rather to limit myself to that part of the field of psychical research and to those methods of study with which I happen to be familiar. The only part of the field with which I can claim more intimate knowledge than acquaintance through books is that generally known as "extra-sensory perception", and the only part of the study of extra-sensory perception with which I can claim familiarity is its study by experimental methods.

If, as an experimental psychologist, I limit myself in my present address to the experimental study of extra-sensory perception, this is not intended to imply that I think that other methods of studying extra-sensory perception, or that fields of study in psychical research other than extra-sensory perception, are unprofitable or unimportant. There seems always in our society a certain measure of disagreement between those whose interests lie in the study of spontaneous cases and those who prefer statistical and experimental methods of investigation. I do not think that there should be any disagreement; the difference is rather a division of interests between two methods of pursuing the same end. In all psychological research the two methods of study are complementary to one another, and neither can be pursued with maximum profit if the other is neglected.

The special function of the study of spontaneous cases is to serve as a guide to the problems to be investigated by experimental methods. It is the clinical experience of disease in the world outside that guides research in the bacteriological laboratory; it is experience of how dogs, cats, and rats behave in our houses and fields that guides (or should guide) research in animal psychology. Neglect of this guidance may lead to sterility and even to absurdity in laboratory experimentation. The worst experimenters in animal psychology are those who seem never to have made the acquaintance of a dog outside their laboratory, and whose work lacks the guidance which would have been given by having a pet dog of their own who could have shown them what were the sensible questions for the experimenter to ask.

So in psychical research, the choice is not between statistics and experiment on the one hand and observation of spontaneous cases on the other. Let us have much more of both. No one would have thought of asking a subject to guess what card has been turned up in a pack, if someone else had not reported spontaneous observations of telepathy or clairvoyance. New problems for experimental investigation may be suggested by new observations of spontaneously occurring phenomena, although also, of course, new problems for experimental investigation may be suggested by experiments themselves.

Still less reason has the student of the experimental approach to extra-sensory perception for throwing doubt on the relative importance of other questions for psychical research, such as that of personal survival after death. I think that few questions can be more important than that of whether we survive the death of our bodies. On the other hand, the experimental techniques acquired in a psychological laboratory do not seem at present to provide a useful way of studying that question. Even if in itself relatively unimportant, the study of extra-sensory perception may have an added importance as a preliminary to the solution of the more difficult question of the evidence for personal survival.

It is generally agreed in discussion of psychical research that if any person can produce correct information on any topic there are three possible explanations in order of increasing intrinsic improbability: (*a*) the information has been gained by normal sensory channels or by rational inference, (*b*) it has been gained by extra-sensory perception, and (*c*) it has been communicated by some discarnate intelligence. It is also generally agreed that evidence for any of these explanations can only be regarded as sufficient if the possibility of the preceding ones (presumed to be less intrinsically improbable) has been excluded. It is obvious that, wherever one adopts such a scale of explanation and such a principle of exclusion, no explanation higher in the scale can ever be established unless the limits of all those lower in the scale are already known. The proof of extra-sensory perception has thus been doubted by those who preferred to suppose that there was an indefinite upward extension of the powers of normal sensory perception (the so-called " sensory hyperaesthesia "). Formally this is obviously correct. If we agree not to regard as evidence for extra-sensory perception any fact that can be explained by normal sensory perception, then it is only by our knowledge of some limits to the possibility of obtaining information by sensory perception that any evidence for extra-sensory perception can be obtained. Explanations of the phenomena of extra-sensory perception by sensory hyperaesthesia have never been generally accepted by psychologists (however sceptical they may have been of the E.S.P. explanation on other grounds) because it has seemed to them that the limits of sensory perception were sufficiently well known, and that in some cases, as, for example, the eye, the limits of possible perception were clearly set by well-known physical factors and that explanations by hyperaesthesia went beyond these limits. Since there is no good evidence for sensory hyperaesthesia in any case, and since, in some cases, well attested facts of extra-sensory perception (such as successful card guessing experiments over long distances) cannot be explained by any extension of hyperaesthesia, the possibility of sensory hyperaesthesia is no longer felt to be a serious obstacle to the acceptance of evidence for extra-sensory perception.

There seems to be no corresponding certainty that we know the limits of extra-sensory perception sufficiently well for it to be possible to regard any evidence for spirit communications as sufficient if we admit the principle that nothing is to be regarded as evidence that might be explained as an effect of extra-sensory perception. The evidence, for example, of the " Lethe " scripts for a communication from Dr Verrall depends on the supposition that the fact that Mrs Willett possessed but had not read the book by Dr Verrall containing the relevant information could not enable her to produce that information by extra-sensory perception.[1] Any argument for accepting such a supposition must remain unconvincing in the absence of any exact knowledge as to what are the limits of extra-sensory perception. It is extremely likely that the possibilities of obtaining information by extra-sensory perception have their own definite limits although these limits are obviously very different from those of sensory perception or rational inference. There also seems every reason to hope that experimental study may one day give us a clear idea of what those limits are. Certainly we do not know yet, but the experimental discovery of the limits of extra-sensory perception has, amongst other things, the value that it may be a necessary preliminary to a satisfactory assessment of the evidence for survival.

I have so far used the term " extra-sensory perception " because that is a name now generally understood. It is not with any enthusiasm for introducing novelties in terminology that I propose now to drop that term and to suggest a new one. The objection to the term " extra-sensory perception " is that it suggests a theory of the nature of the phenomenon in question, and I see no reason to suppose that this is a true theory and some reason for suspecting that it is false. There is some ambiguity in the exact implication of the term (particularly when it is written in the shortened form E.S.P.) since it might be given two very different meanings by the interchange of the hyphen and the space. Thus we might mean " extra sensory-perception " or " extra-sensory perception ".

The first of these is, I think, unobjectionable ; it implies something of a kind not further specified than that it lies outside sensory-perception. This seems to indicate exactly the first objective of experiments on this topic. The experimental subject is asked to give a report on something, such as the turning up of cards, and the set-up of the experiment is directed towards making it certain that he cannot perform the task by means of any

[1] " Some recent scripts affording evidence of personal survival," Rt. Hon. G. W. Balfour, Proc. xxvii, (1914-15), pp. 221-43.

indications provided to him by sensory perception. If he succeeds in the task, we are doing no more than giving an operational definition of the capacity he has displayed if we say that he has succeeded by " extra sensory-perception ", that is, by some means other than sensory perception.

This, however, is not how Rhine (who invented the term) hyphens it, and it is not what he means by it. Rhine's term is " extra-sensory perception ", not " extra sensory-perception ", and that has implications much more questionable. It seems to imply a kind of perception which is outside sensation, that is, which is not occasioned by the operation of sensory cues. That the effects we are speaking of are not occasioned by sensory cues seems clear enough, but are we so sure that it is a kind of perception? Under experimental conditions, a successful subject reports correctly the card turned up more often than he should if his correct answers were merely random. But it appears that he does not know when he is guessing right. That fact alone suggests a wholly different kind of mental operation from that of perception. Can we then accept Osty's term " metagnomy "? I think not for the same reason ; it does not seem to be a kind of knowing. A mere successful reaction not accompanied by any awareness of being right or wrong is not " knowledge " any more than it is " perception ". It must, I think, be admitted that many cases of spontaneously reported telepathy take the form of a mental presentation apparently telepathically determined and that they might, therefore, be considered to be mental events of the same kind as perceptions, but this does not alter the fact that so-called extra-sensory perception need not have this character and that it apparently does not in such cases as card-guessing experiments.

It would be pedantic to object to a misleading terminology unless it, in fact, misleads. I think that this term may be seriously misleading since it leads us to put these effects within a framework of expectations in which ordinary perception is placed. They may belong to a totally different framework, requiring quite different expectations and quite different modes of thought to deal with. Perception lies, for example, within the system of scientific causation, but it may be necessary for our thought to abandon this system of expectations if we are to understand paranormal determination of correct responses.

The same objection may be urged against the earlier terms : " telepathy ", " clairvoyance ", and " precognition ". In Greek, French and Latin respectively, these terms imply that the effects dealt with belong to the class of feeling, of seeing, and of knowing. I do not suppose that the choice of these three different generic terms for these three phenomena was anything but accidental, or that the fact that all three were different meant anything except that those who devised these names felt confusion and uncertainty as to what class of mental fact the phenomena belonged to.

There is, of course, the further objection to these terms that they imply that we have here three different kinds of phenomenon, whereas there may be only one phenomenon in which neither the presence of knowledge in any other person's mind nor restriction of response to the present time is essential. That clairvoyance and telepathy are not separate phenomena is suggested by Rhine's observation that his subjects score about the same whether the experimenter turns up a card and looks at it or simply slips it off the pack without looking at it. Dr Soal has worked with a subject vhose hits were not on the card turned up but on the card one ahead of the ne turned up. In recent experiments I have found a significant excess f hits on the card two ahead. This seems inexplicable on the hypohesis of simple telepathy since the next card but one is no more known y normal means to the agent than it is to the subject. If we use the raditional terms we might call it either " precognitive telepathy " (*i.e.* oreknowledge of something that will be known to the agent in a short me) or " clairvoyance " (of the fact of the card being next but one in the ack although this is not known to the agent), or we might assume teleathic knowledge by the subject of something known by precognition or lairvoyance to the agent.

Alternatively we may adopt the hypothesis that there are not three proesses, but one process, which does not show that restriction to present time nd to present sensory stimulation that is characteristic of the determinaon of right reactions by ordinary sense perception, and which may be nfluenced in some way by knowledge in another person's mind, but which oes not require this as an essential condition for its occurrence. If we dopt this as a working hypothesis, I think we should, for the reasons iven above, avoid the term " extra-sensory perception ", and use some erm that implies no theory as to the kind of mental process this is. I uggest that we should use a term proposed by Dr Wiesner, and call this roup of effects the " psi phenomena ", a term which has the important egative merit that it implies no theory as to their nature.

If experimental psychologists have, on the whole, been slow to accept ne reality of the psi phenomena, this is to be explained partly by the ifficulty of repeating at will the successful results which others have eported, partly by the intrinsic improbability of the phenomena themselves. t is to be hoped that the first of these difficulties will be got over by urther experimentation on the conditions favourable to the appearance of ne phenomena. The literature of the subject contains many hints as to vourable conditions, but these seem generally to be based on the imressions gathered by experimenters in the course of their work rather than n exactly controlled experiments establishing significant differences in cores obtained under one condition and another. Although gathered in ne course of experiments, this evidence is, therefore, largely anecdotal. t is reported, for example, that bodily relaxation and the taking of alcohol re favourable to positive results, while fatigue and illness of the subject re unfavourable. This situation poses, as an important problem to be etermined by experimental methods, the finding by exact methods of the ptimal conditions for the appearance of the psi phenomena. When we now this, it is possible that we shall no longer be dependent on occasional ubjects or on large scale experiments for successful results. It ought o be possible by suitable training of our subjects and by suitably arranged onditions to produce successful results anywhere. When this is possible, ne main rational defence for rejection of the experimental evidence for the henomena will disappear. Anyone will be able to produce the evidence or himself by carrying out the experiments in the right way.

On the intrinsic improbability of the psi phenomena, there seems to be ome difference of opinion. Against those who think the intrinsic imrobability so great that they find themselves unable to be convinced by a weight of evidence far in excess of what would be regarded as decisiv in any other field of research, there are others who seem to find nothin intrinsically unlikely in the facts reported. I should like to suggest th there is a real intrinsic improbability in the psi phenomena, and it ma perhaps be worth while to consider for a few moments what this assertio of intrinsic improbability means.

The psi phenomena are, I think, improbable because they are oppose to an important and well-founded system of expectations, that based o natural science. Scientists occasionally say that the basic principle c science is to follow the evidence wherever it may lead. Undoubtedl there is much to be said for being prepared to be convinced of anythin whatever if there is sufficient evidence for it, but I do not think that it i peculiar to the scientific habit of mind or even particularly characteristi of it. If science had claimed no more than this for itself, I do not think i would have gained the tremendous prestige which it has accumulate during the last three hundred years. I suggest rather that the real strengt of science is that it has claimed to be a method of deciding what kinds c things can happen and what can not. That is how the advance of scienc during the last few hundred years has eliminated belief in magic, i astrology, and all the other things commonly classed as superstitions, no by disproving their reality separately by experimental investigation, but b building up an experimentally based system of expectations which has le men to feel convinced that these are the kind of things that do not happen

If, for example, the scientifically educated man finds himself withou any apparent cause becoming weak or ill, he is not ready to accept th explanation (which he might have accepted if he had lived in the Middl Ages or if he lived in a pre-scientific culture at the present day) that a enemy was working magic against him and had perhaps made a wax imag of him into which he was sticking pins or which he was melting before slow fire. On the other hand, he is ready to accept the scientific explana tion that he is suffering from pernicious anaemia. The remedy he wil expect to be effective is not that somebody should be burned at the stak but that he himself should drink large doses of liver extract.

Yet it is probable that no-one has ever done a properly controlle experiment which has proved that making a wax image of a person an melting it in front of a fire docs not cause that person to fall ill and die To anyone who has within himself the scientific system of expectations such an experiment would be felt to be unnecessary because this is th kind of thing that the scientific system of expectations would lead him t say cannot happen. This system of expectations leads to acceptance o some kinds of alleged causes of illness as effective (such as infections changes in bodily organs, etc.) and others, such as charms and magica practices, as not effective.

If we ask what sort of causes were accepted and what rejected by th scientific system of expectations, the answer in its broad outlines is sufficiently obvious. The essential characteristic of the scientific type o explanation was that one thing could only cause another if there were a continuous chain of physical events between the two. There is the necessary physical continuity between the failure of one's bodily organs t produce a sufficient number of red corpuscles and a wasting disease, but one between the melting of a wax image unknown to the patient and a similar illness. The essential principle is in no wise changed by the fact that a more modern scientist may say that if an Australian aborigine knows that a wax image of himeslf is being melted before a fire, he may produce the symptoms of a wasting disease by suggestion. The chain of continuity of physical causation is then complete, from the fact of the wax image, to the sound waves of a verbal report, to the events in the ear and nervous system of the patient when he hears the report, to the reaction of his bodily organism to that report. The scientist is inclined to welcome such an explanation ; it brings the fact alleged within the system of scientific expectations.

In the same way the facts of ordinary perception, although they may be called wonderful and mysterious, fit very well within the system of expectations of physical science. In visual perception, electro-magnetic waves come from the object to the eye, on the surface of the retina they produce chemical changes which start an impulse along a nerve fibre to the visual area of the cerebral cortex. If we ignore the problem of how a material change in the cerebral cortex is related to the conscious process of perception, and confine ourselves to the physical processes between the emission of rays from a material object and the completion of a muscular or glandular response on the part of the organism, the continuity of the chain of physical events is complete.

If anywhere the chain appears incomplete, it is an accepted method of science to postulate a material event where there appears to be a gap. Let us suppose, for example, that a scientifically educated burglar tries to steal a gold cup protected by a burglar alarm operated by infra-red light. As his hand approaches the cup, a bell rings. His first reaction may be bewilderment ; something has happened which his system of expectations gave him no ground to foresee. If he had touched a wire or a thread, the matter would be simple ; he would suppose that he had established continuity in some way with the bell. But, if he is scientifically educated, he will not fall back on the pre-scientific explanation that might satisfy a savage. He will suppose there is something there, which he cannot see or feel, but which is just as real as a wire or string. Quite likely, remembering what he knows of wireless, he will hit on the correct explanation that his hand has interrupted an invisible series of waves. Then the event will no longer be mysterious ; he will have postulated continuity where a gap appeared in the chain of physical events, and the ringing of the bell will fall within the system of causation to which his scientific education has made him accustomed.

If ordinary visual perception falls within the system of scientific expectations because it preserves the principle of continuity of material causation, the psi phenomenon does not. It is, therefore, inevitable that the first reaction of the scientific mind should be to relegate it to the class of things which science teaches us do not happen. I think it does belong to that class. If so, there are three things to be done about it : (1) to say that the psi effects do not take place and that those who report them are either incompetent experimenters, or frauds, or the unfortunate victims of a vast system of chance coincidences ; (2) to postulate some unknown cause which would reduce the phenomena to a kind admitted by science, as, for example, by postulating some unknown form of radiation ; or (to accept the phenomena as genuine and as not fitting into the system c scientific expectations, and as requiring, therefore, a modification of ou system of expectations.

Before this audience, it is not necessary to argue the impossibility c accepting the first alternative. Even before the laborious and extensiv investigations of Rhine, Soal, and Tyrrel, had produced experimental an statistical evidence of an overwhelming character, it was arguable tha the case was sufficiently proved by sporadic evidence alone. Our distin guished former president, Bergson, in his address to this society, state that he was convinced by evidence of this kind, and Rhine has state that he considered the evidence conclusive for the reality of telepath before his own laborious researches added fresh evidence. There is however, one important advantage for proving the reality of psi tha experimental evidence controlled by a proper use of statistical methods ha over the observation of spontaneous cases and over experimentation t which statistical checks can be less easily applied. This advantage lie in the fact that it is possible to give a figure indicating how unlikely it i that we are being misled by chance coincidences. We may say that a spontaneous case or a series of spontaneous cases shows too many details of correspondence to be explicable by chance. If, however, a sceptic says that he thinks nevertheless that it is due to chance coincidence, we have no reply except to reassert our own interpretation. It is more satisfactory to be able to reply that the odds against a result having occurred by chance is some definite figure—a thousand to one, a million to one, or more. It remains true that some sceptics do say that the odds of billions to one against a chance interpretation of modern experimental results are merely a lucky accident. I do not think that this should be found surprising or particularly disturbing ; no evidence can compel belief in those who are sufficiently determined not to believe. It is still satisfactory to the experimenter that he can produce a figure which is a measure of his opponent's will not to believe.

Odds of more than millions to one against a chance interpretation of results are not particularly interesting since, by the time that limit is reached, the explanation that the whole thing is a conspiracy of fraud becomes at least as likely. That all of the successful experimenters on this subject are deceiving us as to their results or are deceiving themselves is obviously very unlikely, but that too is a possible explanation if we are willing to admit extreme improbabilities rather than the reality of the psi phenomenon. Empirical methods of research can never give certainty ; wildly improbable coincidences are a possibility, and human fallibility is also a possible source of error. All that we can reasonably demand is that evidence for the psi phenomena should be strong enough for reasonable conviction even for an intrinsically unlikely effect. That point has, I think, been passed, and the reality of the phenomena must be regarded as proved as certainly as anything in scientific research can be proved.

Accepting the reality of psi, we may seek to fit it into the existing framework of scientific explanations in the same sort of way as we supposed the burglar fitted in the operation of the infra-red burglar alarm. We may postulate some form of unknown radiation received by some unknown sense organ. Such explanations have had the support of great names in the past (such as Crookes and Ostwald). Against them, we must consider a respect in which the case is not altogether parallel to that of our imagined infra-red burglar alarm. The initial reason for the burglar postulating some unknown form of radiation was to fill in a gap in the chain of physical causation, but, if he stops to experiment, he may find a better reason for confirming this guess. He may find some independent way of showing that there is some radiation occupying the path in which his hand was when the bell rang. If he happened to have a blackened thermo-pile in his kit, he might, for example, trace the path of the radiation by observing its heating effects. The reason for our confidence in the scientific type of explanation is that the entities we postulate to fill such gaps can be shown to have other properties by which their reality can be confirmed. We do not know light waves only by their action on the eye ; if we did there might still be reason for doubting their reality. We know them also by their action on photo-chemical substances, by their heating effects, and so on. An unknown form of radiation of which we have no knowledge except its action in the psi effect and an unknown sense organ similarly known only in this way and undetected by anatomical investigation, lack plausibility as scientific hypotheses and can only be regarded as desperate expedients to save the system of scientific expectations.

There is, of course, also the argument which has often been urged that we know of no kind of radiation which does not lose its effect with distance, and that even if we explain such facts as the success of Rhine's subjects in reading packs of Zener cards " down through " by supposing that some form of radiation proceeded from the cards, it is impossible to see how any form of radiation could enable the subject separately to perceive the cards low down in the pack. Still less is it possible to conceive how any form of radiation could enable a subject to discriminate at a great distance a particular pack from all other packs and all other objects which must be supposed to be sending out similar radiations.

We seem then to be forced to accept the third possibility—that the psi phenomena are real and that they are not explicable in terms of the scientific expectations based on the necessity for physical continuity in chains of cause and effect. This would not, of course, mean that we suppose that the system of scientific expectations is wholly mistaken. That would be absurd in view of the remarkable success which has been achieved in building up a means of controlling the outside world by following that system of expectations. The refinement of optical instruments, the construction and improvement of various types of engines, the control of disease by processes of immunisation and by surgery, are a few only of the practical triumphs of the scientific point of view. They have been attained by attributing to effects the kind of causes which science leads us to expect and by rejecting the kinds of causes that science rejects. It is clear that over a very large field of phenomena, this system of expectations has proved a trustworthy guide. It remains possible that there is also a field in which it is not a trustworthy guide. The psi phenomena appear to be such a field. There may be others. Possibly the human will is one. The denial of the freedom of the human will has been general amongst those accepting the scientific point of view for the last few centuries. But if the principle of continuity as accepted in natural science does not hold for any field, however limited, there remains the possibility that it does not hold in some other field. This opens up disturbing possibilities. Perhaps many would welcome the undermining of an argument against human freedom, but the denial of the universal applicability of scientific expectations seems to open the door to other discarded beliefs, such as the belief in magic, astrology, the evil eye, and other things condemned as superstitions by science. The worst that can happen, however, is that one barrier against these beliefs may go ; there may remain other reasons for rejecting them. If we can no longer accept the scientific system of expectations as an infallible guide, many things become possible that would previously have been regarded as impossible. But not all things become true, and we may still reasonably believe that the things I have mentioned are superstitions without rational foundations ; the only change is that our reasons for rejecting them are somewhat different than they were before.

There is also the very difficult question of how we are to think of psi phenomena if we are not to try to fit them into the framework of the scientific system of expectations. Are we to postulate a type of continuity between cause and effect different from that in the kind of causation recognised by natural science, and if so how are we to think of this continuity? I do not think we are in a position yet to answer these questions. If the way of thinking appropriate to the psi phenomena is to become clear, it must be by more research and by new ranges of speculative thinking. I do not feel myself competent in the field of speculative thinking, and I have no positive suggestions to offer. I have only the negative suggestion to make that the first step is to eradicate from our minds the influence of the deeply ingrained habits of thought which make up the scientific system of expectations. These are deeply ingrained, and they are influencing us when we start thinking in terms of unknown radiations. I think we unnecessarily tempt them to exert their influence when we talk of these phenomena as if they were a kind of perception. It is as part of this negative process of preparation for new ways of thinking that I have suggested the rejection of the term " extra-sensory perception ". As to what are the appropriate ways of thinking about these phenomena, I have no idea. I do not think that we have them yet.

Already I think there are indications in the results of experiments which would guide us in our thinking if we knew how to interpret them. One of the most encouraging signs in the experimental research on this subject at the present time is that we have not merely shown that psi phenomena can be demonstrated under experimental conditions, but that our results are showing odd, unexpected and (at present) inexplicable uniformities which are cropping up independently in different laboratories. No scientific research worker can feel quite so well satisfied with results that come out exactly as he expected them to, as with results which persistently come out as he had no previous reason to expect. Here he feels he is against the hard rock of a reality independent of his expectations ; here is a challenge to his powers of constructive speculation.

For example, Rhine found that if he made his subjects continue their experiments beyond the point at which they were thoroughly bored with them, they began to score persistently below mean chance expectation. At least two other investigators working independently of Rhine have reported the same result. A drop to mean chance expectation would be easy to understand, but a drop that is significantly below mean chance expectation is entirely unexpected. Rhine explains it as an inhibition of the psi function. It may be so ; I shall suggest later that there may be another explanation. In any case, we have here a point at which the experimental facts challenge us to answer the question " Why ?" and when we can answer it we shall be a step nearer to understanding psi.

There is also the discovery, originally made by Mr Whately Carington, of what may be called " temporal dislocation " of responses. He found in his experiments on pictures exposed on successive nights that subjects might not draw a picture resembling the one exposed during the night on which they made the reproduction but one resembling some picture exposed on some other night, or even in some other series of experiments, the probability of such a hit decreasing with the remoteness of the original from the occasion of the reproduction. This observation was not in itself of sufficient statistical significance to carry overwhelming conviction to those who would regard such temporal dislocation as too improbable for belief without coercive evidence. It led, however, to Mr Soal's re-examination of the results he had obtained in his repetition of Rhine's experiments, and, although his intervals were so different from those of Whately Carington (a second or so instead of a day), he found, with some subjects, evidence of correct guessing with temporal dislocation that was of unquestionable significance.

I think that Whately Carington's discovery is of the greatest importance. On the theoretical side, it shows that psi reactions (already proved to be relatively independent of space by Rhine and other experimenters who have obtained successful results over great distances), show also an indefiniteness with respect to time, and that successful psi results may be determined by a future event even when the intention of the experimental subject is to make them refer to the present time.

This discovery also seems to have important consequences for experimental practice. In the past, it has been usual, as in the work of Rhine and Tyrrell, to regard as evidence of psi functioning, the ability to guess correctly the card turned up. Now we can consider that as only a special case of psi activity, and it seems likely that success in psi experiments is even more widespread than is indicated by Rhine's experiments since many people doing a card guessing test may not get appreciably more right than mean chance expectation on the card turned up, but may be consistently getting hits on some other card. Traditional methods of experimenting would erroneously class these as showing no psi success.

Also it is possible that differences between the results of different experimenters may be due to differences between those who get hits on the card turned up and those who do not. For example, Rhine reports that his subjects score equally well under pure clairvoyance conditions and under conditions in which the agent sees the card turned up. Soal does not get success in pure clairvoyance conditions, and I do not. But we were working with subjects showing temporal dislocation and Rhine was working with subjects guessing on the card turned up, and this may be the reason for the difference between our results. The so-called inhibition of psi observed by Rhine and others when subjects have gone on too long at one sitting must also be reconsidered in the light of the fact of temporal dislocation. Suppose that there is no inhibition, but that the result of the continuation of the experiment is that subjects tend to start getting hits one or two ahead of or behind the card turned up. Such temporal dislocation would cause a decrease of score in a pack limited to five cards of each kind such as Rhine uses (though not in a pack of random constitution such as that used by Soal) since a guess determined by some other card would be less likely to be right as judged by the card turned up. An examination of Rhine's score sheets would show whether this was, in fact, taking place. If so, the explanation is quite a different one from that of inhibition.

Another practical point is the desirability, if temporal localisation is possible, of adopting some system of regional scoring, as, for example, by counting hits, not on the card turned up, but on any card within a region of five about the one turned up. This is the scoring method that I am now using. The Zener pack with five cards each of five different kinds is, however, quite unsuitable for such a method of scoring since the mean chance expectation of hits becomes so high as to make the test unworkably insensitive. For this method of scoring, it is desirable to have a larger number of individuals in the pack and consequently a lower mean chance expectation of success. I have, therefore, now returned to the old method of using playing cards, so having fifty-two different individuals in the pack instead of five. Unfortunately it appears as if the rate of psi scoring is decreased as the number of alternative responses is increased, so the greater sensitivity of the playing card pack is partly counterbalanced by a lowered rate of scoring. For many purposes, fifty-two individuals is not enough, and for exact study of temporal displacement, it would be better to have the number of chance determined successes very small compared with those indicating genuine psi success. I am, therefore, experimenting with methods in which the chance expectation is very much smaller than with a pack of playing cards, so far without success.

Another unexpected feature of experimental results is the tendency of temporal dislocation, on the whole, to be in the direction of guessing ahead. I understand that Soal in his latest work has found his subject guessing ahead. In my own experiments, that also is the tendency. If this is general, we must again ask " Why? " Still odder was Soal's finding of a tendency for the guesses just before the card turned up to be below mean chance expectation. I have found indications of the same tendency, although my minimum is not itself significant, and I should attach no importance to it if it had not been more adequately shown by Soal. It looks as if the probability of the response to a card increased to a maximum some seconds before it was turned up, then declined to a point at which it was less likely that that response was given than any other response, as if the subject at the minimum point both knew the card and also knew that it was not a correct response but belonged to the past. Again if we could explain why this is so, we might hope to advance in understanding of the phenomena.

Even the experimental results achieved so far have given us many unsolved problems which give hope of future more complete understanding.

I wish now to turn to another question—that of the aims which experimentalists should now set themselves in studying the psi phenomena. Rhine complained in his first book that, in this field, every experimenter regards it as his task to prove the reality of the phenomena all over again as if it had never been done before. If it was arguably necessary when Rhine started his work, it surely is so no longer. By different methods, a number of workers have obtained under stringent experimental conditions positive results which cannot reasonably be attributed to chance or to experimental error. The work of obtaining these results has been laborious, and great credit is due to those who have undertaken it. A mere repetition of that work now would be a great waste of time. Let us get on to other problems, to be solved by other methods. If we meet with sceptics as to the reality of the phenomena we are studying, let us refer them to the researches of Rhine, of Soal, and of Tyrrell and not succumb to the temptation of trying to satisfy them ourselves.

The reason for calling this a temptation is that the methods appropriate to a research intended to establish the reality of the phenomena are not generally appropriate to a research intended to elucidate the character and the conditions of the phenomena. The investigator seeking to establish the reality of the phenomenon repeats his experiments a large number of times under identical conditions. He aims at getting enormous odds against a chance explanation of his results and is unwilling to introduce variations in method which may be unfavourable to positive results and may therefore reduce the significance of his total score. He also feels it necessary to safeguard himself against critics who will attribute his results to dishonesty or incompetence, so he has one or more impartial witnesses as observers of his experiments whose testimony can establish that he has obtained the results he says he has under the conditions he has described.

Indefinite reduplication of witnessed experiments has been valuable in the past when the primary object of experimenters was to establish the reality of the phenomena. If we agree that this reality has now been sufficiently established, the need for these methods has passed and they should not be allowed to become standardised methods for future experiments. When experimenters have as their primary purpose the understanding and control of the phenomena, frequent variation of conditions is necessary, and those variations will be most fruitful of results which lead to reduction or extinction of the phenomena. Workers must be content with such moderate standards of significance as are used in other branches of scientific research, since the time available for any course of experiments is limited, and time spent in reduplication of experiments under identical conditions is time lost for the more important task of working under variable conditions.

Even the provision of witnesses, valuable for the establishment of the reality of the occurrence of an unlikely phenomenon, may be an impediment to research once that reality has been established. The work of a research worker in any other field (let us say on the psychology of colour vision) would be badly held up if all his experiments had to be witnessed.

Time is limited and possible witnesses with expert knowledge of experimental methods are busy with their own tasks. Insistence on the presence of witnesses must have the effect of reducing frequency of experiment and freedom of variation of condition. Once the reality of the phenomena is taken as established, witnessing is no more necessary than in any other branch of scientific investigation. Undoubtedly there will be error and incompetence in experimental research in this as in all other topics. It will be subjected to the usual check that an erroneous finding by one worker will be corrected by the confirmatory work of others. It is true that not every laboratory has a successful experimental subject available, but already there are probably more independent workers on the psi phenomena than on any other psychological topic, and it is an encouraging fact that even odd and unexpected results are being confirmed by independent workers. The check of independent confirmation is working to a very considerable extent.

It may be objected that although we think the reality of the phenomena is sufficiently established, future ages may not. The early experimentalists were satisfied with results which are now generally regarded as inconclusive. May not future workers adopt more stringent criteria of reality and find our experiments also inconclusive? Certainly they may, and if they do, let them do their own more stringent experiments of verification. Further repetition of the type of experiments for proving the reality of the psi phenomena which we have been doing will not convince future generations if they are not convinced by what has already been done. I wish only to argue that the question of reality has been settled for the present and so far as we can settle it. We must leave to the future the problem of what criteria would satisfy the future.

If we agree that the type of experimental research in the psi phenomena now necessary is one in which we try to find out as much as we can about the phenomena by experiments in which conditions of working are varied as much as possible, we are immediately faced by the practical difficulty which is perhaps the principal obstacle at present to fruitful research along these lines. In any experimental research, mere random variation of conditions of experiment is not enough. We must vary our conditions in such a way as to try to obtain answers to definite questions. The most pressing need to further fruitful research is that we should know what questions to ask. The questions partly arise out of the research itself; they are partly initiated by the propounding of suitable hypotheses.

Thus research on colour vision has been largely directed by the three-colour hypothesis put forward by Young and elaborated by Helmholtz. This hypothesis may be right or wrong, but there can be no question as to its fruitfulness. Researches directed by it have solved many more problems than could have been solved by random or undirected research, although it happens that the problem of whether or not there are three primary colour processes has not yet been solved. It does not matter much now if this hypothesis proves to be wrong. The positive results of investigations inspired by it would remain as evidence of the fruitfulness of the hypothesis.

I think it is a misfortune of our subject that speculation about the psi processes has not been fruitful of problems for research. Speculation there has been in plenty, but not of the kind required by the experimental worker. Speculations, for example, about the psi processes as products of a " subliminal self " may be of genuine value, but they have not this value that they set problems for experimental investigation. It is speculations of a different type that we still need. The test of the speculation which may be expected to be fruitful for the direction of experimental research is that it should lead to clearly defined expectations which may be tested experimentally. Our problem then is to devise an experiment which can prove whether these expectations are fulfilled. If we find they are not, then the hypothesis is proved to be wrong and a step forward has been taken. If they are fulfilled then the hypothesis may be right, and we must find more expectations to test. The fruitful hypothesis is not necessarily the true one. Indeed any hypothesis we think of in our arm-chairs is likely to turn out to be not wholly adequate to the facts. But in the testing of it and the finding out of where it is inadequate, we are achieving our goal of advancing our understanding of the phenomena in question.

It is my impression that we have been lacking in such hypotheses and that consequently much of the research in the psi phenomena has been undirected and unfruitful. It is probably a much more difficult subject to speculate on than is colour vision, and it may demand new and unfamiliar ways of thinking. But let us not forget that it is a need no less urgent than that of continued experimental research. The only limitation of speculation useful in this way is that it should pose definite problems for experimental investigation, that it should lead to experimentally verifiable expectations which we can clearly state.

Let us not be deterred from bold speculation by the fear lest our speculations should be wrong. The process of speculation and experimental testing is a self-correcting one. It does not matter if a speculation is wrong ; if so it will be proved wrong by experiment, and that will be a step forward. The caution which we properly observe in drawing conclusions from our experiments is out of place in the preliminary task of devising hypotheses to be tested.

I cannot, of course, suggest what these fruitful hypotheses may be ; I can only state what has been the hypothesis that has guided my own researches in the psi phenomenon. This is a point of view which has been reached in discussion between Dr. Wiesner and myself and, in what follows, it would be impossible to disentangle the contributions made by us both.

The essential point from which we start is that expressed by Bergson in his Presidential address to this Society. To Bergson, the brain was not an organ whose function is that of transforming material vibrations into mental states but an " instrument of selection charged with choosing, in the immense field of our virtual perceptions, those which are to be actualised ". " I think ", he says, " that we perceive virtually many more things than we perceive actually, and that here once more the part that our body plays is that of shutting out from our field of consciousness all that is of no practical interest to us, all that does not lend itself to our action." Telepathy is the only aspect of the psi function which Bergson considers, and he suggests that this is an action between personalities analogous to the phenomena of endosmosis, and he continues : " If such endosmosis exists, we can foresee that nature will have taken every precaution to neutralise its effect, and that certain mechanisms must be specially charged with the duty of throwing back into the unconscious the presentations so provoked, for they would be very embarrassing in everyday life One or another of these presentations might yet, however, at times pass through as contraband, especially if the inhibiting mechanisms were functioning badly." In other words, Bergson thought of the psi function (or rather the only aspect of it which he considered) as a possible kind of reaction normally suppressed because unserviceable for the demands of practical life, and kept suppressed so long as the organism maintained the attitude of attention to life, but liable to manifest itself under conditions in which that controlling mechanism was thrown out of action by the development of an attitude of inattention to life. It is curious that, having put forward a point of view which seems so suggestive of possibilities for experimental research, Bergson should have shown no interest in this aspect of his hypothesis, but argued instead that it was necessary to start the study of telepathy by the historical and not the experimental method.

Let us adopt Bergson's speculation as a starting point and consider its general plausibility, how we may profitably amplify it, and how it may be used as a guiding hypothesis for experimental research.

First, the plausibility of the point of view must be considered as much greater now than when Bergson put it forward since we are much more inclined to the explanation of mental functions as controlling and limiting more general behaviour possibilities than we were a quarter of a century ago. This conception is, however, still generally an unfamiliar one, and the habit of our minds makes us prone to regard higher mental functions as always extending rather than limiting the possibilities of behaviour and perception. It may, therefore, be worth while to illustrate what is meant by an example drawn from the field of visual perception which is strictly analogous to the relation which Bergson suggests between telepathy and the normal perceptual and intellectual activity of the brain.

If we ask anyone what are the advantages of binocular vision in the higher vertebrates, the answer given in the vast majority of cases is that the use of two eyes with a common field of vision enables its possessor to see depth or distance, and that, without binocular vision, the world would be seen as flat. Yet both common observation and laboratory experiment show that this answer must be wrong. If we close one eye, the world does not look flat ; objects still appear to be at different distances from us. In some cases, the impression of depth is enhanced by using one eye only, as, for example, in looking at pictures which, if they appear flat to two-eyed vision, may appear three-dimensional when looked at with one eye. Moreover, under the simplest conditions of stimulation when the retina of one eye is uniformly illuminated, the impression gained is not that of a flat lighted surface but of looking at a luminous mist with an indeterminate quality of depth. Also, when the efficiency of our visual perception is impaired by brain injury although visual sensations remain, it is not found that the visual world becomes flat. On the contrary, what is lost is the ability to see things as surfaces ; everything appears to have a spongy, indeterminate depth. These facts are inexplicable on the idea that binocular vision causes perception of depth ; they become clear if we adopt the pposite view and suppose that perception of depth is primitive and that he function of the two eyes and of the perceptual process which they erve is to limit the appearance of depth. The idea can be roughly xpressed by saying that the two eyes serve not to enable us to see depth ut to see flatness. The visual perceptual field thus limited is, of course, f more value to the organism possessing it than the original indeterminate epth perception. The limitation of depth perception also means that it ecomes more closely related to real distances in the outside world. There s less depth in a picture looked at with two eyes, but its appearance, herefore, corresponds more closely with the real external fact that it is ainted on a plane surface. By limiting its depth perception by the use f its two eyes, the organism has made its depth perception more useful) it.

Now let us apply this kind of thought to the psi phenomena, acting on ie hints given by Bergson. Let us consider a hypothetical primitive rganism possessing the psi capacity in its most extreme imaginable form, ke Leibniz's monads mirroring all facts in the universe without any mitation with respect to space or time. It would possess a capacity of no ossible biological value to it, since acquaintance with facts is only service-ble so far as it discriminates between near and distant facts and between resent and past or future ones. Also it would be a psi capacity quite ndetectable by any experiment. If we had a subject whose guesses were etermined by the orders of all packs of cards in the universe, past, present nd future, no method of experiment could distinguish his condition om that of a subject with no psi capacity whatever. The possibility of etection of psi determined reactions depends on their being limited in ome degree to a particular pack of cards which is used for scoring. They ıay not be completely limited to that pack, but some degree of limitation necessary to make detection possible.

Our imagination of an organism with completely unlimited psi is, of ourse, purely fantastic. We can suppose that such an organism never kisted, but that any primitive psi function always possessed some degree f limitation. On the other hand, observation and experiment both seem) make it clear that the psi function is less limited in these respects than is ensory perception.

Let us now take a step not taken by Bergson, although it seems to be a atural extension of his thought, and suppose that psi is the primitive way y which organisms oriented themselves to the outside world and that the volution of the sense organs and of sensory perception was a later acquired ıeans, of greater biological usefulness because more limited. Perhaps we ıay take a simple example of what is meant by saying that the more strict mitation in space and time of sensory perception makes it more bio-gically useful than the relatively unlimited psi function. Let us suppose ıat a deer had to rely on its psi function (clairvoyance) to make it aware of ie danger from a tiger. If this psi function worked as it appears to in kperiment, it would combine the advantage of giving warning before the ger came within visual range, with the grave disadvantage of leaving the eer uncertain whether its indications referred to a tiger near by, or to one ıat was two hundred and fifty miles away, and of whether its danger eferred to a tiger in the vicinity now, or to one that would be there tomorrow. The deer would be incomparably better off if he trusted to th strictly limited sense of vision and hearing, when it would be left peacefull feeding until a tiger was near enough for it to be necessary to do somethin about it. The risk that sometimes the tiger would be too near for escap before it was seen would be a small price to pay for the relief from tendency to react which might in the majority of cases be set off by situation in which the danger was remote in space or time.

Or, to take an example nearer home, let us suppose that each one of u present here were so open to psi influences that his thoughts were effec tively determined by those of everyone else in the room. One is perhap wondering whether his clothing coupons will be sufficient for a new suit another is thinking that the speaker is going on too long and that she wil be too late to cook the fish for supper, and so on. If we had such capacity it would not be a wonderfully effective extension of our powers ; it would on the contrary, produce a very ineffective confusion. We are clearl better off if our knowledge of the thoughts of others is limited to th thoughts of the one person who is speaking and so is in auditory communi cation with us. As compared with the possibility of unlimited telepathi powers, this restriction of knowledge of the thoughts of others to thos who are producing the sound waves of speech in our immediate vicinit is a limitation, but clearly a limitation which increases efficiency of inter communication. If we had to choose between the obtaining of informatio by psi and obtaining it through our senses, we should unhesitatingly choos the latter.

This leads us to consider the possibility that the psi function is th earlier in evolutionary history and that it may have been suppressed b the development of the special senses which, with their limitation to what i here and now, serve much more efficiently the biological end of securin the survival and efficient adaptation of the organism. The view her suggested is, of course, radically different from that of F. W. H. Myers who regarded psi as a newly developing human function which woul enormously enhance our range of knowledge and bring us in contact wit the spiritual world. The difference between his view and this may perhap be expressed roughly by saying that he regarded the psi function as human power stretching forward to the angels ; the view that I an suggesting is that it is going back to the amoeba. Rhine too has suggeste that it is a late acquisition in evolution, rather surprisingly, since Rhine is on of the few investigators who has studied the psi function in an animal.

The view that I am suggesting may, of course, not be right ; it is pu forward as an example of a speculation which imposes a number of clea problems for experimental research. So far the expectations it raises seen to be fulfilled. If psi is a more primitive function, normally suppressed b the higher mental activities of perception and reasoning, we should expect on the whole, that conditions favourable to the higher mental activitie would be unfavourable to psi and vice versa. Alcohol is known to b unfavourable to the efficient activity of the higher mental functions ; it i reported to be favourable to psi. The attitude of inattention to the prac tical demands of life induced by muscular relaxation is reported to favour psi, although most people find muscular tension favourable to ordinary cerebral activities. I also have the impression (although I have no experinental proof) that the absence of any effort to guess right is also favourable o psi, although efficiency in any intellectual or perceptual activity requires ustained conation. The standard conditions I have adopted in my own uccessful psi experiments include a moderate dose of alcohol, muscular elaxation, and absence of any effort towards guessing right. The testinony of other workers confirms the effectiveness of the first and second condition, and I think the third is favourable too, but other investigators eem to give no clear indications on the subject.

Clarification of the conditions for psi success is of considerable practical mportance since a knowledge of them may enable us to train subjects to give successful results and, therefore, make psi experiments possible to every research worker who wants to investigate them without it being necessary for him to wait for the lucky chance of finding a suitable subject. The suggestion I am making is that the favourable conditions are those summed up in Bergson's phrase "inattention to life", because these conditions are those in which the higher mental functions are reduced in efficiency so that the more primitive psi function is no longer effectively suppressed. The spontaneously successful psi subjects seem often to be more or less dissociated, and, therefore, to have a pathological attitude of nattention to life. The non-pathological psi subjects may be those who have the power of voluntarily adopting an attitude of inattention to life, a power which probably most people could acquire although we should reasonably expect more difficulty in those subjects who are habitually tense.

There seems to be another indication for experiment in this system of speculations; the possibility that the place to look for psi is amongst the animals low in the evolutionary scale. I know of few researches on this subject, and these are confined to the higher vertebrates. Experiments on invertebrates would be difficult and I do not see how the difficulties can be overcome. I have tried to think of an experiment on a hive of bees (a hopeful place for finding psi phenomena) but the practical difficulties are enormous. Particularly there is the obvious difficulty that we have insufficiently accurate knowledge of the limits of the sensory capacities of animals very remote from ourselves.

May I repeat that the speculations contained in the later part of this address are not to be taken as claimed by me to be true. Perhaps they are flights of fancy with no foundation. Primarily they are intended as guides to the sort of questions we may submit to the test of experimental research. They can be proved to be well or ill founded, not by argument, but by the results of such research.

Finally I should like to suggest that the general result of our survey is that the present position of experimental research in these topics is a hopeful one. Existing researches have not merely proved the existence of the psi phenomena; they have also found out some odd and unexplained things about them that are a challenge to further research. Let us now give up the task of trying to prove again to the satisfaction of the sceptical that the psi effect really exists, and try instead to devote ourselves to the task of finding out all we can about it. With fuller knowledge of its nature, the difficulties of believing in its existence may appear less formidable than they do now.

PROCEEDINGS OF THE SOCIETY FOR PSYCHICAL RESEARCH

PART 167

EXPERIMENTS IN PRECOGNITIVE TELEPATHY

By S. G. Soal and K. M. Goldney

CONTENTS

GLOSSARY

The psi function

Extra sensory perception. A faculty which enables certain persons to obtain knowledge without the use of normal sense perception, or of rational inference based on such sense perception.

Subject

A person being tested for evidence of the *psi* faculty.

Percipient (Sensitive)

A person in whom the *psi* faculty is especially active and/or who is able to bring the results of its activity into the normal conscious field.

Agent

A person from whose mind a Sensitive obtains knowledge by paranormal means (*i.e.*, by the exercise of his *psi* faculty).

Telepathy [1]

The obtaining of knowledge by a Sensitive from the mind of an Agent by means of the *psi* faculty. A telepathy experiment, therefore, involves two persons : the Agent and the Percipient.

Precognitive telepathy [1]

The prehension by a Sensitive, by means of his *psi* faculty, of the *future* contents of the Agent's mind.

Clairvoyance [1]

The obtaining of knowledge by a Sensitive, by means of his *psi* faculty, of some fact in the physical world existing at the moment it is cognised, but which is not in the mind of any other living person (*i.e.*, of an Agent). A clairvoyance experiment, therefore, is one involving a Percipient alone without an Agent.

Deviation

The difference between the score obtained in a guessing experiment and the mean score predicted by the theory of probability. Deviations are *positive* when the observed score exceeds the expected score, *negative* when it is less than the expected score.

[1] N.B.—There are no universally recognised definitions of the terms " telepathy " and " clairvoyance ". The definitions here given indicate the sense in which these terms have been employed in this Report.

Standard Deviation (of a normal or nearly normal distribution)

A deviation, positive or negative, of such numerical magnitude that a deviation of double the magnitude would be equalled or exceeded by chance about once in 21 trials. A deviation of three Standard Deviations, positive or negative, would be exceeded only about once in 370 trials.

Significant score

A score whose positive or negative deviation from the expected value is so large that it cannot reasonably be attributed to chance. Different standards of what is deemed significant are adopted by different workers, but in the present investigation a numerical deviation which approximates to or exceeds three Standard Deviations (say 2·8 or over) is considered significant.

Critical ratio (usually denoted by χ or X).

The deviation (positive or negative) divided by the Standard Deviation. In other words, the number of Standard Deviations contained in a given observed deviation.

Cross-check

An empirical score obtained by comparing a column of guesses with a column of card-presentations with which it was not actually associated in the original experiment. Cross-checks are employed in cases where there is any reason to suspect that neither the guesses nor the presentations are truly random series; they also provide additional confirmation of results obtained by the theory of probability.

P

Probability of getting, by pure chance alone, a positive or negative critical ratio equal to or exceeding the observed critical ratio.

True cognition

A successful guess due to the operation of the *psi* faculty and not to chance coincidence. It is never possible to say which successful hits in a card-guessing experiment are due to the *psi* faculty, but a probable estimate of the number of such hits can be found.

LIST OF ABBREVIATIONS

(These abbreviations are further explained in the text)

(A) = Agent.
(EA) = Experimenter controlling Agent.
(P) = Percipient.
(EP) = Experimenter controlling Percipient.
(O) = Observer.
(PRN) = Prepared Random Numbers.
TP = Telepathy experiments using Pictures.
TA = Telepathy experiments using Associated Words.
TL = Telepathy experiments using initial Letters.
TP/TA = Telepathy experiments with Pictures alternated with Telepathy using Associated Words in sheets of 50 presentations.

TP/TL = Telepathy with Pictures alternated with Telepathy using Initial Letters in sheets of 50 presentations.

TP/CP = Telepathy with Pictures alternated with Clairvoyance using Pictures.

TPN/TPS = Telepathy with Pictures at "Normal" rate alternated with Telepathy with Pictures at "Slow" rate.

E (+1) STEVENS = The Expected number of (+1) hits computed by Stevens' method.

(CC+1) = Empirical number of (+1) hits obtained on the cross-check.

Av. Int. = The average interval in seconds between successive card presentations.

Dev. = Deviation of Observed number from Expected number = O − E.

St. Dev. = Standard Deviation.

χ = Critical Ratio = Dev./St. Dev.

NT = Not timed.

E = Expected number (of hits).

O = Observed number (of hits).

INTRODUCTION

PART ONE

GENERAL

"*The final mystery is oneself. When one has weighed the sun in the balance and measured the steps of the moon and mapped out the seven heavens star by star, there still remains oneself.*"—De Profundis. Oscar Wilde.

THE experiments we are about to describe were conducted over a period of two and a quarter years with a Sensitive who cumulatively demonstrated striking powers of extra sensory perception to an extent which we consider admits of no reasonable denial, and in watertight conditions which we claim admit of no alternative hypothesis.

It was not without considerable discussion that we decided on the bold phrase "Precognitive Telepathy" by which to describe our experiments. Such a title would seem to imply that the authors subscribe to some more or less well-defined theory as to the essential nature of the phenomena they attempt to elucidate. This is true in a limited sense, but such theories as we have suggested are lightly held and we realise that, like most hypotheses in this difficult field, they may have to suffer extensive revision in the light of future knowledge.

We are reasonably convinced, however, that the phenomena belong to that class of unusual mental happenings generally described as "telepathic" inasmuch as they seem to demand the presence of a suitable Agent as well as that of a sensitive Subject: we have found indeed that when some persons try to take the part of Agent the phenomena apparently disappear, and that in one case a change of Agent was followed by a striking variation in the results. And the description "precognitive" telepathy alone indicates the salient feature of the results obtained. We have perforce had to use such terms as are most readily understood to convey the desired meaning, though we are well aware that it is illogical to apply the word "guess" as we have done, for example, to our Subject's powers of card-cognition that we claim show extra sensory perception; and that this word "perception" itself, as Dr R. H. Thouless, our President, pointed out in his Presidential Address,[1] ill describes the operation involved. Such terms serve their purpose well enough until greater knowledge of the phenomena demands a more exact terminology.

All our results were obtained through the medium of one gifted Sensitive, a gentleman who will be referred to as B.S.; and the experiments are experiments in guessing cards with a chance of success equal to one in five.

It is well known to most readers that cards bearing five distinct geometrical symbols, commonly called Zener cards, have been in use for many years in investigations of telepathy and clairvoyance carried out by Dr Rhine and others in America in the psychology departments of Duke and Columbia universities. In many of these experiments a Subject endeavoured to guess the symbol on a card that was being looked at by an experimenter seated behind a screen or in an adjoining room. In some cases it was established beyond reasonable doubt that certain Subjects were able to score over a considerable period many more correct hits than the theory of probability would predict. In these experiments it was the card that was contemporaneously gazed at by the Agent that was, as a general rule, the card on which the Percipient scored his successes. But our own Subject, B.S., possesses the striking idiosyncrasy that it is apparently more natural for him to score a hit, not on the card that the Agent is looking at, but on the cards which immediately precede or follow it in the sequence.

To many readers this may seem strange and improbable, but this strangeness arises in part, we think, from the fact that at the back of our minds we tend to regard extra sensory cognition as only a more mysterious kind of sensory perception.

Visual perception takes place in the "present" moment, or more strictly in a short slab of duration which embraces the "present" moment. Living organisms, as Bergson points out, are supremely interested in the present moment because it is in that moment alone that they react with their material environment by means of their sensory and motor apparatus. Our brains are instruments of action, and normal perception is, according to Bergson, a virtual or incipient action directed towards external bodies. When we look at any object we are exploring the possibilities of action of our bodies upon that object. Thus our normal consciousness is primarily concerned not with the past, or the future, but with the present.

But we have no right to suppose that the extra sensory faculty (or *psi* function as Dr Thouless calls it), operating from a subliminal level, is primarily concerned with the present moment in time or focussed upon our actions and reactions with our material environment. *It* may have no particular preference for the present moment over other moments past or future. We know that mediums will often correctly give incidents

[1] *Proc.* xlvii, 1 ff.

connected with the Sitter's past, and often enough such incidents will be trivial and of no practical importance, indicating perhaps a subliminal functioning very different from the purposeful processes of the conscious mind. If the above surmise as to the nature of the *psi* faculty is correct, it is really rather surprising that Dr Rhine discovered so many Subjects who tended to cognise the card actually in focus at the moment.

It is possible that powers of card cognition are more widespread than has been hitherto supposed and that many persons who fail to score on the actual card are yet successful in cognising certain other cards in the sequence. Such a displacement effect, if it were spread out over a great many positions in the sequence, might easily escape detection by an experimenter who employed cards bearing only five distinct symbols. On the other hand, for Subjects such as B.S., whose faculty of extra sensory cognition seems to be concentrated mainly on positions one or two behind or ahead of the card which is actually in the focus of attention, the use of a random distribution of five cards with five distinct symbols is quite suitable.

The main difficulty that has to be faced in these experiments is not the phenomenon of displacement, but the fact that this displacement is usually towards the future. Moreover it appears to be a real displacement in future time. The simplest hypothesis which will cover the facts is the hypothesis that B.S. somehow becomes cognisant of what is going to be in the Agent's mind about two and a half seconds later. There are possibly other more complicated hypotheses that might be advanced, but none of them has either the clarity or the economy of the one we have just stated.

In many of our experiments the card looked at by the Agent was governed by another experimenter, who drew (by touch) counters from a bag or bowl whose colour determined the card to be looked at. To a small extent, therefore, human volition as well as accident determined the choice of the card which was "foreseen" by the Sensitive (*see* argument p. 53).

Now, many people will be reluctant to admit the possibility of predicting events—or at least certain kinds of events—to whose fulfilment the free-will of human beings is a contributing factor. We can of course make prophecies about the future conduct of a given person upon a basis of our present knowledge of that person's character and habits, and such prophecies may have a high probability of being realised. Today is 21 October 1942 and it would be fairly safe for anyone to predict that 9 a.m. Monday 26 October will find S.G.S. in the Mill Lane lecture rooms at Cambridge. It would be sufficient for the prophet to know (*a*) that S.G.S. is scheduled to lecture at this particular time and place during the present session and (*b*) that he enjoys good health and has not missed a lecture since he came to Cambridge three years ago. But if a clairvoyante were to predict that in the year following the end of the present war K.M.G. would be killed by a falling tile while crossing a street in a storm, and if the prediction were duly fulfilled, it would scarcely be plausible to argue that the prophecy was a deduction based upon the clairvoyante's knowledge of the present state of the universe at the time of the sitting. The concatenation of causes that culminated in the fatal event might involve factors which ranged from an alteration in the courses of the winds produced by the melting of ice in the polar seas, to the victim's decision to shelter for a moment in a doorway and her consequent arrival on the scene of the accident at the precise instant when the tile fell.

However, many well-attested cases have been recorded in which a dreamer foresees an unpredictable future event in his own life in minute and vivid detail as though he were present as an actor or spectator. In such cases the dream seems to be a sort of duplication of a future scene. We need only refer the reader to that admirable collection of first-class cases of " duplicating " prevision made by Mr Saltmarsh from records published in the *Proceedings* and *Journal* of this Society over a period of many years.[1] There are also numerous cases described by Dunne, Osty, Richet and others.

Referring to this kind of precognition Osty writes[2]: " Douze années d'expériences personelles avec un grand nombre de sujets métagnomes et sur un nombre important de personnes, m'ont donné la certitude absolue qu'il est des êtres humains capables de préconnaître le devenir des hommes. De cela j'ai le même degré de certitude que de l'existence de ce que nous appelons la terre, le soleil, les étoiles, les minéraux, les végétaux, les animaux. C'est un fait verifiable par l'expérience et contre lequel ne prévaudront pas longtemps nos préjugés maintenant que des hommes de science ont le courage et la curiosité de se rendre compte."

If future events in the lives of human beings can be foreseen in this way, we can only conclude that in some form or other the future already exists ; and to what extent " free-will " can at the same time remain a reality is indeed a tremendous question.

But for Bergson there can be no prevision of events in which human volitions are involved. The future of a living being cannot be foreseen because it is being *created* at every instant. Real Time, for Bergson, is not the pseudo time which science invented for the measurement of the motion of material bodies, but is the indivisible flow of consciousness itself. It is perpetual change or " becoming ", without any underlying substance that *suffers* change, since all substance *is* change. On this view time is non-spatial and the Space-Time of Minkowski and Einstein is nothing more than a convenient fiction created by the intellect for the purpose of describing the motions of the celestial bodies.

The well-attested facts of " duplicating " precognition cut at the roots of Bergson's theory of time. His theory of memory has great significance in its bearing on the phenomenon of telepathy, but his philosophy of time affords us no help in the study of precognition. Perhaps it was for this reason that Bergson, although once President of this Society, never attempted to work out in detail the application of his philosophy to psychical research and thereby encounter the stumbling block of precognitive phenomena.

More promising for the study of precognition are those theories which regard time as a space-like dimension. Bergson may be right in his contention that the time of experience cannot be truly represented as *space*, but such descriptive hypotheses may be useful even if they are

[1] *Proc.* xlii, 49 ff. [2] *La Connaissance Supra-normale*, p. 177.

fundamentally untrue. The well-known theory of J. W. Dunne is based essentially on the ideas of Hinton, who regarded time as a fourth dimension in which bodies have extension as well as in the three dimensions of space. But Dunne's theory, built up on the conception of an infinite regress of time dimensions, has been criticised by Professor Broad and others. Professors C. D. Broad and H. Habberley Price have put forward a hypothesis to account for the fact of prevision which postulates a second dimension of time at right angles to the first. But we have not the space at our disposal to discuss these theories.

Of the various hypotheses which have been advanced to throw light on the phenomenon of precognition, perhaps that of Mr Saltmarsh has most significance for our experiments. It is expounded by him in his important Paper already cited,[1] and has for its starting point what is known in psychology as the " specious present ". The " present moment " has a definite duration, and an act of perception is not instantaneous but occupies a small slab of duration which includes a bit of the " past " and a bit of the " future ". This interval of duration is known as the " specious present ". Now Mr Saltmarsh supposes that this span of perception is greatly extended at subconscious levels below the threshold of normal consciousness. Thus a future event which is outside the " specious present " of consciousness at the normal level, may yet lie well within the " specious present " of consciousness at a subliminal level. That is to say, an event which is still in the " future " for the normal consciousness is already a " present " event for some stratum of the subliminal mind. In normal persons there may be no mechanism whereby this knowledge at the subliminal level can pass into the normal consciousness, but in the case of psychical Sensitives there may be a kind of osmosis which allows the subliminal content to infiltrate into the normal " specious present ". If this takes place, it will appear as though the Sensitive has precognised a future event. This of course is no more than a skeleton outline of Mr Saltmarsh's conception and does no sort of justice to it. In his Paper he works out the theory in great detail and with much ingenuity.

Yet it is not easy to apply Mr Saltmarsh's theory to the kind of precognition exhibited by B.S. For our Sensitive does not obtain precognitive knowledge of what he is going to see through his own eyes in, say, a couple of seconds' time. What he apparently precognises is what is going to be in the mind of another person in two seconds' time. The phenomenon is not one of simple precognitive perception ; it is enormously complicated by the addition of telepathy. But there does seem to be an intimate connection between the results of our experiments and Mr Saltmarsh's theory of an extension of the specious present.

We have alluded to Bergson's theory of memory. We cannot elaborate this here but, in brief, he postulates two types of memory embodying our individual past : one, the habit or bodily memory which is simply the capacity of our physical body and brain to reproduce certain organised movements by means of motor mechanisms which have been built up within the nervous system ; and the other, what Bergson names the Pure Memory, a psychical memory which represents the conservation of all our past mental states. The Pure Memory must not however be regarded

[1] *Proc.* xlii, 49 ff.

as a mere lifeless and inert record of such past mental states. It must be considered rather as something essentially dynamic and fluid, constantly added to at every moment of our lives, being created at each act of perception, and capable of transmuting itself into a thousand mental images in the present moment. Our past does not survive in the form of mental images: the mental image is a materialisation, an intellectualisation of an unconscious mental state which is nevertheless in a sense its equivalent.

Telepathy, we suggest, is essentially a case of the influence of one Pure Memory by another. It is not, we postulate, the propagation of a mental image from one brain to another, but the development into imagery by a mind (P) of some unconscious equivalent which is in the Pure Memory of a mind (A). The Pure Memory does not occupy space, and the metaphor of "transferring" or "transmitting" an idea is probably an entirely misleading one. If we adopt Bergson's theory of memory we shall never attempt to visualise the telepathic process as a transmission from one brain to another, after physical models like radiation or electric waves. And with the abandonment of these spatial metaphors we shall cease to think of the Agent as an active sender and the Percipient as a passive receiver. It may indeed be the Percipient who is the more active partner in the transaction, since it is he who has to materialise the unconscious memory-states of another. We say "the memory-states of another", but it may, again, be that memory in the unconscious state is not such a private thing as we are wont to suppose. It may be, as William James has suggested, that our memory states pass into a great dynamic reservoir of unconscious mental life. No doubt our individual memories carry with them a seal of personality, and our attention to life and action generally prevents us from materialising the memories of other people; but in conditions not as yet understood it may well be that a Sensitive can materialise the memories of another—much in the same way as he normally materialises his own (however that may be).

The above remarks refer to telepathy in general. But just as there are different varieties of normal mental activity, so research may reveal several widely differing types of telepathic functioning. The processes which take place, for instance, in a case of spontaneous telepathy are probably very different from those brought into play by experiments in card-cognition.

Our experiments, it may be feared, will raise problems rather than solve them. The problem of their interpretation bristles with difficulties. But if it be admitted that we have demonstrated the existence of precognitive powers in our Sensitive, we shall at least have contributed valuable experimental data bearing upon the problem of psychological Time. It is but a beginning. We trust that our report may stimulate others to prosecute similar enquiries. The way is long and tedious, but the prize is the adventure of climbing a new peak in Darien and scanning a fresh horizon.

PART TWO

ORIGIN OF THE PRESENT EXPERIMENTS

By S. G. Soal

I first made the acquaintance of B.S. in the early part of the year 1936. At that time I was carrying out an extensive repetition of Dr Rhine's experiments with Zener cards. During this investigation, which lasted five years, I gave individual tests for telepathy and clairvoyance to 160 persons and recorded 128,350 guesses. The results are discussed in my Paper " Fresh light on Card Guessing—Some new effects.".[1]

Except for one group of Percipients whose work in " Pure Clairvoyance " showed a just significant tendency to score below chance expectation, the results in general appeared at first sight to be in agreement with what the laws of probability would predict. There seemed indeed little evidence of a direct kind that the persons tested, whether considered as individuals or in the mass, possessed any faculty for either clairvoyance or telepathy.

However, in the autumn of 1939, at the suggestion of Mr Whately Carington who was carrying out experiments in telepathy, I re-examined a large number of my records in order to ascertain whether any of the guessers had scored hits, not on the cards focussed by the Agent, but on the immediately preceding or following cards. The two examples given below illustrate the sort of effect I was trying to discover.

(i)	Actual Card	Guess
	+ ↖	O
	W ←	↘ +

(ii)	Actual Card	Guess
	O ←	S
	S ↙	W

(In the above examples the horizontal arrows point to the card which was being looked at by the Agent while the Percipient was making his guess.)[2]

N.B.—Example (i) shows a *post*-cognitive effect, for the successful hit is, as it were, delayed by one place: while example (ii) shows a *pre*-cognitive effect, for the Percipient scores a successful hit before the card is actually lifted from the pack for the Agent to look at.

In my previous Paper I called example (i) a (+1) hit and example (ii) a (−1) hit. But in order to conform with the notation adopted by Dr Thouless, who is himself experimenting along these lines, I now propose to call the postcognitive hits with displacement of one card backwards

[1] *Proc.* xlvi, 152 ff. [2] The symbols used in this diagram are Zener symbols.

(-1) hits, and the precognitive hits with displacement of one card forwards $(+1)$ hits. This reverses the notation employed in my first Paper. Similarly when the successful hit refers to the card two places ahead, I shall call it a precognitive $(+2)$ hit, and when it is made on the card two places behind the "actual" card, I shall call it a postcognitive (-2) hit; thus again reversing the custom of my previous Paper. If we consider time as a linear dimension, it seems more natural to refer to the past as *minus* and the future as *plus* than vice versa.

Among the records of the 160 persons previously tested, two persons were found whose results showed highly significant successes on both $(+1)$ and (-1) displacements. One of these, referred to as Mrs S., obtained in addition a lesser degree of success on the "actual" card looked at by the Agent. Her success on the actual card, however, petered out after the first 1,000 trials while the postcognitive (-1) and precognitive $(+1)$ successes continued to be highly significant over a series of 2,000 guesses. The second person, Mr B.S., the Subject of the present experiments, obtained on the whole no success on the "actual" card—though the first few runs at least suggested that he began by scoring on the actual card but soon switched off to (± 1) hits. B.S. scored about equally well on both precognitive $(+1)$ and postcognitive (-1) guesses. In fact, in a series of 768 $(+1)$ trials he obtained 194 hits, and in the same number of (-1) trials he won 195 hits. These correspond to 3·65 and to 3·74 Standard Deviations respectively.

It will be understood that both Percipients as well as myself (the Experimenter) were totally unaware that any success was being obtained on precognitive or postcognitive hits at the time when the experiments were carried out (1936), for these effects were not even suspected till three years later.

Another extremely interesting effect was observed in the records of both Percipients. It was found that when the card to be guessed—*i.e.*, the "actual" card—was sandwiched between two cards of the same denomination, a greater proportion of (± 1) hits was noted than the theory of probability would predict. In fact it appeared that in cases where both a $(+1)$ success and a (-1) success were *simultaneously possible*, the two effects seemed in some degree to reinforce one another. In my previous Paper I called such guesses "multiply-determined" (MD) guesses. These are illustrated by the following examples:

$$\text{(I)}\ \begin{array}{l} + \\ \text{o} \leftarrow \\ + \end{array} \qquad\qquad \text{(II)}\ \begin{array}{l} + \\ + \leftarrow \\ + \end{array}$$

In each case the arrow points to the card actually looked at by the Agent, before and after which come cards of the same denomination. Previously I counted only the MD guesses as in example (I), but in the present Report we have included both examples (I) and (II) in the same count, since there seems little reason for making a distinction between the two types.

I should like to mention before proceeding that neither of these successful Sensitives was discovered by the blind method of applying routine tests indiscriminately to a large number of persons in the hope that a genuine telepathic Percipient would soon appear. I am convinced now that this is a forlorn hope and that other methods should be adopted in the search for Sensitives. I visited Mrs S. at her house in Richmond only after hearing from her husband that she possessed undoubted psychical gifts. B.S. came to the Laboratory where I was carrying out the tests, after reading an article on the experiments which appeared in a Sunday newspaper. He came because he had been successfully amusing his friends for years by his card-cognition feats and because he felt confident he could demonstrate telepathy. Applying tests at random to all and sundry is, I now believe, wasteful of both time and energy. It is far better to seek out those persons who are reputed among their friends to possess paranormal powers.

Nor do I think—in spite of Rhine's experience to the contrary with his own students—that the English University student is promising material for psychical investigation. Of the 80 odd students tested at University College, there was not one who provided even reasonable evidence that he or she possessed any gift for extra sensory perception, and I believe Dr Thouless's experience agrees with mine. It is among those who cultivate intuition and feeling rather than intellect that we should prosecute our enquiries. I offer these observations since it appears to be the rule that whenever a Professor of psychology takes up psychical research he almost always experiments with students. The mind of the average British student is a more or less logical fact-assimilating machine, which is at opposite poles to the mind of the intuitive Sensitive. And I should like to add that many psychological laboratories are ill-adapted to the study of extra sensory cognition. Too often they are noisy places with students scurrying along the corridors to their class-rooms. The Sensitive demands freedom from distraction, the presence of friendly people who are prepared to adapt themselves to his mental idiosyncrasies, and, above all, the absence of formality or fuss. Neglect of these conditions, which experience has shown to be essential, will lead inevitably to frustration, to the accumulation of negative results and, in the end, to the psychologist abandoning the study of a delicate faculty the laws of whose emergence have not been properly understood.

The experiments of 1936 with B.S., described in Part II of my previous Report,[1] left many questions unanswered. It will be remembered that the Zener cards were lifted one by one from a pack of 25 cards in random order, each card being exposed for a few seconds to the gaze of an Agent who sat with the experimenter on one side of an opaque screen while the Sensitive, B.S., sat on the other side. When B.S. had recorded his guess, he tapped on the table with his pencil and the next card was then exposed. Now when B.S. scored a (+1) precognitive success, it was not clear from what source he was obtaining his knowledge of the card which was one place ahead. At the moment when he made his guess this card was resting face downwards on top of the pack. Was he cognising clairvoyantly the symbol inscribed on this top card, or was he foreseeing what would be in the Agent's (or Experimenter's) mind in a few seconds' time? In the former case *future time* would play no part in the affair and the Percipient's success could be ascribed to an act of clairvoyant cognition

[1] *Proc.* xlvi, 178 ff.

of an event in the present. In the latter case the Sensitive would be truly precognising a future mental event which did not " exist " (in common parlance) at the moment when he wrote down his guess. The 1936 experiments with B.S. or Mrs S. did not allow me to decide between these two alternatives.

Again, was it *necessary* for anyone to look at the cards for the experiment to succeed, or would it have worked equally well if the Experimenter or Agent had merely lifted them one by one from the pack without anyone seeing the symbols on their faces?

In December 1940, having discovered in his previous records the displacement effects described, I sought out B.S. with a view to further experiments. Fortunately I found him reinstated in his Studio after being discharged from the Army owing to ill-health. I thought it advisable to tell him something about the interesting effects we had discovered, but I did not go into much detail as he made no objection to my trying some fresh experiments. He suggested that as he was a busy man the new experiments should take place in his Studio. With the precautions I intended to adopt, there was no objection to this suggestion; and my earlier records had shown that he obtained positive results when the experiments were carried out away from his own premises.

The question I now had to decide was whether the experiments were to be again carried out with Zener cards inscribed with the five geometrical symbols, or whether to substitute a different sort. The Zener cards had the obvious advantage of a clear-cut one in five chance of success, but on the other hand I had, after having used them for five years, become sick of the very sight of the Zener symbols. I decided eventually to effect a compromise by substituting for the Zener cards others bearing five pictures of different animals. I thus retained the great advantage of a one in five chance of success. In war-time, however, it was not easy to obtain large quantities of cards with identical backs, such as I had used previously, and I therefore modified the technique of the experiment so that five cards only would be required instead of the 40 packs used in the earlier experiments.

My colleague, Mrs Goldney (K.M.G.), scarcely needs any introduction to members of this Society, of which she herself has been a member for 16 years. She has travelled widely, has had great practical experience in many branches of psychical research, and is expert in the detection of fraud. We have worked together on many occasions in the past. For more than a year she collaborated with me in the investigation of " Marion ", the well-known vaudeville telepathist. She also organised the physiological experiments carried out in connection with our joint work with the medium Eileen Garrett and contributed a Paper on this subject to Proceedings.[1] Owing to her medical experience she has been specially selected to take a leading part in investigations of physical mediums. Since February 1939 she has held the post of Assistant Regional Administrator at W.V.S. Headquarters in London, covering Region 10 with its 4 counties of Lancashire, Cheshire, Cumberland, and Westmorland.

No one who knows K.M.G. will question either her great ability or her integrity. It was to her that I turned again when seeking a collaborator

[1] *Proc.* xlv, 43 ff.

for the present series of experiments, with the request that she would co-operate with me not only in the conduct of the experiments, but in this Report on them. It is however at her own request that I record that she has not the mathematical qualifications necessary for handling or criticising the statistical methods employed here, and to that extent she has played a subsidiary part in the work entailed. She asks me to say that she would prefer to be considered as my Assistant rather than as a partner, but I am happy to record that I do not share her views on this point.

With this much as my own account of the events leading to the present experiments, we will now resume our joint report.

B.S., our Sensitive, is a photographer, an artist in his profession, with an arresting style and original conception of treatment, well-known for his striking portrait studies. A man in perhaps the late thirties, he has a large Studio in the west end of London. He passed the early part of his life in South Africa with which country his family has connections. Not until he was about 23 years of age did he become aware that he possessed unusual psychical gifts. So far as we can ascertain, he has no particular interest in Spiritualism; nor does he practise automatic writing. He tells us he has on various occasions applied his faculty of intuition to the forecasting of winners in a horse race with much profit to himself. None of his relatives appears to possess a similar paranormal faculty.

B.S. claims to be able to sum up a person's character by a flash of intuition, and his thumb-nail sketches are often very amusing and accurate.

B.S., unfortunately, does not enjoy good health, and suffers from duodenal trouble and the loss of one kidney.

When we gave an account of our experiments with him at a Meeting of the Society on 2 October 1942, the President, Dr Thouless, asked us to convey the best thanks of the Society to B.S. for the splendid way in which he has put both his time and his great gift at our service. It is no small thing that he has been willing over a period of more than two years to persevere in the monotonous task of guessing at the names of five animals and to suffer our intrusions into his Studio. We extend to him our very heartiest thanks.

We have obtained highly significant results with three different Agents, none of whom was a friend of B.S. These we shall refer to as Miss R.E., Mrs G.A., and Mr J.Al. None of the Agents has received any remuneration for his or her assistance and neither has B.S.

We owe a special debt of gratitude to Miss R.E., who worked with us regularly for a whole year. R.E. had previously given valuable assistance with the 1934-1939 experiments after these had been transferred to the psychological laboratory at University College, London. She also undertook an immense amount of work in making various counts in connection with the 1934-1939 experiments—work which she carried out with scrupulous care. All observers who have watched her at work in the present experiments testify to her reliability and accuracy. In addition to acting as Agent, R.E. assisted in the making of the duplicate records which were sent to Professor Broad in Cambridge at the conclusion of each day's work.

Our thanks are also due to the other two Agents, Mrs G.A. and Mr J.Al. Mr J.Al. in particular, though a busy man, made many journeys from South London in order to assist at the experiments.

Our thanks are extended to the numerous persons who often travelled long distances to witness and assist at the experiments. Their names will appear in the course of the Report. Our special thanks are due to Dr E. J. Dingwall for his advice and help; and to Miss Jephson, Mrs Johnstone, Mrs Woollard, Mr Chibbett, Mr Medhurst, Mr Redmayne, Mr Richmond, and Mr Rozelaar, who carried out the complete re-check of all original and duplicate scoring sheets. Mr Gerhard Wassermann, B.Sc., a young mathematician, on some occasions compiled lists of random numbers for use in the experiments, as did also Mr C. U. Blascheck, of Clare College, Cambridge.

And finally we wish to record our appreciation of the interest taken in the experiments by Dr Thouless, who has been ever ready to assist us with his statistical experience and knowledge of experimental psychology.

REPORT ON EXPERIMENTS IN PRECOGNITIVE TELEPATHY

1941—1943

ABSTRACT

In the present Report the authors describe experiments in card-cognition carried out during the years 1941-1943 with a Sensitive, Mr B.S., whose work in 1936 with Zener cards had shown evidence of precognitive and post-cognitive effects.

The bulk of the experiments took place in 1941, mostly at weekly intervals; they were resumed, after a 4 months' gap, in 1942; and after August 1942 three isolated experiments were carried out in the early months of 1943. All experiments to date are included in this Report.

Throughout the present series the Agent and Percipient were in separate rooms, and stringent precautions were taken to eliminate the possibilities of normal leakage, fraud, and collusion.

The material for transmission consisted of five animal pictures, but the experiments were equally successful when, in place of the pictures, cards were substituted inscribed with the initial letters of the animals' names or with " associated " words.

It is shown that the precognitive and postcognitive effects obtained were almost certainly of a telepathic (rather than clairvoyant) character.[1]

B.S. scored highly significant results with three Agents. Superficial tests with ten other Agents were negative. (Some of these were for a few calls only.)

Successful results were obtained using (1) the method of selecting the card to be looked at by lists of random digits (1-5) prepared before the experiment, and (2) the method of selecting the card to be looked at by drawing coloured counters one by one by touch from a bag while the experiment was in progress.

Interesting results were also obtained by the use of non-random lists of digits.

With two of the three successful Agents it was found that, when the time interval between successive card presentations varied from 2·1 to 3·3 seconds (the " normal " rate), significant precognitive (+1) successes were scored;

[1] See Glossary, p. 22.

while with the third Agent both significant precognitive $(+1)$ and significant postcognitive (-1) hits were obtained. The direct hits on the whole series show no significance.

It was also found that when the rate of calling was speeded up so that the interval between successive calls was reduced to 1·5 seconds (the " rapid " rate), the $(+1)$ cognitions disappeared and were replaced by $(+2)$ precognitive successes.

When on the other hand the rate of calling was slowed down to an interval of five seconds between successive presentations (the " slow " rate), no " beyond chance " results of any kind were obtained.

In so-called " clairvoyance " experiments in which the Agent touched but did not look at the card to be guessed, no significant results of any kind were obtained, and this irrespective of whether B.S. knew or did not know whether the experiment was one of " clairvoyance " or of " telepathy " (in which the Agent looked at the cards).

The effects of " sandwiching " [1] noted during the work of 1936 are fully confirmed in the present series, and there is strong evidence of a similar effect in connection with (± 2) guesses at the " rapid " rate of calling.

B.S.'s impressions or pre-judgments as to whether his guessing was successful or not appeared to bear no relation to the actual results obtained.

Scoring was not equally successful on each of the five animal picture cards. Most success was scored with the Elephant and least with the Lion.

A number of persons witnessed the successful scoring and all testified to the fraud-proof character of the methods employed.

Statistically the precognitive $(+1)$ results on the whole series are highly significant. Including every single experiment between the dates 24.1.41 and 6.1.43 at which an Agent was present, we have a total of 11,378 $(+1)$ precognitive trials.[2] This total includes tests in both " telepathy " and " clairvoyance " and tests at *all* rates of calling and with *all* Agents; *i.e.*, we include those conditions which consistently led to negative results as well as conditions which conduced to success. The number of $(+1)$ successes on this grand total is 2,890 compared with an expectation of 2,308·17 by Stevens' method.[3] We have thus an excess of $(+1)$ hits amounting to 581·83 and equivalent to 13·6 Standard Deviations, with odds of more than 10^{35} to 1 against chance.

PART ONE [4]

ACCOUNT OF THE DIFFERENT TYPES OF EXPERIMENT WITH DESCRIPTION OF THE TECHNIQUES EMPLOYED AND EVALUATION OF THE RESULTS

N.B.—Owing to her war work, which entailed travelling to the north of England, K.M.G. was unable to be present at a few of the experiments. S.G.S. attended every experiment except one held on 16 January 1942 when, however, K.M.G. was present. Every experiment held between the dates 24 January 1941 and 15 April 1943 is recorded in the present Report and with the exception of the two final sittings (Nos. 39 and 40) is included in

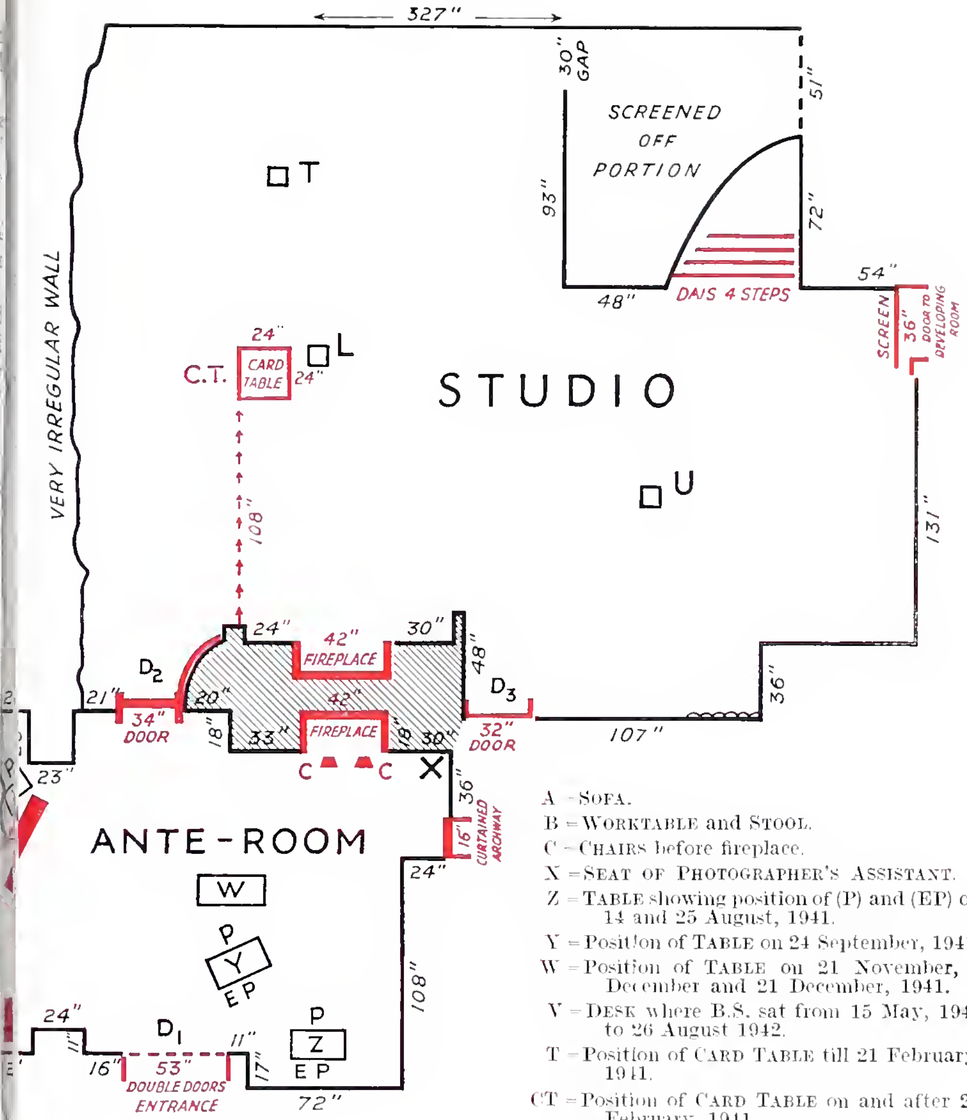
[1] See p. 31.

[2] The results of two experiments held at the Society's rooms (Sittings 39 and 40) arrived too late to be included in this total but if they were added the effect would be to increase still further the Critical Ratio.

[3] See pp. 43-44.

[4] Part 1 should be read in conjunction with Part 2 (List of Scores, p. 88).

PLATE I



A = Sofa.
B = Worktable and Stool.
C = Chairs before fireplace.
X = Seat of Photographer's Assistant.
Z = Table showing position of (P) and (EP) on 14 and 25 August, 1941.
Y = Position of Table on 24 September, 1941.
W = Position of Table on 21 November, 5 December and 21 December, 1941.
V = Desk where B.S. sat from 15 May, 1942 to 26 August 1942.
T = Position of Card Table till 21 February, 1941.
CT = Position of Card Table on and after 21 February, 1941.
L = Photographer's Lamp.
U = Position of R.E. on 5 June 1942.

$\left.\begin{array}{l} D_1 \\ D_2 \\ D_3 \end{array}\right\}$ = Doors.

PLATE II

SCORING SHEET :

NAME B.S.

SHEET 1

DATE 14 November '41

	G	A
	2	5
	3	1
direct hit	4	4
	1	3
5	1	1
	3	2
	5	5
	4	3
	2	2
10	3	2
	5	5
(+1)	3	4
precognitive hit	4	3
	2	2
15	2	5
	3	1
	5	3
	5	1
	4	4
20	3	2
	5	3
	1	2
(-1)	4	1
postcognitive	1	5
hit. 25	1	4

Totals (+1)(0)(-1)

6/7/3

(+2) = 3
(-2) = 5

(CC+1) = 6

Totals

5/5/5

(+2) = 1
(-2) = 7

(CC+1)=3

TO BE FILLED IN BEFORE LEAVING THE LABORATORY.

Experimenter's Name..........

Witness's Name..........

Guesser's Name..........

Remarks or Suggestions :

[Continue overleaf]

CODE

Z	P	G	E	L
1	2	3	4	5

G	A
4	3
5	2
4	5
3	2
4	1
2	4
4	2
3	3
3	4
5	4
1	5
4	4
5	3
3	3
1	4
4	2
1	5
2	1
3	3
4	5
3	5
5	2
4	4
2	1
3	1

Time Not timed.

Time 58 secs.

TOTAL CORRECT=..............

This independent record has been checked with the duplicate and found to agree..........

Signature of Experimenter..........

Signature of Witness..........

TOTAL CORRECT=..............

This independent record has been checked with the duplicate and found to agree..........

Signature of Experimenter..........

Signature of Witness..........

the tabulated totals. No experiment held in the presence of either S.G.S. or K.M.G. at any time has been omitted. Two preliminary experiments carried out by S.G.S. on first contacting B.S. are recorded in Appendix H.

Types of Experiment. There were two main types of experiment. In the first the card to be looked at by the Agent was chosen by means of a list of random numbers (1 to 5) prepared beforehand by S.G.S., or in a few cases by another person. This type of experiment will be referred to as a PRN (Prepared Random Numbers) experiment. In the second type of experiment, the card to be looked at was chosen by one of the experimenters selecting by touch a counter from a bag or bowl which contained equal numbers of counters in five different colours. This will be called a Counters experiment.

We shall begin with a description of the PRN experiments. The technique which we shall describe was for all practical purposes standardised by 7 March 1941 in the seventh sitting of the series. A screen was in use on the card-table from Sitting 1 on 24 January 1941, but the additional precaution of enclosing and screening the five cards inside a box was adopted on and after 7 March 1941. The introduction of a second experimenter whose function was to control B.S., the Percipient, first took place on 7 February 1941 at Sitting No. 3.

Personnel. In general four persons take part in the experiments. They are

(1) The Percipient (B.S.) referred to as - - - - - (P)
(2) The Agent referred to as - - - - - - - (A)
(3) The Experimenter controlling Agent referred to as - - (EA)
(4) The Experimenter controlling Percipient referred to as - (EP)
In addition a fifth person is present on most occasions, who is called the Observer - - - - - - (O)

From January 1941 till June 1941 the rôle of (EA) was assumed by S.G.S., and that of (EP) by K.M.G. and various other persons. On and after 14 August 1941, K.M.G. usually played the part of (EA) while S.G.S. acted as (EP). This change-over gave to each experimenter experience of the different rôles. (See Part 2, List of Scores, for personnel at each experiment.)

The Studio and Ante-Room.[1] Our experiments are conducted in B.S.'s Studio. This is below the level of the street and none of the rooms has any windows. The rooms consist in the main of a large Studio and an ante-room. There are, in addition, some small private apartments which are reached from the ante-room through a curtained archway and from the Studio through the door D3.[2] The ordinary entrance to the Studio from the ante-room is by door D2. The folding entrance-doors, D1, lead to a short passage from which stairs ascend to the outer door of the building which opens on the street. The shaded area in Plan between ante-room and Studio is intra-mural and hollow, and the walls are not solid but built-up with plywood, plaster-covered, as are also the doors. The screened-off portion shown in the right hand corner of the Studio is a temporary plywood platform (for photographic purposes).

[1] *Cf.* Plate I. [2] See Plate I.

Card Table and Cards.[1] The card table (size 24 in. square and 25 in. high) is situated in the Studio at a distance of about 9 feet from the dividing wall between Studio and ante-room. It is lighted by a powerful photographer's lamp, L. Standing on this card table is a screen (size 31 in. wide by 26 in. high) with a square aperture (3 inches square) in its centre. The Agent is seated on that side of the screen remote from the ante-room and the experimenter (EA) sits or stands on the side nearest to the ante-room. The plane of the screen is about parallel to that of the dividing wall. Resting on the table on the Agent's side of the screen is a rectangular box with its open face towards the Agent.[2] Inside this, on the floor of the box and entirely screened by it, are five cards with backs like those of playing cards and bearing on their faces pictures of the five animals

Lion	(L)
Elephant	(E)
Zebra	(Z)
Giraffe	(G)
Pelican	(P)

The pictures are in appropriate colours. On the table in front of (EA) are five cards on which are printed in large bold type the numbers 1, 2, 3, 4, 5.

Position of Percipient. The Percipient, B.S., sits in the ante-room while he is guessing the cards, in one of the following positions. For the first 18 sittings, between 24 January 1941 and 13 June 1941, he sat on one of the chairs (C) with the experimenter (EP) beside him in front of the fireplace. On and after 14 August 1941 and until 21 December 1941, B.S. and (EP) sat at a small table nearly towards the far end of the ante-room. On and after 15 May 1942 B.S. and (EP) sat at a desk (V).[3]

Scoring Sheets.[4] S.G.S. brings to each sitting scoring sheets, foolscap size, each designed to accommodate two columns. For each column there are two divisions: the one on the left headed G (for Guesses) and the one on the right headed A (for Actual cards). The two divisions are divided into 25 rectangular cells, and for convenience in keeping count these cells are numbered at intervals of five. The left hand column with its two divisions of 25 cells is referred to as the (*a*) column and the right hand one with its two divisions of 25 cells as the (*b*) column. Thus sheet 4(*a*) means the left-hand column (with its two divisions of 25 cells) on the fourth sheet; sheet 4(*b*), the right-hand column (with its two divisions of 25 cells) on the fourth sheet.

S.G.S., before coming to the sitting, fills in the A divisions on all the sheets to be used by (EA) with a random sequence of the digits 1, 2, 3, 4, 5. In general S.G.S. prepares these lists from the last digits of the seven-figure logarithms of numbers selected at intervals of 100 from Chambers' Tables.[5] In some cases, however, Tippets' random numbers were used. These lists are compiled by S.G.S. at his lodgings in Cambridge with no-one

[1] See Plate III. [2] See Plate IV. [3] See Plate I.
[4] See Plate II. [5] See *Proc.* xlvi, 156.

present but himself, and they are kept under lock and key until the day of the sitting. They are then brought to London in a suit-case which is never out of S.G.S.'s sight till the experiment is about to start. At the last moment S.G.S. takes the suit-case into the Studio, extracts the compiled lists, and hands them to (EA). (P), therefore, who never enters the Studio till the experiments are finished, has no opportunity of seeing these sheets at any time.

S.G.S. also hands (P) some empty scoring sheets similar to those in the possession of (EA) and both (EA) and (P) number the first sheet they are about to use " 1 ". (P) records his guesses in the G divisions on each sheet.

The lists of random numbers are made out in blue-black ink, but (P) finds it more convenient to use a pencil in recording his guesses.

(P) and (EP) now seat themselves in the ante-room. The door D3 is kept closed.[1] In the earlier experiments, till sitting No. 10, the door D2 was completely closed also. After sitting No. 9 however, it was left an inch or two ajar in order to facilitate hearing. From where he sits (P) is quite unable to see either (EA) or the screen. Still less is he able to see the box on the far side of the screen or the Agent (A). The purpose of the box is to screen the five cards from the view of any person who might by hypothesis be concealed in the Studio or who might be gazing down into the Studio through some hypothetical hole in the ceiling. In fact, with the cards inside the box, no-one could see them unless he were standing directly behind (A), in which case his presence would be apparent at once.

The five cards are now shuffled by either the Agent (A) or the observer (O), if an observer is present. Throughout the experiments we have adopted as a cardinal principle the rule that neither (EA) nor (EP) shall shuffle the five cards nor witness the shuffling. Hence, since (EA) does not know the order of the cards inside the box he can give nothing away to (P) by any inflections of the voice when he calls (see ff); and since (EP) is also unaware of the order, he cannot help (P) in any way when the latter records his guesses. If (EA) looks through the square aperture in the screen, he can see only (A) and the top of the box.

The Call. The experiment begins with (A) and sometimes (O) shuffling the cards out of sight of (EA) and laying them face downwards in a row on the floor of the box. (EA) then shouts " Are you ready? " and on receiving the answer " Yes " from (EP) in the ante-room, lifts to the aperture in the screen the printed card bearing the number which comes first in the (a) column of the first sheet. He pauses for the merest fraction of a second and calls " One ". On seeing, say, the number 4 at the aperture, (A) lifts up slightly the fourth card from left to right and having noted the picture on its face lets the card fall back face downwards on the floor of the box, without of course disturbing the order of the five cards. On hearing the word " One ", (P) writes down in the first cell of the G division of the (a) column of his first scoring sheet the initial letter of one of the animals : L, E, Z, G, or P. The momentary pause by the experimenter (EA) is to ensure that (A) has lifted the card at the instant (P) hears the call. (EP) verifies that (P) synchronises his recording

[1] See Plate I.

with (EA)'s calls. While the guessing is in progress (A) remains absolutely silent. To summarise: (A) or (O), the only persons who know the order of the cards, never speak at all; (EA), the only person who speaks, does not know the order of the cards. The possibility of a code being conveyed by the voice is therefore precluded.

Until sitting No. 9 (21 March 1941), (P) used to shout "Right" immediately he had recorded his guess; but after a time this became unnecessary since (P) got into the habit of writing down his guess at the instant he heard (EA)'s call or at an interval scarcely ever exceeding ·4 of a second after the call (timed by S.G.S. with a stop-watch).

(EA) now exhibits the next random number at the aperture and calls "Two" after a fraction of a second's pause. On seeing the number card at the aperture, (A) lifts the appropriate animal card and lets it fall back in its place. (P) on hearing "Two" immediately records his guess in the second cell of his G division, and so the guessing continues. (EA) calls the numbers 2, 3, 4, up to 25 at a rhythmical rate, keeping the intervals as constant as possible.[1] At guess number one (EA) starts a stop-clock which is stopped at guess number twenty-five. At the end of the (a) or left hand column of 25 guesses, there is a pause of at least six or seven seconds, after which (EA) shouts "Next column", and on hearing (EP)'s "Right" begins again with "One", "two", "three", etc. until the right hand or (b) column of the sheet is run through.

Taking Down the Code. When the sheet of 50 calls is completed, there is a break of perhaps a couple of minutes for "taking down the code". (EA) goes round to the other side of the screen, and, watched by (O), turns over the five cards without disturbing their order. He records the code at the bottom of the scoring sheet thus:

E	L	G	P	Z
1	2	3	4	5

This, say, is the order of the five cards as seen and lifted by the Agent from left to right. (P) in the meantime remains with (EP) in the ante-room.

Before commencing their second sheet (which both (EA) and (P) now number "2"), the five cards are re-shuffled by (A) or (O) out of sight of both (EA) and (EP), and this is done each time before starting on a new sheet.

Decoding and Checking-Up. The decoding and counting of successful hits is carried out by (EA), (EP), and (O), with (A) looking on.

(EA) first brings the random number sheets into the ante-room (or (P)'s sheets into the Studio) and lays sheet No. 1 on a table by the side of (P)'s "guess" sheet No. 1. One of the experimenters reads aloud (P)'s guesses and as he does so (O) or the other experimenter copies down in the appropriate cell of (EA)'s G column the code-number for each of (P)'s letters, which he obtains by referring to the code at the bottom of (EA)'s sheet. As this number is entered, either (O) or the other experimenter

[1] In the earliest experiments the word "next" was called instead of the serial number (see p. 44).

checks it, while (A) checks the letter read out by the first experimenter. Thus each member of the *active* pair is checked by a looker-on. All decoded numbers are entered in ink.

When a column of 25 guesses is filled in on (EA)'s sheet, the numbers of successes are counted in the order

(*a*) direct hits
(*b*) precognitive (+1) hits
(*c*) postcognitive (−1) hits

with, as a rule, at least three people checking the counts. These numbers are then entered in ink at the top of (EA)'s sheet thus:

$$\frac{(+1)}{6} \;/\; \frac{(0)}{4} \;/\; \frac{(-1)}{3}$$

Ticks are entered opposite the direct hits.

The checking-up is usually done immediately the experiments are completed, though in some of the earlier sittings it was done at the end of each sheet, and later on at the end of three sheets. The two experimenters as well as (O) append their signatures at the bottom of each scoring sheet.

In the earlier sittings B.S. often watched the decoding and checking-up as a passive observer, but after June 1941 he was seldom present at the checking-up, having left the Studio. (The exact procedure on each occasion is noted in Part III: *The Chronicle*). He would usually return after we had finished, but as a rule he was (after June 1941) not told his exact scores. After a successful sitting we would remark to him " The results were first-rate today " or something of that sort. If the results were poor we would tell him " Not so good today ". He was never much depressed by poor scores, but neither was he unduly elated by good scores. His general attitude was one of detachment and often he seemed quite indifferent as to the outcome of his performance. Certainly there was none of that emotional stress which some experimenters have described as existing in their Subjects.

In connection with the above method of decoding and checking-up, it must be understood that we have made many variations and changes of personnel in order to discount the criticism that the same two persons playing the same rôles might be in collusion to falsify the records. All the independent observers were satisfied that the task of checking-up was performed in a straightforward manner, and many have testified to this in writing. The experimenters frequently asked the Observer if he would like to re-check independently some of the higher scores. Professor H. Habberley Price, for instance, himself selected three columns of high scores and re-checked independently both the decoding and the counting of hits. No errors were found.

We may cite one or two examples of variation:

(1) On 21 December 1941 Mr C. A. Mace stood between K.M.G. and S.G.S. so that he could check both the letter read out by S.G.S. and the correctness of the number entered (in ink) by K.M.G. who did the decoding. He reported no errors and highly significant (+1) scores were recorded. (See List of Scores, p. 114 and *The Chronicle*.)

(2) On 5 December 1941, Sir Ernest Bennett read aloud B.S.'s guesses while K.M.G. decoded and entered the numbers (in ink) with S.G.S. looking on and checking her. R.E. also watching. Results highly significant. (See List of Scores, p. 113 and *The Chronicle*.)

(3) On 24 October 1941, Dr C.E.M. Joad decoded the first three sheets entirely alone. The numbers were entered (in ink) by Mrs Woollard, and Joad was checked by S.G.S. while R.E. checked Mrs Woollard. At the end of Sheet 3, Joad had to leave for a lecture and the remaining sheets were decoded by Mrs Woollard with R.E. and S.G.S. checking her. Scores significant. (See p. 109 and *The Chronicle*.)

(4) At the four sittings on 8, 15, and 22 May 1942, and 8 April 1943, Mr R. G. Medhurst, B.Sc., at the request of S.G.S., carried out entirely alone the whole of the decoding, entering (in ink) and counting of scores. This was confirmed by Medhurst in writing. Scores highly significant on these occasions. (See List of Scores, pp. 117, 118, 119, 125; and *The Chronicle*.)

At the end of the decoding and the counting of hits, the totals for each of the classes (+1), (0), and (−1), and the corresponding chance expectations, are reckoned by S.G.S. and re-checked independently by K.M.G. with (O) usually looking on. The Standard Deviation and Critical Ratios are approximately estimated before the experimenters leave the premises.

Duplicate Records. The final task consists of making duplicates in ink of all the completed scoring sheets. This work is usually shared between K.M.G., S.G.S. and R.E. After being signed by both experimenters, the duplicates are, in full view of all present, placed in an envelope that is stamped and addressed to Professor C. D. Broad, Trinity College, Cambridge. The envelope is posted in the presence of not less than three people in the post-box in the street a few yards from B.S.'s Studio.

By this time the Observer, if any, has usually departed. K.M.G. and S.G.S. now adjourn to a restaurant where scores are re-checked, results discussed, and plans made for future experiments.

S.G.S. takes the record sheets back with him to Cambridge or to his home in Essex as the case may be, and there proceeds to make a complete re-check of both the decoding and counts of successes, and also counts all (+2) (precognitive) and all (−2) (postcognitive) hits and records the totals for each column. No error in the whole period was ever discovered in the decoding, but on one occasion it was found that a single precognitive (+1) hit had been overlooked in a count. This is testimony to the accuracy of those concerned in the decoding and counting.[1]

The Statistical Method. The statistical methods employed in the straightforward evaluation of the results of experiments in card-cognition are now so well established, and have been the subject of such thorough discussion since the year 1937, that no-one, we think, will any longer question their validity. The statistical battle was fought and won by the Duke University experimenters and since 1938 no further opposition has been encountered on this score.

[1] For results of independent re-check of all scoring sheets, see pp. 86-87.

In the present investigation the results obtained are so manifestly above chance expectation that any critic would be simply wasting his time if he ventured to attack them on statistical grounds. The use of prepared lists of random digits (1-5) removes the last objection based on the supposition that the shuffling of packs of cards may not result in ideal random distributions. It also renders ineffective those objections to the use of the binomial formulae which were urged so persistently and (from a practical standpoint) with such lack of success against the work of Rhine.

The reader who desires to know something of the history of these statistical controversies will find a summary of them in *Extra Sensory Perception After Sixty Years* (New York, Henry Holt & Co. 1940). In a series of admirable appendices to the same book is given a collection of statistical formulae which are of use in card-cognition tests. The formulae referred to as " Stevens " formulae were first established by W. L. Stevens in 1938 and his Paper appeared in the Annals of Eugenics, Vol. VIII, Part III, pp. 208-44.

METHODS OF EVALUATION.[1] Throughout this investigation we have made no attempts to count beyond (± 2) displacements. The five scores (-1), (0), $(+1)$, (-2), $(+2)$ only are considered. As our (PRN) distribution of card presentations is a random series, the expectations on these five scores are theoretically independent of each other. Since there seems to be some misapprehension on this point, we shall illustrate the independence of $(+1)$ and (-1) scores by taking an extreme case.

Each column of 25 calls provides 24 $(+1)$ trials and 24 (-1) trials. The expected number of $(+1)$ hits is clearly (on an average) $\frac{1}{5}$ of $24 = 4{\cdot}8$ and there is the same expectation of (-1) hits. Let us suppose that by an extraordinary fluke a Percipient got all the 24 $(+1)$ (precognitive) guesses right. Then, with the exception of a possible success on the last or twenty-fifth presentation, his *only* (-1) (postcognitive) successes would arise from multiply-determined presentations of the form

$$\begin{matrix} \text{L} & & \text{L} \\ \text{E} & \text{or} & \text{L} \\ \text{L} & & \text{L} \end{matrix} \quad \text{etc. } (cf.\ \text{p. } 31)$$

and on these and similar types he would score *certain* hits. But in a *random* distribution there would be a mean expectation of $\frac{1}{5} \times 23$ such types. Hence his total mean expectation for (-1) successes would be

$$\tfrac{1}{5} + \tfrac{1}{5} \times 23 = 4{\cdot}8$$

(since his expectation on the last presentation would be $\frac{1}{5}$). Thus his high $(+1)$ score has not affected his (-1) expectation.

We should point out that throughout this report the probability corresponding to a deviation X from mean chance expectation is understood to be the probability of a deviation lying outside the range $-X$ to $+X$. In other words, we estimate our probability on the assumption that we are just as interested in negative deviations as in positive deviations.

(I) In a series of N trials the mean expectation of successes is $N/5$ and the standard error is $0{\cdot}4\sqrt{N}$. These formulae of course apply when we are dealing with any of the five types (0) (± 1) (± 2).

[1] *Cf. Proc.* xlvi, pp. 168-9.

(II) The expectation and variance can be found for each set of, say, 24 (+1) trials by Stevens' method and the results summed for all sets.[1] It was shown in the 1934-1939 experiments that the variances obtained by Stevens' method are almost without exception very slightly less than those given by the formula $4N/25$. This was confirmed by scoring a large batch of the present results by both methods (I) and (II). We are therefore on the safe side if we use $0{\cdot}4\sqrt{N}$ as the theoretical Standard Error instead of that given by Stevens' formula which is tedious to evaluate. The expectations have been found from Stevens' formula.

(III) An empirical " cross-check " for each sheet in the case of the (+1) scores was made by scoring column (a)G against column (b)A and column (b)G against column (a)A. That is to say, the " guesses " (G) division of the left hand half of the sheet was scored against the " actual card " (A) division of the right hand half, and vice versa.

The " Normal " Rate of Guessing. A fairly rapid speed of guessing seems essential if B.S. is to obtain significant results, and in this respect he resembles G.N.M.Tyrrell's Sensitive[2] and differs from many of the Sensitives studied by Dr Rhine.

If the interval between successive card presentations is increased beyond 4 seconds, B.S. grows unhappy, restive, and irritable. At an interval of as much as 5 seconds, he does not seem to be able to score any kind of success. We made no systematic attempts at timing the guessing till 21 March 1941, as we did not at first realise the effects the varying rates had upon the results.

As already recorded, in the experiments previous to sitting 9 (21 March 1941), (EA) on showing each number at the aperture in the screen called " Next ", and immediately he had recorded his guess B.S. shouted " Right ". This was a signal for (EA) to exhibit the next number at the aperture. Until 21 March 1941, therefore, the rate of calling was determined by B.S. himself. He adapted himself to a rate that was fairly quick but comfortable for both himself and the experimenters. In order to reassure us that he was keeping in step with (EA) he would call out at the end of every 5 guesses " Five ", " Ten ", " Fifteen ", " Twenty ", " Twentyfive ". But on and after 21 March 1941 (P) dispensed with calling " Right " after each guess and (EA) no longer shouted " Next " but called out instead the serial numbers " One ", " two ", " three " . . . " twentyfour ", " twentyfive ", to facilitate and ensure the synchronisation of (P)'s recording with (EA)'s calling.

Obviously synchronisation is of primary importance for any evaluation of the results. It was noted by several Observers who acted as (EP) in the earlier experiments, that on occasion B.S. wrote down his eighteenth guess, say, the merest fraction of a second *before* (EA) called " eighteen ". When we discovered this tendency, we were constantly warning him against it and by 16 May 1941 he was writing down his guesses almost simultaneously with or about ·4 of a second after the call from (EA). During the six months that S.G.S. acted as (EP), he frequently timed the intervals between (EA)'s calls and (P)'s recording of his guess; and during the

[1] *Cf. Proc.* xlvi, pp. 168-9.

[2] See *Proc.* vol. xliv, Part 147.

whole of this period S.G.S. noted that on no occasion did (P) record his guess before (EA)'s call. It was as a rule recorded at about ·2 to ·4 seconds *after* the call.

We had hoped to use a metronome to keep the intervals between successive calls constant, but (P) said it was quite impossible for him to work with " that confounded thing ticking away in the other room ". So we had to abandon this plan. Nor, in fact, would it be an easy thing for (EA) to regulate with completely accurate timing to the split second, and at the required rate, the difficult task of reading off accurately the list of figures, quickly picking up the correct card, and presenting it neatly at the small aperture in the screen. This is a task which requires considerable dexterity and concentration at the best!

B.S.'s comfortable rate of guessing varies between limits of about 50 seconds and 80 seconds for a column of 25 calls, and we entitle it the " Normal Rate ". Throughout a single sitting it remains fairly constant but varies somewhat with the person who plays the part of (EA). Of 255 columns of 25 calls timed at " normal rate ", the average time for 25 calls is 62·33 seconds, which corresponds to an average interval of 2·60 seconds between successive calls. The standard error for a column is 11·38 seconds (observed value).

An inspection of Table XXXV Appendix (F), shows that it is safe to conclude that significant successes on (+1) guesses have been obtained at practically all rates between 50 and 80 seconds for a column of 25 calls. We shall later discuss the effects of more rapid calling.

Explanation of Table I (p. 46). This Table records all the precognitive (+1) guesses and hits made by B.S. over the total period when working with Prepared Random Numbers at the " normal rate " in conjunction with our first Agent, R.E. It will be seen that the cumulative results over eleven months amount to over 13 times the Standard Deviation. The odds against such a result being due to chance are, on a conservative estimate, indicated by a figure consisting of 10 with 35 noughts added, to one.

All the experiments recorded in Table I are " telepathy " experiments, but on some occasions sheets of " clairvoyance " experiments were interspersed among, or regularly alternated with the telepathic tests (see p. 49 ff). Except on the dates 14 and 25 August 1941, the material for telepathic transmission consisted of pictures of the five animals. On the dates mentioned, experiments with the animal pictures were alternated with experiments in which cards inscribed with the associated words Trunk, Mane, Beak, Stripes, and Neck were substituted for the corresponding animal pictures (see pp. 71-72). These latter experiments are of course included in Table I, but all " clairvoyance " experiments are excluded and evaluated separately (Tables II, III, IV and V, pp. 49-51).

On 24 September 1941 and 14 November 1941 the sheets for experiments at " normal " speed were alternated with sheets for experiments in which the interval between successive card presentations was increased to 5 seconds. These " Slow Rate " experiments are of course not included in Table I, which is confined to experiments at " Normal Rate ". Table X gives the results for all calls at " Slow Rate " (see p. 54).

The second column (Average Interval) gives the average interval in seconds between successive card presentations. The third column gives the total numbers of (+1) trials on each date. It will be seen that these numbers are multiples of 24 except on 23.5.1941 when the total is 189 instead of 192, owing to the fact that Dr Wiesner, acting as (EP), reported that B.S. missed the last four calls in one column. Columns four and five give, respectively, the actual number of (+1) hits scored on each date and the expected number (E) computed by Stevens' method. Column six (CC+1) gives the empirical number of hits obtained on the cross-check; and in column seven is the value of $\chi = \text{Dev/S.D.}$ (*i.e.* the Critical Ratio) for each date. For explanation of the abbreviations in the last column see pp. 23-24.

TABLE I. (PRN) (Tel.) (Normal Rate) (Agent : R.E.)

(recording all the precognitive (+1) guesses and hits over the total period when working with Prepared Random Numbers at the "normal" speed with R.E. as Agent.)

1 Date	2 Average Interval	3 (+1) Trials	4 (+1) Hits	5 E(+1) Stevens	6 CC (+1)	7 Value of χ	8 Type of Experiment
24. 1.41	N.T.	192	67	39·66	47	+4·93	TP
31. 1.41	N.T.	192	30	40·62	38	−1·92	TP
7. 2.41	N.T.	144	39	28·54	29	+2·18	TP
14. 2.41	N.T.	240	88	50·29	61	+6·09	TP
21. 2.41	N.T.	192	50	39·83	36	+1·84	TP
28. 2.41	N.T.	288	99	60·75	68	+5·63	TP
7. 3.41	N.T.	144	52	30·54	25	+4·47	TP
14. 3.41	N.T.	144	48	30·04	25	+3·74	TP
23. 5.41	2·74	189	61	38·84	48	+4·03	TP
14. 8.41	2·58	384	105	78·21	73	+3·42	TP/TA
25. 8.41	2·45	288	78	57·42	61	+3·03	TP/TA
24. 9.41	3·10	192	58	38·79	41	+3·47	TPN/TPS
10.10.41	2·56	144	29	29·42	26	−0·09	TP
24.10.41	2·84	192	56	39·04	37	+3·06	TP/CP
7.11.41	2·56	144	30	28·08	29	+0·40	TP/CP
14.11.41	2·43	192	54	38·50	45	+2·80	TPN/TPS
21.11.41	2·99	144	40	28·71	35	+2·35	TP/CP
5.12.41	2·33	192	59	40·12	36	+3·41	TP/CP
21.12.41	2·15	192	58	38·42	38	+3·53	TP/CP
TOTALS	—	3789	1101	775·82	798	+13·2	

N.B.—The probabilities corresponding to the values of χ in column 7 should all be multiplied by 5 in order to find the odds that at least one of the five categories (0), (±1), (±2) shows a numerical deviation exceeding the magnitude recorded.

COMMENTS ON TABLE I. (A) It will be noted that the total expectation of (+1) precognitive hits as evaluated by Stevens' method (775·82), is somewhat higher than $N/5 = 3789/5 = 757{\cdot}8$. The reason for this is not far to seek. Since there is a high degree of paranormal cognition, it is seen that when a column contains an excess of presentations of some particular symbol, (P) tends to guess that particular symbol more times than if it were not present in excess. This is confirmed by some later experiments. Hence in Stevens' formula ($E = \sum_{r=1}^{5} a_r g_r / N$), if a high value of a_r is associated with a high value of g_r and vice versa, the expectation is increased as a general rule. Thus in our experiments the use of Stevens' formula instead of N/5 is on the safe side. The difference between the two expectations is very small where there is no extra sensory cognition.

(B) The totals of Table I give

(+1) Trials	(+1) Hits	(E)	Dev.	St. Dev.	χ
3789	1101	775·82	325·18	24·62	13·2

It follows that the chance of getting a (±) deviation of this magnitude or greater is certainly less than 10^{-35}. Since, however, our score was chosen as the *best* out of five scores (±2)(0)(±1), the chance of getting at least one of the five scores with a ± deviation as large as the above will (approximately) be less than 5×10^{-35}. The factor 5 of course scarcely affects the order of the result. The probabilities which correspond to the values of χ given under different dates in Table I should of course all be multiplied by 5.

(C) As the total scores on (±2) and (0) and (−1) are not significant, we have not evaluated their expectations by Stevens' method. These totals are given in Table IA below.

(D) The cross-check total under (CC+1) is in close agreement with expectation.

TABLE IA (Appendix to Table I)

(PRN) (Tel.) (Normal Rate) (Agent: R.E.)

N.B.—O = Observed number of hits. E = expected number.

	(−2)	(−1)	(0)	(+1)	(+2)
Trials - -	3630	3788	3946	3789	3632
E - -	726	757·6	789·2	775·82	726·4
O - -	714	768·0	829·0	1101·00	703·0
Dev. - -	−12	+10·4	+39·8	+325·18	−23·4
St. Dev. -	24·10	24·62	25·13	24·62	24·11

From Table IA it is clear that apart from the large precognitive (+1) deviation, none of the other deviations is anywhere near significance.

There is however some evidence that under the influence of suggestion, sufficiently prolonged, B.S. is on occasion able to score significantly upon the actual card being looked at by the Agent (*i.e.* direct hits).

On 24 January 1941 (the first experiment), he scored significantly on (+1) hits, while on the " actual card " he obtained only 40 hits in 200 trials, which is clearly a chance result. At the close of the experiments on this date, S.G.S. asked B.S. to keep reminding himself during the following week that at the next sitting on 31 January he was going to score *direct* hits and not precognitive hits. Each day during the week S.G.S. kept repeating aloud " B.S. will score direct hits next Friday ". The experiment was a striking success. On 31 January B.S. scored an almost significant *negative* deviation on $(+1)$ guesses but on the actual card itself he won 76 hits in 200 trials: an excess of $+36$ above chance expectation, and equivalent to 6·36 times the Standard Deviation with $P < 10^{-8}$ (For the actual scores see Part II, List of Scores, p. 89, Sitting No. 2).

At the end of the experiment on 31 January, S.G.S. asked B.S. to concentrate during the following week on the idea of scoring $(+1)$ precognitive successes. S.G.S. during the week preceding the next experiment again repeated aloud several times a day "B.S. will score precognitive hits next week". The results were disappointing, but this may have been due to the presence of K.M.G. and Mr H. Chibbett, both of whom B.S. was meeting for the first time. It has been noted by Rhine, Tyrrell, and others, that the effect of introducing a new personality on the scene is sometimes a temporary lowering of the score. On 150 "direct" trials B.S. scored 43 hits, which is an excess of $+13$ above expectation and equivalent to 2·65 times the Standard Deviation; while on 144 $(+1)$ trials he won 39 $(+1)$ hits, equivalent (on Stevens' scoring) to 2·18 times the Standard Deviation. The chance of getting at least two of the five scores (0), (± 1), (± 2), with a deviation as high as 2·18 Standard Deviations is approximately $10 \times (\cdot 0293)^2 = 0\cdot 0086$, which corresponds to odds of the order 100 : 1. But contrary to the suggestion given, the "direct" score is a little higher than the $(+1)$ score (see p. 90, Sitting No. 3).

After this we did not continue with the method of suggestion described above. During the succeeding weeks, in which there was an influx of fresh visitors, the interest of everyone became centred on the precognitive scores. We talked to B.S. only of "precognition". All this may have had the effect of directing his faculty into the precognitive channel from which it scarcely ever strayed for a whole year.

As certain people seem afraid of experiments in which the odds against chance are clear and decisive, and apparently prefer results of dubious significance, a few words may not be out of place on the question of high scores. Odds of millions to one against chance are the inevitable consequence of the consistent operation of a real *psi* faculty. Odds of only seventy or a hundred to one soon give rise, if repeated for a few weeks, to odds of astronomical magnitude: since probabilities are combined by the law of multiplication and not of addition. *Consistency* is the real test. Are positive results obtained week after week under first-class experimental conditions and in the presence of intelligent observers? If so, it is a proof that we are investigating a *real* faculty and not dealing with a few vagaries of chance-coincidence.

If, say, an experimenter makes his Subject do a thousand guesses and obtains a positive deviation from chance expectation of only 2·8 times the Standard Deviation, and then goes on to put the Subject through another ten thousand trials with only chance results—it is open to the experimenter to claim that the Sensitive showed extra sensory cognition for the first thousand trials but that the faculty then petered out. It is however equally open to another to maintain, with a much greater show of reason, that there never was any faculty in operation and that all that happened was that the Subject had an initial run of luck. But if we, in these experiments with our Sensitive, go on getting (as we do) odds of hundreds or thousands to one time after time over a period of two years, and if, moreover, we find (as we do) that when the experimental conditions are varied in the same fashion the results exhibit consistent changes in character—then we are in a strong position which is proof against the theory of mere flukes of chance.

"Clairvoyance" Experiments. Our first variation with the (PRN) method was to introduce, unknown to B.S., sheets of 50 calls during which the Agent did not know the order of the five cards in the box but merely touched their backs at each call without turning them up. A couple of sheets of these "clairvoyance" tests were interspersed each week more or less randomly among the ordinary "telepathy" tests. Before commencing on a "clairvoyance" sheet, the Agent, R.E., would shuffle the five cards with her eyes shut and lay them on the floor of the box without having seen their faces. On a good many occasions the five cards were shuffled by an Observer (O) out of sight of the Agent or of anyone else in the room.[1] When (EA) showed the random number card at the aperture in the screen, the Agent, R.E., *touched* the corresponding card without lifting it to look at it. But as there was no other difference in procedure, B.S. was kept in ignorance that any variation had been introduced.

Considered as an experiment in Pure Clairvoyance the test was far from satisfactory. It could, for instance, have been suggested that the Agent might succeed in recognising the five cards by noting specks etc. on their backs (though probably the light inside the box was not good enough to make this possible.) Had (P) obtained any significant degree of success in this admittedly defective test, we should have gone on to perfect it and seal the cards up in opaque envelopes etc. But these refinements proved unnecessary since the "clairvoyance" experiments invariably failed.

Between 24 January 1941 and 28 February 1941, ten sheets of "clairvoyance" experiments were completed, and the results—completely negative on all five scores—are given in Table II.

TABLE II

(PRN) (Clairvoyance) (First Series) (Normal Rate)
(Results : completely negative)

Date	−2 Trials	−2 Hits	−1 Trials	−1 Hits	0 Trials	0 Hits	+1 Trials	+1 Hits	E(+1) Stevens	CC (+1)	+2 Trials	+2 Hits
24.1.41	92	22	96	20	100	21	96	21	19·17	22	92	13
31.1.41	92	19	96	21	100	20	96	20	19·33	18	92	12
7.2.41	92	15	96	29	100	19	96	19	18·79	27	92	16
14.2.41	92	17	96	20	100	13	96	20	19·37	17	92	27
28.2.41	92	21	96	16	100	25	96	18	19·37	17	92	20
Totals	460	94	480	106	500	98	480	98	96·03	101	460	88

An inspection of this Table shows that all scores are close to the expected values. The cross-check total under (CC + 1) for precognitive (+1) guesses is in close agreement with expectation.

[1] See *The Chronicle*.

Clairvoyance Experiments, Second Series. A further series of experiments was carried out in which sheets of " clairvoyance " were alternated not randomly but rigorously with sheets of " telepathy ". In this second series B.S. was told that the sheet was for " clairvoyance " or " telepathy " as the case might be. Thus (EA) would call out " Sheet No. 1, Telepathy "—" Sheet No. 2, Clairvoyance "—" Sheet No. 3, Telepathy ", and so on. The rest of the procedure was the same as in the first series. The regular alternation of the two categories would tend to eliminate any differences due to fatigue and show up clearly B.S.'s reaction to the two types of experiment. While the " telepathy " sheets produced highly significant scores, the " clairvoyance " results were again only what chance might be expected to produce.

TABLE III

(PRN) (Clairvoyance, Second Series, alternated with Telepathy in sheets of 50 calls), (Normal Rate) (Clairvoyance results completely negative)

Extracted " Clairvoyance " Sheets

Date	−2 Trials	−2 Hits	−1 Trials	−1 Hits	0 Trials	0 Hits	+1 Trials	+1 Hits	E(+1) Stevens	CC (+1)	+2 Trials	+2 Hits
24.10.41	138	26	144	32	150	32	144	29	30·21	27	138	25
7.11.41	138	31	144	27	150	30	144	20	29·08	30	138	24
21.11.41	138	25	144	30	150	31	144	36	29·52	26	138	26
5.12.41	184	38	192	48	200	58	192	42	39·54	46	184	33
21.12.41	138	32	144	22	150	33	144	33	28·29	25	138	24
Totals	736	152	768	159	800	184	768	160	156·64	154	736	132

The values of χ for the five total scores are as follows :

	(−2)	(−1)	(0)	(+1)	(+2)
χ	+·44	+·49	+2·12	+·30	−1·38

The one just significant value, +2·12 for (0), can be ascribed to chance since it is selected from five scores.

It is interesting to compare the " telepathy " scores in this second series with the alternated " clairvoyance " scores on the precognitive (+1) trials (See Part II, List of Scores for all Experiments, under the above dates).

TABLE IV

(Scoring on alternated " Telepathy " and " Clairvoyance " Sheets)

	(+1 Trials)	(+1 Hits)	E. (Stevens)	St. Dev.	χ
Telepathy -	864	243	174·37	11·76	5·84
Clairvoyance -	768	160	156·64	11·08	<1

For the telepathy results $P<10^{-7}$ on a very conservative estimate. In other words, the " telepathy " experiments show odds against chance of at least ten million to 1, whereas the " clairvoyance " experiments give results entirely consistent with chance.

TABLE V

Clairvoyance (Series 1 and 2 combined) Result: entirely negative.

E = expected number of hits: O = observed number of hits

	(−2)	(−1)	(0)	(+1)	(+2)
Trials - -	1196	1248	1300	1248	1196
E - -	239·2	249·6	260	252·67	239·2
O - -	246	265	282	258	220
Dev. - -	+6·8	+15·4	+22·0	+5·33	−19·2
St. Dev. - -	13·83	14·13	14·42	14·13	13·83
χ - -	< 1	+1·1	+1·5	< 1	−1·4

Obviously none of the five deviations is significant.

The cross-check (CC + 1) total for the combined series is 255 which is in close agreement with the (Stevens') expectation (252·67).

The average intervals between card presentations for the five experiments of Series 2 were 2·85; 2·56; 2·99; 2·30; and 2·06 seconds. Series 1 was not timed.

Experiments with Counters. The experiments described in the preceding sections made it highly probable that B.S. succeeds only when the Agent knows the order of the five pictures in the box. Our next step was to discover whether the experiment would succeed if, instead of using prepared lists of random numbers, the experimenter (EA) determined the Agent's selection of cards by drawing counters from a bag or bowl at random. This would produce *during the process of the experiment* the " number factor " governing the cards, in place of a series of prepared random numbers already in existence before the experiment began.

For this purpose 200 bone counters of the same make and size but in five different colours were thoroughly mixed inside a cloth bag, there being equal numbers of each colour. The five colours stood for the digits 1-5 in the following order:

White = 1; Yellow = 2; Green = 3; Red = 4; and Blue = 5.

In order to assist the Agent, five counters with colours in the above order from left to right were placed in a row on top of the box, so that when (EA) showed, say, a red counter at the aperture in the screen, (A) would merely have to lift up the card directly beneath the red counter before her. In actual practice the association between the card positions and colours is rapidly memorised, so that the appearance of a colour at the aperture results in the Agent's almost automatic selection of the correct card.

After an abortive attempt by S.G.S. who proved to be far too slow in extracting the counters at the required speed (by no means easy), this job was relegated to K.M.G. who was much more successful in presenting the counters at a constant and " normal " rate. On 14 March 1941, however, in K.M.G.'s absence Miss Ina Jephson selected the counters. In the first two experiments, 7 March and 14 March, (EA) drew the counters from a cloth bag, replacing each counter in the bag after it had been exposed at the aperture in the screen. After the sitting on 7 March, K.M.G. wrote a short report in which she described her method of extracting the counters at the required speed. She writes :

"I arranged the bag so that the counters were easily accessible. I then dipped each hand in alternately and showed a counter at the screen-aperture with one hand while the other hand was already delving in the bag for the next counter. At intervals I hesitated just long enough to give the bag a quick shake, and always picked out counters from all corners, and above and below, in order to avoid picking the same counters up twice so far as possible."

On and after 21 March, K.M.G. found it more practicable to place all the counters in an open bowl. In order to avoid conscious selection, which would destroy the desired random character of the presentations, she stood up, looking straight over the top of the screen, and selected the counters with alternate hands, letting them fall back into the bowl, and extracting them by touch alone and without looking towards the bowl.

Now, although this method of selection appears to give approximately equal numbers of each of the five colours when the extraction is done at "normal" speed, this is certainly no longer the case when the speed of calling is doubled, as it subsequently was. Even at a comfortable "normal" rate we doubt if the distribution is "random" in the sense that a series of numbers taken from logarithmic tables (with due precautions) is a random sequence. But when the rate of selection of the counters is increased to, say, 35 seconds for a run of 25, the numbers become very unequal. This, we think, is probably due to the fact that at this increased speed there is not sufficient time for the experimenter's hand to delve into all parts of the bowl as described in K.M.G.'s note quoted above. If, therefore, there happens to be, say, an excess of white counters on the surface of the bowl, the white counters may be picked up more frequently than those of the other colours.

In their *Introduction to the Theory of Statistics* (p. 339), the authors *Yule* and *Kendal* say :

"Sight is not the only sense which may bias a sampling method. In certain experiments counters of the same shape but of different colours were put into a bag and chosen one at a time, the counter chosen being put back and the bag thoroughly shaken before the next trial. On the face of it this appears to be a purely random method of drawing the counters. Nevertheless there emerged a persistent bias against counters of one particular colour. After a careful investigation the only explanation seemed to be that these particular counters were slightly more greasy than the others owing to peculiarities of the pigment, and hence slipped through the sampler's fingers."

We cannot comment here on their explanation, but quote it as somewhat relevant to the point we are discussing.

A *machine* which produced a random sequence of presentations at the required rapid rates would of course have served our purpose better than the bag or bowl. The ingenious machine designed by Mr Geoffrey Redmayne, based essentially on the principle of the roulette wheel,

PLATE III

PLATE IV

seemed at first sight to meet all our requirements. It could be used to generate a random series of 5 numbers, or pictures, or symbols of any sort. But unfortunately, owing to the time taken for the wheel to come to rest, the interval between successive presentations would have exceeded five seconds whereas, as we have already stated, this rate is too slow if B.S. is to score significant results.

Moreover, human volition would not enter into the working of a machine in the same way as it is manifest when a person draws counters from a bag or bowl. The sampler can, for instance, exercise choice as to the corner of the bag in which he feels for his counter. If he has merely to press a button which sets a machine in motion (as in G. Redmayne's machine), he does not possess the same degree of freedom. Had a mechanical selector been available, we should certainly have made use of it, since a random sequence was desirable for our immediate purpose ; though, as suggested earlier, the introduction of an element of choice into experiments resulting in precognition on the part of (P), raises a far more extensive and intriguing problem.

As K.M.G. showed the counters at the aperture and called to (P), S.G.S., sitting beside (A) and acting as Recorder, recorded the corresponding numbers in the (A) column of the empty scoring sheets. In sittings 7-10, S.G.S., while recording, sat by the side of the box in such a position that he could not see the cards. But in subsequent sittings at which counters were used he sat behind the Agent in such a position that he could not only record the counters but was able at the same time to check the cards selected by the Agent. As in all experiments (*cf.* p. 40), the only person to speak a single word was (EA) who did not know and could not possibly see the order of the cards. While K.M.G. and S.G.S. were occupied with the counters in the Studio, an observer (O) acted as (EP) in the ante-room. The new method did not affect the successful scoring.

Variation of Experiments with Animal Picture Cards. On 28 March 1941, instead of using pictures of animals, white cards were used on which had been printed in block letters the initials of the five animals L, E, G, P, Z. The backs of the cards were blank. In order that the Agent should be able to recognise immediately the letter on the card even if the latter were only raised slightly, the initial was repeated three times in miniature at the top and bottom.

B.S. was not told of this change in presentation material until four sheets at " normal rate " had been completed. The results show that the substitution of initial letters for pictures of the animals did not materially affect the successful scoring.[1]

In the next two experiments of this sort on 18 April and 6 June 1941, " Initial Letters " and " Pictures of Animals " were alternated in sheets of 50 at the beginning of the experiment (B.S. being informed of the change). On 13 June 1941 pictures were used for the first sheet only. The pictures were then removed and replaced by white cards on which had been written in block letters the " Associated Words " Trunk, Mane, Beak, Stripes, and Neck. B.S. was not informed that the cards had been

[1] See Table VI (p. 54); and again at a later date and the experiments recorded in Table XV (p. 65) and XVI (p. 66).

changed till the end of the sitting. He was then told that there had been a change but he was not enlightened as to its nature. This was the first occasion on which the "Associated Words" were employed. The experiment on this occasion showed a negative result, but it was resumed in August with a better technique and with positive results.[1]

It is interesting to note that B.S. explained to Mr. Richmond (at sitting No. 6 on 28 February 1941) that he seldom gets a mental picture of the animal, but writes down the initial letter "almost automatically". That is to say, all he experiences is a motor impulse to write the initial letter.

Explanation of Table VI. This Table records the precognitive (+1) guesses and hits made by B.S. in all the experiments with counters at the "normal rate" in conjunction with the first Agent, R.E. Without exception the experiments are "telepathy" experiments.

TABLE VI

(Counters) (Tel.) (Normal Rate) (Agent: R.E.)

(Precognitive (+1) guesses and hits in *all* experiments with counters at normal rate with Agent R.E.)

Date	Av. Int.	(+1) Trials	(+1) Hits	E(+1) Stevens	(CC) (+1)	Value of χ	Type
7.3.41	NT	144	59	29·33	35	6·18	TP
14.3.41	3·26	144	50	31·17	25	3·92	TP
21.3.41	2·77	144	61	29·62	24	6·54	TP
28.3.41	3·12	192	57	39·37	40	3·18	TL
18.4.41	2·78	138	37	26·62	30	2·21	TP/TL
6.6.41	2·60	144	49	28·54	27	4·26	TP/TL
13.6.41	3·02	48	11	10·04	11	—	TP
13.6.41	2·49	240	41	49·21	55	−1·32	TA
3.1.42	2·12	384	74	77·29	80	−0·42	TP
Totals	—	1578	439	321·19	327	7·4	

Comments on Table VI. (A) We note once more that the total expectation (321·19) as found by Stevens' method is somewhat higher than $N/5 = 315{\cdot}60$. The total on the cross-checks (327) is in excellent agreement with either value of the expectation.

(B) The critical ratio (χ) on the 1578 trials is 7·4 and, on a very conservative estimate, the chance that at least one of the five scores (0), (± 1), (± 2) should attain a critical ratio of this magnitude is $5 \times 2{\cdot}56 \times 10^{-12} = 12{\cdot}8 \times 10^{-12}$, indicating an enormously significant result.

(C) It will be noted that on 18 April 1941 there were only 138 (+1) trials instead of 144. Dr. Wiesner, who was acting as (EP) on this occasion, reported that at call No. 20 on sheet 3a, B.S. hesitated and got completely out of step. The last six (+1) trials were therefore not taken into account.

(D) The failure with TA (Telepathy with Associated Words) on 13 June 1941 may have arisen from our having started the experiment with pictures and having then made the sudden change, without giving B.S. the slightest warning. On the other hand, if we had continued with

[1] See p. 71 ff.

the pictures the experiment might have failed equally. At a later date, as stated above, this same experiment succeeded.

(E) Some remarks are offered on the failure of experiments on 3 January 1942. After B.S. had completed the first two sheets, the experimenters made the suggestion to B.S. that he should now re-orient his mind and try only for "direct" hits on sheets 3 and 4. He made the effort but said it caused mental confusion. At the end of sheet 4 we suggested that he should try for (+1) hits throughout sheets 5 and 6. Finally he was urged to try for "direct" hits in sheets 7 and 8. It will be seen on referring to the List of Scores (p. 115) that during the first two sheets, before any disturbing suggestion had been made, the (+1) scores ran: 5, 7, 8, 8, which is quite a good beginning. Immediately after the suggestion was given to "re-orient" and try for "direct" hits, the (+1) scores fell to 2, 3, in the next sheet and never recovered. Nor was there any significant success on "direct" hits in sheets 3, 4, 7 and 8. This sort of suggestion—forced on to (P) in the middle of an experiment—may well upset the working of the *psi* faculty, and is, of course, very different from that mentioned in connection with the experiment of 31 January 1941 (see pp. 47-48) in which the suggestion of scoring on "direct" hits was maintained over a whole week preceding the experiment.

Experiments with Counters at Rapid Rate. On 21 March 1941 when Mr Kenneth Richmond (Editor, *S.P.R. Journal*) acted as (EP), we made an important discovery. After completing three sheets at the "normal" speed (av. int: = 2·77 secs. between successive card presentations), it was suggested that the experiment should be speeded up so that the interval between each call was halved, if this proved to be practicable. In order to facilitate this rapid rate of presentation, K.M.G., who had been drawing counters from a cloth bag, now emptied them into a bowl, and used the technique already described on p. 52.

Richmond sat with B.S. to see that he kept in step, noting hesitations and gaps, if any, and using a stop-watch. S.G.S. sat next the Agent by the side of the box, so that he could not see the cards, and recorded the counters. It was impressed on B.S. that he must keep in step at all costs. If, on hearing the call for a certain cell, B.S. found that his response did not arrive quickly enough, he was instructed to leave that cell a blank so that he would be ready to fill in the next cell the instant its serial number was called.

The new experiment was a strain on all concerned. After three sheets (Nos. 4, 5, 6) were completed, we checked up the results. We all expected the experiment would be a failure, and we found in fact no significant results on the categories (0), (+1) or (−1) although the preceding experiments at "normal" rate had yielded significant scores on (+1) trials. Then the suggestion was made—whether by K. Richmond or S.G.S. or K.M.G. we cannot now recall—that a count should be made of the (+2) precognitive hits. When this was done, it was at once obvious that B.S. had been scoring significantly on (+2) presentations instead of (+1). (See List of Scores, p. 95). In other words, when the speed of the calling was approximately doubled, his precognitive faculty skipped the (+1) presentation and pounced upon the one which immediately followed it.

In order to make sure of our discovery, three more sheets (after a fifteen minutes' rest) were done at the same rapid speed. These were sheets Nos. 7, 8, 9. The results confirmed our previous observation.

In sheets 4, 5, 6, B.S. had left no gaps and had kept perfectly in step with the calling. But at column 7a, B.S. shouted "Stop" after recording his entry for call No. 13. After a few seconds' pause he shouted "Continue No. 14". K.M.G. then continued "14", "15", etc. at the rapid rate till the end of the column. During column 7b, B.S. left three blank spaces after call No. 5, but did not get out of step. He left another blank space at call No. 15, and completed the column.

For the total six sheets at rapid rate, the average interval between successive calls was 1·44 seconds, equivalent to an average time of 34·6 seconds for 25 calls. With our present methods we have not found it humanly possible to work at any substantially higher speed than that recorded in this experiment.

All trials at the "rapid" rate with the Agent R.E. are included in Tables VII and VIIa (below). The first Table is devoted to (+1) scores and the second to (+2) scores. The results should be compared and show that at the "rapid rate" the (+1) successes disappear but that the (+2) scores become highly significant.

As already reported on p. 52, it was discovered later that when counters are selected by touch at the "rapid" speed and dropped back into the bowl, the distribution so obtained is not strictly random and the numbers of the different colours are somewhat unequal. Stevens' method of evaluating the expectation of hits allows of course for the inequality of the numbers, but at the same time it requires that the presentation objects are randomly mixed. It would perhaps have been better if instead of returning the counters to the bowl we had used a much larger number of them and had let each counter fall on the floor after being exposed.

However, with our third Agent, Mr J. Al., we have obtained abundant confirmation of the reality of the (+2) displacement when the speed is doubled, by using prepared random numbers instead of counters. Hence we have no hesitation in asserting that the effect is a genuine effect. Moreover it will be seen (Table VIIA) that in the above counters experiments a cross-check (CC+2) for (+2) trials produced 148 hits, which agrees excellently with the (+2) expectation obtained from Stevens' method. This is an additional confirmation.

TABLE VII

(Counters) (Tel.) (Rapid Rate) (Agent : R.E.)

(+1) Scores

Date	Av. Int.	(+1) Trials	(+1) Hits	E(+1) Stevens	Value of χ	Type
21.3.41	1·44	280	57	56·44	<1	TP
28.3.41	1·40	181	34	34·94	<1	TL
18.4.41	1·40	227	34	46·09	$-2·01$	TL
6.6.41	1·49	143	29	29·36	<1	TP
Totals	—	831	154	166·83	$-1·1$	

TABLE VIIA

(Counters) (Tel.) (Rapid Rate) (Agent : R.E.)

(+ 2) Scores

Date	Av. Int.	(+ 2) Trials	(+ 2) Hits	E(+ 2) Stevens	Value of χ	Type	CC + 2
21.3.41	1·44	265	84	53·60	+4·67	TP	50
28.3.41	1·40	173	54	33·56	+3·88	TL	29
8.4.41	1·40	219	63	44·58	+3·11	TL	37
6.6.41	1·49	137	35	27·71	+1·56	TP	32
Totals	—	794	236	159·45	6·79	—	148

N.B.—Table VII shows that at a rapid rate of calling the (+ 1) successes have disappeared to be replaced by the highly significant (+ 2) score given in Table VIIA.

COMMENTS ON TABLES VII AND VIIA. (A) Even when the (+ 2) score of 6·79 × St. Dev. is considered as the best of the five scores (± 1), (± 2), (0), the odds against chance are more than one hundred millions to 1, and therefore highly conclusive.

(B) The simplest hypothesis by which to account for this conversion of (+ 1) precognitive hits into (+ 2) hits is to suppose that B.S. possesses a span of telepathic precognition which ranges between 2 and $3\frac{1}{2}$ seconds into the future, or possibly between 2 and 4 seconds. If the object of presentation is closer to his " present instant " than, say, two seconds, it is perhaps too near in time for him to perceive it and so he cognises the next object which is within his span. We cannot of course attempt to fix too accurately the upper and lower limits of this supposed span of precognition, but the fact that B.S. invariably fails to cognise telepathic presentations at 5 seconds ahead of his " present instant " (see ff p. 58) strongly suggests that the upper limit is somewhere around $3\frac{1}{2}$-4 seconds. All that it is permissible to affirm is the possible existence of upper and lower limits. What these exact limits are could only be determined, if at all, by more accurate kinds of experiment. With our present rough methods it is impossible to do more than to suggest lines for future research.

An alternative hypothesis is to postulate that B.S. developed a fixed precognitive time-*habit* engendered by the " normal " rate of calling to which he had become accustomed during the earlier experiments. If his span into the future remained fixed while we doubled the rate of calling, his successes would fall on (+ 2) guesses instead of on (+ 1). The fact that he fails entirely to precognise presentations at 5 seconds ahead, might be due to the fact that at this slow rate of calling the activity permitted to his conscious mind militates against the emergence of the *psi* faculty from the subliminal level. The conception of a " censor " at the turnstile between conscious and subconscious levels, opposing the emergence of the *psi* faculty into our everyday lives, is a hypothesis suggesting further types of experiment.

EXPLANATION OF TABLES VIII and IX. Table VIII shows the total scores in the five categories (0), (±1), (±2) for the "counters" experiments at "normal" rate, with R.E. as Agent. Except in the case of the (+1) scores, expectations are evaluated as $N/5$.

Table IX is a similar table for the "rapid" experiments with counters with R.E. as Agent.

TABLE VIII

(Counters) (Normal Rate) (Agent: R.E.)

E = expected number of hits; O = observed number of hits.

	(−2)[1]	(−1)	(0)	(+1)	(+2)[1]
Trials -	1510	1577	1644	1578	1512
E - -	302·0	315·4	328·8	321·19	302·4
O - -	300·0	302·0	312·0	439·00	286·0
Dev. - -	−2·0	−13·4	−16·8	+117·81	−16·4
St. Dev. -	15·54	15·88	16·22	15·89	15·55

It is seen from the above Table that there are no significant deviations except on (+1) scores.

TABLE IX

(Counters) (Rapid Rate) (Agent: R.E.)

	(−2)	(−1)	(0)	(+1)	(+2)
Trials -	789	829	868	831	794
E - -	157·8	165·8	173·6	166·83	159·45
O - -	147·0	168·0	182·0	154·0	236·00
Dev. - -	−10·8	+2·2	+8·4	−12·83	+76·55
St. Dev. -	11·23	11·52	11·78	11·53	11·27

In the above Table "E" for (+1) and (+2) trials is evaluated by Stevens' method. None of the deviations is significant except the (+2) precognitive deviation.

EXPERIMENTS AT SLOW RATE OF CALLING. Between 6 June 1941 and 14 November 1941 we carried out experiments in which successive calls were separated by an interval of just five seconds as timed by a stop-clock. Counters were used in the first test of this kind but in subsequent tests prepared random numbers were employed. On each occasion that this slow rate was employed, B.S. grew very irritable, saying it was useless for him to continue and that "it was enough to drive him mad". He invariably failed to score significantly at the five seconds rate. Suggestions that he would eventually succeed appeared to have no effect.

In the experiments of 24 September 1941 and 14 November 1941, the five seconds rate tests were regularly alternated with experiments at "normal" rate in batches of 50 calls (See Table X, TPS/TPN). The results of all (+1) scores at 5 seconds rate with R.E. as Agent are shown in Table X.

[1] The numbers of (+2) and (−2) trials are not precisely equal owing to the effects produced by P.'s losing step and to occasional gaps, as described.

TABLE X

(Counters or PRN) (Tel.) (Slow Rate) (Agent : R.E.)

(Results of all (+1) scores at 5 seconds rate of calling with R.E. as Agent)

Date	(+1) Trials	(+1) Hits	E (+1) Stevens	Value of χ	Type	
6. 6.41	192	40	38·38	<1	Counters	TPS
24. 9.41	192	38	37·54	<1	(PRN)	TPS/TPN
10.10.41	192	43	37·83	<1	(PRN)	TPS
14.11.41	192	36	37·96	<1	(PRN)	TPS/TPN
Totals	768	157	151·71	<1		

None of the scores in the other four categories, (0), (±2), (−1), is significant.

It is interesting to compare the (+1) scores at " slow " rate with those at " normal " rate in the " alternated " series (See List of Scores under dates 24 September 1941 and 14 November 1941, pp. 107 and 111).
We have

TABLE XA

	(+1) Trials	(+1) Hits	E (Stevens)	St. Dev.	χ
Slow - -	384	74	75·50	7·84	<1
Normal - -	384	112	77·29	7·84	4·43

Thus in the alternated series for " slow " rate we have $\chi = <1$, while at " normal " rate $\chi = 4{\cdot}43$ with $P < \frac{1}{2}{\cdot}10^{-4}$ when the (+1) score is considered as the best of the five scores (0), (±1), (±2). In other words, in these two experiments the results on (+1) scores at " normal " rate show odds against chance of about 5,000 to one, whereas at the " slow " rate the results are consistent with chance. No further comparison is required.

Experiments with Other Agents. Twelve other persons besides R.E. have at various times endeavoured to act as Agent in conjunction with B.S. (two for only a few calls). With two of these, Mrs G.A. and Mr J.Al., the results were quite as brilliant as those obtained with our first Agent, R.E. Quite early on in the investigation B.S. tried to obtain results with his wife as Agent, but was unsuccessful. None of the scores was significant and the reader is referred to List of Scores, pp. 90, 93, for the details.

On 21 February 1941, after R.E. had acted as Agent for four sheets, K.M.G. took her place, while R.E. watched K.M.G. R.E. sat about three feet away but in a position where she could certainly have seen the cards in the box if she had chosen to do so. It is quite possible therefore that R.E. was the unconscious Agent on this occasion, though she reports that she was watching K.M.G. the whole while and did not look at the cards. But there is an interesting difference between the results scored with K.M.G. as Agent and those previously obtained earlier in the sitting with R.E. as Agent. This will be apparent from the following Tables, XI and XIA.

TABLE XI

(PRN) (C. Normal Rate) (Agent : R.E.)

Results on 21 February 1941 with R.E. as Agent

	(−2)	(−1)	(0)	(+1)	(+2)	Type
Trials -	184	192	200	192	184	(TP)
E - -	36·8	38·4	40	39·83	36·8	
O - -	31	33	56	50	28	
Dev. - -	−5·8	−5·4	+16	+10·17	−8·8	
St. Dev. -	5·43	5·54	5·66	5·54	5·43	
χ - -	−1·07	<1	+2·83	+1·84	−1·59	

TABLE XIa

(PRN) (C. Normal Rate) (Agent : K.M.G. (?))

Results on 21 February 1941 with K.M.G. (?) as Agent

	(−2)	(−1)	(0)	(+1)	(+2)	Type
Trials -	184	192	200	192	184	(TP)
E - -	36·8	38·4	40	39·21	36·8	
O - -	40	34	44	61	33	
Dev. - -	+3·2	−4·4	+4·0	+21·79	−3·8	
St. Dev. -	5·43	5·54	5·66	5·54	5·43	
χ - -	<1	<1	<1	3·93	<1	

A comparison of the two tables shows that while with R.E. as Agent (P) scored just significantly on the actual card (O) ($P \sim 5 \times \cdot 005 = \cdot 025$), he did not on this occasion obtain any significant success on (+1) guesses. When working with K.M.G. on the other hand, (P) scored no success on the actual card but obtained a highly significant score on (+1) guesses ($P < 5 \times \cdot 0001 = \cdot 0005$). It may be therefore that K.M.G. did have some influence on the results even if R.E. was the unconscious Agent : though perhaps one should hardly suggest conclusions on so limited a number of guesses with a new Agent. K.M.G. acted as Agent again on 25 April 1941 when R.E. was absent. Three sheets only of (PRN) were completed, but the results were not significant.

Results with Mrs G.A. as Agent. Mrs G.A., who is a friend of K.M.G., visited the Studio on 16 May 1941 and 23 May 1941 and acted as Agent on both occasions. On the first of these dates R.E. was absent, but on the second she was present and advantage was taken of the fact that Mrs G.A. had proved herself a good Agent to carry out a special experiment in which two Agents worked together.

The results which B.S. obtained when working with Mrs G.A. appear to be similar in character to those obtained with R.E. That is to say, at the "normal" rate he scored significantly on (+1) guesses, but not on any of the other categories.

On 16 May 1941, on 288 (+1) trials B.S. scored 82 (+1) hits as against an expectation (by Stevens) of 57·79. This is equivalent to 3·56 × Standard Deviation ($P \sim 5 \times \cdot 0004 = 002$).

Special Experiment with Two Agents : 23 May 1941. During the first part of this sitting, R.E. acted as Agent for 200 (PRN) calls. This was R.E.'s first reappearance at the Studio since 18 April, her absence having been necessitated by her mother's illness and death. K.M.G. was unavoidably absent on this date. Throughout these 200 calls, Dr Wiesner acted as (EP) and S.G.S. as (EA) while Mrs G.A. sat by the side of the box in a position from which she could not see the five cards. As may be seen from Table I (p. 46) B.S.'s score on (+1) trials reached $4{\cdot}03 \times$ Standard Dev. After these results had been decoded and checked, we commenced on the special experiment. R.E. and Mrs G.A. were each handed four small cards on which were inscribed the following choices of codes

R.E.	Mrs G.A.
G L E P Z	L G P Z E
P E Z G L	L Z P E G
P E L G Z	Z G P L E
G E L P Z	Z P G L E

These are arranged so that no letter in any one of R.E.'s four codes is the same as the letter of the corresponding position in any one of Mrs G.A.'s four codes. The two Agents were asked to select any *three* from their four codes in any order, unknown both to S.G.S. and to Dr Wiesner. B.S., who remained in the ante-room and knew nothing whatever of the proposed special experiment and the arrangements being made in the Studio, was merely told that for the next three sheets Mrs G.A. would be the Agent. He was given no hint that while Mrs G.A. sat behind the screen R.E. would be seated a few feet farther back with a second set of five animal picture cards in front of her. The two Agents were asked to select their codes privately in any order, and to change from one code to another at the end of each sheet of 50 calls. The two sets of codes being mutually exclusive, it followed that at each call the two Agents were looking at different animal pictures. Mrs G.A. sat in front of the box in the usual position, while R.E. sat at a small table some six feet behind her and so that she also could see the random numbers as they appeared at the aperture in the screen. S.G.S. acted as (EA) while Dr Wiesner sat with B.S. in the ante-room acting as (EP).

The two Agents having each selected a code from the four available on their cards, arranged their five picture-cards in the order indicated by the code: the cards being as usual laid face downwards on the floor of the box or table. The experiment then proceeded in the ordinary way and the results were decoded and checked at the end of the third sheet. At every call the two Agents were competing in opposition. The result was that R.E. failed absolutely to influence B.S., although she had succeeded earlier in the afternoon when working alone as Agent. B.S. had been told that Mrs G.A. was the Agent, and things happened just as if R.E. had not been present.

A similar phenomenon is often observed when two persons sit in the same room with a medium. If the medium is told at the beginning of a successful sitting that visitor (A) is the sitter, and visitor (B) the note-taker, the medium makes contact with (A) and not with (B), and the information which is forthcoming generally applies to the circumstances of (A) and not to those of (B).

The difference between the two sets of scores is very striking. Thus we find for (+1) scores:

Agent	Sheet No.	5a	5b	6a	6b	7a	7b	Totals
Mrs G.A.	- -	8	10	11	10	10	8	57
R.E.	- - -	4	3	4	3	5	6	25

There is, of course, no question with such scores as to who was the real Agent in this experiment.

For Mrs G.A. we have $\chi = 5{\cdot}45$
For R.E. we have $\chi = < 1$

It would be of interest to make obvious variations on the theme of two Agents, but these must wait for favourable circumstances. The scores made by the two Agents on the remaining categories (± 2), (0), (-1) are of no interest. See List of Scores, p. 102.

TABLE XII. In Table XII are shown all the (+1) results with Mrs G.A. as Agent.

TABLE XII

(PRN) (Tel.) (Normal Rate) (Agent: Mrs G.A.)
(All precognitive (+1) results with Mrs G.A. as Agent)

Date	Av. Int.	(+1) Trials	(+1) Hits	E(+1) Stevens	CC(+1)	Values of χ	Type
16.5.41	3·01	288	82	57·79	53	3·56	TP
23.5.41	2·88	144	57	30·83	29	5·45	TP (2 Agents)
Totals	—	432	139	88·62	82	6·06	

(For $\chi = 6{\cdot}06$ we have $P = 5 \times 1{\cdot}97 \times 10^{-9} = 9{\cdot}8 \times 10^{-9}$)

EXPLANATION OF TABLE XIII: "GROUP 1" comprises *all* "telepathy" experiments at "normal" rate with Agents R.E. and Mrs G.A., plus two other scores—see ff.

As the results obtained with our third Agent, Mr J.Al., have distinguishing characteristics of their own, we propose to exclude them from Group 1 and devote a special section to them (Group 3). In addition to the three principal Agents (R.E., Mrs G.A., and Mr J.Al.) more or less superficial tests were made with ten other persons with whom the results were negative. These trials, except in the case of Mrs B.S., seldom exceeded 150 calls, and it is quite possible that had we gone on with some of these people significant results would have ultimately appeared. These tests with other Agents were mostly carried out while R.E. was absent owing to her mother's illness (25 April—16 May 1941). The Agents with whom B.S. failed will be collated in a single group (Group 2).

Group 1 comprises the totals for *all* "telepathy" at "normal rate" done with the Agents R.E. and Mrs G.A. In addition, however, we have included the highly significant scores obtained on 21 February 1941 when, it will be remembered, K.M.G. acted as Agent with R.E. sitting in a position from which she also could have seen the cards (*cf.* pp. 59-60). There is also included (by inadvertence) one set of 50 calls during which Dr C. E. M. Joad took R.E.'s place as Agent for one sheet only (see *The Chronicle*, Sitting No. 26).

There seems no point in adding together the results of Groups 1 and 2, but the reader is at liberty to do this if he pleases : the significance of the results will be scarcely affected. He may, if he wishes, add on all the negative " clairvoyance " tests and the negative tests at " slow rate ". The resulting (+1) score will still be immensely significant.[1] But there seems no point in mixing together results which are obtained under experimental conditions widely differing from one another. We have taken together the work of R.E. and Mrs G.A. in Group 1 because the results with these two Agents are similar in character and so form a group which is distinct from Groups 2 and 3. If the experiments with J.Al. (Group 3) were added the significance of the scoring would be greatly increased.

TABLE XIII

Group 1 : *all* "telepathy" experiments with Agents R.E. and G.A., also K.M.G. (?) and C.E.M.J. (50 calls)

(PRN or COUNTERS) (Tel.) (Normal Rate)

	(−2)	(−1)	(0)	(+1)	(+2)
Trials - -	5784	6037	6290	6039	5788
E - -	1156·8	1207·4	1258	1234·51	1157·6
O - -	1134	1213	1288	1755	1094
Dev. - -	−22·8	+5·6	+30	+520·49	−63·6
St. Dev. - -	30·42	31·08	31·72	31·08	30·43
χ - - -	<1	<1	<1	+16·7	−2·09

It is seen that apart from the very large (+1) deviation none of the other scores is significant.

TABLE XIV

Group 2 : Eight other (unsuccessful) Agents (Tel.) (Normal Rate) (+1) Scores

Date	Agent	Av. Int.	(+1) Trials	(+1) Hits	E(+1) Stevens	CC (+1)	Value of χ	Type of Experiment
14.2.41	Mrs B.S.	NT	48	10	10·42	4	−0·1	(PRN) TP
7.3.41	,,	NT	192	40	38·83	41		(PRN) TP
25.4.41	K.M.G.	4·01	144	28	28·88	31	−0·2	(PRN) TP
,,	S.G.S.	2·80	144	30	28·83	23	+0·2	(PRN) TP
,,	Miss H.	2·77	48	8	10·17	6	—	(PRN) TP
2.5.41	Dr W.	3·24	144	31	29·38	25	+0·3	Counters TP
,,	Mr B.	2·60	144	24	28·50	27	−0·9	,, ,,
9.5.41	Mr Ch.	NT	96	9	19·17	22	−1·53	,, ,,
16.1.42	,,	2·66	96	21	19·33	21		,, ,,
16.1.42	Miss K.	2·54	96	21	19·96	18	+0·3	,, ,,
Totals	—	—	1152	222	233·47	218	<1	

Note.—K.M.G. is included in this table as well as in Table XIII as K.M.G.(?). One other person who acted as Agent for a few calls only is excluded.

[1] 13·6 × St. Dev.

COMMENTS ON TABLE XIV. An inspection of Table XIV shows that neither individually nor collectively did these 8 Agents score any success on (+1) guesses. Nor are any of the other categories significant. On the other hand, the examination of these eight Agents was a superficial one. With our access to such first-class Agents as R.E., Mrs G.A. and J.Al., there was no need for us to waste time putting B.S. through long series of tests with inferior Agents.

It may be asked " Do the three good Agents possess any common mental characteristic? " We think that two of them (R.E. and J.Al.) display great passivity of temperament. Both are very intelligent and at the same time very quiet. B.S., on the other hand, is a nervous type, assertive, and a good talker, and has the quick temperament associated with artists. But whether this contrast in psychological type between Agent and Percipient is important, we do not know. The contrast in type between B.S. and Mrs G.A. is not so marked.

EXPERIMENTS WITH MR J.AL. (Group 3). At the end of December 1941, R.E. took up a full-time war job which left her with little leisure for helping with the experiments. During the early winter months of 1942 B.S. considered that his Studio was too cold for us to continue with the experiments in the evenings, especially as his health was not so good as usual. We therefore abandoned the investigation until the beginning of May 1942. K.M.G. visited B.S. on a single occasion during this period (16 January 1942) and conducted some experiments which have been included for the sake of completeness. (See *The Chronicle*, sitting No. 30, and List of Scores, p. 116.)

In the meantime S.G.S. got into touch with Mr J.Al., who had been one of the principal Agents in the 1936 experiments. S.G.S. has known J.Al. for a good many years and can vouch for his integrity. When working with this Agent in 1936, B.S. scored about equally well on both (+1) guesses and on (−1) guesses.[1] We had been puzzled by the fact that with both R.E. and Mrs G.A. he had scored significant results on (+1) guesses only. We were therefore interested to discover whether B.S. would continue to score only (+1) successes with J.Al. or whether the (−1) scores would now become significant as well.

Throughout the work with J.Al. as Agent (9 sittings), the Initial Letters were used instead of the Animal Pictures, but before commencing the experiments J.Al. was shown the five animal pictures and it was explained to him that the letters were the initials of the five names of the animals. A week before the first resumed sitting, S.G.S. told B.S. that a new Agent was coming the following week, but he was not told that this was J.Al. whom he had not seen since June 1936. To the best of S.G.S.'s knowledge B.S. is unaware of J.Al's address. K.M.G. was absent from the first three sittings (May 8, 15, 22) and her place was taken by R. G. Medhurst, B.Sc. As already recorded, Medhurst himself carried out the whole of the work of decoding and counting of hits on each of these three dates.

It will be seen by reference to Table XV that significant (−1) postcognitive results were in evidence at the first two sittings while the (+1) precognitive

[1] See *Proc.* xlvi, 186.

scores were barely significant. This was a great surprise. However, at two subsequent sittings on 12 and 26 August, the (+1) precognitive results grew much stronger while significant (−1) postcognitive effects were still being produced. As is evident from Table XV, B.S. on the four sittings taken together scored equally well on (+1) and (−1) trials.

TABLE XV

(PRN) (Tel.) (Normal Rate) (New Agent: J.Al.)
(Initial Letters used in place of Animal Pictures)

			Postcognitive			Precognitive		
Date	Av. Int.	No. of Trials	(−1) Hits	E(−1) Stevens	Value of χ	(+1) Hits	E(+1) Stevens	Value of χ
8.5.42	2·72	192	52	36·79	2·74	46	36·17	1·77
15.5.42	2·53	168	50	35·54	2·79	42	35·87	1·18
12.8.42	2·52	168	51	33·50	3·38	56	34·08	4·23
26.8.42	2·28	192	54	38·50	2·90	59	38·71	3·66
Totals	—	720	207	144·33	5·84	203	144·83	5·42

Comments on Table XV. (A) It is clear that B.S. has obtained highly significant scores on both (+1) and (−1) guesses. If p is the (small) chance of getting a deviation as high as $5{\cdot}42 \times$ St. Dev., it is easily seen that the chance of getting *at least two* of the five scores (± 1), (0), (± 2) with deviations as high as $5{\cdot}42 \times$ St. Dev., is approximately $10p^2$.

But for $\chi = 5{\cdot}42 \quad p < 6 \times 10^{-7}$

Hence
$$P < 10 \times 36 \times 10^{-14}$$
$$= 3{\cdot}6 \times 10^{-12}$$

(B) In 1936 with J.Al. as Agent, B.S. working on Zener cards obtained 5·38% of "true cognitions" (*i.e.* successful hits not due to chance) on (± 1) guesses taken together. It can be shown from the results of Table XV that in the present (1942) series with J.Al. he scored 10·59% of "true cognitions" on (± 1) trials taken together. Surprisingly however this difference is NOT significant, even though one percentage is almost double the other. For discussion see Appendix A, p. 131.

(C) In the present series of experiments with J.Al. as Agent at "normal" rate there is, as in the 1936 series, plenty of evidence that the (+1) and the (−1) effects tend to reinforce each other in the case of the multiply-determined presentations (*cf.* Introduction, p. 31). This reinforcing effect is entirely absent throughout the work with R.E. and Mrs G.A. as Agent, as might perhaps be expected, since in the presence of these Agents only (+1) cognitions were registered. For a full discussion of the statistical problems involved and the experimental data, see Appendix B, p. 134 ff.

(PRN) Experiments at Rapid Rate. On 22 May 1942, 5 June 1942, and 6 January 1943, we carried out "rapid" rate experiments using prepared random numbers instead of counters. The modified procedure on all three occasions was as follows:

The screen which stood on the card-table was removed, and the five cards which bore the numerals 1-5 were arranged in order on the *top* of the box which was in its ordinary place on the table. The Agent, J.Al., sat in the usual position, and having shuffled the five " Letter " cards out of sight of (EA) and (EP), laid them face downwards in a row inside the box. (EP) sat with B.S. at his desk in the ante-room and the latter had before him five " Letter " cards similar to those in front of the Agent. (EA) stood facing J.Al. on the near side of the box, with his prepared lists of random numbers. He called 1—2—3—25 at a rapid rate, and as he called each serial number he touched with a pencil the number card on top of the box corresponding to the figure on his prepared list. As J.Al. saw the numeral touched with (EA)'s pencil, he instantly jerked up the corresponding card inside the box and dropped it down into its place again. As B.S. heard the serial number of the call, he instantly touched with a pencil one of the five letters L, E, G, P, Z, in front of him. (EP) recorded the letter touched in the appropriate cell of the G column.

The experiment went without a hitch on all three occasions; there were no gaps and B.S. was never out of step. It should be noted that as J.Al. was new to the technique, on each of the first two occasions the real experiment was preceded by a rehearsal in which (EA) and J.Al. took part using the previous week's lists of random numbers. During this try-out S.G.S. sat by the box to verify that J.Al. was able to synchronise the lifting of the cards with (EA)'s calls at the required rapid rate.

The results are recorded in Tables XVI and XVIA.

TABLE XVI

(PRN) (TL) (Rapid Rate) (Agent : J.Al.)

(+1) and (−1) Scores

		Postcognitive				Precognitive				
Date	Av. Int.	(−1) Trials	(−1) Hits	E(−1) Stevens	Value of χ	(+1) Trials	(+1) Hits	E(+1) Stevens	Values of χ	Type
22.5.42	1·37	168	37	33·75	<1	168	39	34·42	<1	TL
5.6.42	1·39	192	46	37·79	+1·48	192	44	38·42	+1·01	TL
6.1.43	1·44	192	29	38·21	−1·66	192	43	38·25	<1	TL
Totals	—	552	112	109·75	<1	552	126	111·09	1·59	

TABLE XVIA

(PRN) (TL) (Rapid Rate) (Agent : J.Al.)

(+2) and (−2) Scores

		Postcognitive				Precognitive				
Date	Av. Int.	(−2) Trials	(−2) Hits	E(−2) Stevens	Value of χ	+2) Trials	(+2) Hits	E(+2) Stevens	Value of χ	Type
22.5.42	1·37	161	45	31·91	2·57	161	50	32·52	3·44	TL
5.6.42	1·39	184	36	34·92	<1	184	57	37·35	3·62	TL
6.1.43	1·44	184	70	36·22	6·23	184	42	36·61	0·99	TL
Totals	—	529	151	103·05	5·21	529	149	106·48	4·62	

COMMENTS ON TABLES XVI AND XVIA. (A) The results of the three (PRN) experiments at "rapid" rate with J.Al. as Agent appear to confirm the discovery made in the counters experiments when R.E. was Agent (see pp. 55-56). In the present series not only do the (+1) precognitive hits tend to be replaced by (+2) hits, but on two of the three occasions the postcognitive (−1) hits appear to undergo a temporal shift and become (−2) hits.

On the occasion of the first experiment (22 May 1942) these two effects seem to proceed simultaneously, but in the second experiment (5 June 1942) it is only the precognitive hits that are affected and neither the (−1) nor the (−2) effects are significant. The third experiment (6 January 1943) was carried out after B.S. had had about 17 weeks' rest and the results are very remarkable. Apparently there were no precognitive effects at all, but instead a violent switch over to (−2) postcognitive effects is noted.

(B) An analysis of the sitting on 6 January 1943 from the standpoint of multiple-determination reveals a very curious result. There are in all 23 cases of (±2) multiple-determination—equivalent to 46 (±2) trials. On these 46 trials there are no less than 32 (±2) successes as compared with an expectation of 18·80. (This expectation is computed on the mutually exclusive hypothesis, see Appendix B, p. 134 ff).

Hence we have the following Table showing the successes (S) and failures (F) in the expected (E) and observed (O) classes.

6 January 1943. (Agent: J.Al.) (Rapid Rate)

	S.	F.	Totals (±2 M.D. trials)
O - - - - - -	32	14	46
E - - - - - -	18·80	27·20	46

With Yates's correction $\chi^2 = 14{\cdot}508$ or $\chi = 3{\cdot}80$: a highly significant result ($P < 10^{-4}$).

It would seem that though on this occasion the precognitive (+2) effect was so weak that it showed no direct indication of its presence, yet when reinforced by the strong (−2) effect it was revealed by the large proportion of multiply-determined cognitions.

The fact that B.S.'s *psi* faculty can still flare up to 6·23 standard deviations two years after the commencement of the experiments testifies to the strength of his paranormal ability.

Mr L. A. Rozelaar, M.A. (Lecturer in French at Queen Mary College, London University) who acted as (EA) on this occasion wrote:

"I carried out every step of decoding and checking-up, with Mr S.G. Soal (EP) checking every step of the process. The Agent did not speak during the guessing nor did I notice any signs of his signalling by shuffling of feet, coughing or in any other manner. I inspected and recorded the code at the end of every 50 guesses."

Mr Rozelaar took away with him a list of the totals in each of the five categories (0), (±1), (±2). He also compared each original scoring sheet carefully with the corresponding duplicate and himself posted the duplicates to Professor Broad.

Experiments with Non Random Series. Mr C. A. Mace, M.A., who witnessed our experiments on 21 Dec., 1941, made the suggestion that it would be of great interest to discover whether B.S. would succeed with a series of number presentations chosen not at random but pre-arranged in some improbable order.

On the three dates 15 May 1942, 22 May 1942 and 12 August 1942, it was arranged that among the (PRN) sheets should come a single column of 25 calls in which the first twelve numbers were all the same digit, and the last thirteen the same (different) digit. B.S. was given no hint that the " doctored " column differed in any way from the others. The results of the first experiment of this kind are not so striking as those of the other two.

TABLE XVII

Experiments with Non-random Series

(i)

15 May 1942 (Tel.) (Normal Rate) Sheet 1 column (b)

A	5	5	5	5	5	5	5	5	5	5	5	5	3	3	3	3	3	3	3	3	3	3	3	3	3
G	2	3	3	4	4	4	3	3	2	4	4	5	4	1	3	2	3	3	4	5	4	3	3	3	1

(ii)

22 May 1942 (Tel.) (Rapid Rate) Sheet 1 column (b)

A	1	1	1	1	1	1	1	1	1	1	1	1	3	3	3	3	3	3	3	3	3	3	3	3	3
G	3	3	5	1	1	1	5	3	1	1	1	3	1	4	5	3	3	3	4	3	3	3	5	5	2

(iii)

12 August 1942 (Tel.) (Normal Rate) Sheet 2 column (a)

A	3	3	3	3	3	3	3	3	3	3	3	3	1	1	1	1	1	1	1	1	1	1	1	1	1
G	3	4	3	1	2	5	3	3	3	3	1	2	5	3	5	2	1	1	1	5	1	1	2	1	1

A =" Actual Card " Column.
G =" Guess " Column.

It will of course be understood that (P) does not actually guess the digit itself but the corresponding animal (Initial Letter) card looked at by (A). It is only when decoding has taken place that we get the animal cards represented by figures.

In all three cases the Agent was J.Al. It will be seen that the total numbers of " direct " hits are :

7 (first occasion)
12 (second occasion)
(13) third occasion)

In example (i) the Percipient does not seem to respond to the experiment until the second half of the column.

We may examine the question statistically as follows : the " relevant " digits are 5 and 3 in example (i). We may count how many 5's and 3's there are in the " correct " part of the column and how many in the " wrong ". As 3 and 5 were guessed 12 times, we might expect 6 relevant digits to be in the " correct " part and 6 in the " wrong " part. Thus we have the following Table :

TABLE XVIIa

Date	Correct Part	Wrong Part	Total	Digits	Letters
15.5.42	7	5	12	(3, 5)	(L, E)
22.5.42	12	5	17	(1, 3)	(P, G)
12.8.42	13	3	16	(3, 1)	(Z, P)
Observed Number	32	13	45		Totals
Expected Number	22·5	22·5	45		,,

With Yates's correction $\chi^2=7{\cdot}20$; $\chi=2{\cdot}7$ which is definitely significant.

But there is another method of approach. We may ask: When, as in example (i), a 5 is presented a large number of times, does B.S. tend to guess more of this digit than he does on ordinary occasions? From the totals given for each of the 5 digits in the G columns of the scoring sheet, we may, *on the assumption that B.S.'s guesses are fairly randomly mixed*, estimate the number of 5's that would be expected to appear in a column of 25 calls and similarly in the case of all the other " relevant " digits. We thus obtain :

TABLE XVIIb

	Relevant Digits	Non-Relevant	Total
Observed Number - -	32	43	75
Expected Number - -	14·873	60·127	75

This gives $\chi^2=23{\cdot}19$; $\chi=4{\cdot}8$—with Yates's correction, a highly significant result.

An examination of B.S.'s calls shows that the assumption of random mixing is very well justified. There seems therefore to be a decided tendency for B.S. to guess a given digit more frequently when that digit is presented in excess in a set of 25 calls than when its frequency is normal.

The results of the three " non-random " columns which had of set purpose been mixed among the (PRN) columns are of course *not* included in Tables XV and XVI which record the (PRN) experiments of these dates.

Special Experiment of 26 August 1942 (" Lift and Touch " Experiment). It was explained to B.S. before we began that we wished to compare results obtained with the Agent J.Al.

(*a*) having sensation (sight) of the letter at each call, and
(*b*) the Agent having only memory content without sensation at each call.

The experiment was carried out as follows : During the (*a*) column of each sheet, J.Al. lifted the cards and looked at them as usual, thus getting to know their order ; but during the succeeding (*b*) column, the Agent merely touched the backs of the cards without lifting them or looking at the faces. The order of the five cards in the box remained the same until the end of the whole sheet, when the cards were as usual re-shuffled by (A). We designate the two types of test as "Lift" and "Touch" respectively.

There appears to be no significant difference between the two methods. Apparently, so far as the limited data indicate, experiments with "memory content" only seem to be as successful as those where there is "sensation content" as well. The experiments were all at "normal" rate. Taking (± 1) guesses together, the results for "Lift" and "Touch" are given in Table XVIII. [For further experiments on these lines, *cf.* Sittings 39 and 40, p. 75 ff.]

TABLE XVIII

(TL) (Normal Rate) (Agent: J.Al.)

Type	(± 1 Trials)	(± 1 Hits)	E(± 1) (Stevens)	Value of χ
"Lift"	192	60	39·04	3·79
"Touch"	192	53	38·17	2·69
Totals	384	113	77·21	4·56

COMMENTS ON TABLE XVIII. It is seen that both types yield significant results. Using Fisher's method for the comparison of means of small samples we have for 24 trials

$$\bar{x} = \text{Mean Score for "Lift"} = 7{\cdot}500 \text{ with } n_1 = 7$$
$$\bar{x}' = \text{Mean Score for "Touch"} = 6{\cdot}625 \text{ with } n_2 = 7$$
$$S(x-\bar{x})^2 = 30{\cdot}000$$
$$S(x'-\bar{x}')^2 = 27{\cdot}875$$
$$s^2 = 57{\cdot}875/14 = 4{\cdot}134$$
$$t = \frac{\bar{x}-\bar{x}'}{s}\sqrt{\frac{(n_1+1)(n_2+1)}{n_1+n_2+2}} = 0{\cdot}861$$
$$\text{whence } 0{\cdot}5 > P > 0{\cdot}4$$

a value which is *not* significant.

Otherwise: If we suspected that each "Lift" score was positively correlated to the immediately following "Touch" score we might proceed as follows:

Sheet	L (± 1) Score	T (± 1) Score	$L-T=x$
1 - - - -	20	15	+5
2 - - - -	10	11	−1
3 - - - -	15	15	0
4 - - - -	15	12	+3
Totals - - - -	60	53	+7

$$\text{Whence Mean of } (L-T) = +1{\cdot}75 = \bar{x}$$

Now $S(x^2) = 35\,;\ S(x-\bar{x})^2 = 35 - 7(1{\cdot}75) = 22{\cdot}75$

And $\dfrac{s^2}{n'} = \dfrac{22{\cdot}75}{12} = 1{\cdot}8958$

Hence $$t = \frac{1{\cdot}75}{\sqrt{1{\cdot}8958}} = 1{\cdot}27$$

Whence with $n = n^1 - 1 = 3$, $\cdot 3 > P > \cdot 2$

which again is without significance.

EXPERIMENTS WITH "ASSOCIATED WORDS" 14 AND 25 AUGUST 1941. In these experiments, as already recorded,[1] we used five white cards in the centre of which were printed in large block capitals the five words MANE, TRUNK, NECK, BEAK, and STRIPES—words having obvious associations with the five animals LION, ELEPHANT, GIRAFFE, PELICAN, and ZEBRA. The same words were printed at the tops and bottoms of the cards in smaller letters so that the Agent would recognise the card immediately the end was raised from the table. The Percipient, B.S., was never shown these cards, which S.G.S. took home with him at the end of each sitting.

On 14 and 25 August 1941 we regularly alternated the ordinary "picture cards" with those bearing the "associated words" in batches of 50 calls. Thus on 14 August during Sheets 1, 3, 5, 7 the Agent (R.E.) looked at pictures of animals, while on sheets 2, 4, 6, 8 she looked at the associated words. (EA) would call out "First Sheet—Pictures"; "Second Sheet—White Cards"; etc., so that while B.S. knew that there was *some* change, he was unaware of its nature. The experiment was continued at the next sitting on 25 August. At B.S.'s own request on these two occasions five cards bearing the pictures of the animals were laid on the table in front of him and instead of B.S. himself writing down his guess he merely touched one of the five pictures with a pencil and the guess was recorded by S.G.S., acting as (EP). S.G.S. noted that B.S. always touched the card from ·2 to ·4 of a second after the call. The delay was never more than ·4 of a second as timed with a stop-watch, but more often it was about ·2 of a second. B.S. used this method of touching a card instead of writing his guess because he felt it involved less conscious effort. We employed it on several subsequent occasions. The combined results for these two Sittings are given in Table XIX.

TABLE XIX

(PRN) (Tel.) (Normal Rate) (Agent: R.E.)

(Experiments with Animal Pictures alternated with Associated Words)

Type of Experiment	(+1) Trials	(+1) Hits	E(+1) Stevens	Value of χ
Pictures - - - -	336	94	68·21	3·52
Words - - - -	336	89	67·42	2·94
Totals - - - -	672	183	135·63	4·57

COMMENTS ON TABLE XIX. It is seen that on both "Pictures" and "Associated Words" significant results were registered. The mean scores are worked out for a set of 24.

[1] See p. 45.

We have $\bar{x}$ = Mean Score for Pictures = 6·71

$\bar{x}'$ = ,, ,, ,, Words = 6·36

$$n_1 = 14 - 1 = 13 \qquad n_2 = 14 - 1 = 13$$

$$s^2 = \tfrac{1}{26}\,[55{\cdot}26 + 80{\cdot}96] = 5{\cdot}24$$

Where $S(x-\bar{x})^2 = 55{\cdot}26$

and $S(x'-\bar{x}')^2 = 80{\cdot}96$

Also $\bar{x} - \bar{x}' = 0{\cdot}35$

Hence $$t = \frac{{\cdot}35}{\sqrt{5{\cdot}24}} \times \sqrt{\frac{14 \times 14}{28}} = 0{\cdot}404 \text{ with } n = n_1 + n_2 = 26$$

Whence $0{\cdot}7 > P > 0{\cdot}6$

Hence the difference (·35) in mean scores is *not* significant.

We may also test if there is any significant difference in the variances of the two scores. With the usual notation the two observed variances s_1^2 and s_2^2 are given by

$$s_1^2 = \frac{S(x-\bar{x})^2}{13} = \frac{80{\cdot}96}{13} = 6{\cdot}228 \quad (n_1 = 13)$$

$$s_2^2 = \frac{S(x'-\bar{x}')^2}{13} = \frac{55{\cdot}26}{13} = 4{\cdot}251 \quad (n_2 = 13)$$

$$z = 1{\cdot}1513\,(\log_{10} 6{\cdot}228 - \log_{10} 4{\cdot}251)$$

$$= 0{\cdot}191$$

For $n_1 = 13$, $n_2 = 13$ the 5% point for z is about ·473.
Hence there is *no* significant difference in variance.

We might also consider the mean of the differences between each "Picture" (P) score and the "Word" (W) score that immediately follows it. Thus we should have:

TABLE XX

Sheets	P	W	$P - W = x$
1, 2 - - - - - -	13	18	−5
3, 4 - - - - - -	15	11	+4
5, 6 - - - - - -	15	11	+4
7, 8 - - - - - -	11	11	0
9, 10 - - - - - -	16	10	+6
11, 12 - - - - - -	14	14	0
13, 14 - - - - - -	10	14	−4

$$\bar{x} = +{\cdot}71$$

$$S(x^2) = 109 \quad S(x-\bar{x})^2 = 109 - 5\,({\cdot}71) = 105{\cdot}45$$

$$\frac{s^2}{n'} = \frac{105{\cdot}45}{7 \times 6} = 2{\cdot}5107$$

$$t = \bar{x} \Big/ \frac{s}{\sqrt{n'}} = \frac{{\cdot}71}{1{\cdot}584} = 0{\cdot}448$$

Hence with $n = n' - 1 = 6$

$$0{\cdot}7 > P > 0{\cdot}6$$

which agrees with the first method.

The experiment therefore succeeds equally well whether Pictures or Associated Words are looked at by the Agent. We have also seen that it succeeds when the Initial Letters of the animal names are used.[1] We think it very probable that *any* symbols which the Agent interpreted as meaning Lion, Elephant, etc. would serve just as well.

Experiment with Zener Cards. We have carried out only one experiment during the present series with Zener cards. This test was carried out at the end of the highly successful " Lift and Touch " experiment on 26 August 1942. The technique was the same, except that the five Zener symbols were substituted for the Initial Letters of animals as used in the " Lift and Touch " experiment. B.S. made his guess each time by touching one of the five Zener cards laid on the table in front of him, his guesses being recorded by S.G.S. Three sheets were completed but none of the five scores (0), (± 1), (± 2) was significant.

The results are given below :

TABLE XXI

	(−2)	(−1)	(0)	(+1)	(+2)
Trials - - -	138	144	150	144	138
Hits - - -	27	32	30	36	27
Expected - -	27·6	28·8	30	28·8	27·6
χ - - -	<1	<1	0	$+1{\cdot}5$	<1

The result was a failure, but coming as it did at the end of more than eighteen months' work with the animal symbols, we may conjecture that a considerable re-education would be necessary before B.S. became habituated to the use of the Zener symbols again.

Special Experiment with Counters (Influence of (P) on (EA)). Two special experiments were carried out on 9 May 1941 and 7 August 1942, in the absence of the ordinary Agent, in order to discover whether B.S. could influence K.M.G.'s selection by touch of counters from a bowl. It had been suggested by someone that the apparent precognitive successes in the counters experiments (*cf.* p. 51 ff.) might in part be attributed to some influence exerted by the mind of B.S. (the Percipient) on the mind of K.M.G. (acting as (EA)), which caused her to select as her next choice the counter corresponding to the animal just recorded by B.S. in his preceding guess. Such an hypothesis would of course suppose telepathic rapport between B.S. and K.M.G. and the possession of clairvoyance on the part of K.M.G., unless we assume that she sees normally the counters she draws from the bowl in spite of conscious effort to the contrary. It would require, further, that B.S. was aware of the order of the five cards in the box either by telepathy from (A) or by clairvoyance.

[1] See p. 53, Table VI (p. 54), Table XV (p. 65), Table XVI and XVIa (p. 66).

The hypothesis seemed to us complicated and improbable in view of the fact that the experiment succeeds when prepared random numbers are used instead of counters; but we thought it worth while to see if any results could be obtained.

The experiment of 9 May 1941 was conducted as follows:

Dr Wiesner (EP) sat in the ante-room with B.S. who had in front of him the five differently coloured counters. B.S. was also given a number of scoring sheets whose "A" columns had been previously filled in by S.G.S. with random digits 1-5. Dr Wiesner shuffled the five counters so that they stood in a row in any order, the order being changed after the completion of each sheet. B.S. touched the counter whose position in the row corresponded to the random number on his sheet. He was checked by Dr Wiesner. As he touched each counter, B.S. called out "Right". On hearing this signal, K.M.G. in the Studio immediately chose a counter from the bowl while looking straight over the top of the screen as she did on ordinary occasions in the counters experiments. K.M.G. let the counters drop back into the bowl, the contents of which she stirred up at frequent intervals as usual. S.G.S., seated on the other side of the screen in the place normally occupied by (A), recorded the numbers standing for the five colours as they appeared at the aperture. After four sheets had been completed, the sheets of random numbers were decoded into colours according to the code records kept by Dr Wiesner. The sheets filled in by S.G.S. were similarly decoded and successes in the five categories counted.

None of the scores was significant. The totals are given below.

TABLE XXII

	(−2)	(−1)	(0)	(+1)	(+2)
Trials	184	192	200	192	184
Hits	38	46	37	41	32
Expected	36·8	38·4	40	38·4	36·8
x	<1	$+1{\cdot}4$	<1	<1	<1

In the experiment on 7 August 1942, no lists of random numbers were used. B.S. sat at his desk in the ante-room with five coloured counters in front of him arranged in the order

W Y G R B[1]

S.G.S. sat opposite B.S. and recorded the ordinal numbers from left to right as (P) chose and touched the counters. K.M.G. sat in the Studio at the card table with a friend, Mrs Wykeham-Martin. In front of K.M.G. was a bowl containing 245 counters, there being equal numbers of each of the five colours. B.S. touched the counters one by one with a pencil at "normal" speed, and as he touched them called aloud the serial numbers 1, 2, 3....25. On hearing the serial number K.M.G., who sat throughout with her eyes closed, drew out a counter, letting it fall back in the bowl. The corresponding number (in the order W.Y.G.R.B.) was

[1] The capitals denote the initals of the five colours.

recorded in the A column of a scoring sheet by Mrs Wykeham-Martin. At the end of each column of 25 calls there was a pause of 30 seconds—1 minute, during which K.M.G. thoroughly re-shuffled the counters in the bowl. Then the work was resumed until 10 sheets were completed.

The sequence of 500 counters drawn by K.M.G. appears to satisfy most of the tests of a random distribution, but B.S.'s selections do not produce a random distribution. The totals for the five scores (0), (± 1), (± 2) are given in Table XXIII.

TABLE XXIII

	(-2)	(-1)	(0)	$(+1)$	$(+2)$
Trials - - -	460	480	500	480	460
Hits - - -	88	83	103	99	112
Expected - -	92	96	100	96	92
χ - - -	<1	$-1{\cdot}48$	<1	<1	$+2{\cdot}33$

The value 2·33 is *not* significant since the chance of getting at least one of the scores with a χ as high as 2·33 is approximately

$$5 \times {\cdot}0198 = 0{\cdot}099 \text{ (\textit{i.e.}, odds of nearly 10 : 1)}$$

From the above experiments we obtain no evidence that B.S. can influence K.M.G.'s selection of counters from the bowl.

Two Additional Experiments at the Society's Rooms, 31 Tavistock Square, London, W.C. 1. Although nobody who witnessed our experiments suggested at any time that the conditions were not entirely watertight, it was suggested by one critic at a later date that it might be of value to hold a couple of additional sittings away from the Subject's own premises, to see whether this affected the results. B.S. was quite ready to fall in with this suggestion and two sittings were held at the Society's rooms in April 1943. Both occasions yielded highly successful results. By this time all calculations on the experiments to date had been completed, and the results of these two experiments are therefore not included in any of the Tables in this Report. The scores however are given in the Lists of Scores (sittings 39 and 40) and the procedure is described in detail in *The Chronicle*, under dates 8 and 15 April 1943.

On each occasion B.S. was taken from his Studio to 31 Tavistock Square by taxi, accompanied by S.G.S. and the Agent, J.Al. (K.M.G. being away on war work). There they were met by independent persons invited by S.G.S. to assist in the experiments. B.S. brought no personal friend or Assistant with him.

Agent and Percipient sat in separate rooms, and the conditions were similar to those in force at B.S.'s Studio.

Experiment on 8 April 1943. The experiments on this date were " Lift and Touch " experiments (as already described on p. 69). Three persons were present assisting with the experiments: Mr R. G. Medhurst, B.Sc., Mr D. Parsons, M.Sc., and Miss J. Fairbairn, B.Sc. All the decoding, checking, counting of hits, evaluating of results, and duplicating were done by Mr Medhurst and Miss Fairbairn with S.G.S. merely looking on. Mr Parsons assisted in checking the duplicates against original scoring sheets. All sheets were signed by all four experimenters, and Medhurst posted the duplicates to Professor Broad and took away with him a private record of the scores in all three categories (–1), (0), (+1).

The results for (+1) and (–1) are given in Table XXIV.

TABLE XXIV

(PRN) (TL) (Normal Rate) Agent : J.Al.

Sheets 1a, 2a, 3a, 4a = " Lift " Sheets 1b, 2b, 3b, 4b = " Touch "

Category	Trials	Hits (Both " Lift and Touch ")	E(Stevens)	Dev.	St. Dev.	χ
(+1)	192	67	39·58	+27·42	5·542	4·94
(–1)	192	56	40·46	+15·54	5·542	2·80

It will be seen that significant scores were recorded on both (+1) and (–1) trials, as is typical when B.S. works with the Agent J.Al. at "normal" speed.

The ($\pm$1) combined scores for both " Lift and Touch " are given in Table XXV.

TABLE XXV

($\pm$1) scores combined

Type	$\pm$ Trials	$\pm$ Hits	E(Stevens)	Dev.	χ
" Lift "	192	61	40·66	+20·34	3·67
" Touch "	192	62	39·37	+22·63	4·08

From Table XXV it appears that the combined ($\pm$1) score for " Touch" (memory content only) is just higher than the score for " Lift " (sensation + memory), but the difference is obviously without any significance. This confirms the results obtained on 26 August 1942 (*cf.* pp. 69-71).

Experiment on 15 *April* 1943. J.Al. was again Agent and Mr D. J. West was invited to assist at the experiment. The experiment was a variation of the " Lift and Touch " method used on 8 April. Before each sheet was called, the Agent shuffled the five cards and laid them face upwards on the floor of the box. He then gazed at the cards for about 15 seconds, studying their order. The cards were then turned face downwards without changing their order, and as each random number was shown at the aperture in the screen by (EA), (A) merely touched the corresponding card without lifting it to look at its face. Throughout the guessing therefore (A) had no sensation content of the faces of the cards, but memory content only. (P) recorded his own guesses in pencil. All the sheets were signed by both experimenters, but all the decoding, checking, counting of hits, and duplicating were done by Mr. West alone with S.G.S. merely watching. Mr. West posted the duplicate sheets to Professor Broad and also took away with him a private record of scores in the three categories (+1), (0), (−1). The results are given in Table XXVI.

TABLE XXVI

(PRN) (TL) (Normal Rate) Agent : J.Al.

Method : " Gaze and Touch "

Category	Trials	Hits	E(Stevens)	Dev.	χ
(+1)	192	60	38·583	+21·417	3·86
(−1)	192	53	38·333	+14·667	2·65

The above Table shows that highly significant scores are obtained by the " Gaze and Touch " method.

The combined results for the two sittings held at the Society's rooms on 8 and 15 April are given in Table XXVII.

TABLE XXVII

(PRN) (TL) (Normal Rate) Agent : J.Al.

Category	Trials	Hits	E(Stevens)	Dev.	χ
(+1)	384	127	78·166	+48·834	6·23
(−1)	384	109	78·791	+30·209	3·85

(P)'s Claim to Pre-Judge Success and Failure. Can B.S. pre-judge his scoring with any degree of success? Owing to the rapid rate at which all guessing was done, it would have been quite impracticable for B.S. to have graded his guesses A, B, C, D according as he felt them to be " very good ", " good ", " indifferent " or ' bad ". Any attempt to impose such a grading would certainly have ruined the experiments. However, as he went along, B.S. from time to time volunteered comments and marked a sequence of, say, five guesses as being probably better than the rest. Sometimes he would mark a whole column of 24 as " jolly good " ; or " this felt good " ; or " this felt better than the rest " etc. Such marked groups occur on most dates between 24 January 1941 and 14 August 1941. After the latter date he ceased to mark his guesses or express verbal opinions about special groups. We have therefore counted all such marked (+1) guesses at " normal " rate which were registered between 24 January 1941 and 14 August 1941 whether these occurred during telepathy tests or clairvoyance tests, and irrespective of the person acting as Agent (*i.e.* all Agents are included in the counts). In the absence of proper grading of all guesses, the method can only be regarded as a *very rough one*, but it affords no evidence that B.S. is able to pre-judge successful hits.

The following contingency table for (+1) guesses shows totals of successes on all " marked " guesses and corresponding totals for the " unmarked " guesses on the same dates.

TABLE XXVIII

(S = successes F = failures)

	S	F	Totals
Marked - -	100	296	396
Unmarked - -	1331	3352	4683
Totals - - -	1431	3648	5079

Whence (with Yates's correction) $\chi^2 = 1{\cdot}66$ *i.e.* $\chi = 1{\cdot}29$—a result which is without any significance.

EFFECT OF (P)'S HEALTH ON HIS SCORING. Does B.S.'s health affect his scoring? As was mentioned on p. 34, B.S. seldom enjoys good health and on account of this fact we made no systematic records of his state of health at each sitting. On certain occasions however he did complain of feeling exceptionally unwell and a special note was made at the time. The following Table sums up the scanty information in our possession.

TABLE XXIX

SCORING IN RELATION TO HEALTH ETC.

(N.B.—By the term " True Cognition " is meant successful hits not due to chance.)

Date	B.S.'s Remarks	% of True Cognitions
7. 2.41	B.S. complained at start of a " bad hangover" from the previous night when he had been to a party.	8·85%
14. 3.41	Before commencing the experiments B.S. complained of feeling unwell and advised that if results were not good after first two sheets we ought to stop the experiment.	17·53%
18. 4.41	B.S. reported that he didn't feel like getting good results as he felt tired after the very bad " blitz " of two days earlier—" but of course one can't say for sure."	8·51% (Normal rate) 10·96% (Rapid rate)
23. 5.41	Before the experiment B.S. reported " kidney trouble " had been bad during the past few days and that he was still in pain.	15·34% with Agent R.E. 24·48% with Agent G.A.
10.10.41	B.S. was in a bad humour from the start. He complained of feeling exasperated after a heavy day's work.	Chance results.

Date	B.S.'s Remarks	% of True Cognitions
7.11.41	After the results were found to be chance results B.S. told us that this was the first day for some time that he had been free from pain.	Chance results.
14.11.41	Before the experiment B.S. said that he was suffering from severe kidney trouble and did not expect to get good results as he was in pain.	10·16%
3. 1.42	Before starting the experiment B.S. said he had had an attack of duodenal trouble and that he would be very surprised if he got any good results that day.	Chance results (but see p. 55 (E.))
16. 1.42	B.S. in a bad humour and feeling ill. Also complained of cold weather.	Chance results, but the Agents were new.
15. 5.42	B.S. in a wrought-up state. Before commencing experiment B.S., who had been kept waiting owing to the lateness of J.Al., the Agent, remarked " You know my nerves are in a terrible state and doing this sort of thing is absolute torture . . . I am completely tired and run-down and am going away for a day or two."	(+1) 6·25% (−1) 12·20%
5. 6.42	B.S. in a very irritable state owing to J.Al., the Agent, being half an hour late again. Before checking up B.S. said J.Al.'s lateness had so disturbed his mind that he felt sure the results would be those due to mere guessing.	Rapid Rate (+2) 13·72% (−2) Chance.

An inspection of the above remarks and scores certainly suggests that on several occasions when B.S. has made complaints of ill-health, bad nerves, etc., he has nevertheless won a very high score ; and so far as the limited data go we see no reason to connect failure with ill-health.

Although somewhat irrelevant, it may be of interest to record a personal note made by K.M.G., acting as (EA), at the conclusion of the experiment on 5 December 1941. She made and filed this note :

" During today's experiments, I had a feeling of ' How absurd this is. There is nothing happening at all. There will be nil results I'm certain.' Commenting on this feeling and intending my remarks to be merely conversationally descriptive of it, I remarked to R.E., acting as Agent, ' I'm sure today will be an absolute blank.' R.E. remarked ' Why do you say that?' and I rejoined ' I just feel it in my bones; it feels as flat as flat can be.' Results however were good (3·41 × St. Deviation)."

The note indicates well enough the fact that even our most successful days produced no " atmosphere " of attainment during the process of the experiments, though naturally enough we experienced feelings of elation on some occasions when particularly good or interesting results were brought to light during the later analysis of the scoring sheets.

Discussion of the Experimental Methods Employed

We gave much thought and discussion throughout to the question of rendering the conditions in which these experiments took place proof, so far as was humanly possible, against even the possibility of fraud, on the part of Percipient and experimenters alike. In this we were helped by the nature of the *psi* function displayed by B.S., since any form of fraud or collusion which would cover *precognitive* telepathic cognition would obviously be extremely difficult, if not impossible.

As we have recorded, we invited at intervals independent persons to come and witness the experiments. Appendix I gives the names of all such persons who attended the sittings as Observers or otherwise assisted us in the experiments. Invitations were issued to several more people, but their war-work and difficulties of war-time travel prevented their accepting. Only those who have had practical experience of organising regular experiments and meetings will appreciate the difficulty of getting five and six persons gathered together week after week, some travelling considerable distances—a difficulty enormously increased in war-time. S.G.S. was himself evacuated to Cambridge and had to travel to London for the experiments. This made it impossible to hold them normally more often than once a week at week-ends. On no occasion was a request to us to attend and witness the experiments refused ; indeed, we should have more than welcomed it had some of the Observers found themselves able to come more often. At a first visit the experiments were fully explained, and Observers were given complete freedom to do what they pleased and were asked to furnish us subsequently with their criticisms and suggestions. Many of their observations are quoted in the following pages, and no single unfavourable comment has been withheld.

(1) *Signalling*. All the Observers testified that they considered the results were not due to any kind of signalling, direct or indirect, between Agent and Percipient. In the first place the rapid and uniform rate of the calling in itself precludes the successful use of such signals. Visual signals are clearly impossible. Even if anyone were concealed in the Studio in such a position that he could see the cards inside the box (which is not considered possible by anyone who has seen the Studio), this person would be unable to convey the information so acquired in time for it to be of any use to B.S. If B.S. were receiving visual signals it would soon be obvious to the person sitting next to him, for he would have to keep looking up from his scoring sheet. Further, it is not the card being turned up by (A) that would have to be signalled, but the card that is one card ahead. B.S. cannot leave any gaps in his scoring column to be filled in later, since (EP)—who is frequently the independent Observer—is watching him all the time. He must fill in cell No. 11 immediately after call

No. 11. Mr Kenneth Richmond, Editor, *S.P.R. Journal*, has perhaps said the last word on the impossibility of any *visual* aid reaching B.S. He wrote :

" It is obvious that even if he had had a television screen before him giving a plain view of each card as it was selected, he would have been no better able to note the succeeding cards before they were selected."

Dr B. P. Wiesner, consulting biologist, wrote :

" It seems to me that the obvious explanations (signals) are fully excluded both by the set-up and by the consistent manner in which the experiment was performed."

But what of auditory signals? The Agent sitting on the far side of the screen could not (unless she were in collusion with (EA)) signal by any sound the *next* card, because she has no means of knowing what this card will be. She is unable to see the sheets of random numbers from her side of the screen. But suppose she IS in collusion with (EA), it might be asked : Is this credible when we remember that (EA) has been S.G.S., K.M.G., Miss Jephson, Mrs Woollard, R. G. Medhurst, L. A. Rozelaar, and D. J. West ; and that (A) has been R.E., Mrs G.A., and J.Al.? Again, all the Observers who have sat next to (A) while she is turning up the cards, testify most emphatically to the fact that she never speaks, whispers, coughs, scrapes her feet, moves her chair, or acts in any way which even the most suspicious could look at askance, or which could convey any code information. H. Chibbett, a most sceptical observer, wrote :

" Miss E. kept silent during the whole of each experiment. There was no whispering or muttering on her or anybody else's part."

Miss Ina Jephson, a pioneer in card-cognition experiments and S.P.R. Council member, wrote :

" I saw no sign of a code between any of the people involved, heard no unnecessary talking, coughing or whispering."

Professor H. Habberley Price, Wykeham Professor of Logic in the University of Oxford and ex-President of the *S.P.R.*, wrote :

" I should like to say that, so far as I can judge, the methods you have adopted are perfectly watertight and fool-proof. It seems to me impossible that the Percipient should obtain knowledge of the card that is being shown, in any ' normal ' manner; still less of the card which is going to be shown a few seconds later."

Sir Ernest Bennett, M.P., member of the S.P.R. Council, wrote :

" I am convinced that no exchange of signals, audible or visual, was possible between Agent and Percipient, and that under the conditions which prevailed any form of collusion was ruled out."

Mr C. A. Mace, Reader in Psychology, University of London, wrote :

" In general the precautions against fraud seemed to me to be very thorough. My previous acquaintance with (EP) [S.G.S.] was sufficient to dispel suspicion in that direction and (EA) [K.M.G.], previously a stranger to me, seemed to be adequately guaranteed by (EP). (P) and (A) were in a different position. In their case I could certainly observe nothing to suggest the possibility of deceitful collusion, but everything here depends upon the conditions of the test making collusion impossible. Visual and auditory signals were, I think, adequately precluded, but to meet any suspicion of other kinds of signal it would perhaps be advisable to conduct a series of tests in another environment." [1]

What other kind of signal Mr Mace had in mind besides visual and auditory signals we do not know, but we should perhaps point out that a "timing code" (i.e., a code conveyed by varying the interval between calls, or similar device) is not possible, since (EA) who calls the serial numbers is unaware of the order of the cards in the box. Neither can (EA) give anything away by the inflections of her voice for the same reason. As stated previously, (A) or (O), the only persons who know the order of the cards, never speak at all; (EA), the only person who speaks, does not know the order of the cards; and the attention of (O) is particularly put on guard to safeguard this very point. The supposition that K.M.G. acting as (EA) is the signaller and that she is in collusion with (A) or (P) is not credible, since the same highly significant scores achieved with our chief Agent and K.M.G. are continued with Miss Jephson, Mr Medhurst, Mrs Woollard, and Mr Rozelaar acting the part of (EA) and with two other persons playing the part of (A). It is not plausible to assume that outside visitors of the highest character would immediately enter into collusion with someone they had met for the first time.

It should be added that with *all* the Observers cited above significant precognitive results were obtained. They are none of them describing watertight conditions on occasions when nothing happened. They took part in the checking and decoding, and watched the evaluation of the results. They all carried away with them impressions of high scoring. Persons like Miss Jephson, Mr Mace, Professor Price, Dr Wiesner, are all sufficiently acquainted with Rhine's work to know that the average score for 25 calls is 5, and that when they witness long series of sevens, eights, tens etc. with scarcely a single score below 5, they have witnessed something that is out of the ordinary. Mrs Woollard, a member of the S.P.R., who had assisted Mr Herbert, Research Officer, in his experiments with Zener cards on many occasions, remarked that our scores were "thrilling" and quite different from any she had seen with Mr Herbert.

On some occasions (noted on each occasion in *The Chronicle*), Mr B.S.'s Assistant, Miss Jervaise, sat at the work-table (B)[2] in the far corner of the ante-room, continuing her normal photographic work; but she did not enter the Studio while the guessing was in progress and it was impossible for her or anyone else in the ante-room to see the card-table. But we have frequently obtained high significant results on occasions when no-one was present on the premises except (EA), (EP), (A), (P), and (O).

[1] To meet Mr Mace's suggestion two additional experiments were subsequently carried out at the rooms of the Society for Psychical Research. (See pp. 75-77).

[2] See Plan, Plate I.

(2) *Collusion.* Let us suppose that everything is pre-arranged between B.S., the experimenters, and the Agent before the sitting. This pre-arrangement would be upset by the fact that on many occasions it was (O), the outside observer, who shuffled the five cards at the commencement of each sheet of 50 calls. Moreover, on such occasions (O) sat by the side of (A) and had the cards in view from the moment of shuffling till the end of the series of, say, 150 calls. Highly significant results were obtained under such conditions. Mr Chibbett wrote :

" I shuffled the cards face downwards for each experiment in such a way that neither S.G.S. nor Miss E. nor myself (being the only persons in the room at the time) could have any knowledge of their code-order on the table."

Actually, during this shuffling (A) and (EA) moved away to a remote part of the Studio and only returned when (O), who retained his seat at the card-table, intimated that the cards were ready.

Miss Jephson wrote :

" I am satisfied that when I shuffled the cards at the beginning of each series it was done out of sight of both S.G.S. and Miss E. (and of course of Mr B.S. in the other room). I am quite satisfied that the cards could not have been seen by anyone not standing immediately behind Miss E. I am also satisfied that Miss E. turned up the correct card indicated by the printed random-number held at the little opening by S.G.S. (acting as (EA)). I very carelessly did not notice whether foot-shuffling or signalling took place under the table, but my impression is that this did not happen. I was satisfied that the lay-out of the five cards, the decoding, the checking of score-sheets was allowed thorough supervision and that I was allowed to re-check any series I chose. When at the end of the experiments I had to leave the Studio to go and turn on my car lights, S.G.S. gave me all the records of that afternoon's work to take with me, so that there should be no opportunity of their being tampered with before they were posted to Dr Broad. I brought the records back, copies were made, and the records posted by me at once."

Mr Richmond wrote :

" On the occasions when I was witness of the procedure at the experimenters' table, I myself shuffled the cards before each experiment out of sight of the experimenter who thus had no opportunity till the experiment was ended of knowing in what order they were placed before the Agent."

In order to meet the possible suggestion that S.G.S., who prepared the sheets of random numbers, might be in collusion with (P) and (A), it was arranged that on three occasions these sheets should be compiled by another person who posted them direct either to K.M.G. or to Mrs Woollard.

For the experiment on 5 December 1941, at which Sir Ernest Bennett was present, Mr Gerhard Wassermann, B.Sc., compiled eight sheets of random digits (1-5) in his lodgings at Cambridge with no-one present but himself. He posted these sheets, which S.G.S. had no opportunity of seeing, to K.M.G. at her home in London, 153 Rivermead Court, Hurlingham, S.W. 6. On each sheet G.W. had written in his own handwriting " Prepared by Gerhard Wassermann ". K.M.G. brought the envelope unopened to the sitting on 5 December, and immediately before the experiments started it was opened by Sir Ernest Bennett in the Studio. S.G.S., who was (EP), remained in the ante-room till the experiment was finished.

For the experiment on 21 December 1941, at which Mr Mace was present, eight sheets of random numbers were prepared by Mr C. U. Blascheck, of Clare College, Cambridge, in his rooms and posted by him to Mrs Woollard at 7 North Hill, Highgate, N. 6 on 3 December. Mrs Woollard had instructions to receive this envelope (marked " Not to be opened till immediately before the experiment ") and to keep it unopened in safe custody. When it was known definitely that Mr Mace was coming to see the experiment on 21 December, S.G.S. wrote to Mrs Woollard instructing her to seal up the envelope in a larger one and post it to Mr C. A. Mace at Vale Farm, Hobbesley, Woodbridge, Suffolk. Mr Mace brought the envelope to the sitting unopened, and only opened it in the Studio immediately before the experiment began. He handed the sheets to K.M.G., who was acting as (EA), one at a time as required, keeping charge of the others. On each sheet Mr Blascheck had written " Prepared by C. U. Blascheck ".

For the sitting on 5 June 1942, Mr Wassermann again prepared in Cambridge four sheets of random numbers from logarithmic tables, and after sealing the envelope containing them with three large seals, posted it to K.M.G. in London. K.M.G. brought the envelope unopened to the sitting on 5 June and immediately before the experiment started she opened it in the Studio in the presence of Mr J.Al. alone. On inspection the seals showed no signs of tampering.

On each of these occasions the independent compiler signed a statement made out in his own handwriting. We will merely quote that by G. D. Wassermann written on 2 June 1942.

" I hereby certify that on Saturday May 30th 1942 I prepared from logarithmic tables four sheets of random numbers 1 to 5 at my lodgings 15 Victoria Park, Cambridge, nobody but myself being present. Subsequently on the same day I sealed the envelope with three seals and posted it to Mrs Goldney, 153 Rivermead Court, S.W. 6. Mr Soal could have had no opportunity of seeing the sheets, and till the time of posting I had constant control of the envelope. On each of the four sheets I had written ' Prepared by (signature).'

Signed

(Gerhard Wassermann)

2.6.42."

Unless of course it is assumed that Mr Wassermann and Mr Blascheck were in collusion with S.G.S., it is difficult to see how on these three occasions the latter could influence the results in any way. On all these three occasions highly significant results were scored by B.S. (see Part 2, List of Scores, under appropriate dates).

(3) *Statistical.* On his visit to the Studio on 21 December 1941, Mr Mace asked this pertinent question : " How are you going to prove to a sceptic that the results with high scores are not the most striking results selected from a much larger quantity of data? " In other words, would it not have been possible for us to carry out an enormous number of real

(or fictitious) experiments, select from them the scores of highest significance, and say nothing about the remainder?

We at once told Mace that we had an adequate reply. It is as follows:

An inspection of Appendix I and List of Scores (Part 2) shows that there are 13 sittings at each of which there was present an outside observer well-known to the S.P.R. and at which the value of χ (critical ratio) for at least *one* of the five categories (0), (± 1), (± 2), exceeds 3. Now the *chance* of getting one at least of the five scores with a deviation numerically greater than $3 \times$ Standard Deviation is nearly $5 \times \frac{1}{370} = \frac{1}{74}$. Hence (on an average) to get 13 such deviations we should have required about $74 \times 13 = 962$ sittings at the majority of which observers known to the S.P.R. would have had to be present. Let us however suppose that in reality we *had* managed to crowd this number of sittings into $2\frac{1}{4}$ years, always inviting a person known to the S.P.R. When this Report appears, some 900 persons (or at any rate several hundred) would be asking why *their* sitting was not mentioned in it.

But in point of fact this estimate of 962 sittings is a gross underestimation, since the values of χ obtained at the 13 sittings are usually much higher than 3 and often rise to 4, 5, or even 6. A hundred thousand unrecorded sittings in the $2\frac{1}{4}$ years would be a truer estimate, but it is clearly not worth while pursuing the argument further. We state emphatically that every single sitting and every single experiment has been recorded in this report from beginning to end of our experiments to date.

In his comments, Mr Mace (who apparently was unaware of Mr Richmond's account of his two visits to the Studio in the *Journal* for June-July 1941), suggested the use of counters instead of random numbers (which had already been done) and the substitution of other persons for (A), (EA), and (EP)—variations which had also been previously effected. S.G.S. wrote to Mr Mace pointing out that a good many of his suggestions had in fact already been adopted and he replied:

" Dear Soal,

Thank you for writing so fully in reply to my observations. You were, as I anticipated, in a position to meet fully nearly all, if not quite all my points. I cannot find time just at present to write anything further—nor I imagine is it necessary. But some time we might meet and discuss outstanding points of interest and possible variations of the experiments . . ." (the rest of letter irrelevant to matter in hand.)

Checking up of Results. We have already mentioned that none of the Observers has made any adverse criticism of the methods of decoding and counting of hits. It may perhaps not be out of place to quote the comments of Observers who made written allusion to the question. We have already given Miss Jephson's remarks (p. 83) and the comments of Mr Rozelaar (p. 67).

Sir Ernest Bennett wrote: " I took part personally with Mrs Goldney and Mr Soal in the final checking-up of results and can testify to the accuracy of the record."

Mr Richmond wrote: " Mr Soal's habitual exactitude in method is well-known, but I may note for completeness that I observed all the processes of checking-up results in detail on the record sheets, and saw no loopholes for error."

The *only* Observer who was unlucky enough to witness only negative results was Dr H. G. Baynes, the well-known psycho-therapeutist. The reason was possibly that we were on this occasion using " Associated Words " for the first time, without giving B.S. any warning of a change in the cards. Dr Baynes wrote : " I am not skilled at all in appraising the apparatus and methods used in these experiments. But the method I witnessed the other night is certainly proof against any interference other than that of infernal powers."

THE RECORD SHEETS. Professor Sidgwick, in his first Presidential address to this Society, said " We have done all that we can when the critic has nothing left to allege except that the investigator is in the trick. But when he has nothing else left to allege he will allege that ".[1] So perhaps the last resort of the sceptic will be to suggest that the record sheets were tampered with by the experimenters themselves at the conclusion of the sittings. *Both* experimenters would need to have been in collusion : it would not have been possible for one to have tampered with the figures without the connivance of the other, since both experimenters checked the results together and affixed their signatures to each sheet, and the duplicates were posted to Professor Broad immediately on leaving the Studio, in full sight of both experimenters and the Agent, R.E.

We cannot deny that if it is assumed that both K.M.G. and S.G.S. were bent on trickery, it would have been possible, on those occasions when no Observer was present, to have made out false record sheets and duplicates to agree with them. Many Observers were unable to stay long enough to watch the lengthy business of re-checking and duplicating the scoring sheets : they had trains to catch, other appointments, etc. But this was not the fault of the experimenters, who expected and asked them to remain. On the other hand, many Observers, such as Mr Chibbett, Miss Jephson, Mr Medhurst, Sir Ernest Bennett, Mr. Rozelaar, Mr West, *did* stay on to the end (see *Chronicle*), and on these occasions the original scoring sheets were directly under their observation from the time of the first checking-up to the final posting of the duplicates to Prof. Broad (posted on such occasions by the Observer himself). An inspection of the List of Scores (Part 2) on occasions when Observers were present, reveals long runs of highly significant (+1) scores in which scarcely a single column shows a figure lower than 5 correct hits.

The experimenters themselves, however, were anxious to have a check not on their honesty but on their accuracy. Each separate sheet of figures had involved checking 240 pairs of figures for the five counts (−2) to (+2), making the duplicates was a boring business which might easily have led to carelessness, and all this had had to be done at the end of a long day's work and before a late supper. Still further copying of the totals had had to be done for the typing of the List of Scores. We therefore distributed all the original scoring sheets and a carbon copy of the typed List of Scores (Part 2) among the following persons : Mr Richmond, Miss Jephson, Mr Redmayne, Mrs Woollard, Mr Medhurst, Mr Chibbett, Mrs Johnstone, and Mr Rozelaar. The first five of these are members of

[1] *Proc.* I, 12.

this Society. At the same time Professor Broad was asked to post them the corresponding duplicate record sheets in his possession. Each recipient was asked to check up all scores in the five categories (± 1), (0), (± 2) on the typewritten List of Scores against the results obtained by actual recounting of the hits on the original scoring sheets. They were also asked to check the totals written in ink at the top of each column (*e.g.*, 7/8/3)[1] against the results obtained by direct counting. This was important since any tampering with the figures in the column itself would have involved a corresponding alteration in these totals, which were written at the actual time of the sitting in the presence of both the experimenters and any Observer present. The checker was also asked to compare the original records with Professor Broad's duplicates, and to sign each of the typewritten Lists of Scores as being in agreement with the original and duplicate. In cases where the checker himself had acted as the Observer at any of the experiments, he was asked to examine his signature. The originals were returned to S.G.S. together with the signed Lists of Scores, and the duplicates were returned direct to Professor Broad.

In all the original scoring sheets so checked, covering the experiments over the total period, less than a dozen isolated errors were found, none of them being in the precognitive groups. They practically cancel each other out and are of no significance whatsoever. There were similarly a very few unimportant copying errors in the duplicate scoring sheets returned to Professor Broad. The List of Scores, Part 2, as now published includes the corrections made as the result of this independent re-checking.

There were six occasions on which the whole of the checking up and entering of figures was carried out by the Observer himself, who never lost control of the original scoring sheets from the instant when they were first handed to him to the moment when he posted the duplicates to Professor Broad (*cf. The Chronicle*, under dates 8, 15, 22 May 1942 and 6 January and 8 and 15 April 1943). Statistically these six occasions alone would be sufficient to establish the paranormal faculty of B.S. (*cf.* List of Scores for these dates).

On some occasions the Observers (including Mrs Woollard, Mr Medhurst, Mr West and Mr Rozelaar) copied out the totals from the original scoring sheets and took them home for retention. This was an additional safeguard.

This carries the description of experiments up to April 1943. When leisure and other favourable circumstances permit, we hope to continue our investigation of B.S.'s remarkable powers. Many questions are necessarily left unanswered. For instance :

" What happens at rates of calling which range between 80 and 120 seconds for a column of 25 calls? Is there a gradual falling off of significance as we slow down the tempo, or does some other remarkable effect make its appearance? "

" Could B.S. be trained to succeed at the 120 seconds tempo by the aid of persistent suggestion carried out over a long period? "

" Would the experiment succeed if carried out at a great distance by means of the telephone? "

[1] Cf. Plate II.

" Does B.S. succeed only with Agents who possess a peculiar psychological make-up, and if so what are the distinguishing characteristics of the successful Agent? "

" Do the onlookers or experimenters have any influence upon the results?"

" Would the experiment be successful if the random digits were selected mechanically and not by human agency? "

" Would B.S. continue to score precognitively in the counters experiments if the counters were selected by sight (conscious choice) instead of by touch alone? "

B.S. has obtained successful precognitive results over a period of 2¼ years. " Will his powers decline gradually, or cease suddenly as happened in the case of the Subjects investigated by Rhine, Reiss, Pratt and other experimenters; or will he continue in possession of his remarkable powers? "

We do not know. Whatever the future may have in store we may safely hazard the opinion that B.S. has made psychological history and that his name will rank among those whose gifts have added to our knowledge of that " final mystery—oneself ".

Note.—Students of the subject are referred to the private files of the Society for Psychical Research for the duplicate scoring sheets which were sent during the progress of the experiments to Professor Broad in Cambridge, and for certain correspondence of interest in connection with the case; and to S. G. Soal for the originals of the scoring sheets.

BOOKS AND PAPERS RECOMMENDED FOR FURTHER STUDY

E.S.P. AFTER 60 YEARS (New York, H. Holt & Co.: 1940).

EXTRA SENSORY PERCEPTION by J. B. Rhine. Faber & Faber (6s).

REPORT ON CASES OF APPARENT PRECOGNITION by H. F. Saltmarsh. *Proc.*: vol. 42, part 134.

FURTHER RESEARCH IN EXTRA SENSORY PERCEPTION by G. N. M. Tyrrell. *Proc.*: vol. 44, part 147.

EXPERIMENTS ON THE PARANORMAL COGNITION OF DRAWINGS by Whately Carington. *Proc.*: vol. 46, part 162.

FRESH LIGHT ON CARD GUESSING—SOME NEW EFFECTS by S. G. Soal. *Proc.*: vol 46, part 162.

PART TWO

LIST OF SCORES FOR ALL EXPERIMENTS[1]

Note: " Clairvoyance " scores are in heavy type. The significant scores on each date are printed in square brackets, and it will be seen that, with few exceptions, these come in the precognitive (+1) or (+2) columns.

(CC+1) = Cross-check for (+1) guesses.
(PRN) = Prepared random numbers.
(TP) = Telepathy experiment using pictures.
(TL) = Telepathy experiment using initial letters.
(TA) = Telepathy experiment using associated words.
(CP) = Clairvoyance experiment using pictures.

[1] The figures show the number of successful hits in each column of 25 calls.

SITTING No. 1. 24 January '41 (3 p.m.)

" Telepathy " and " Clairvoyance " experiments, with prepared random numbers governing selection of cards (*cf.* p. 49).

Present : B.S. =(P) , S.G.S. =(EA) , Miss R.E. =(A).

Sheet Nos. 1, 2, 4, 5 =(PRN) (TP) : *Agent*, R.E.
Sheet Nos. 3, 6 =(PRN) (CP) : *Agent*, R.E.

No. of Sheet	(−2)	(−1)	(0)	(+1)	(+2)	(CC+1)
1a - - -	3	7	7	8	5	3
1b - - -	5	5	5	7	4	7
2a - - -	0	6	6	10	5	6
2b - - -	7	2	7	8	4	7
3a - - -	**7**	**4**	**4**	**8**	**2**	**6**
3b - - -	**7**	**5**	**10**	**3**	**2**	**6**
4a - - -	3	9	5	9	4	5
4b - - -	5	7	2	8	6	6
5a - - -	3	1	4	8	4	4
5b - - -	5	4	4	9	6	9
6a - - -	**5**	**4**	**3**	**5**	**4**	**3**
6b - - -	**3**	**7**	**4**	**5**	**5**	**7**
Totals -						
T - - -	31	41	40	[67]	38	47
C - - -	**22**	**20**	**21**	**21**	**13**	**22**

SITTING No. 2. 31 January '41 (c. 3 p.m.)

" Telepathy " and " Clairvoyance " experiments, with prepared random numbers governing selection of cards (*cf.* p. 49).

Present : B.S. =(P); S.G.S. =(EA); Miss R.E. =(A).

Sheet Nos. 1, 2, 3, 6 =(PRN) (TP) : *Agent*, R.E.
Sheet Nos. 4, 5 =(PRN) (CP) : *Agent*, R.E.

No. of Sheet	(−2)	(−1)	(0)	(+1)	(+2)	(CC+1)
1a - - -	6	4	12	4	6	5
1b - - -	5	9	11	9	7	3
2a - - -	1	6	11	2	6	4
2b - - -	5	7	8	2	4	4
3a - - -	2	4	7	6	3	4
3b - - -	5	6	8	5	3	6
4a - - -	**3**	**6**	**5**	**4**	**3**	**3**
4b - - -	**6**	**3**	**6**	**6**	**2**	**8**
5a - - -	**4**	**8**	**3**	**5**	**5**	**2**
5b - - -	**6**	**4**	**6**	**5**	**2**	**5**
6a - - -	2	3	8	2	4	7
6b - - -	5	6	11	0	2	5
Totals						
T - - -	31	45	[76]	30	35	38
C - - -	**19**	**21**	**20**	**20**	**12**	**18**

SITTING No. 3. 7 February '41 (c. 3 p.m.)

" Telepathy " and " Clairvoyance " experiments, with prepared random numbers governing selection of cards (*cf*. p. 49).

Present : B.S. =(P); S.G.S. =(EA); K.M.G. =(EP);
Mr H. Chibbett =(O); Miss R.E. =(A).

Sheet Nos. 1, 2, 4 =(PRN) (TP) : *Agent*, R.E.
Sheet Nos. 3, 5 =(PRN) (CP) : *Agent*, R.E.

No. of Sheet	(−2)	(−1)	(0)	(+1)	(+2)	(CC+1)
1a - - -	2	3	8	9	5	6
1b - - -	2	2	9	6	5	3
2a - - -	5	4	8	6	5	6
2b - - -	3	7	9	8	3	4
3a - - -	**4**	**8**	**6**	**6**	**4**	**8**
3b - - -	**4**	**5**	**5**	**4**	**5**	**5**
4a - - -	3	4	4	3	7	5
4b - - -	1	2	5	7	5	5
5a - - -	**2**	**9**	**6**	**5**	**2**	**7**
5b - - -	**5**	**7**	**2**	**4**	**5**	**7**
Totals						
T - - -	16	22	[43]	[39]	30	29
C - - -	**15**	**29**	**19**	**19**	**16**	**27**

SITTING No. 4. 14 February '41 (4.15 p.m.)

" Telepathy " and " Clairvoyance " experiments, with prepared random numbers governing selection of cards (*cf*. p. 49).

Present : B.S. =(P); S.G.S. =(EA); Mr. H. Chibbett =(O);
Miss R.E. =(A).

Sheet Nos. 1, 2, 3, 4, 8 =(PRN) (TP) : *Agent*, R.E.
Sheet Nos. 5, 6 =(PRN) (CP) : *Agent*, R.E.
Sheet No. 7 =(PRN) (TP) : *Agent*, Mrs B.S.

No. of Sheet	(−2)	(−1)	(0)	(+1)	(+2)	(CC+1)
1a - - -	4	4	3	11	4	8
1b - - -	3	6	5	9	7	2
2a - - -	2	8	1	12	2	7
2b - - -	5	4	5	10	5	6
3a - - -	5	3	6	10	7	7
3b - - -	5	6	3	10	5	5
4a - - -	6	7	6	10	2	4
4b - - -	3	7	4	6	4	9
5a - - -	**4**	**3**	**4**	**4**	**6**	**6**
5b - - -	**2**	**7**	**1**	**4**	**7**	**8**
6a - - -	**5**	**7**	**6**	**7**	**8**	**2**
6b - - -	**6**	**3**	**2**	**5**	**6**	**1**
7a - - -	5	6	3	5	3	1
7b - - -	3	7	5	5	3	3
8a - - -	4	4	4	6	5	5
8b - - -	3	3	3	4	7	8
Totals						
T. R.E. - -	40	52	40	[88]	48	61
T. Mrs B.S. -	8	13	8	10	6	4
C. R.E. - -	**17**	**20**	**13**	**20**	**27**	**17**

SITTING No. 5. 21 February '41 (4 p.m.)

" Telepathy " experiments, with prepared random numbers governing selection of cards.

Present : B.S. =(P).

Sheets 1-4, S.G.S. =(EA); R.E. =(A);
Sheets 5-8, S.G.S. =(EA); K.M.G. (?)[1] =(A); Miss R.E. =(O).
N.B.—K.M.G. arrived during sheet 4a.

Sheet Nos. 1, 2, 3, 4 =(PRN) (TP) : *Agent*, R.E.
Sheet Nos. 5, 6, 7 8 =(PRN) (TP) : *Agent*, K.M.G. (?)[1].

No. of Sheet	(−2)	(−1)	(0)	(+1)	(+2)	(CC+1)
1a - - -	3	4	7	5	4	4
1b - - -	3	2	7	5	2	4
2a - - -	4	5	5	7	4	6
2b - - -	5	6	10	6	2	8
3a - - -	4	5	11	8	5	1
3b - - -	4	4	7	8	3	4
4a - - -	1	3	7	7	6	5
4b - - -	7	4	2	4	2	4
Totals - - -	31	33	[56]	50	28	36
5a - - -	3	5	8	8	4	7
5b - - -	5	4	7	7	5	3
6a - - -	4	2	5	8	2	9
6b - - -	5	8	7	6	5	6
7a - - -	8	6	4	7	2	2
7b - - -	0	5	4	7	5	4
8a - - -	6	2	3	13	6	3
8b - - -	9	2	6	5	4	5
Totals - - -	40	34	44	[61]	33	39

[1] See pp. 59-60.

SITTING No. 6. 28 February '41 (4.30 p.m.)

"Telepathy" and "Clairvoyance" experiments with prepared random numbers governing selection of cards (*cf.* p. 49).

Present : B.S.=(P); S.G.S.=(EA); Mr Kenneth Richmond=(O) and (EP); Miss R.E.=(A). K.M.G. arrived near the end of sheet 5a, but took no active part in experiments.

Sheet Nos. 1, 2, 3, 5, 6, 8=(PRN) (TP): *Agent*, R.E.
Sheet Nos. 4, 7 =(PRN) (CP): *Agent*, R.E.

No. of Sheet	(−2)	(−1)	(0)	(+1)	(+2)	(CC+1)
1a - - -	5	4	5	5	4	8
1b - - -	2	3	8	5	5	7
2a - - -	6	5	8	10	6	6
2b - - -	4	5	5	8	7	6
3a - - -	4	7	5	9	6	2
3b - - -	8	6	4	7	4	8
4a - - -	**5**	**6**	**5**	**3**	**8**	**4**
4b - - -	**5**	**3**	**8**	**8**	**2**	**1**
5a - - -	6	6	4	11	1	8
5b - - -	9	5	3	3	5	6
6a - - -	4	5	5	11	6	3
6b - - -	6	4	6	10	5	5
7a - - -	**5**	**4**	**6**	**5**	**5**	**8**
7b - - -	**6**	**3**	**6**	**2**	**5**	**4**
8a - - -	4	4	7	13	4	3
8b - - -	4	4	6	7	2	6
Totals						
T - - -	62	58	66	[99]	55	68
C - - -	**21**	**16**	**25**	**18**	**20**	**17**

SITTING No. 7. 7 March '41 (c. 4.30 p.m.)

" Telepathy " experiments, with prepared random numbers, and with counters drawn from a bag, governing selection of cards (*cf.* p. 51).

Present : B.S. =(P); S.G.S. =(EA) for (PRN) and Recorder for (Counters); K.M.G. =(EP) for (PRN) and (EA) for (Counters); The Hon. Mrs Alfred Lyttelton =(O) and (EP); Miss R.E. and Mrs B.S. =(A).

Sheet Nos. 1, 2, 3 =(PRN) (TP) : *Agent*, R.E.
Sheet Nos. 4, 5, 6 =(Counters) (TP) : *Agent*, R.E.
Sheet Nos. 7, 8, 9, 10 =(PRN) (TP) : *Agent*, Mrs B.S.

No. of Sheet	(−2)	(−1)	(0)	(+1)	(+2)	(CC+1)
1a - - -	6	8	8	11	3	3
1b - - -	2	5	5	9	6	5
2a - - -	8	4	8	9	5	1
2b - - -	4	8	6	8	4	8
3a - - -	2	2	5	8	7	1
3b - - -	8	8	5	7	4	7
Totals - - -	30	35	37	[52]	29	25
4a - - -	7	1	3	10	4	6
4b - - -	7	5	5	11	4	7
5a - - -	5	5	4	10	4	4
5b - - -	3	4	8	10	3	7
6a - - -	3	5	1	12	3	4
6b - - -	6	4	4	6	6	7
Totals - - -	31	24	25	[59]	24	35
7a - - -	8	5	2	3	3	3
7b - - -	5	6	4	4	5	8
8a - - -	4	7	6	6	2	5
8b - - -	4	4	3	4	4	7
9a - - -	7	9	7	6	4	5
9b - - -	8	3	6	2	6	3
10a - - -	3	5	7	10	4	5
10b - - -	3	6	5	5	7	5
Totals - - -	42	45	40	40	35	41

SITTING No. 8. 14 March '41 (c. 5 p.m.)

" Telepathy " experiments, with prepared random numbers, and with counters drawn from bag, governing selection of cards (*cf.* p. 51).

Present : B.S. = (P); S.G.S. = (EA) for (PRN) and Recorder for (Counters); Miss I. Jephson = (O), and (EA) for (Counters); Mrs Oliver Gatty = (EP); Miss R.E. = (A).

Sheet Nos. 1, 2, 3 = (PRN) (TP) : *Agent*, R.E.
Sheet Nos. 4, 5, 6 = (Counters) (TP) : *Agent*, R.E.

No. of Sheet	(−2)	(−1)	(0)	(+1)	(+2)	(CC+1)
1a - - -	4	3	5	6	7	4
1b - - -	6	5	4	11	5	4
2a - - -	7	4	3	7	5	4
2b - - -	7	5	2	11	3	6
3a - - -	4	6	4	6	8	3
3b - - -	7	4	4	7	3	4
Totals - - -	35	27	22	[48]	31	25
4a - - -	4	4	7	7	7	2
4b - - -	4	7	3	11	2	3
5a - - -	3	4	8	9	2	5
5b - - -	5	4	6	7	2	4
6a - - -	5	4	6	9	1	5
6b - - -	8	6	7	7	6	6
Totals - - -	29	29	37	[50]	20	25

SITTING No. 9. 21 March '41 (5 p.m.)

" Telepathy " experiments, with counters drawn from bag and from bowl governing selection of cards, at " normal " rate and " rapid " rate (*cf.* p. 55).

Present : B.S. = (P); S.G.S. = Recorder for (Counters); K.M.G. = (EA); Mr Kenneth Richmond = (O) and (EP); Miss R.E. = (A).

Sheet Nos. 1, 2, 3 = (Counters) (TP) (Normal Rate) : *Agent*, R.E.
Sheet Nos. 4, 5, 6, 7, 8, 9 = (Counters) (TP) (Rapid Rate) : *Agent*, R.E.

No. of Sheet		(−2)	(−1)	(0)	(+1)	(+2)	(CC+1)
1a	-	3	3	3	11	3	4
1b	-	1	3	7	12	4	4
2a	-	3	4	7	14	1	6
2b	-	5	3	8	9	5	3
3a	-	7	3	5	7	5	2
3b	-	1	2	4	8	2	5
Totals	-	20	18	34	[61]	20	24

							(CC+1)	(CC+2)
4a	-	1	4	3	8	7	3	6
4b	-	2	5	5	6	6	6	6
5a	-	4	2	6	6	5	4	4
5b	-	2	6	7	0	4	4	4
6a	-	2	3	6	6	10	9	5
6b	-	4	6	3	4	12	7	4
7a	-	4	7	6	6	5	10	4
7b	-	2	3	6	2	9	6	3
8a	-	5	4	7	7	5	4	3
8b	-	6	5	8	4	6	3	3
9a	-	6	5	6	4	10	4	5
9b	-	6	4	3	4	5	4	3
Totals	-	44	54	66	57	[84]	64	50

TIMES

Sheets

Normal Rate	-	1a	1b	2a	2b	3a	3b						
Seconds	- -	60	60	68	76	65	70						
Rapid Rate	- -	4a	4b	5a	5b	6a	6b	7a	7b	8a	8b	9a	9b
Seconds	- -	35	40	35	40	35	37	37	33	29	33	28	33

Note.—In sheets 7a and 7b there were one or two gaps. We have

Sheet 7a : (+2) trials = 20; (+1) = 22; (0) = 24; (−1) = 22
Sheet 7b : (+2) trials = 15; (+1) = 18; (0) = 21; (−1) = 18

SITTING No. 10. 28 March '41 (c. 5 p.m.)

" Telepathy " experiments, with counters governing selection of cards. Initial letters substituted for animal pictures. " Normal " rate and " rapid " rate (*cf.* p. 53 ff.).

Present : B.S. = (P); S.G.S. = Recorder for (Counters); K.M.G. = (EA); Mrs Kenneth Richmond = (EP); Miss R.E. = (A).

Sheet Nos. 1, 2, 3, 4 = (Counters) (TL) (Normal Rate) : *Agent*, R.E.
Sheet Nos. 5, 6, 7, 8 = (Counters) (TL) (Rapid Rate) : *Agent*, R.E.

No. of Sheet		(−2)	(−1)	(0)	(+1)	(+2)	(CC+1)
1a	-	2	2	5	6	4	8
1b	-	2	6	6	7	2	7
2a	-	2	7	7	8	7	5
2b	-	3	7	1	7	8	4
3a	-	3	6	9	8	4	2
3b	-	8	9	3	6	5	9
4a	-	7	4	3	7	5	2
4b	-	4	4	6	8	1	3
Totals	-	31	45	40	[57]	36	40

		(−2)	(−1)	(0)	(+1)	(+2)	(CC+1)	(CC+2)
5a	-	6	4	5	1	7	4	5
5b	-	5	5	4	5	6	3	6
6a	-	3	5	7	3	7	10	1
6b	-	8	5	7	4	8	4	2
7a	-	3	4	5	3	2	3	1
7b	-	4	5	4	3	11	2	8
8a	-	4	0	2	9	4	5	2
8b	-	3	5	5	6	9	5	4
Totals	-	36	33	39	34	[54]	36	29

TIMES

Sheets

Normal Rate -	-	1a	1b	2a	2b	3a	3b	4a	4b
Seconds -	-	65	95	?	?	72	70	?	80
Rapid Rate -	-	5a	5b	6a	6b	7a	7b	8a	8b
Seconds -	-	35	33	35	34	32	32	33	35

It was reported by (EP) that (P) got out of step after call No. 15 in sheet 7a. In sheet 8a (P) left a gap at call No. 16 but did not get out of step.

For sheet 7a : (−2) trials = 13; (−1) = 14; (0) = 14; (+1) = 13
For sheet 8a : (−2) trials = 22; (−1) = 23; (0) = 24; (+1) = 23

SITTING No. 11. 18 April '41 (5 p.m.)

" Telepathy " experiments, with counters governing selection of cards Animal pictures and initial letters of animals used. " Normal " rate and " rapid " rate.

Present : B.S. = (P); S.G.S. = Recorder for (Counters); K.M.G. = (EA); Dr B. P. Wiesner = (EP) and (O); Miss R. E. = (A).

Sheet Nos. 1, 3 = (Counters) (TL) (Normal Rate) : *Agent*, R.E.
Sheet No. 2 = (Counters) (TP) (Normal Rate) : *Agent*, R.E.
Sheet Nos. 4, 5, 6, 7, 8 = (Counters) (TL) (Rapid Rate) : *Agent*, R.E.

No. of Sheet	(−2)	(−1)	(0)	(+1)	(+2)	(CC+1)
1a - - -	3	4	1	6	5	6
1b - - -	4	4	7	6	3	5
2a - - -	2	1	5	10	7	5
2b - - -	5	4	3	4	6	7
3a - - -	1	6	3	1	5	4
3b - - -	6	2	2	10	4	3
Totals - - -	21	21	21	[37]	30	30
						(CC+2)
4a - - -	7	7	4	2	8	5
4b - - -	2	5	7	2	7	4
5a - - -	4	4	2	2	9	5
5b - - -	4	6	6	5	5	4
6a - - -	7	3	5	3	3	3
6b - - -	2	6	5	4	5	2
7a - - -	6	5	3	4	9	4
7b - - -	2	7	3	2	8	3
8a - - -	2	3	10	5	6	5
8b - - -	3	1	5	5	3	2
Totals - - -	39	47	50	34	[63]	37

TIMES

Sheets

Normal Rate -	1a	1b	2a	2b	3a	3b				
Seconds - -	65	75	65	65	66	65				
Rapid Rate -	4a	4b	5a	5b	6a	6b	7a	7b	8a	8b
Seconds - -	34	30	32	33	35	33	33	35	36	34

Notes.—(EP) reported that in Sheet 3a (P) hesitated at call No. 20 and was then out of step.

In sheet 5a (Rapid Rate) (P) got out of step at call No. 20.

In sheet 6a (Rapid Rate) (P) recorded no guesses after call No. 18.

In sheet 4b (Rapid Rate) (P) left a blank space at call No. 10 but kept in step.

After making allowances for gaps and the cancelling of trials that were out of step, we have the following totals for trials :

	(−2)	(−1)	(−0)	(+1)	(+2)
Sheets 1-3 - - - - -	130	137	144	138	132
Sheets 4-8 - - - - -		225	235	227	219

SITTING No. 12. 25 April '41 (c. 5 p.m.)

"Telepathy" experiments, with prepared random numbers governing selection of animal picture cards. "Normal" rate.

Present: B.S. =(P); Miss Hocken (visitor) arrived at end of sheet 6, and acted as Agent for sheet No. 7.
Sheets 1-3, K.M.G. =(A) and S.G.S. =(EA);
Sheets 4-6, S.G.S. =(A) and K.M.G. =(EA).

Sheet Nos. 1, 2, 3 =(PRN) (TP) (Normal Rate): *Agent*, K.M.G.
Sheet Nos. 4, 5, 6 =(PRN) (TP) (Normal Rate): *Agent*, S.G.S.
Sheet No. 7 =(PRN) (TP) (Normal Rate): *Agent*, Miss Hocken.

No. of Sheet	(−2)	(−1)	(0)	(+1)	(+2)	(CC+1)
1a - - -	8	7	4	2	9	4
1b - - -	7	4	6	4	4	5
2a - - -	3	4	5	5	5	5
2b - - -	7	3	4	7	7	6
3a - - -	2	7	8	6	3	10
3b - - -	7	5	2	4	3	1
Totals - - -	34	30	29	28	31	31
4a - - -	6	4	6	6	3	4
4b - - -	6	5	6	4	6	5
5a - - -	5	3	8	5	7	3
5b - - -	3	7	5	7	3	3
6a - - -	5	3	1	6	5	3
6b - - -	5	3	5	2	3	5
Totals - - -	30	25	31	30	27	23
7a - - -	13	4	6	6	4	2
7b - - -	5	6	5	2	6	4
Totals - - -	18	10	11	8	10	6

TIMES

Sheets

Normal Rate	1a	1b	2a	2b	3a	3b	4a	4b	5a	5b	6a	6b	7a	7b
Seconds	?	70	?	?	95	?	70	65	75	65	63	65	not timed	

N.B.—The times missed were about the same rate as those recorded.

SITTING No. 13. 2 May '41 (5.45 p.m.)

" Telepathy " experiments, with counters governing selection of animal picture cards. " Normal " rate.

Present : B.S. =(P); S.G.S.; K.M.G.; Dr B. P. Wiesner; Mr H. A. Berens.
Sheets 1-3, Dr. Wiesner acted as Agent; Mr Berens as (EP); S.G.S. as Recorder for (Counters); K.M.G. as (EA).
Sheets 4-6, Mr Berens was Agent; Dr Wiesner was (EP); S.G.S. Recorder of (Counters); K.M.G. =(EA).

Sheet Nos. 1, 2, 3 =(Counters) (TP) (Normal Rate) : *Agent*, Dr Wiesner.
Sheet Nos. 4, 5, 6 =(Counters) (TP) (Normal Rate) : *Agent*, Mr Berens.

No. of Sheet	(−2)	(−1)	(0)	(+1)	(+2)	(CC+1)
1a	2	7	3	6	6	2
1b	2	2	4	4	3	3
2a	5	2	8	5	4	5
2b	4	4	9	6	3	8
3a	7	4	5	5	4	4
3b	2	8	5	5	4	3
Totals	22	27	34	31	24	25
4a	3	4	7	7	4	2
4b	2	5	5	5	4	6
5a	6	7	2	1	8	2
5b	2	4	5	2	3	6
6a	6	4	8	4	6	5
6b	3	3	2	5	7	6
Totals	22	27	29	24	32	27

TIMES

Sheets

Normal Rate	1a	1b	2a	2b	3a	3b	4a	4b	5a	5b	6a	6b
Seconds	90	85	80	75	?	?	70	70	63	58	58	55

SITTING No. 14. 9 May '41 (c. 5 p.m.)

Special Test—see pp. 73-74. Also " telepathy " experiments with counters governing selection of animal picture cards. " Normal " rate.

Present : B.S. = (P); S.G.S.; K.M.G.; Dr B. P. Wiesner; Mr H. Chibbett.
Sheets 1-4 Special test. Influence of (P) on (EA).
Sheets 5 and 6 Mr Chibbett = (A); Dr Wiesner = (EP); K.M.G. = (EA); S.G.S. = Recorder for (Counters).

Sheet Nos. 1, 2, 3, 4 = Special Test.
Sheet Nos. 5, 6 = (Counters) (TP) (Normal Rate): *Agent*, Mr. Chibbett.

No. of Sheet	(−2)	(−1)	(0)	(+1)	(+2)	(CC+1)
1a	4	3	3	5	2	
1b	3	6	4	4	5	
2a	6	7	5	4	4	
2b	4	7	4	5	1	
3a	3	5	3	2	2	
3b	6	6	8	6	5	
4a	5	4	4	9	8	
4b	7	8	6	6	5	
Totals	38	46	37	41	32	
5a	5	7	1	1	7	4
5b	4	7	11	2	8	4
6a	5	6	6	5	2	9
6b	4	6	4	1	7	5
Totals	18	26	22	9	24	22

N.B.—No times taken.

SITTING No. 15. 16 May '41 (5 p.m.)

"Telepathy" experiments, with prepared random numbers governing selection of animal picture cards. "Normal" rate.

Present : B.S. = (P); S.G.S = (EA); K.M.G. = (EP); Mrs G.A. = (A).

Sheet Nos. 1, 2, 3, 4, 5, 6 = (PRN) (TP) (Normal Rate) : *Agent*, Mrs G.A.

No. of Sheet	(−2)	(−1)	(0)	(+1)	(+2)	(CC+1)
1a - - -	1	6	4	6	4	3
1b - - -	4	4	5	8	7	5
2a - - -	5	8	6	5	4	4
2b - - -	2	6	5	10	2	6
3a - - -	4	3	3	10	3	5
3b - - -	4	9	4	8	1	5
4a - - -	2	4	9	6	5	3
4b - - -	5	3	6	5	1	3
5a - - -	4	7	7	6	3	6
5b - - -	7	5	10	4	4	7
6a - - -	7	7	2	5	5	4
6b - - -	5	3	4	9	5	2
Totals - - -	50	65	65	[82]	44	53

TIMES

	Sheets											
Normal Rate -	1a	1b	2a	2b	3a	3b	4a	4b	5a	5b	6a	6b
Seconds - -	70	75	75	70	66	75	?	?	75	?	?	?

N.B.—The times missed were about the same rate as those recorded.

SITTING No. 16. 23 May '41 (5 p.m.)

" Telepathy " experiments, with prepared random numbers governing selection of animal picture cards; " normal " rate. Also special experiment with two Agents working together (*cf.* p. 60).

Present : B.S. =(P); S.G.S. =(EA); Dr B. P. Wiesner =(EP);
Mrs G.A. and Miss R.E. both Agents.

Sheet Nos. 1, 2, 3, 4 =PRN) (TP) (Normal Rate) : *Agent*, R.E.
Sheet Nos. 5, 6, 7 =(PRN) (TP) (Normal Rate) : *Agents*, R.E. and G.A. working together.

No. of Sheet	(−2)	(−1)	(0)	(+1)	(+2)	(CC+1)
1a - - -	4	6	7	8	6	9
1b - - -	6	5	7	8	1	10
2a - - -	5	4	5	6	5	6
2b - - -	4	4	3	8	6	7
3a - - -	5	3	4	7	5	3
3b - - -	3	4	4	7	5	5
4a - - -	3	6	4	11	4	3
4b - - -	7	7	6	6	3	5
Totals - - -	37	39	40	[61]	35	48

	(−2)		(−1)		(0)		(+1)		(+2)	
	R.E.	G.A.	R.E.	G.A.	R.E.	G.A.	R.E.	G.A.	R.E.	G.A.
5a	5	1	6	5	1	8	4	8	9	6
5b	6	1	3	7	2	6	3	10	6	5
6a	5	4	6	4	6	1	4	11	5	4
6b	5	4	6	4	7	4	3	10	2	4
7a	3	7	1	6	6	4	5	10	6	2
7b	2	5	6	8	1	6	6	8	4	3
Totals	(26	22)	(28	34)	(23	29)	(25	[57])	(32	24)

TIMES

	Sheets								
Normal Rate	1a	1b	2a	2b	3a	3b	4a	4b	(Agent, R.E.)
Seconds	65	71	65	65	?	?	64	70	

Normal Rate	5a	5b	6a	6b	7a	7b	(two Agents working together).
Seconds	75	70	65	65	70	70	

SITTING No. 17. 6 June '41 (3 p.m.)

" Telepathy " experiments, with counters governing selection of animal picture cards and initial letter cards. At " normal ", " rapid ", and " slow " rates (*cf*. p. 58).

Present : B.S.=(P); S.G.S.=Recorder for (Counters); K.M.G.=(EA); Professor H. H. Price=(O) and (EP); Miss R.E.=(A).

Sheet Nos. 1, 3 =(Counters) (TP) (Normal Rate) : *Agent*, R.E.
Sheet No. 2 =(Counters) (TL) (Normal Rate) : *Agent*, R.E.
Sheet Nos. 4, 5, 6, 7 =(Counters) (TP) (Slow Rate) : *Agent*, R.E.
Sheet Nos. 8, 9, 10 =(Counters) (TP) (Rapid Rate) : *Agent*, R.E.

No. of Sheet	(−2)	(−1)	(0)	(+1)	(+2)	(CC+1)
1a	2	2	6	8	7	7
1b	8	3	4	9	1	7
2a	3	3	6	5	4	2
2b	5	3	2	8	5	3
3a	4	7	6	10	4	5
3b	3	1	4	9	4	3
Totals	25	19	28	[49]	25	27
4a	8	5	2	8	3	4
4b	7	7	2	4	6	6
5a	2	8	6	3	6	6
5b	6	3	1	8	6	3
6a	3	6	3	5	2	4
6b	5	5	2	5	3	6
7a	8	7	5	0	4	8
7b	4	5	6	7	3	2
Totals	43	46	27	40	33	39
						(CC+2)
8a	3	5	6	3	4	6
8b	6	4	2	5	8	4
9a	7	3	4	6	5	6
9b	5	6	3	4	5	3
10a	4	7	7	3	6	6
10b	3	9	5	8	7	7
Totals	28	34	27	29	35	32

TIMES

	Sheets							
Normal Rate	1a	1b	2a	2b	3a	3b		
Seconds	64	64	67	66	57	57		
Slow Rate	4a	4b	5a	5b	6a	6b	7a	7b
	120 seconds for each column							
Rapid Rate	8a	8b	9a	9b	10a	10b		
Seconds	40	36	36	34	35	33		

SITTING No. 18. 13 June '41 (4 p.m.)

"Telepathy" experiments, with counters governing selection of animal picture cards and also "associated word" cards (see pp. 53-54). "Normal" rate.

Present : B.S. =(P); S.G.S. =Recorder for (Counters); K.M.G. =(EA); Dr H. G. Baynes =(O) and (EP); Miss R.E. =(A).

Sheet No. 1 =(Counters) (TP) (Normal Rate) : *Agent*, R.E.
Sheet Nos. 2, 3, 4, 5, 6 =(Counters) (TA) (Normal Rate) : *Agent*, R.E.

No. of Sheet	(−2)	(−1)	(0)	(+1)	(+2)	(CC+1)
1a - - -	6	4	1	8	8	4
1b - - -	4	5	7	3	3	7
Totals - - -	10	9	8	11	11	11
2a - - -	3	7	12	4	1	7
2b - - -	5	8	3	4	3	5
3a - - -	2	2	8	4	3	6
3b - - -	10	5	3	4	3	5
4a - - -	7	6	4	4	5	4
4b - - -	12	9	0	2	1	6
5a - - -	4	4	6	4	3	3
5b - - -	4	5	5	5	3	6
6a - - -	3	6	1	3	4	7
6b - - -	10	5	6	7	5	6
Totals - - -	60	57	48	41	31	55

TIMES

	Sheets											
Normal Rate -	1a	1b	2a	2b	3a	3b	4a	4b	5a	5b	6a	6b
Seconds - -	80	65	62	?	60	63	65	55	57	62	57	57

SITTING No. 19. 14 August '41 (5.30 p.m.)

" Telepathy " experiments, with prepared random numbers governing selection of animal picture cards and of " associated word " cards alternately (*cf.* pp. 71-73). " Normal " rate.

Present : B.S. = (P); S.G.S. = (EP); K.M.G. = (EA); Miss R.E. = (A).

Sheet Nos. 1, 3, 5, 7 = (PRN) (TP) (Normal Rate) : *Agent*, R.E.
Sheet Nos. 2, 4, 6, 8 = (PRN) (TA) (Normal Rate) : *Agent*, R.E.

No. of Sheet	(−2)	(−1)	(0)	(+1)	(+2)	(CC+1)
1a - - -	7	8	5	8	3	10
1b - - -	5	8	5	5	5	6
3a - - -	3	3	7	7	5	4
3b - - -	6	5	2	8	3	1
5a - - -	4	8	7	7	3	3
5b - -	3	4	2	8	4	1
7a - - -	5	4	6	8	5	3
7b - - -	4	8	5	3	3	8
Totals - - -	37	48	39	[54]	31	36
2a - - -	5	6	4	6	4	2
2b - - -	5	4	1	12	2	5
4a - - -	6	3	5	6	1	5
4b - - -	3	1	7	5	1	6
6a - - -	7	5	3	3	8	5
6b - - -	1	2	7	8	8	4
8a - - -	4	5	3	5	5	5
8b - - -	7	4	7	6	4	5
Totals - - -	38	30	37	[51]	33	37

TIMES

Sheets	1a	1b	2a	2b	3a	3b	4a	4b	5a	5b	6a	6b	7a	7b	8a	8b
Normal Rate -																
Seconds - -	72	65	56	57	55	54	57	60	56	57	54	55	55	56	57	56

SITTING No. 20. 25 August '41 (5.30 p.m.)

" Telepathy " experiments, with prepared random numbers governing selection of animal picture cards and " associated word " cards alternately (*cf*. pp. 71-73). " Normal " rate.

Present : B.S. =(P); S.G.S. =(EP); K.M.G. =(EA); Miss Jervaise (B.S.'s Lady-Assistant) =Checker; Miss R.E. =(A).

Sheet Nos. 1, 3, 5 =(PRN) (TP) (Normal Rate) : *Agent*, R.E.
Sheet Nos. 2, 4, 6 =(PRN) (TA) (Normal Rate) : *Agent*, R.E.

No. of Sheet	(−2)	(−1)	(0)	(+1)	(+2)	(CC+1)
1a - - -	4	4	5	11	6	5
1b - - -	7	3	7	5	5	5
3a - - -	2	8	2	8	8	8
3b - - -	2	3	6	6	3	3
5a - - -	3	4	3	4	3	6
5b - - -	5	5	8	6	6	1
Totals - - -	23	27	31	[40]	31	28
2a - - -	5	6	5	7	4	4
2b - - -	7	6	3	3	3	6
4a - - -	7	2	5	7	5	8
4b - - -	4	2	5	7	4	9
6a - - -	4	5	5	10	5	3
6b - - -	3	7	1	4	10	3
Totals - - -	30	28	24	[38]	31	33

TIMES

Normal Rate - (*Sheets*)	1a	1b	2a	2b	3a	3b	4a	4b	5a	5b	6a	6b
Seconds - -	60	60	60	61	60	57	57	60	58	56	60	57

SITTING No. 21. 24 September '41 (4.30 p.m.)

"Telepathy" experiments, with prepared random numbers governing selection of animal picture cards. "Normal" rate and "slow" rate alternately (*cf.* pp. 58-59).

Present : B.S. =(P); S.G.S. =(EP); K.M.G. and Mrs. Woollard =(EA); Miss R.E. =(A).

Sheet Nos. 1, 3, 5, 7 =(PRN) (TP) (Normal Rate) : *Agent*, R.E.
Sheet Nos. 2, 4, 6, 8 =(PRN) (TP) (Slow Rate) : *Agent*, R.E.

No. of Sheet	(−2)	(−1)	(0)	(+1)	(+2)	(CC+1)
1a - - -	3	7	5	7	2	7
1b - - -	4	5	4	8	7	6
3a - - -	3	5	3	9	3	5
3b - - -	3	6	1	6	8	4
5a - - -	5	3	7	5	2	3
5b - - -	5	8	8	8	2	4
7a - - -	2	4	4	6	11	8
7b - - -	3	5	3	9	6	4
Totals - - -	28	43	35	[58]	41	41
2a - - -	5	5	6	4	3	
2b - - -	4	8	3	5	7	
4a - - -	6	3	6	6	4	
4b - - -	4	3	3	6	2	
6a - - -	4	5	10	5	1	
6b - - -	10	5	4	2	6	
8a - - -	9	5	4	7	6	
8b - - -	6	7	4	3	6	
Totals - - -	48	41	40	38	35	

TIMES

	Sheets							
Normal Rate -	1a	1b	3a	3b	5a	5b	7a	7b
Seconds - -	75	77	75	75	72	72	75	77
Slow Rate - -	2a	2b	4a	4b	6a	6b	8a	8b
Seconds - -	120 seconds per column							

SITTING No. 22. 10 October '41 (5 p.m.)

"Telepathy" experiments, with prepared random numbers governing selection of animal picture cards at both "normal" and "slow" rates.

Present : B.S. =(P); S.G.S. =(EP); K.M.G. =(EA); Miss R.E. =(A).

Sheet Nos. 1, 2, 3 =(PRN) (TP) (Normal Rate) : *Agent*, R.E.
Sheet Nos. 4, 5, 6, 7 =(PRN) (TP) (Slow Rate) : *Agent*, R.E.

No. of Sheet	(−2)	(−1)	(0)	(+1)	(+2)	(CC+1)
1a - - -	7	9	5	5	4	4
1b - - -	4	2	9	5	2	4
2a - - -	6	6	6	4	6	6
2b - - -	7	7	5	3	3	5
3a - - -	3	6	9	8	5	3
3b - - -	7	6	4	4	3	4
Totals - - - -	34	36	38	29	23	26
4a - - -	5	5	3	5	7	
4b - - -	5	4	5	3	2	
5a - - -	3	3	5	7	2	
5b - - -	2	5	6	5	4	
6a - - -	5	3	4	7	6	
6b - - -	5	4	6	5	1	
7a - - -	3	5	6	6	2	
7b - - -	4	7	4	5	4	
Totals - -	32	36	39	43	28	

TIMES

	Sheets							
Normal Rate -	1a	1b	2a	2b	3a	3b		
Seconds - -	62	60	61	66	59	61		
Slow Rate - -	4a	4b	5a	5b	6a	6b	7a	7b
	120 seconds per column							

SITTING No. 23. 24 October '41 (5 p.m.)

" Telepathy " and " Clairvoyance " experiments alternated in sheets of 50 guesses (*cf.* p. 50 ff.), with prepared random numbers governing selection of animal picture cards. " Normal " rate.

Present : B.S. =(P); S.G.S. =(EP); Mrs Woollard =(EA);
Dr C.E.M. Joad =(O); Miss R.E. =(A).

Sheet Nos. 1, 3, 5, 7 =(PRN) (TP) (Normal Rate) : *Agent*, R.E.
Sheet Nos. 2, 4, 6 =(PRN) (CP) (Normal Rate) : *Agent*, R.E.

No. of Sheet	(−2)	(−1)	(0)	(+1)	(+2)	(CC+1)
1a - - -	7	3	7	7	4	5
1b - - -	9	3	0	10	5	6
3a - - -	6	7	4	8	2	5
3b - - -	1	4	3	9	3	5
5a - - -	8	5	4	7	1	6
5b - - -	6	2	7	7	3	2
7a - - -	7	6	1	3	4	4
7b - - -	2	5	3	5	5	4
Totals - - -	46	35	29	[56]	27	37
2a - - -	**4**	**2**	**6**	**2**	**2**	**5**
2b - - -	**6**	**8**	**5**	**4**	**6**	**5**
4a - - -	**4**	**6**	**5**	**6**	**4**	**4**
4b - - -	**2**	**5**	**5**	**5**	**6**	**2**
6a - - -	**4**	**6**	**4**	**7**	**4**	**6**
6b - - -	**6**	**5**	**7**	**5**	**3**	**5**
Totals - - -	**26**	**32**	**32**	**29**	**25**	**27**

TIMES

	Sheets							
Telepathy - -	1a	1b	3a	3b	5a	5b	7a	7b
Seconds - -	65	60	70	70	75	65	70	70
Clairvoyance -	2a	2b	4a	4b	6a	6b		
Seconds - -	65	65	70	65	75	70		

SITTING No. 24. 7 November '41 (5 p.m.)

" Telepathy " alternated with " Clairvoyance " experiments in sheets of 50 guesses (*cf.* p. 50 ff.), with prepared random numbers governing selection of animal picture cards. " Normal " rate.

Present : B.S. = (P); S.G.S. = (EP); K.M.G. = (EA); Mrs Woollard = (O); Miss R.E. = (A).

Sheet Nos. 1, 3, 5 = (PRN) (TP) (Normal Rate) : *Agent*, R.E.
Sheet Nos. 2, 4, 6 = (PRN) (CP) (Normal Rate) : *Agent*, R.E.

No. of Sheet	(−2)	(−1)	(0)	(+1)	(+2)	(CC+1)
1a	5	1	4	5	8	6
1b	2	9	7	4	7	5
3a	5	2	4	3	2	5
3b	2	4	7	6	4	3
5a	6	2	4	5	5	5
5b	6	4	3	7	3	5
Totals	26	22	29	30	29	29
2a	**7**	**6**	**5**	**4**	**5**	**1**
2b	**3**	**3**	**5**	**3**	**5**	**10**
4a	**4**	**5**	**8**	**3**	**3**	**4**
4b	**6**	**3**	**4**	**4**	**4**	**6**
6a	**6**	**3**	**4**	**5**	**4**	**1**
6b	**5**	**7**	**4**	**1**	**3**	**8**
Totals	**31**	**27**	**30**	**20**	**24**	**30**

TIMES

	Sheets					
Telepathy	1a	1b	3a	3b	5a	5b
Seconds	60	63	62	63	62	59
Clairvoyance	2a	2b	4a	4b	6a	6b
Seconds	60	61	61	63	60	60

SITTING No. 25. 14 November '41 (c. 5 p.m.)

"Telepathy" experiments, with prepared random numbers governing selection of animal picture cards, at "normal" rate and "slow" rate alternately (*cf.* pp. 58-59).

Present : B.S. =(P); S.G.S. =(EP); K.M.G. =(EA); Miss R.E. =(A).

Sheet Nos. 1, 3, 5, 7 =(PRN) (TP) (Normal Rate) : *Agent*, R.E.
Sheet Nos. 2, 4, 6, 8 =(PRN) (TP) (Slow Rate) : *Agent*, R.E.

No. of Sheet	(−2)	(−1)	(0)	(+1)	(+2)	(CC+1)
1a - - -	5	3	7	6	3	6
1b - - -	7	5	5	5	1	3
3a - - -	4	5	3	7	6	4
3b - - -	2	2	9	12	4	5
5a - - -	7	3	4	7	5	3
5b - - -	5	2	3	6	6	8
7a - - -	5	8	7	4	4	6
7b - - -	5	3	7	7	5	10
Totals - - -	40	31	45	[54]	34	45
2a - - -	4	5	2	4	9	2
2b - - -	5	7	4	2	2	5
4a - - -	7	5	5	5	3	5
4b - - -	7	2	6	7	4	8
6a - - -	4	5	4	4	2	8
6b - - -	0	5	9	3	8	7
8a - - -	7	3	5	6	5	3
8b - - -	5	3	9	5	4	8
Totals - - -	39	35	44	36	37	46

TIMES

	Sheets							
Normal Rate -	1a	1b	3a	3b	5a	5b	7a	7b
Seconds - -	58	58	61	53	62	60	59	55
Slow Rate - -	2a	2b	4a	4b	6a	6b	8a	8b
Seconds - -	120 seconds per column							

SITTING No. 26. 21 November '41 (5 p.m.)

"Telepathy" and "clairvoyance" experiments alternated in sheets of 50 guesses (*cf.* p. 50 ff.) with prepared random numbers governing selection of animal picture cards. "Normal" rate.

Present : B.S. = (P); S.G.S. = (EP); Mrs Woollard = (EA); Dr C.E.M. Joad = (O); Miss R.E. = (A).

Sheet Nos. 1, 5, 7 = (PRN) (TP) (Normal Rate) : *Agent*, R.E.
Sheet No. 3 = (PRN) (TP) (Normal Rate) : *Agent*, Dr C.E.M. Joad [1]
Sheet Nos. 2, 4, 6 = (PRN) (CP) (Normal Rate) : *Agent*, R.E.

No. of Sheet	(−2)	(−1)	(0)	(+1)	(+2)	(CC+1)
1a - - -	5	2	5	7	2	4
1b - - -	3	9	3	6	6	5
5a - - -	2	6	5	8	4	2
5b - - -	3	5	8	10	4	6
7a - - -	4	6	7	4	2	6
7b - - -	4	6	3	5	7	4
Totals - - -	21	34	31	[40]	25	27
3a[1] - - -	5	4	5	7	3	4
3b[1] - - -	3	6	4	8	1	4
Totals - - -	8	10	9	[15]	4	8
2a - - -	3	1	7	10	6	5
2b - - -	6	7	3	4	3	3
4a - - -	3	7	1	7	4	4
4b - - -	5	5	6	4	2	2
6a - - -	5	5	5	3	4	6
6b - - -	3	5	9	8	7	6
Totals - - -	25	30	31	36	26	26

TIMES

	Sheets							
Telepathy - -	1a	1b	3a	3b	5a	5b	7a	7b
Seconds - -	80	75	60	70	70	80	70	70
Clairvoyance -	2a	2b	4a	4b	6a	6b		
Seconds - -	80	70	70	75	65	70		

[1] Just before sheet 3 began, Dr Joad went into the studio and, unknown to (P) and (EP) in the ante-room, asked R.E. (A) to leave her seat while he himself sat in it, reshuffled the cards, and acted as Agent during this sheet. R.E. later reported that she had moved out of sight of the cards while Dr Joad acted as Agent.

SITTING No. 27. 5 December '41 (5 p.m.)

" Telepathy " alternated with " clairvoyance " experiments in sheets of 50 guesses (*cf.* p. 50 ff.), with prepared random numbers governing selection of animal picture cards. " Normal " rate.

Present : B.S. = (P); S.G.S. = (EP); K.M.G. = (EA); Sir Ernest Bennett, = (O); Miss R.E. = (A).

Sheet Nos. 1, 3, 5, 7 = (PRN) (TP) (Normal Rate) : *Agent*, R.E.
Sheet Nos. 2, 4, 6, 8 = (PRN) (CP) (Normal Rate) : *Agent*, R.E.

No. of Sheet	(−2)	(−1)	(0)	(+1)	(+2)	(CC+1)
1a - - -	6	5	5	8	4	7
1b - - -	1	5	9	8	5	5
3a - - -	1	6	2	11	7	4
3b - - -	5	7	5	9	6	3
5a - - -	5	6	5	8	7	7
5b - - -	2	4	4	7	7	3
7a - - -	6	5	5	4	0	4
7b - - -	8	3	2	4	4	3
Totals - - -	34	41	37	[59]	40	36
2a - - -	8	8	8	4	4	8
2b - - -	3	5	6	6	4	9
4a - - -	2	6	8	7	6	3
4b - - -	4	5	7	6	6	5
6a - - -	7	7	6	4	3	7
6b - - -	2	5	8	7	3	5
8a - - -	6	6	8	4	4	3
8b - - -	6	6	7	4	3	6
Totals - - -	38	48	[58]	42	33	46

TIMES

	Sheets							
Telepathy - -	1a	1b	3a	3b	5a	5b	7a	7b
Seconds - -	53	60	60	61	52	52	55	55
Clairvoyance -	2a	2b	4a	4b	6a	6b	8a	8b
Seconds - -	53	60	55	56	51	60	55	52

SITTING No. 28. 21 December '41 (12.30 p.m.)

"Telepathy" experiments alternated with "clairvoyance" experiments in sheets of 50 guesses (*cf.* p. 50 ff.), with prepared random numbers governing selection of animal picture cards. "Normal" rate.

Present : B.S. = (P); S.G.S. = (EP); K.M.G. = (EA); Mr C. A. Mace = (O); Miss R.E. = (A).

Sheet Nos. 1, 3, 5, 7 = (PRN) (TP) (Normal Rate) : *Agent*, R.E.
Sheet Nos. 2, 4, 6 = (PRN) (CP) (Normal Rate) : *Agent*, R.E.

No. of Sheet	(−2)	(−1)	(0)	(+1)	(+2)	(CC+1)
1a	8	4	3	7	5	6
1b	2	5	4	9	5	2
3a	6	5	4	7	4	7
3b	6	5	3	7	3	8
5a	4	9	3	9	2	5
5b	5	9	4	7	5	3
7a	7	0	4	6	3	4
7b	6	4	9	6	2	3
Totals	44	41	34	[58]	29	38
2a	**8**	**5**	**5**	**3**	**5**	**6**
2b	**5**	**2**	**7**	**6**	**3**	**5**
4a	**4**	**2**	**4**	**7**	**4**	**3**
4b	**5**	**6**	**6**	**4**	**1**	**2**
6a	**6**	**3**	**7**	**7**	**5**	**2**
6b	**4**	**4**	**4**	**6**	**6**	**7**
Totals	**32**	**22**	**33**	**33**	**24**	**25**

TIMES

	Sheets							
Telepathy	1a	1b	3a	3b	5a	5b	7a	7b
Seconds	50	50	50	?	50	51	55	55
Clairvoyance	2a	2b	4a	4b	6a	6b		
Seconds	49	48	50	49	51	50		

SITTING No. 29. 3 January '42 (5 p.m.)

" Telepathy " experiments, with counters governing selection of animal picture cards. Suggestion given to score (+1) hits for some sheets and direct hits for others (*cf.* p. 55) " Normal " rate.

Present : B.S.=(P); S.G.S.=Recorder for (Counters); K.M.G.=(EA); Miss R.E.=(A).

Sheet Nos. 1, 2, 5, 6=(Counters) (TP) (Normal Rate) : *Agent*, R.E. Suggestion given to score (+1) hits.

Sheet Nos. 3, 4, 7, 8=(Counters) (TP) (Normal Rate) : *Agent*, R.E. Suggestion given to score (0) hits (direct hits).

No. of Sheet	(−2)	(−1)	(0)	(+1)	(+2)	(CC+1)
1a - - -	3	4	8	5	5	3
1b - - -	3	6	7	7	5	7
2a - - -	1	6	4	8	7	11
2b - - -	4	7	4	8	7	6
5a - - -	2	7	4	2	6	3
5b - - -	8	4	1	3	3	1
6a - - -	5	5	3	5	5	4
6b - - -	7	4	3	6	6	4
Totals - - -	33	43	34	44	44	39
3a - - -	4	6	5	4	5	5
3b - - -	3	5	3	4	4	5
4a - - -	5	4	4	4	8	4
4b - - -	5	7	3	4	1	3
7a - - -	3	4	5	2	4	7
7b - - -	6	3	6	5	8	5
8a - - -	7	4	7	4	7	9
8b - - -	7	4	4	3	8	3
Totals - - -	40	37	37	30	45	41

TIMES

	Sheets							
Normal Rate -	1a	1b	2a	2b	5a	5b	6a	6b
Seconds - -	47	47	50	47	48	46	48	52
Normal Rate -	3a	3b	4a	4b	7a	7b	8a	8b
Seconds - -	52	52	50	56	50	54	54	60

SITTING No. 30. 16 January '42 (7.30 p.m.)

"Telepathy" experiments, with counters governing selection of animal picture cards. "Normal" rate.

Present : B.S. =(P); K.M.G. =(EA); Dr Wiesner = Recorder for (Counters) and (EP); Mr H. Chibbett and Miss Kennedy =(A) and (EP).

Note.—S.G.S. not present. These experiments were not intended to be part of the ordinary series. K.M.G. called in at the Studio with friends and a few experiments were tried. They are included for sake of completeness in order that no experiment, from first to last, should be left unrecorded in this Report.

Sheet Nos. 1, 2 =(Counters) (TP) (Normal Rate) : *Agent*, Mr Chibbett.
Sheet Nos. 3, 4 =(Counters) (TP) (Normal Rate) : *Agent*, Miss Kennedy.

No. of Sheet	(−2)	(−1)	(0)	(+1)	(+2)	(CC+1)
1a - - -	7	7	4	5	2	3
1b - - -	2	4	3	7	6	3
2a - - -	6	3	6	5	5	5
2b - - -	4	4	5	4	6	9
Totals - - -	19	18	18	21	19	20
3a - - -	3	5	4	6	3	5
3b - - -	4	7	7	5	4	5
4a - - -	5	4	6	4	6	2
4b - - -	6	4	4	6	3	6
Totals - - -	18	20	21	21	16	18

TIMES

Sheets								
Normal Rate -	1a	1b	2a	2b	3a	3b	4a	4b
Seconds - -	63	66	66	60	65	60	60	59

SITTING No. 31. 8 May '42 (7.10 p.m.)

(Experiments resumed after a gap of four months)

"Telepathy" experiments, with prepared random numbers governing the selection of initial letter cards (*cf.* pp. 64-65) "Normal" rate and "slow" rate.

Present : B.S. = (P); S.G.S.' = (EP); Mr R. G. Medhurst = (EA); Mr J.Al. = (A).

Sheet Nos. 1, 2, 3, 4 = (PRN) (TL) (Normal Rate) : *Agent*, J.Al.
Sheet Nos. 5, 6 = (PRN) (TL) (Slow Rate) *Agent*, J.Al.

No. of Sheet	(−2)	(−1)	(0)	(+1)	(+2)
1a	6	2	4	6	3
1b	4	6	4	4	2
2a	3	9	7	8	7
2b	5	9	9	7	4
3a	4	8	0	4	4
3b	8	7	0	7	5
4a	6	4	4	6	7
4b	4	7	4	4	5
Totals	40	[52]	32	46	37
5a	7	3	7	4	4
5b	4	6	8	5	3
6a	2	1	4	6	7
Totals	13	10	19	15	14

Note.—After call No. 20 in Sheet 6a, B.S. threw down his pencil saying it was waste of time for him to continue guessing at this (5 seconds) rate. He had been growing more and more irritated.

TIMES

	Sheets							
Normal Rate	1a	1b	2a	2b	3a	3b	4a	4b
Seconds	65	65	65	65	68	65	N.T.	N.T.
Slow Rate	5a	5b						
Seconds	120 seconds per column							

SITTING No. 32. 15 May '42 (7.15 p.m.)

"Telepathy" experiments, with prepared random numbers and also a non-random sequence of numbers governing selection of initial letter cards (*cf*. p. 68) "Normal" rate.

Present: B.S. = (P); S.G.S. = (EP); Mr R. G. Medhurst = (EA); Mr J.Al. = (A).

Sheet Nos. 1a, 2, 3, 4 = (PRN) (TL) (Normal Rate): *Agent*, J.Al.
Sheet No. 1b = (TL) (Normal Rate): *Agent*, J.Al. A non-random sequence of presentations consisting of 12 digits (5) followed by 13 digits (3).

No. of Sheet	(−2)	(−1)	(0)	(+1)	(+2)
1a - - - - - -	5	6	5	4	3
2a - - - - - -	3	9	6	9	3
2b - - - - - -	6	11	9	8	8
3a - - - - - -	6	8	6	6	5
3b - - - - - -	5	6	6	6	6
4a - - - - - -	5	6	3	4	7
4b - - - - - -	6	4	3	5	9
Totals - - - - -	36	[50]	38	42	41
1b - - - - - -	7	7	7	6	5

TIMES

Normal Rate -	*Sheets* 1a	1b	2a	2b	3a	3b	4a	4b
Seconds - -	?	62	62	59	60	61	60	61

SITTING No. 33. 22 May '42 (7.30 p.m.)

"Telepathy" experiments, with prepared random numbers and also a non-random sequence of numbers governing selection of initial letter cards. "Rapid" rate (*cf.* p. 65 ff.).

Present: B.S.=(P); S.G.S.=(EP); Mr R. G. Medhurst=(EA); Mr J.Al.=(A).

Sheet Nos. 1a, 2, 3, 4=(PRN) (TL) (Rapid Rate): *Agent*, J.Al.
Sheet No. 1b =(TL) (Rapid Rate): *Agent*, J.Al. A non-random sequence of presentations consisting of 12 digits (1) followed by 13 digits (3)—*cf.* p. 68.

No. of Sheet	(−2)	(−1)	(0)	(+1)	(+2)	(CC+2)
1a - - -	6	6	7	2	6	3
2a - - -	5	5	2	5	6	7
2b - - -	8	6	3	7	10	4
3a - - -	7	5	4	4	4	3
3b - - -	7	7	5	7	11	8
4a - - -	7	3	3	8	7	3
4b - - -	5	5	5	6	6	1
Totals - - -	[45]	37	29	39	[50]	29
1b - - -	13	13	12	13	12	

TIMES

	Sheets 1a	1b	2a	2b	3a	3b	4a	4b
Rapid Rate - -	1a	1b	2a	2b	3a	3b	4a	4b
Seconds - -	36	37	35	36	34	31	28	26

SITTING No. 34. 5 June '42 (7.25 p.m.)

" Telepathy " experiments, with prepared random numbers governing selection of initial letter cards. " Rapid " rate (*cf.* p. 65 ff.).

Present : B.S. = (P); S.G.S. = (EP); K.M.G. = (EA); R.E. = Checker; Mr J.Al. = (A).

Sheet Nos. 1, 2, 3, 4 = (PRN) (TL) (Rapid Rate) : *Agent*, J.Al.

No. of Sheet	(−2)	(−1)	(0)	(+1)	(+2)	(CC+2)
1a - - -	9	6	2	5	6	5
1b - - -	8	5	4	5	11	7
2a - - -	1	5	5	5	5	4
2b - - -	3	6	8	3	7	6
3a - - -	5	5	4	3	7	2
3b - - -	4	7	7	7	7	3
4a - - -	3	7	3	7	7	5
4b - - -	3	5	7	9	7	5
Totals - - -	36	46	40	44	[57]	37

TIMES

Sheets								
Rapid Rate - -	1a	1b	2a	2b	3a	3b	4a	4b
Seconds - -	36	32	34	38	33	32	32	30

SITTING No. 35. 7 August '42 (6 p.m.)

Special test designed to show whether (P) influences (EA) in the selection of counters (in the (Counters) experiments). (*Cf*. pp. 73-74.)

Present : B.S. = (P); S.G.S = (EP); K.M.G. = Selector of counters; Mrs Wykeham-Martin = Recorder of counters.

Sheet Nos. 1-10 = Special Test.

No. of Sheet	(−2)	(−1)	(0)	(+1)	(+2)
1a	7	4	5	7	3
1b	5	6	3	6	6
2a	5	4	5	3	6
2b	5	3	7	7	7
3a	1	3	3	5	4
3b	3	3	8	5	7
4a	6	6	5	9	5
4b	4	3	6	6	4
5a	4	3	5	7	4
5b	4	2	3	7	6
6a	3	5	5	4	5
6b	3	4	9	6	2
7a	2	4	5	3	5
7b	8	5	4	3	9
8a	1	6	5	3	7
8b	7	3	4	3	8
9a	6	6	5	3	3
9b	5	7	8	4	5
10a	5	5	7	4	5
10b	4	1	1	4	11
Totals	88	83	103	99	112

SITTING No. 36. 12 August '42 (6.15 p.m.)

" Telepathy " experiments, with prepared random numbers and also a non-random sequence of numbers governing selection of initial letter cards. (*Cf.* p. 68) " Normal " rate.

Present : B.S. = (P); S.G.S. = (EP); K.M.G. = (EA); Mr J.Al. = (A).

Sheet Nos. 1, 2b, 3, 4 = (PRN) (TL) (Normal Rate) : *Agent*, J.Al.
Sheet No. 2a = (TL) (Normal Rate) : *Agent*, J.Al. A non-random sequence of presentations consisting of 12 digits (3) followed by 13 digits (1).

No. of Sheet	(−2)	(−1)	(0)	(+1)	(+2)
1a	4	6	2	11	6
1b	3	10	2	8	3
2b	8	7	·5	6	2
3a	3	6	4	8	6
3b	2	11	2	8	8
4a	6	8	7	9	2
4b	4	3	5	6	4
Totals	30	[51]	27	[56]	31
2a	13	12	13	12	12

TIMES

	Sheets 1a	1b	2a	2b	3a	3b	4a	4b
Normal Rate								
Seconds	60	60	61	61	59	65	58	60

SITTING No. 37. 26 August '42 (c. 6 p.m.)

Alternate " Lift " and " Touch " experiments (" telepathy ") with prepared random numbers governing the selection of initial letter cards (*cf.* p. 69). Also " telepathy " experiments with Zener symbols using prepared random numbers (*cf.* p. 73).

Present: B.S. = (P); S.G.S. = (EP); K.M.G. = (EA); Mr J.Al. = (A).

Sheet Nos. 1a, 2a, 3a, 4a = (PRN) (TL) (Normal Rate).
Agent, J.Al. lifting and looking at each card.

Sheet Nos. 1b, 2b, 3b, 4b = (PRN) (TL) (Normal Rate).
Agent, J.Al. touching backs of cards only.

Sheet Nos. 5, 6, 7 = (PRN) (Zener symbols) (Normal Rate):
Agent, J.Al. lifting and looking at cards.

No. of Sheet	(−2)	(−1)	(0)	(+1)	(+2)
1a	3	11	2	9	3
2a	3	6	6	4	7
3a	8	8	3	7	5
4a	4	8	2	7	6
Totals	18	[33]	13	(27]	21
1b	6	7	4	8	7
2b	6	3	5	8	5
3b	6	7	2	8	5
4b	5	4	6	8	4
Totals	23	[21]	17	[32]	21
5a	4	4	4	4	5
5b	3	9	7	8	2
6a	7	1	4	7	5
6b	3	5	3	7	5
7a	4	7	4	6	6
7b	6	6	8	4	4
Totals	27	32	30	36	27

TIMES

Normal Rate (*Sheets*)	1a	1b	2a	2b	3a	3b	4a	4b
Seconds	52	55	55	53	53	58	55	57

Normal Rate	5a	5b	6a	6b	7a	7b
Seconds	57	55	55	55	55	53

SITTING No. 38. 6 January '43 (6.30 p.m.)

(Gap of 4½ months since last sitting. This extra sitting arranged in order to supplement the data on the "rapid" rate experiments.)

"Telepathy" experiments, with prepared random numbers governing selection of initial letter cards. "Rapid" rate.

Present : B.S. = (P); S.G.S. = (EP); Mr L. A. Rozelaar = (EA); Mr J.Al. = (A).

Sheet Nos. 1, 2, 3, 4 = (PRN) (TL) (Rapid Rate) : *Agent*, J.Al.

No. of Sheet	(−2)	(−1)	(0)	(+1)	(+2)	(CC−2)
1a	9	5	5	4	7	2
1b	9	4	3	5	7	4
2a	9	2	5	6	6	6
2b	10	3	3	9	5	4
3a	10	4	2	4	7	2
3b	9	4	1	7	5	3
4a	7	1	2	4	3	3
4b	7	6	4	4	2	4
Totals	[70]	29	25	43	42	28

TIMES

Rapid Rate	*Sheets* 1a	1b	2a	2b	3a	3b	4a	4b
Seconds	N.T.	42	40	39	35	29	28	29

SITTING No. 39. 8 April '43 (6.10 p.m.)

(Gap of 3 months since last sitting. One of two additional sittings held at the rooms of the Society for Psychical Research. See p. 75.)

Alternate " Lift " and " Touch " experiments (" telepathy "), with prepared random numbers governing the selection of initial letter cards. " Normal " rate.

Present : B.S. = (P); S.G.S. = (EP); Mr R. G. Medhurst = (EA); Miss J. Fairbairn and Mr D. Parsons = Checkers; Mr J.Al. = (A).

Sheet Nos. 1a, 2a, 3a, 4a = (PRN) (TL) (Normal Rate) : *Agent,* J.Al. Agent lifts each card and looks at it.

Sheet Nos. 1b, 2b, 3b, 4b = (PRN) (TL) (Normal Rate) : *Agent,* J.Al. Agent touches backs of cards only.

No. of Sheet	(−2)	(−1)	(0)	(+1)	(+2)
1a	6	8	4	7	7
2a	4	8	4	7	2
3a	1	6	7	8	2
4a	9	7	3	10	6
Totals	20	[29]	18	[32]	17
1b	1	7	4	9	5
2b	4	9	5	11	4
3b	6	4	5	8	2
4b	3	7	5	7	1
Totals	14	[27]	19	[35]	12

TIMES

	Sheets							
Normal Rate	1a	1b	2a	2b	3a	3b	4a	4b
Seconds	55	63	70	67	59	66	68	70

SITTING No. 40. 15 April '43 (6.10 p.m.)

(Held at rooms of the Society for Psychical Research.)

Variation of "Lift and Touch" experiment, with prepared random numbers governing the selection of initial letter cards (*cf.* p. 76). "Normal" rate.

Present : B.S. = (P); S.G.S. = (EP); Mr D. J. West = (EA); Mr J.Al. = (A).

Sheet Nos. 1, 2, 3, 4 = (PRN) (TL) (Normal Rate) : *Agent*, J.Al. concentrates on faces of cards for 15 seconds before each sheet; then touches backs of cards at each call without lifting them.

No. of Sheet	(−2)	(−1)	(0)	(+1)	(+2)
1a	8	10	4	9	1
1b	3	7	6	7	2
2a	4	6	1	5	9
2b	2	6	3	7	4
3a	1	6	2	14	4
3b	6	5	3	4	10
4a	3	6	1	8	4
4b	2	7	3	6	5
Totals	29	[53]	23	[60]	39

TIMES

	Sheets							
Normal Rate	1a	1b	2a	2b	3a	3b	4a	4b
Seconds	90	74	70	72	72	66	66	70

PART THREE

CHRONICLE OF EXPERIMENTS

It was originally intended to print the Chronicle of each sitting, giving in minute detail the conditions and happenings at each experiment. Owing to war-time necessity for paper economy, it has been decided to omit this lengthy record. For those who wish to make a study of the Chronicle, duplicated copies can be obtained on application from the Society for Psychical Research, and references to the Chronicle in the text have, therefore, been left intact.

The Chronicle for three sittings only is given below, to indicate the conditions in force.

SITTING No. 8. 14 March '41 (5 p.m.)

" Telepathy " experiments, with prepared random numbers and with counters drawn from bag governing selection of cards (*cf.* p. 51, and List of Scores, p. 94).

Items

1. *Present:* B.S. =(P); R.E. =(A); S.G.S. =(EA) for (PRN) experiments and Recorder for (Counters) experiments; Miss Ina Jephson =(O), and (EA) for (Counters) experiments; Mrs Oliver Gatty =(EP);

   During sheets 1-3, B.S.'s Assistant sat in the ante-room at (X)[1] re-touching photographs. She did not enter Studio and left at end of sheet 3.
2. B.S. seated by fire in ante-room with Mrs Gatty as (EP).
3. (A) and (EA) sit in Studio separated by screen; cards screened inside box.
4. Doors D_2 and D_3[1] closed throughout progress of experiments.
5. Distance between (P) and (A) about 12 feet in a straight line drawn through intervening wall between the two rooms. Mirror in Studio covered by large cloth, away from card-table.
6. (EA) gave signal " Next " for calls, and (P) answered " Right " immediately he had recorded his guess.
7. During sheets 1-3 (PRN), Miss Jephson sat at the experimenters' table and shuffled the cards before each 50 calls out of sight of both (A) and (EA). She also checked (A')s selection of the cards and the taking down of the code. During sheets 4-6, Miss Jephson drew counters from the bag for the (Counters) experiments and showed them at the aperture in screen. S.G.S. acted as Recorder, sitting to the side of the box so that he could not see the cards inside it.

   Mrs Gatty had to leave at the end of sheet 3, after taking part in the checking-up of results with Miss Jephson, S.G.S., and R.E.

   During sheets 4-6, B.S. sat alone in the ante-room.

   Cards for sheets 4-6 shuffled by (A) out of sight of (EA) and Recorder.
8. Code recorded by (EA).
9. Results decoded for sheets 4-6 at the end of sheet 6 by Miss Jephson, S.G.S., and R.E. Miss Jephson invited to re-check any sheets she chose.

[1] See Plate I.

Before duplicates were made out, Miss Jephson had to leave premises to attend to her car-lights. She took with her all the original scoring sheets.

10. On her return she assisted S.G.S. and R.E. in making out the duplicates and posted them herself to Professor Broad in Cambridge.

Remarks.—Mrs Gatty reported in writing:

(1) "Mr B.S. filled in all his spaces in strict order one after the other, leaving no gaps to return to afterwards.

(2) He wrote down his first guess *at once* on hearing the first call from S.G.S."

For B.S.'s comments see Report, p. 78.

SITTING No. 28. 21 December '41 (12.30 p.m.)

"Telepathy" experiments alternated with "Clairvoyance" (*cf.* p. 50 ff.) in sheets of 50 guesses, with prepared random numbers governing selection of animal picture cards. "Normal" rate. See List of Scores, p. 114.

Items

1. *Present:* B.S. = (P); R.E. = (A); S.G.S. = (EP); K.M.G. = (EA); Mr C. A. Mace = (O).
2. B.S. and (EP) now no longer sit in front of fire-place, but at table marked (W) in Plan.[1]
3. (A) and (EA) separated by screen at card-table in Studio; cards screened inside box.
4. Door D_3 closed throughout experiment. Door D_2[1] left very slightly ajar to facilitate (P) hearing the serial numbers called by (EA).
5. From where (P) sat it was quite impossible for him to see the card-table. Mirror in Studio covered by large cloth—away from card-table.
6. (EA) calls serial numbers 1-25; B.S., (P), keeps in step with these numbers for his guesses.

   N.B.—At B.S.'s own request an innovation was made in the method of recording guesses. B.S. and (EP) sat facing each other on opposite sides of the table. In front of (P) were five cards bearing pictures of the five animals and placed faces upwards. When (P) heard the serial number of the call, he spoke his guesses in a low tone and S.G.S. recorded the initial letter of the animal's name in the appropriate cell of the G column. The interval between (EA)'s call and (P)'s guess was always less than half a second. (P) was told that the sheet was for "telepathy" or "clairvoyance" as the case might be, this being called out by (EA).

   As described on pp. 83-84, the envelope containing lists of random digits which Mr Mace brought with him to the sitting was opened by him inside the Studio. The sheets were handed to K.M.G., (EA), one by one as required. They were never seen by S.G.S. or by B.S. until the end of the sitting.

   During sheet 1, Mr Mace sat near (P) in ante-room. During the remaining sheets he was in the Studio except during sheet 3, when he returned to the ante-room and sat near (P).
7. Cards shuffled by (A) out of sight of (EA).
8. Code recorded by (EA).

[1] See Plate I.

9. At the end of sheet 7, Mr Mace returned to the ante-room and collected (P)'s sheets which he took into the Studio. They were laid on the table for decoding etc. S.G.S. read out (P)'s guesses which were decoded and entered by K.M.G. with Mr Mace checking both. Mr Mace watched the counting of hits and the evaluation of the critical ratio for (+1) hits. Before he left, Mr Mace said he thought the experiment could hardly have been done better. The rate of calling was a little quicker than usual.
10. Mr Mace signed the record sheets but was unable to stay to watch the making of duplicates. These were done as usual by K.M.G., S.G.S., and R.E., and posted immediately in the box outside the Studio to Professor Broad in Cambridge.

Remarks.—As this experiment was done on a Sunday morning, B.S.'s Lady-Assistant was not present at work. Before Mr Mace's arrival, K.M.G. tried calling in time to a metronome (practice only), but B.S. declared that it would be hopeless to expect him to get results " with that confounded thing ticking away in the other room ", so the idea was abandoned.

During sheet 3, while Mace was in the ante-room, B.S. declared that he was " getting more vibrations from Mr Mace than from the Agent ". So at the end of this sheet [Mr Mace] returned to the Studio, as described above.

SITTING No. 39. 8 April '43 (6.10 p.m.)

Held at the rooms of the Society for Psychical Research, 31 Tavistock Square, London, W.C. 1. (See p. 75.) Alternate " Lift " and " Touch " (" telepathy ") experiments, with prepared random numbers governing the selection of initial letter cards. " Normal " rate. See List of Scores, p. 125.

1. *Present:* B.S. = (P); J.Al. = (A); S.G.S. = (EP); Mr R. G. Medhurst = (EA); Miss J. Fairbairn and Mr D. Parsons = Checkers.
2. (P), (EP), and D. Parsons were seated in the Society's séance room at or near a small table. From his position here, (P) was quite unable to see into the next room in which the Agent sat. (EP) recorded (P)'s guesses.
3. (A) and (EA), as well as Miss Fairbairn, sat in the ante-room adjoining séance room, at a table on which were the screen and box enclosing cards (as used in B.S.'s Studio and brought here). (A) and (EA) separated by screen : cards inside box in front of (A).
4. Door between séance room and ante-room left very slightly ajar to facilitate hearing of (EA)'s call.
5. Distance between (A) and (P) in a straight line drawn through wall between the two rooms c. 12 feet.
6. Serial numbers 1-25 called by (EA) as guide to (P) for recording his guesses.
7. Medhurst, (EA), showed random numbers at the aperture in screen, at " normal " rate. Miss Fairbairn recorded the times, and assisted (EA) with " taking down the code " after each 50 calls.
8. When four sheets were completed, Medhurst and Miss Fairbairn, working together, decoded (P)'s guesses and entered the numbers in the G. column; counted the hits for the three categories (+1), (0), and (−1); and watched S.G.S. evaluate the critical ratio. They then made

duplicate records and these were checked against the original sheets by Medhurst and Parsons. Medhurst took charge of the duplicates (after the originals had been signed by all four experimenters) and posted them to Professor Broad. He also took away with him a private record of the totals in all three categories. S.G.S. took no active part in any of the decoding, checking, counting of hits, or making of duplicates, but merely watched Medhurst and Miss Fairbairn.

APPENDIX A

1. Percentage of True Cognitions.
2. B.S. scores at a higher rate on some animal symbols than on others.

1. *Percentage of True Cognitions.*

We shall define a " true cognition " as a successful hit that is not due to chance.

(*a*) If s = observed number of hits
x = number of true cognitions
N = number of trials,

then the most probable value of x is given by the equation

$$s = x + \tfrac{1}{5}(N - x).$$

Hence
$$x = \frac{5s - N}{4}.$$

And the percentage of true cognitions in the N trials

$$= \frac{100x}{N} = \frac{125s}{N} - 25 = P \text{ (say)}.$$

Let
$$\frac{s}{N} = p.$$

The mean value p_0 of p is not known. But if we have two samples N_1 and N_2 with observed hits s_1 and s_2 respectively, the *best value* for p_0 will be

$$p_0 = \frac{s_1 + s_2}{N_1 + N_2}.$$

Then assuming a binomial distribution (*which may not always be justifiable*) the standard error of P will be

$$125\sqrt{\frac{p_0(1 - p_0)}{N}} \text{ for } N \text{ trials.}$$

Hence for our two samples N_1 and N_2 the standard error of the difference in the percentages P_1 and P_2 of true cognitions will be

$$125\sqrt{p_0(1 - p_0)}\left[\frac{1}{N_1} + \frac{1}{N_2}\right]^{\frac{1}{2}}.$$

(*b*) We may use this result to compare the percentages of true cognitions obtained in work with the Agent R.E., (*a*) using (PRN) and (*b*) Counters—both at "normal" rate.

For (PRN) (Tel.) we have $N_1 = 3789 \quad s_1 = 1101.$

,, Counters ,, ,, $N_2 = 1578 \quad s_2 = 439.$

Hence $$p_0 = \cdot 28694$$

and substituting in the above formula we find for the Standard Error of $P_1 - P_2$, S.E. $= 1 \cdot 69\%$.
But the observed value of $P_1 - P_2 = 1 \cdot 39\%$.

Hence the difference is not significant ($\chi = 0 \cdot 82$).

(*c*) We may check this by comparing mean observed scores $(+1)$ for 24 trials.

We have mean score for PRN	=	6·974
,, ,, ,, Counters	=	6·677
Difference	=	+0·297

Variance for a set of 24 with (PRN) $= 5 \cdot 5689$
,, ,, ,, Counters $= 8 \cdot 0278$

$$\text{Hence variance of mean for PRN} = \frac{5 \cdot 569}{157 \cdot 875} = \cdot 0353$$

$$\text{,, ,, ,, Counters} = \frac{8 \cdot 028}{65 \cdot 75} = \cdot 1221$$

$$\text{Variance of difference of means} = \cdot 0353 + \cdot 1221 = \cdot 1574$$

$$\text{S.E. of difference} = \sqrt{\cdot 1574} = \cdot 396$$

Hence $\chi = 0 \cdot 75$ and difference is not significant.

(*d*) As a second example we may compare the percentages of "true cognitions" on (± 1) guesses taken together for (*a*) work with J.Al. as Agent in 1936 using Zener Cards, and (*b*) in 1942 with the same Agent using Initial Letters ("normal" rate). We have :

	(± 1) trials	(± 1) hits	% of Cognitions
1936 - - -	$576 = N_2$	$140 = S_2$	$5 \cdot 38\% = P_2$
1942 - - -	$1440 = N_1$	$410 = S_1$	$10 \cdot 59\% = P_1$
			$P_1 - P_2 = 5 \cdot 21\%$

But $$p_0 = \frac{550}{2016} = 0 \cdot 27282$$

$$\frac{1}{N_1} + \frac{1}{N_2} = \cdot 002431.$$

Hence S.E. of difference $P_1 - P_2 = 2 \cdot 74\%$
And $\chi = 1 \cdot 90$ which is not quite significant.

(*e*) Again we may check by Fisher's method for the difference of means of two small samples. We have

J.Al. 1942. Observed mean for 24 trials $=\bar{x}_1=6{\cdot}833$
J.Al. 1936. „ „ „ $=\bar{x}_2=5{\cdot}833$

Hence $$\bar{x}_1-\bar{x}_2=1{\cdot}000$$

$$n_1=60-1=59 \quad n_2=24-1=23$$

$$\Sigma(x_1-\bar{x}_1)^2=264{\cdot}47$$

$$\Sigma(x_2-\bar{x}_2)^2=121{\cdot}38$$

$$s^2=\tfrac{1}{82}\,[264{\cdot}47+121{\cdot}38]=4{\cdot}705.$$

Hence $$t=\frac{1}{\sqrt{4{\cdot}705}}\quad\sqrt{\frac{1440}{84}}=1{\cdot}9088.$$

No. of degrees of freedom $=n=82$

But $t\sqrt{\dfrac{n-2}{n}}$ can be taken to be a normal deviate with *unit* standard error.

Hence $$\sqrt{\frac{80}{82}}\;t=1{\cdot}885$$

a value which agrees very closely with $\chi=1{\cdot}90$ obtained by the previous method.

Had there been a significant difference in percentages of cognitions, we could of course have drawn no conclusions as to the factors which produced it.

(*f*) *Test for Variance.*

For J.Al. 1936, we have $s_1{}^2=5{\cdot}277$ $\quad n_1=23$
„ „ 1942 „ $s_2{}^2=4{\cdot}483$ $\quad n_2=59$

$$z=1{\cdot}1513\,(\log_{10}5{\cdot}277-\log_{10}4{\cdot}483)=0{\cdot}0815$$

For the 5% point $z>0{\cdot}2654$
Hence the difference in variance is not significant.

2. *Does B.S. score at a higher rate on some animal symbols than on others?*

Denote the five symbols Z, E, L, G, P by 1, 2, 3, 4, 5 respectively.

Suppose that in n trials $(+1)$ the symbols are presented a_1, a_2, a_3, a_4, a_5 times and guessed g_1, g_2, g_3, g_4, g_5 times. Let the *observed* numbers of hits on the five symbols be $S_1\ S_2 \ldots S_5$ and the numbers of "true cognitions" be $x_1\ x_2 \ldots x_5$. Then clearly x_1 is given by the equation

$$S_1=x_1+\frac{(a_1-x_1)\,(g_1-x_1)}{n-X}$$

where $$X=x_1+x_2+x_3+x_4+x_5.$$

Now X is the most probable number of *cognitions* in *n trials* and can be found from the equation $S=X+\frac{1}{5}\,(n-X)$ where S is the total observed number of hits. Whence x_1 can be found as the positive root of a quadratic equation, and similarly we can find $x_2, x_3 \ldots x_5$.

Thus for Group 1 [1], Agents R.E., G.A., C.E.M.J., K.M.G.(?), a Normal Rate, we have for (+1) hits

	(1)	(2)	(3)	(4)	(5)	Totals
$g_1 \ldots g_5$ - -	1118	1393	1196	1206	1126	6039
$a_1 \ldots a_5$ - -	1224	1206	1213	1177	1219	6039
$S_1 \ldots S_5$ - -	349	414	316	352	324	1755

Hence X is given by the equation

$$1755 = X + \tfrac{1}{5}(6039 - X)$$

Or

$$X = 684$$

And x_1 is given by the quadratic

$$349 = x_1 + \frac{(1224 - x_1)(1118 - x_1)}{6039\text{-}684}.$$

Solving this and the four similar quadratics we obtain for $x_1 \ldots x_5$ the values

x_1	x_2	x_3	x_4	x_5	Total
157·84	182·73	79·79	149·14	115·94	685·44

The total 685·44 is sufficiently close to 684 to afford a good check on the work.

Now the phrase " equal degrees of success on the five symbols " might be interpreted to mean that

$$\frac{x_1'}{a_1} = \frac{x_2'}{a_2} = \frac{x_3'}{a_3} = \frac{x_4'}{a_4} = \frac{x_5'}{a_5} = \frac{X}{n}.$$

On this interpretation we can calculate the " expected " values of $x_1' \ldots x_5'$ and compare them with the " observed " most probable values. We obtain

	x_1	x_2	x_3	x_4	x_5	Totals
Observed - -	157·84	182·73	79·79	149·14	115·94	685·44
Expected - -	138·93	136·88	137·68	133·59	138·36	685·44
Deviations - -	+18·91	+45·85	−57·89	+15·55	−22·42	

Whence $\chi^2 = 47{\cdot}7$ and with $n' = 4$, $P < {\cdot}001$

Hence there are widely different degrees of success with the five animals. Especially noticeable is the failure to score on (3) the Lion. This may possibly be due to some inhibition caused by the subconscious emotion of fear, since the lion is the most dangerous of the five animals chosen as presentation objects.

We might however interpret " equal degrees of success " to imply that

$$\frac{x_1'}{g_1} = \frac{x_2'}{g_2} = \frac{x_3'}{g_3} = \frac{x_4'}{g_4} = \frac{x_5'}{g_5} = \frac{X}{n}.$$

[1] See pp. 62-63.

On this interpretation we obtain

	x_1	x_2	x_3	x_4	x_5	Totals
Observed - -	157·84	182·73	79·79	149·14	115·94	685·44
Expected - -	126·90	158·11	135·75	136·88	127·80	685·44
Deviations - -	+30·94	+24·62	−55·96	+12·26	−11·86	

Whence $\chi^2 = 36{\cdot}6$ with $n' = 4$

Again, $P < {\cdot}001$ and there are highly significant differences in degrees of success with the five symbols. The LION (3) is again much below expectation.

APPENDIX B

EFFECTS OF MULTIPLE-DETERMINATION

A (± 1) guess is said to be multiply-determined if the " actual " card presentations which immediately precede and follow it are presentations of the same symbol.

$$\text{Thus}\quad \begin{array}{c} \text{L} \\ \to\text{E} \\ \text{L} \end{array} \quad\text{and}\quad \begin{array}{c} \text{L} \\ \to\text{L} \\ \text{L} \end{array}$$ where the arrow denotes the point in the sequence at which the guess is made, are examples of multiple-determination for (± 1) guesses.

$$\text{Similarly} \begin{array}{c} \text{L} \\ \text{G} \\ \to\text{E} \\ \text{P} \\ \text{L} \end{array} \quad\text{or}\quad \begin{array}{c} \text{L} \\ \text{L} \\ \to\text{E} \\ \text{G} \\ \text{L} \end{array} \quad\text{or}\quad \begin{array}{c} \text{L} \\ \text{G} \\ \to\text{L} \\ \text{L} \\ \text{L} \end{array}$$ etc. are cases of multiple-determination for (± 2) guesses.

Confining our attention at present to (± 1) guesses, let us suppose that in a sequence of n guesses there are m of these that are multiply-determined.

Now suppose that in the whole sequence of n trials the number of ($+1$) true cognitions is pn and the number of (-1) true cognitions is qn.

There are three hypotheses to consider :

(*a*) That ($+1$) and (-1) true cognitions are mutually exclusive. That is to say, in cases where it is possible for both a ($+1$) cognition and a (-1) cognition to occur at the same guess, only *one* of these and *not both* can occur.

(*b*) That ($+1$) and (-1) true cognitions are independent of each other. That is to say, when a guess is sandwiched between two cards of the same symbol, there may occur a ($+1$) cognition only or a (-1) cognition only or *both* a ($+1$) and a (-1) cognition.

(*c*) That in the case of the guess sandwiched between two cards of the same symbol, the ($+1$) and (-1) presentations may tend to reinforce each other : so that the chance of (say) a ($+1$) cognition is higher in this case than it would be where the guess lay between two cards of *different* symbols.

Of the two hypotheses (*a*) and (*b*), the second (*b*) is the more natural, but on the other hand hypothesis (*a*) leads to the greater theoretical expectation of (± 1) hits.

Take first hypothesis (*a*).

In the case of the m multiply-determined guesses, the expectation of $(+1)$ true cognitions will be pm and the expectation of (-1) true cognitions is qm. Hence the expected number of (± 1) hits taken together corresponding to these true cognitions will be $2(p+q)m$. Of the remainder the number of (± 1) hits due to *chance* will be $\frac{2}{5}(m - pm - qm)$.

Hence the total expectation of hits (± 1) on the m multiply-determined guesses is

$$2(p+q)m + \tfrac{2}{5}(m - pm + qm) = \frac{2m}{5}[1 + 4(p+q)].$$

Now take hypothesis (*b*).

In the case of a multiply-determined guess, the chance of a $(+1)$ cognition is p and that of a (-1) cognition is q. Hence the chance that *neither* a $(+1)$ *nor* a (-1) cognition will occur is $(1-p)(1-q)$ since the chances are independent.

Clearly then the expected number of (± 1) hits corresponding to *true cognitions* will be

$$2m[1 - (1-p)(1-q)] = 2m[p + q - pq].$$

On the remaining guesses the number of (± 1) hits arising from *chance* will be $\frac{2m}{5}[1 - p - q + pq]$.

Hence the total expectation of (± 1) hits on the m multiply-determined guesses will be $2m[p + q - pq] + \frac{2m}{5}[1 - p - q + pq]$.

$$= \frac{2m}{5}[1 + 4(p+q)] - 4pq].$$

Hence hypothesis (*a*) leads to the greater expectation though in practice the term $\frac{8mpq}{5}$ is usually small, and the difference between the two expectations is not very important.

Hypothesis (*c*).

In order to test whether hypothesis (*c*) is probable, we shall choose the higher of the two expectations to be on the safe side.

The above analysis is more accurate than the method of treatment adopted in the previous paper by S.G.S.[1] but the conclusions arrived at in that paper are quite unaffected.

We shall first apply the method to the work of B.S. with the Agent J.Al. with whom, it will be remembered, (-1) as well as $(+1)$ cognitions were scored.

There were 1440 (± 1) trials. In a random distribution the expected number of M.D. guesses is $\frac{1}{5} \times 60 \times 23 = 276$ (± 1) M.D. trials.

[1] *Proc.* xlvi, 152 ff.

The observed number of (± 1) M.D. trials is 288—in good agreement with expectation.

Now on the 720 ($+1$) trials B.S. scored 203 ($+1$) hits, and 207 (-1) hits on the 720 (-1) trials. (See Table XV, p. 65.)

It follows that there are 73·75 ($+1$) true cognitions and 78·75 (-1) true cognitions.

Hence
$$p = 73{\cdot}75 \div 720 = 0{\cdot}10243$$
$$q = 78{\cdot}75 \div 720 = 0{\cdot}10937.$$

Hence taking hypothesis (*a*) (mutually exclusive case) we find the following expectations and observed values for successful hits (S) and failures (F).

	S	F	Total (± 1) (M.D. trials)
E - - -	106·31	181·69	288
O - - -	142	146	288

Whence, with Yates' correction $\chi^2 = 18{\cdot}36$ and $\chi = 4{\cdot}28$, a highly significant result ($P < 3 \times 10^{-5}$).

Taking hypothesis (*b*) we have $E = 103{\cdot}82$ instead of 106·31 and the result would be still more significant ($\chi = 4{\cdot}62$).

On the other hand the Group 1 Agents [1] with whom B.S. obtained only ($+1$) cognitions do not produce any M.D. effect on (± 1) guesses.

For these Agents we have 12,077 (± 1) trials at "normal" rate. Hence expected number of (± 1) M.D. trials

$$= \frac{12077}{24} \times \frac{23}{5} = 2315$$

while the counted number of such trials is 2276 which is in good agreement with expectation.

No. of ($+1$) true cognitions $= 684$
" (-1) " " $= 4$

Hence
$$p = 684/6039 = 0{\cdot}11326$$
$$q = 4/6038 = 0{\cdot}00066$$

Hence on hypothesis (*a*) we find:

	S	F	Total (± 1) (M.D. trials)
E - - -	662·63	1613·37	2276
O - - -	640	1636	2276

Whence with Yates' correction $\chi^2 = 1{\cdot}04$, a result which has no significance.

We may also apply the method to the results obtained by B.S. with all Agents in 1936.[2]

On hypothesis (*a*) we have

	S	F	Total (± 1) (M.D. trials)
E - - -	59·46	134·54	194
O - - -	90	104	194

Whence $\chi^2 = 21{\cdot}89$ or $\chi = 4{\cdot}68$ with Yates' correction—a highly significant result.

[1] See p. 62 f. [2] *Proc.* xlvi, 190 (Table 14).

Effect of Multiple-Determination on (± 2) *hits at " Rapid " Rate.*

We find equally good evidence that when the experiment is carried out at "rapid" rate with J.Al. as Agent, the (± 2) presentations tend to reinforce each other when the guess is of the multiply-determined type.

In the three experiments with J.Al. at " rapid rate ", we have 23 columns comprising in all $23 \times 23 \times 2 = 1058$ (± 2) trials. As the distribution is random we should expect to find $23 \times 2 \times 21 \times \frac{1}{5} = 193{\cdot}2$ (± 2) guesses of the multiply-determined type, since the maximum number for a single column is 2×21.

The actual observed number of such M.D. trials is 204, which is in good agreement with expectation.

Now on the 529 ($+2$) trials (P) scores 149 ($+2$) hits

On „ 529 (-2) „ „ „ 151 (-2) hits.

Whence, proceeding as in the previous section, we obtain on hypothesis (a) (mutually exclusive case) the following expectations (E), and observed values (O), for (± 2) successful hits (S) and (± 2) failures (F).

	S	F	Total (± 2) (M.D. trials)
E - - - -	74·89	129·11	204
O - - - -	104	100	204

Whence with Yates' correction $\chi^2 = 17{\cdot}27$ or $\chi = 4{\cdot}1$,—a highly significant result in favour of the hypothesis of reinforcement ($P < 4 \times 10^{-5}$).

APPENDIX C

Single Successes and Success Groups

It was the impression of both experimenters while the work was in progress that the successful hits were not randomly distributed in the series but tended to crowd together into runs, as though extra-sensory cognition occurred in short spasms. Subsequent analysis however proved this impression to be a mistaken one.

We have counted for Group 1 Agents (*cf.* pp. 62-63) the numbers of singletons (*i.e.* isolated hits), doubles (*i.e.* runs of 2 successes), " triples ", etc. on the ($+1$) trials at " normal " rate in order to discover if there were any grounds for the above hypothesis of " close packing ". There are none.

We have computed the expectations of " singletons ", " doubles ", etc. from the approximate formulae

$$E_1 = np(1-p)^2 + 2p^2(1-p)$$

$$E_2 = np^2(1-p)^2 - p^2(1-p)(1-3p)$$

$$E_3 = np^3(1-p)^2 - 2p^3(1-p)(1-2p)$$

$$E_4 = np^4(1-p)^2 \text{ approx.}$$

$$E_5 = np^5(1-p)^2 \quad \text{,,} \qquad \text{etc.}$$

using the " observed " value of $p = 1755/6039 = 0{\cdot}29061$ instead of $p = 1/5$.

The results are given in the following table:

(N.B.—Counts have been carried on from the end of one column of 25 to the beginning of the next without any breaks in the sequence.)

TABLE XXX

(Tel.) (Normal Rate) (1755(+1) hits)

Group I: Agents: R.E.; G.A.; K.M.G.(?); C.E.M.J.

Runs of	1	2	3	4	5 or more
Expected -	883·29	256·65	74·58	21·67	8·86
Observed -	884	227	84	26	11

The number of "singletons" is curiously close to the expectation computed by this method, but this is of course a fluke. However, we have also computed the number of singletons from Stevens' formula

$$E_1 = \frac{ab(b-1)}{(a+b-1)(a+b-2)}$$

where a = number of "hits", b = number of failures, so that $a+b=n=6039$ and $a=1755$. The result is $E_1 = 883{\cdot}4$ which is in close agreement with that derived from our method. As all the counts have been carefully checked, the unusually close agreement with the observed number must be considered as an interesting fluke, as already stated. The other categories also provide observed numbers sufficiently close to expectation to dispose of the theory of the crowding of hits.

We have further computed the expected number of "success groups" by Stevens' formula

$$E = \frac{a(b+1)}{a+b} \text{ with variance equal to } \frac{E \times b(a-1)}{(a+b)(a+b-1)} = V.$$

Writing $a=1755$, $b=4284$ in these formulae we obtain $E=1245{\cdot}3$, $V=256{\cdot}62$. Hence Standard Error of E is $\sqrt{256{\cdot}62}=16{\cdot}02$.
But Observed number of "Success Groups" is $1232=O$.
The difference $E-O=13{\cdot}3$, which is less than the Standard Error.

(N.B.—In such a run as $f\overset{1}{s}ff\ \overset{2}{s}sf\ \overset{3}{s}ss\ ff\ \overset{4}{s}sss\ f\overset{5}{s}f$, where s denotes a success and f a failure, there are 5 "success groups", as indicated.

This calculation brought home to us how easy it is to be deceived by impressions resulting from a cursory inspection of the data, especially when one is looking for or hoping for a special effect.

APPENDIX D

1. Distribution of (+1) Hits in Places 1-24

The distribution of the 1755 (+1) hits for Group I Agents (*cf.* pp. 62-63) in the "telepathy" experiments at "normal" rate is given in the following table:

TABLE XXXI

(+1) hits (Tel.) ("Normal" Rate) (PRN) and (Counters)

Agents: R.E.; G.A.; K.M.G.(?); C.E.M.J.

Place

1	2	3	4	5	6	7	8	9	10	11	12
53	76	59	77	66	57	74	81	80	79	85	74
13	14	15	16	17	18	19	20	21	22	23	24
80	77	58	84	82	68	81	73	89	66	70	66

On the assumption of a random distribution of the (+1) successes over the 24 places in the column, the mean expectation for each place is 1755/24 $=73\cdot125$. We have $\chi^2=29\cdot60$ which with 23 degrees of freedom gives $P=0\cdot17$, a value which is *not* abnormal.

2. *The distribution of* (+2) *hits for all experiments at "Rapid" Rate up to* 5 *June* 1942 *is given in the Table which follows:*

TABLE XXXII

(+2) hits (Tel.) ("Rapid" Rate) (Counters) and (PRN)

Agents: R.E. (Counters) and J.Al. (PRN)

No. of (+2) hits$=343$

Place

1	2	3	4	5	6	7	8	9	10	11	12
15	22	16	15	11	20	17	14	11	10	14	16
13	14	15	16	17	18	19	20	21	22	23	Total
24	11	10	12	17	16	14	12	14	18	14	343

For (+2) hits the expectation for each of the 23 places is $14\cdot91$. We have $\chi^2=19\cdot84$ which with 22 degrees of freedom gives $P=0\cdot6$ nearly,—a result without significance.

APPENDIX E

(Carry-on of (+1) Hits from Column (*a*) to Column (*b*)

The interesting question arises: Is there any carry-over of (+1) hits from call No. 25 of Column (*a*) to call No. 1 of Column (*b*)? We should not expect this to occur, since there is always an interval of at least 6 or 7 seconds between the completion of Column (*a*) and the start of Column (*b*), and we have seen that B.S.'s span of precognition does not reach 5 seconds. Actual counts confirm this expectation.

We have:

TABLE XXXIII

		(+1) Trials	(+1) hits
Group I[1]	(PRN)	93	19
,,	(Counters)	33	5
J.Al.	(PRN)	16	5
	Totals	142	29

Thus out of 142 "carry-over" trials at "normal" rate we have 29 (+1) hits, which is obviously a chance result.

APPENDIX F

List of (+1) Scores at (variable) "Normal" Rates

Times of calling were not recorded till 21 March 1941 and even after that date odd columns were sometimes left untimed.

The following table gives "telepathy" scores at each recorded time for a column of 25 calls. We have included Group I Agents and the Agent J.Al., but excluded Group II Agents.[2] It will be seen that with the successful Agents significant (+1) scores were obtained at practically all rates lying between 80 seconds and 50 seconds for a column of 25 calls.

It will be noticed that there are more scores at 60, 65, 70, 75 seconds than at the intermediate rates. This is due to the fact that on many occasions (EA) deliberately aimed at finishing the column in a number of seconds which was an exact multiple of 5. But it is also probable that (EA) sometimes "rounded off" a time of say 74 or 76 seconds and made it 75.

TABLE XXXIV

(Tel.) ("Normal" Rate) Agents: R.E.; G.A.; K.M.G.(?); C.E.M.J; and J.Al.

Time in Seconds for 25 calls	(+1) Scores
95	7
85	7
80	8, 8, 10, 7

[1] See pp. 62-63. [2] Unsuccessful Agents, see p. 62.

77	8, 9
76	9
75	9, 7, 6, 8, 5, 8, 6, 8, 7, 9, 6, 6, 7, 6
72	5, 7, 8, 8, 8
71	8
70	8, 8, 6, 6, 10, 6, 10, 10, 8, 5, 4, 8, 8, 5, 3, 9, 8
68	14, 7
67	5
66	3, 8, 7, 10, 1
65	4, 7, 6, 6, 10, 4, 10, 8, 6, 8, 11, 10, 3, 4, 5, 7, 7, 6, 4, 8, 8
64	8, 9, 11
63	4, 4, 6, 7
62	4, 5, 5, 3, 5, 7, 6, 9
61	3, 4, 4, 7, 9, 6, 5, 12, 6, 7
60	11, 12, 4, 5, 11, 5, 7, 8, 7, 10, 5, 10, 5, 6, 7, 8, 11, 3, 6, 4, 11, 8, 6
59	8, 7, 4, 8, 8
58	4, 6, 5, 9, 8
57	10, 9, 4, 3, 7, 12, 6, 8, 5, 6, 7, 4, 8
56	6, 7, 3, 6, 6, 4
55	2, 7, 7, 4, 4, 6, 6, 8, 8, 8, 4, 7
54	8, 8, 5, 4
53	12, 8, 8, 7
52	8, 7, 4, 4, 6, 9
51	7
50	7, 9, 7, 9, 8, 4, 2
48	2, 5
47	5, 7, 8
46	3

Taking class intervals of 5 seconds, and putting half the total score at, say, 55 seconds into the class 50-55 and the other half in the class 55-60, and doing the same for the upper and lower rates (taken as 80 and 50 seconds respectively), we have the following Table :

TABLE XXXV

Rate	(+1) Score	No. of Scores	Mean score for 24
75-80	91·5	12·0	7·62
70-75	154·0	21·5	7·16
65-70	187·0	27·0	6·93
60-65	312·0	47·0	6·64
55-60	308·5	46·5	6·63
50-55	163·5	24·5	6·67

It must however be pointed out that the different scoring rates are *not* randomly distributed among the experiments. As a matter of fact the higher times (*i.e.* slower rates) occur for the most part in the earlier experiments with R.E. as Agent while S.G.S. was (EA), whereas the rates round about 50-60 seconds occur as a rule after K.M.G. became (EA). All that can be inferred safely is that varying the rate between 50 and 80 seconds does not appear to inhibit significant scoring of (+1) hits.

(2) *Timing at "Rapid" Rate.*

At "rapid" rate there is very little variation in the time for a column of 25 calls, since in this case the experiment was carried out as fast as the apparatus permitted.

Using (Counters) and with R.E. as Agent, the mean time for a column is 34·41 seconds; while with J.Al. as Agent and using a modified form of (PRN), the mean time is 33·12 seconds.

For both Agents it is 34·00 seconds and the Standard Error for a column is only 2·84 seconds.

APPENDIX G

Additional Data

1. *Distribution of number presentations over the places 1-5 of cards in the box in all experiments at "normal" rate between 24 January* 1941 and 16 *January* 1942 *inclusive.*

In the Table given below we have counted 25 presentations to each column, and "clairvoyance" as well as "telepathy" experiments are included. Experiments after 16 January 1942, with J.Al. as Agent, are not included, as these computations were completed before he took part in the experiments.

TABLE XXXVI

Places - - -	1	2	3	4	5	Totals
(PRN) - - -	1439	1462	1494	1414	1366	7175
Counters - -	472	455	414	460	449	2250
Totals - - -	1911	1917	1908	1874	1815	9425

For (PRN) we have: expectations for each place = 1435 whence with

$$n=4,\ \chi^2=6{\cdot}45\ [0{\cdot}2>P>0{\cdot}1]$$

For (Counters): expectation for each place = 450 whence with

$$n=4,\ \chi^2=4{\cdot}07\ [0{\cdot}5>P>0{\cdot}3]$$

For (PRN) + (Counters): expectation for each place = 1885 whence with

$$n=4,\ \chi^2=3{\cdot}76\ [{\cdot}5>P>{\cdot}3]$$

The distributions of presentations at "normal" rate agree substantially with expectation.

2. *Variance from Observed Mean.*

For Group I Agents,[1] ("Telepathy") (PRN) at "Normal" Rate (+1) guesses:

We have:	No. of sets of 24 (+1) trials	= 185·875
	Mean score for 24 trials	= 7·080
	Observed variance from this mean	= 5·462

Expected variance on assumption of a binomial distribution in which $p=0{\cdot}29500$, $q=0{\cdot}70500=24\times pq=4{\cdot}9914$.

The Standard Error of the above expected variance = 0·514 (*cf. Fisher*: "*Statistical Methods for Research Workers*", p. 73).

[1] See pp. 62-63.

Hence the observed variance is not inconsistent with the assumption of a binomial distribution.

For (COUNTERS) at " Normal " Rate (+1) trials (Agent = R.E.) we have :

No. of sets of 24 (+1) trials $=65{\cdot}75$
Mean score for 24 trials $=\bar{x}=6{\cdot}677$
Observed variance from this mean $=8{\cdot}0278$

Expected variance on assumption of a binomial distribution (in which $p=0{\cdot}27820$, $q=0{\cdot}72180$) $=24pq=4{\cdot}8193$

Standard Error of the above expected variance $=0{\cdot}838$

Whence $OV-EV=8{\cdot}0278-4{\cdot}8193=+3{\cdot}2085$—a difference which is nearly 4 times the Standard Error.

The scores of the (COUNTERS) experiments are clearly not consistent with the assumption of a binomial distribution, but the reason for this abnormal " scatter " would seem to be that on the dates 13 June 1941 and 3 January 1942 there were large batches of trials which gave only chance results. That is to say the material is not homogeneous.

The difference between the two variances 8·0278 (COUNTERS) and 5·462 (PRN) is just (dubiously) significant. With the usual notation we have

$$z=1{\cdot}1513\,(\log_{10}8{\cdot}028-\log_{10}5{\cdot}462)=0{\cdot}1925$$

With $n_1=64{\cdot}75$; $n_2=184{\cdot}875$, we find by the Interpolation method (*cf.* Fisher, *op. cit.* p. 225) 5% point for $z=0{\cdot}1610$

1% " " $=0{\cdot}2269$

Hence there exists a (not very) significant difference between the two variances.

(3) *Variance from Theoretical Mean.*

It was advocated by Kellogg (" New Evidence (?) for Extra-Sensory Perception ", The *Scientific Monthly*, October 1937, vol. XLV, pp. 331-41)—on grounds that to us appear to be unjustified—that in working out the critical ratio the *observed* variance from the theoretical mean should be substituted for the *theoretical* variance.

But even if we do this, our results will still be very highly significant. We have for Group I Agents[1] (Telepathy) (PRN) (" Normal " Rate).

Actual mean of hits for 24 (+1) trials	$=7{\cdot}080$
Theoretical " " "	$=4{\cdot}800$
Difference	$=+2{\cdot}280$

Observed variance from $4{\cdot}8=10{\cdot}688$

Hence Standard Error for mean $=\sqrt{10{\cdot}688}/\sqrt{185{\cdot}875}=0{\cdot}240$ and $\chi=9{\cdot}5$

Similarly for (COUNTERS) at " Normal " Rate, we have

Actual mean of hits for 24 (+1) trials	$=6{\cdot}677$
Theoretical " " "	$=4{\cdot}800$
Difference	$=1{\cdot}877$

Observed variance from $4{\cdot}8=11{\cdot}606$

Hence Standard Error for mean $=\sqrt{11{\cdot}606}/\sqrt{65{\cdot}75}=0{\cdot}420$ and $\chi=4{\cdot}47$.

[1] See pp. 62-63.

APPENDIX H

Two Preliminary Experiments

Two experiments were carried out on first contacting B.S. again, before the commencement of the series proper. The conditions were far from ideal, the sittings being merely for the sake of preliminary exploration. We record them however in this Appendix, in order that no single experiment, from first to last, should be left unrecorded. We shall refer to them as Experiment A and Experiment B.

Experiment A 31 Dec. 1940; S.G.S. = (EA); R.E. = (A); B.S. = (P).
Experiment B 17 Jan. 1941; S.G.S. = (EA); R.E. = (A); B.S. = (P).

Conditions.—These were to all intents and purposes identical at each sitting. Doors D_2 and D_3[1] were kept closed during progress of experiments. B.S. sat alone in ante-room in front of fireplace recording his guesses. His Lady-Assistant was also in ante-room at work at work-table (B)[1] and did not enter Studio while experiments were in progress. (EA) and (A) sat at card-table in Studio. Five plain white cards, shuffled by (A), were spread out, face downwards, in front of the Agent, on the faces of which had been pasted strips of differently coloured paper—brown, blue, green, yellow, red—the other sides of the cards remaining blank. S.G.S. referred to previously prepared sheets of random digits 1-5; he pointed with a pencil to the card before Agent corresponding in turn to the figures on his list; the card was lifted and looked at by (A) and dropped back into place. (EA) called " Are you ready? "—" First Card "—" Next "—" Next " to (P) in the next room, who recorded his guess on the sheets with which he had been provided and called back " Right " each time after doing so. At the end of every 5 guesses he added " 5 ", " 10 ", " 15 ", " 20 ", as the case might be, to ensure that he was keeping in step with (EA). At the twenty-fifth call (EA) shouted " Last guess ". There was then an interval of six or seven seconds before (EA) called " Second column. Are you ready? First card—Next—Next " etc. The code for (A)'s cards was recorded at end of 50 guesses, and checking up of B.S.'s guesses was also carried out after each sheet of 50 guesses. The cards were then re-shuffled by (A) and the next sheet proceeded with in the same way. At the end of the fifth and last sheet, signatures of S.G.S., B.S. and R.E. were entered on each sheet. No duplicate records were made on either of these first two occasions. Scoring sheets were taken home by S.G.S. and re-checked.

RESULTS OF EXPERIMENT A

	Sheet	1a	1b	2a	2b	3a	3b	4a	4b	5a	5b	Totals	χ
(Precog.)	(+1)	7	3	2	8	8	7	4	6	9	8	62	+2·26
(Direct)	(0)	5	8	7	3	9	6	6	7	6	4	61	+1·74
(Postcog.)	(−1)	4	2	2	5	3	6	7	1	5	6	41	−1·13

Note.—S.G.S. did not trouble to evaluate experiments A and B by Stevens' method. As there are 10 columns, the number of precognitive trials is $10 \times 24 = 240$, and the expected number of precognitive $(+1)$ hits is $\frac{1}{5}$ of $240 = 48$. The actual number of $(+1)$ hits in experiment A being 62,

[1] See Plate I.

we have a positive deviation of $+14$ which, compared with a Standard Error of $6{\cdot}197$, gives $\chi = 2{\cdot}26$ with $P = {\cdot}02$. As however this result is chosen as the best of the three scores $(+1)$, (0), and (-1), the chance of getting at least one of the three scores with a deviation (either + or −) of this magnitude is approximately $3 \times {\cdot}02 = {\cdot}06$. The result therefore can scarcely be regarded as significant.

RESULTS OF EXPERIMENT B

	Sheet	1a	1b	2a	2b	3a	3b	4a	4b	5a	5b	Totals	χ
(Precog.)	(+1)	2	5	6	6	9	11	6	7	3	4	59	+1·77
(Direct)	(0)	4	4	8	6	10	7	2	6	7	7	61	+1·74
(Postcog.)	(−1)	4	6	3	5	5	7	8	4	6	5	53	+0·81

Note.—The results in Experiment B on Precognitive (+1) and Direct (0) hits are not separately significant though they both point towards significance.

If, however, we combine the results of the two experiments A and B, we obtain

		Trials	Hits	Deviation	χ
(Precog.)	(+1) - - -	480	121	+25	+2·85
(Direct)	(0) - - -	500	122	+22	+2·45
(Postcog.)	(−1) - - -	480	94	− 2	−0·23

The results are seen to be significant on both (+1) (Precognitive) hits and on (0) (Direct) hits, but the (−1) (Postcognitive) score can be ascribed to chance. As has been recorded in the Report, B.S. has never been able to score any significant postcognitive successes while working with R.E. as Agent.

Times were not recorded for either experiment A or B, but the average interval between successive calls was probably within the limits of the " normal " rate : *i.e.* between 2 and 4 seconds.

The defects of the above-mentioned technique are obvious. No screen separated (A) from (EA) ; (EA) might catch a glimpse of the cards as they were lifted by (A) and therefore act as unconscious Agent in addition to (A) ; having seen the card, it would become possible for him to convey a code to (*P*) by inflection of the voice in making the calls. (*P*) was not controlled in any way and his recording of guesses was not checked. After these two preliminary experiments and at the commencement of the series proper, due precautions were taken to eliminate all such obvious objections. As recorded, animal picture cards were used in future and the colour-cards discarded, B.S. having told us he found colours confusing and that he felt he would do better with pictures of concrete objects.

APPENDIX I

Persons who witnessed and took part in the experiments (see *The Chronicle*)

Dr H. Godwin Baynes, 11 Mansfield Street, London, W. 1 and c/o S.P.R., 31 Tavistock Square, London, W.C. 1. Sitting No. 18.

H. A. Berens, Esq., 42 Mount Park Crescent, Ealing, London, W. 5. Sitting No. 13.

Sir Ernest Bennett, M.P., c/o S.P.R., 31 Tavistock Square, W.C. 1. Sitting No. 27.

H. Chibbett, Esq., 4 Palace Road, Bowes Park, London, N. 11. Sitting Nos. 3, 4, 14, 30.

Miss Joyce Fairbairn, B.Sc., 69 Carlton Avenue East, Wembley, Mdx. Sitting No. 39.

Mrs Oliver Gatty, c/o S.P.R., 31 Tavistock Square, W.C.1. Sitting No. 8.

Miss Ina Jephson, c/o S.P.R., 31 Tavistock Square, W.C. 1. Sitting No. 8.

Dr C. E. M. Joad, Birkbeck College, University of London. Sitting Nos. 23, 26.

Miss Kennedy (address not known). Sitting No. 30.

The Hon. Mrs Alfred Lyttelton, G.B.E., c/o S.P.R., 31 Tavistock Square, W.C. 1. Sitting No. 7.

C. A. Mace, Esq., M.A., Reader in Psychology, University of London, and c/o S.P.R., 31 Tavistock Square, W.C. 1. Sitting No. 28.

R. G. Medhurst, Esq., B.Sc., c/o S.P.R., 31 Tavistock Square, W.C. 1. Sitting Nos. 31, 32, 33, 39.

Denys Parsons, Esq., M.Sc., c/o S.P.R., 31 Tavistock Square, W.C. 1. Sitting No. 39.

Professor H. Habberley Price, New College, Oxford, and c/o S.P.R., 31 Tavistock Square, W.C. 1. Sitting No. 17.

Kenneth Richmond, Esq., c/o S.P.R., 31 Tavistock Square, W.C. 1. Sitting Nos. 6, 9.

Mrs Kenneth Richmond, c/o S.P.R., 31 Tavistock Square, W.C. 1. Sitting No. 10.

L. A. Rozelaar, Esq., M.A., Queen Mary College, London University. Sitting No. 38.

D. J. West, Esq., c/o S.P.R., 31 Tavistock Square, W.C. 1. Sitting No. 40.

B. P. Wiesner, Esq., D.Sc., Ph.D., 9 Weymouth Street, W. 1, and c/o S.P.R., 31 Tavistock Square, W.C. 1. Sitting Nos. 11, 13, 14, 16, 30.

Mrs Woollard, c/o S.P.R., 31 Tavistock Square, W.C. 1. Sitting Nos. 21, 23, 24, 26.

Mrs Wykeham-Martin, 2 Ashburn Gardens, London, S.W. 7. Sitting No. 35.

Persons who prepared sheets of random figures for use in the experiments. (*cf.* pp. 83-84)

C. U. Blascheck, Esq., Clare College, Cambridge.

Gerhard Wassermann, Esq., B.Sc., Queen Mary College, London University and 15 Victoria Park, Cambridge.

APPENDIX J

List of Tables

Note: Principal Agents = R.E., G.A., and J.Al (results highly significant). More or less superficial tests (seldom exceeding 150 calls) were made with ten other Agents (results negative).

[1] Group I comprises all "Telepathy" experiments at "normal" rate with Agents R.E. and G.A., plus two other short scores by K.M.G.(?) and C.E.M.J. (see p. 62).

APPENDIX K

INDEX of main subjects discussed in Introduction and Report (Part I).

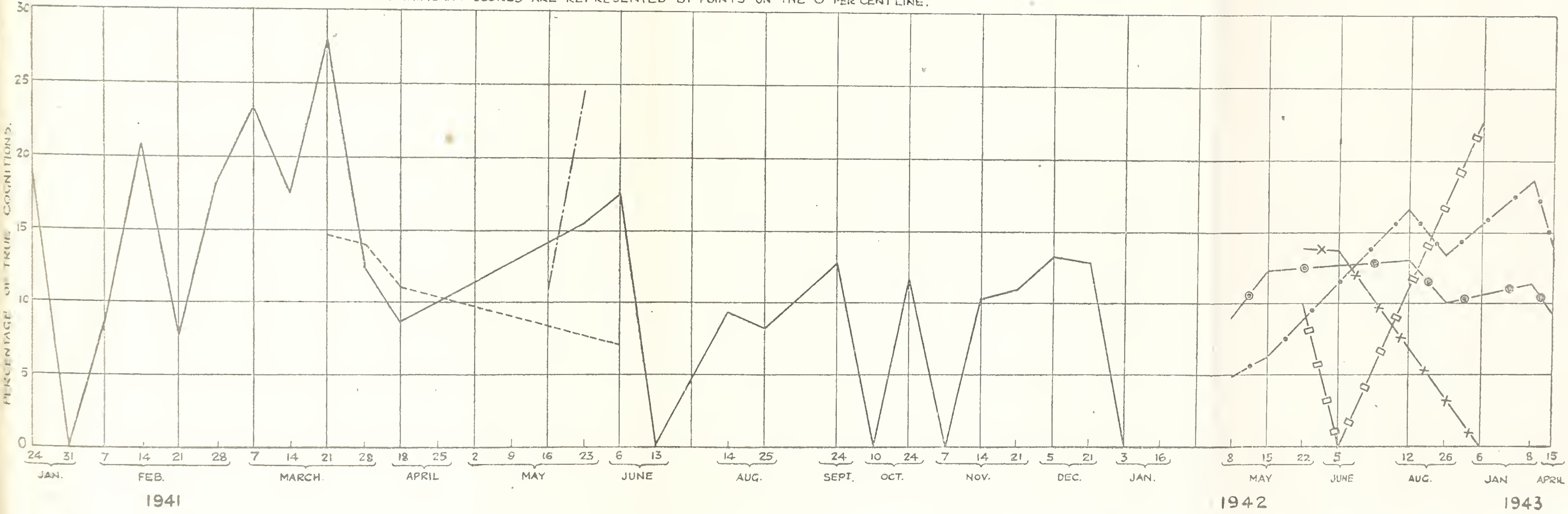
APPENDIX L

GRAPHS SHOWING PERCENTAGES OF TRUE COGNITIONS ON DIFFERENT DATES.

EXPLANATIONS:–

AGENT (R.E.)	————	= NORMAL RATE	(+1)	COGNITIONS.
	- - - - - -	= RAPID RATE	(+2)	COGNITIONS.
AGENT (G.A.)	—·—·—·—	= NORMAL RATE	(+1)	COGNITIONS.
AGENT (J.A.L)	—•——•—	= NORMAL RATE	(+1)	COGNITIONS.
	—⊙——⊙—	= NORMAL RATE	(−1)	COGNITIONS.
	—X——X—	= RAPID RATE	(+2)	COGNITIONS.
	—▯——▯—	= RAPID RATE	(−2)	COGNITIONS.

NON-SIGNIFICANT SCORES ARE REPRESENTED BY POINTS ON THE 0 PER CENT LINE.



OBITUARY

Mr H. F. Saltmarsh

The death of Mr H. F. Saltmarsh on 24 February 1943, deprived the S.P.R. of a most loyal and valuable member who had not only made important contributions to the *Proceedings* but had also devoted much time and trouble and business ability to the financial affairs of the Society.

Saltmarsh was born at Highgate on 16 July 1881. As a young man he was engaged in a business connected with the shipping of cargoes to and from the Baltic, South American and other ports. He was compelled by ill-health to retire at an early age, and he was a semi-invalid for the rest of his life; but he derived considerable benefit from the treatment of an unorthodox medical practitioner whose death some few years ago was a serious misfortune for him. Saltmarsh married in 1904, and he and his wife lived for many years at Lynton. Mrs Saltmarsh is a keen musician, whilst Saltmarsh, to his great regret, had little natural ear for music; but he learned through her to love the works of certain composers, in particular her own favourites, Beethoven, Chopin and Grieg. He had a strong love of nature, for wild flowers and for birds and their song; and he and his wife were able to indulge this freely by driving about the beautiful country near Lynton in their little car. Before the war it was an annual pleasure of the present writer, when visiting Mr and Mrs Hugo Mallet at their house near Lynton, to meet the Saltmarshes and to exchange ideas with them on congenial topics.

Saltmarsh was a man of wide interests and varied accomplishments, both mental and bodily; and this, combined with his training in business and his experience of men and things, made him an exceptionally complete and balanced person. He was a skilful worker with his hands, constructing in his workshop all kinds of useful and ingenious articles for the house and doing most of the repairs which lesser men have to employ tradesmen to do for them. He was a skilled and courageous sailor and an enthusiastic fisherman, and it was remarkable that a man of his delicate health could bear the hardships and dangers to which he freely exposed himself. He not only sailed boats, but also built them; and the last of three little dinghies which he constructed is still in use in Lynmouth harbour.

Saltmarsh had a high sense of his duty towards his neighbour. He might have been excused if he had made his ill-health a pretext for retiring into his armchair, but instead he gave unstintingly his time and his financial experience to the service of the public and of individuals in Lynton and the neighbourhood. In one case, which happened to come to the present writer's notice, Saltmarsh took great pains to extricate from its difficulties and to set upon its legs a tea-shop which had good possibilities but was in a poor way when he took charge. Anyone who worked with Saltmarsh soon recognised his business acumen, his thoroughness, and his extreme conscientiousness and high standards of rectitude.

In Psychical Research he showed all the qualities which marked his practical activities. But here there entered another factor also, viz., his interest in philosophical questions. As a young man he had made a considerable study of theosophical literature, and in later life he studied and enjoyed the more austere reflexions of contemporary critical philosophers. Two philosophical problems interested him particularly and were closely connected with his work in Psychical Research, viz., the nature of the human self and personal identity, and the nature of time and causation. The former led him to ask insistently : " What precisely is meant by the proposition that so-and-so has survived the death of his body ? " The latter became pressing in reference to his studies in the evidence for Precognition. On both topics he made suggestions which were ingenious and stimulating, even if they cannot be accepted in the precise form in which they are offered.

Saltmarsh's two most important contributions to Psychical Research are his *Report on the Investigation of some Sittings with Mrs Warren Elliott* (*Proceedings*, Vol. XXXIX) and *Report on Cases of Apparent Precognition* (*Proceedings*, Vol. XLII). Both are models of their kind. In the Mrs Warren Elliott investigation two classes of sitting were held, viz., those at which the sitter was present in person (" Present Sitter ") and those in which he was represented only by a " relic " sent long beforehand in a sealed package to the S.P.R. rooms and chosen at random by the note-taker on the day of the sitting ("Absent Sitter "). The records of each sitting were annotated both by the sitter or the owner of the relic, as the case might be, and by strangers who were asked to treat them as if they themselves had been the sitters or the relic-owners (" Pseudo-Sitters "). Saltmarsh then scored, in accordance with a system which he explains, each record (i) as annotated by the real sitter, present or absent, and (ii) the same record as annotated by one or more pseudo-sitters. In this way he tried to test whether the scores where significantly higher in the real sittings than in the pseudo-sittings. Then he considered whether there was any significant difference between the scores of real present-sitter and real absent-sitter sittings ; between the amount of correct post-mortem and ante-mortem statements ; and so on. Lastly, on the basis of his findings, he draws some tentative conclusions about the psychological mechanism of the mediumistic trance, about the functions of " relics ", and about various alternative theories which have been proposed to account for ostensible communications from the dead.

In the same volume (XXXIX) of the *Proceedings* there is a short but important joint paper by Saltmarsh and Mr Soal, entitled *A Method of Estimating the Supernormal Content of Mediumistic Communications*. The essential problem is to devise a method of scoring such that true statements shall get positive marks which increase with the improbability of their being true by chance and false statements get negative marks which increase with the improbability of their being false by chance ; such that the marks for a number of statements are additive ; such that the most probable aggregate score for a large number of purely random statements is zero ; and such that the standard deviation of various possible aggregate scores about the most probable value is calculable. Mr Soal devised such a method and it was submitted to Professor Fisher, who introduced certain modifications. Saltmarsh applied it to score the results of certain of the Warren Elliott sittings. Saltmarsh had to make assumptions about the antecedent probabilities of statements of various kinds being true. Since then attempts have been made on a large scale to determine empirical frequencies and substitute them for such conjectural antecedent probabilities.

The *Report on Cases of Apparent Precognition* in Vol. XLII is much the best survey of the material which exists in English. It is admirably arranged, subdivided, and classified. Saltmarsh was compelled to recognise the occurrence of non-inferential precognition, and he propounds a theory of an extended sub-liminal specious present to account for it. He also considers in some detail the question whether the occurrence of veridical non-inferential precognition is compatible with free-will. In this connexion he uses a number of ingenious mechanical analogies. If, as appears to the present writer, Saltmarsh did not carry heavy enough metaphysical guns to attack with much hope of success the hardest of all philosophical problems, he made a gallant attempt and he failed where no-one else has come within sight of victory.

The cream of this paper is skimmed and served up in the very useful little book on Precognition which Saltmarsh contributed to Messrs. Bell's admirable series of monographs on Psychical Research. He is responsible for another book in this series, viz., that on Cross-Correspondence. This is a triumphant exhibition of Saltmarsh's powers of extracting the essence of a complicated mass of material and presenting it in a clear and agreeable form to the intelligent layman.

The three remaining papers in the *Proceedings* are of slighter importance and all bear on the same topic. They are *Is Proof of Survival possible?* (Vol. XL) ; *Some Comments on Mr Tyrrell's Paper on Individuality* (Vol. XLIV) ; and *Ambiguity in the Question of Survival* (Vol. XLVI). These are all theoretical papers, dealing with the vital question of what is meant by the survival of a personality and what are the criteria by which to judge whether such and such empirical facts are evidence for survival.

The above account of his life and work should suffice to show how much the S.P.R. and Psychical Research have lost by Saltmarsh's death. Those who had the privilege of knowing him personally will feel that they have lost a friend who was a firm rock of courage and honour and good sense, and they will be inspired and somewhat abashed at the thought of what he accomplished in the face of constant ill-health and frequent severe discomfort. C. D. Broad.

Mr Saltmarsh joined the Society in 1921 and lost little time in taking an active part in our work. In 1931 he became a member of Council and in February, 1939, on the retirement of Mr Piddington, he was appointed a trustee of the Research Endowment Fund, and kindly consented to serve as the "Acting Trustee", making himself specially responsible for looking after the investments of the Fund and keeping the Fund accounts. When on the outbreak of War our Hon. Treasurer, Admiral Strutt, went on active service, Mr Saltmarsh also took over the work of acting Treasurer of the Society. In both capacities his business training, his exact habit of mind, and his keen and prompt attention to detail were of the greatest service to the Society. He was, in fact, from 1939 until his death the key man of the Society's administration. All who have been during these years concerned with the administrative side of the Society's work have very special reasons for feeling the loss of so highly endowed a colleague.

Professor Broad has referred to his high sense of duty towards his neighbour and his unsparing efforts to be of practical help. He found opportunities to help in many directions, not least in drawing on his great experience in psychical research for the benefit of those who through bereavement or other causes stood in need of sympathetic advice. When discussing in our *Journal* and *Proceedings* the question of human survival he very properly wrote with the detachment of a philosopher and scientific inquirer, but this was not in any degree incompatible with a sympathetic understanding of the personal side of the problems he had constantly in mind.

I take the liberty of quoting from two very sympathetic letters which he wrote to a friend bereaved by the war. " I suggest that we are wrong in identifying the ' me which I now recognise as myself ' as the total true me. The first of these two ' me's '—call it for brevity, the superficial me—is a composite being, largely composed of elements derived from the physical body ; it is ephemeral seeing that the compound will be broken up at death and one set of elements, viz., the physical, dispersed. . . . I do not believe in the survival of the superficial me, nor do I desire it. . . . In honesty, I must confess that I am not completely convinced that there is any survival at all. I am inclined to think there is, but am not quite sure."

" If by what little I have been able to contribute to psychical research I have added anything towards arriving at a solution—and by solution I mean not only the optimistic but also the pessimistic view—I am satisfied, but it is necessary to the value of any contribution to knowledge that it should be unbiassed.

" I hate dogmatism but I am so uncertain of the correctness of my opinions that I would never seek to persuade where I cannot convince."

W. H. S.

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