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THE AMERICAN SOCIETY FOR PSYCHICAL RESEARCH
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PURPOSE AND SCOPE OF THE SOCIETY

1. The investigation of claims of telepathy, clairvoyance, veridical hallucinations and dreams, psychometry, precognition, dowsing, and other forms of supernormal cognition; of claims of supernormal physical phenomena, such as raps, telekinesis, materialization, levitation, fire-immunity, poltergeists; the study of automatic writing, trance speech, hypnotism, alterations of personality, and other subconscious processes: in short, all types of the phenomena called psychic, mediumistic, supernormal, parapsychological and metapsychic, together with the bordering subjects.

2. The collection, classification, study and publication of reports dealing with the above phenomena. Readers are asked to report incidents and cases. Names must be given, but on request will be treated as confidential.

3. The maintenance of a library on psychical research and related subjects. Contributions of books and periodical files will be welcomed.

4. Co-operating in the above tasks with qualified individuals and groups who will report their work to the Society.

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Members, who receive the Proceedings and the Journal, pay an annual fee of $10. (One may become a Life Member or endow a Memorial Membership on payment of $200.) *Associates,* who receive the Journal only, pay an annual fee of $5. (Life Associate membership, $100.) *Fellows,* who receive all publications of the Society, pay an annual fee of $25. (Life Fellowship, $500.) *Patrons and Founders*: For those who wish to make a still larger contribution to the Society's work, these classes are open at $1000 and $5000, respectively.

It is to be remembered that membership in a scientific society means more than merely a subscription to its publications. The work must be carried on largely through the income from membership fees. Therefore members, old and new, are urged to make their membership class as high as they feel they can. *If a comparatively small proportion of the present members went one class higher, the money available for research would be more than doubled.*

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Notice of Annual Meeting of the Voting Members of the American Society for Psychical Research, Inc.

The Annual Meeting of the Voting Members of the American Society for Psychical Research, Inc., will be held at the office of the Society, 40 East 34th Street, Room 916, Borough of Manhattan, City of New York, on Tuesday, January 28, 1947, at 4:00 o'clock in the afternoon, for the election of six Trustees and for the transaction of such other business as may properly come before the meeting.

Lydia W. Allison, *Secretary*

Resolutions and Membership Lectures

At a Special Meeting of the Board of Trustees, held on Tuesday, October 1, 1946, at the office of the Society, 40 East 34th Street, New York 16, N. Y., Dr. George H. Hyslop, the President, presented the resignation of Mr. Lawson Purdy, Treasurer, to take effect on that day, October 1, 1946. The following resolution was unanimously adopted:

Resolved, that the Board of Trustees of the American Society for Psychical Research, Inc. express its regret at the resignation of Mr. Lawson Purdy as Treasurer, and its appreciation for the long and faithful service he has given the Society, and that this resolution appear in the next number of the JOURNAL.

We have also to announce the resignation of Miss Adele Wellman, Executive Secretary of the Society since 1930, who has accepted a post in another field. At the Regular Meeting of the Board of Trustees, held on Tuesday, September 24, 1946, at the office of the Society, the following resolution was unanimously adopted:

Resolved, that in accepting Miss Adele Wellman's resignation as Executive Secretary, the Trustees of the American Society for Psychical Research, Inc. express their gratitude for her long years of intelligent and faithful service, and wish her every success in her new opportunity.

The first Lecture and Tea of the current season was held at the rooms of the Society on November 20, 1946. Miss Gertrude Odgen Tubby, for many years Secretary of the Society, was the speaker. Her subject was "Psychics and Psychical Research in the United States." The lecture was followed by a discussion period in which a number of Members and their guests took part.

The Committee wishes to notify Members that a list of books relating to the topics of this season's lectures has been prepared and is posted on the bulletin board.

Notices of forthcoming monthly lectures, together with the names and topics of speakers, will be sent to Members ten days in advance.

Personality Appraisal and the Paranormal

GARDNER MURPHY

Many of the problems investigated in psychical research relate to *conditions which favor the paranormal,* with no direct interest in individual differences. When, for example, Pratt and Woodruff[1] report that *novelty* of stimulus material appears to favor good scoring —the scores declining as the newness wears off—they properly mass the data from all subjects and state their findings in terms of a generalization. When both the Duke University experimenters[2] and the A.S.P.R. experimenters[3] find evidence of "decline effects" (Midas Touch) in ESP results, they report on the entire work of all participating subjects. Indeed, individual scores in such experiments are not as a rule independently significant. The whole approach is different when the focus of attention is upon the individual. It is safe to say that nearly everything that is known concerning paranormal gifts from the standpoint of experimental work is in terms of general working principles, rather than in terms of individual differences.

With spontaneous cases, and with special sensitives, however, the case is somewhat different. To be sure, some attempts have been made to discover the general psychology of readiness for mediumship and for spontaneous experiences—indeed, we have made our own attempts along this line.[4] But in both cases there must of course be careful preliminary studies of individuals if much is to be accomplished: in some well-known instances, such as Mrs. Sidgwick's study of Mrs. Piper[5] and Gerald Balfour's study of Mrs. Willett,[6] attention is focused upon personal factors, and, indeed, upon those personal *idiosyncrasies* which are uniquely present in an individual.

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[1] Pratt, J. G., and Woodruff, J. L., "Size of Stimulus Symbols in Extra-Sensory Perception," *Journal of Parapsychology,* Vol. 3, December, 1939, pp. 121-158.

[2] See *Extra-Sensory Perception After Sixty Years,* by Pratt, J. G., *et al,* Henry Holt and Company, New York, 1940, pp. 284 ff.

[3] Taves, Ernest, and Dale, L. A., "The Midas Touch in Psychical Research," JOURNAL A.S.P.R., Vol. XXXVII, April, 1943, pp. 57-83.

[4] Murphy, Gardner, "Psychical Phenomena and Human Needs," JOURNAL A.S.P.R., Vol. XXXVII, October, 1943, pp. 163-191.

[5] Sidgwick, Mrs. Henry, "A Contribution to the Study of the Psychology of Mrs. Piper's Trance Phenomena," *Proc.* S.P.R., Vol. XXVIII (1915), pp. 1-652.

[6] Balfour, Gerald W., "A Study of the Psychological Aspects of Mrs. Willett's Mediumship, and of the Statements of the Communicators Concerning Process," *Proc.* S.P.R., Vol. XLIII (1935), pp. 43-318.

Today we are witnessing a rapid and striking shift in research interest, in the fact that this concern for individual personality, long restricted to special cases, is spreading rapidly into experimental studies with groups, so that the question about "favorable or unfavorable conditions for the paranormal" is supplemented by the question of "conditions favorable or unfavorable for this or that *kind of person,*" or indeed, "conditions favorable for *Miss Smith,* unfavorable for *Mr. Jones.*"

This trend is, of course, completely dependent upon the development of adequate methods of appraising personalities. It does not help us to say that cheerful people succeed and gloomy people fail in a task, unless we have a fairly objective and dependable method of determining who is cheerful and who is gloomy. Our subject may have gotten out of the wrong side of the bed today; or, indeed, your standards of what it takes to be "cheerful" may be different from mine. Psychologists and psychiatrists concerned with the assessment of personality have learned this the hard way, and in the last two decades have found that it is imperative to develop objective and dependable tests for personality assessment. They require that a test be "reliable" (by this they mean that it must give comparable results when applied on different occasions), and "valid" (that is, it must agree with an independent, objective criterion; for example, if the test purports to test punctuality, its results must agree well with actual records of arrival on time for appointments). Such tests do not achieve the reliability and validity of physical measurements, partly because personality remains, in some degree, hidden and inaccessible to all existing approaches, partly because it has a flexibility or plasticity which makes any rigid or absolute determination meaningless. Such tests have, however, achieved a reliability and validity sufficient to make them useful in psychiatric practice, in industrial personnel work, and in the guidance of college students.

When, therefore, a need is felt for personality appraisal in relation to paranormal abilities, it is to these personality tests that we turn. This does not mean that every personal trait of every person is necessarily better appraised by a test than it could be by some other method (such as the testimony of a friend, for example). It means, rather, that in an experimental study of a group we need a uniform method applicable to all members of the group, and a method whose known reliability and validity can be counted on to prevent arbitrary judgments. There remains a very large place for other methods when unique attributes of a single person are the focus of attention, or when we encounter personality traits for which no suitable test has been devised.

Qualities Calling for Appraisal

What are the sorts of personal qualities which we might want to test in psychical research? In accordance with the theory of the paranormal which we have sketched in recent papers[7] we should want to know *first* about the individual's *need* for paranormal experience in general; and his specific needs for information as to what is happening, or will happen, to himself or to those emotionally close to him. *Secondly,* we should want to inquire about the barriers which he himself is setting up against receipt of paranormal impressions. *Thirdly,* we should be interested in the devices available to him for permitting paranormally received impressions to circumvent barriers and find a means of expression, as in dreams, visions, automatic writing, etc.[8]

Corresponding to these three kinds of attributes we might expect to find usefulness, *first,* in tests that help us to understand the individual's psychological needs, including those unconscious needs about which the subject can tell us nothing; *second,* in tests which show the specific techniques by which the individual keeps himself in ignorance of his own needs and activities; *third,* in tests that throw light on individual proneness to automatisms (*i.e.,* tests of the capacity to carry out sensory or motor acts automatically while giving primary attention to something else). Naturally, the personality tests which have arisen in recent years have not been devised primarily for use in parapsychology. Yet there are tests which, without too much difficulty, may be used to serve all three purposes indicated.

The Available Personality Tests

Three main kinds of tests meet our requirements: questionnaires, behavior tests, and projective tests. The projective tests are by far the most important. The questionnaire, being dependent on the individual's testimony about himself, is seldom satisfactory. C. E. Stuart[9] has ingeniously used a questionnaire dealing with the intensity of the individual's *likes* and *dislikes* as a clue to his paranormal abilities, and certainly there is a large field here to be explored; but in general, the questionnaire allows for too much self-deception. The term "behavior tests" is used here to include all those tests which, by showing the manner in which a subject carries out a *standardized experimental task,* predict how he will carry out a real task in a life situation. An

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[7] See footnote 4.

[8] Murphy, Gardner, "Removal of Impediments to the Paranormal," JOURNAL A.S.P.R., Vol. XXXVIII, January, 1944, pp. 2-23.

[9] Stuart, C. E., "An Interest Inventory Relation to ESP Scores," *Journal of Parapsychology,* Vol. 10, September, 1946, pp. 154-161.

example is the successful test program of the Office of Strategic Services[10] which, by presenting very grilling tasks, fairly accurately predicted how men would behave in difficult overseas assignments. In psychical research we may, in time, expect good results from behavior tests—for example, from tests of ability to carry on two activities at once without interference, since this ability may be related to the automatisms which express the paranormal. At this writing, however, neither the questionnaire nor the behavior test has anything at all comparable to what the projective tests have already offered; so we shall examine these more closely.

A projective test is any test in which the individual records his *outlook upon life* in a free situation permitting him to proceed in his own way; in L. K. Frank's words, the projective test reveals the "private world" of the individual. Thus children in nursery school, dabbling in finger paints, or playing with toys, show by choice of color or by the form of their play activity, how they feel about their environment. A timid child, for example, usually paints with subdued colors in a corner of the sheet of paper, not "venturing out" into the great unknown. He arranges and plays with doll house furniture and miniature life toys in such a way as to reveal clearly what it is that he fears, and how he finds his way to love and safety. So, too, the way in which a picture is interpreted by a subject, whether adult or child, is a clue to his fears and hopes, the scope and richness of his imagination. His *expressive movements,* as in his spontaneous drawings, have proved highly valuable as clues to his impulsive and creative life. Everyone sees in his friend's drawings—quite aside from all question of artistic merit—something characteristic of the individual. The problem of the projective test is to study these self-revealing qualities so as to throw the greatest possible light upon the way in which personality discloses itself.

It is in this last area, that of free drawings, that the first systematic and extensive use of personality tests in relation to the paranormal has been published: the work of B. M. Humphrey at Duke University.[11] A large amount of material was made available to her by C. E. Stuart, also of Duke University, in the form of drawings which Stanford University students had made for him during his experiments there. His subjects, that is, had attempted to reproduce drawings and other picture material concealed in opaque envelopes

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[10] Murray, H. A., and MacKinnon, D. W., "Assessment of OSS Personnel," *Journal Consulting Psychol.,* Vol. 10, 1946, pp. 76-80.

[11] Humphrey, B. M., "Success in ESP as Related to Form of Response Drawings: I. Clairvoyance Experiments," and "Success in ESP as Related to Form of Response Drawings: II. GESP Experiments," *Journal of Parapsychology,* issues of June and September, 1946.

or to reproduce such material being looked at by an "agent"; these tests had been evaluated by his "preferential matching" technique.[12]

The question arose: can the *form of the drawings* executed by individual subjects tell us whether they are likely to be good or poor subjects at a clairvoyance or GESP task? The theory of the projective tests would lead us to expect an affirmative answer: we should expect people to show in their drawings something about their conscious or unconscious needs, and their habitual way of coping with the problem which their needs present.

Now, as it happened, there was available an excellent study of the possibilities of free-drawing as a projective technique, by Paula Elkisch,[13] who had shown, among other things, that "expansion" and "compression" in free drawings can be gauged with fair reliability. Accordingly, Humphrey studied the criteria of expansion and compression and learned to score the drawings in these terms. She was then able to show that *expansive subjects,* according to these criteria, scored significantly higher on the *clairvoyance* tests than did compressive subjects. The *compressive* subjects had a curious tendency to score not on the target at which they were consciously aiming at the time, but on the target used in the test just preceding. We have here a suggestion that the expansive people may reach out more freely to make clairvoyant contact. The compressive people may, perhaps, be fighting against making a response which would indicate successful clairvoyant contact; but their significant scores on the just-preceding target seem to betray the fact that they did actually function paranormally, though in such a way that it would never have been known if the data had not been carefully studied. In both groups, then, the technique is helpful in showing the operation of needs (the need to reach out and make contact, the need to avoid contact), and of the devices by which the unconscious paranormal contact with the target gets into *direct* or *indirect* expression. A great deal can reasonably be expected from further use of this approach, which we hope will be repeated and confirmed by other workers.

In a later experiment, Humphrey applied the "expansive-compressive" procedure to the results of GESP tests—tests in which there is a possibility for telepathy to operate—and found that here it was the *compressive* subjects who scored significantly higher than the

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[12] In this technique, each drawing by a subject is ranked, both by the subject and by the experimenter, in terms of its degree of association with each of four target drawings; and each of four target drawings is ranked in terms of its degree of association with each drawing made by the subject (the judges not knowing which target had been used at each one of the four trials).

[13] Elkisch, Paula, "Children's Drawings in a Projective Technique," Psychol. Monogr., 1945, 58, pp. 1-31.

expansive subjects. She points out that "this reversal of relationship raises a problem that has to be left unsolved for the present." One possible hypothesis would be that in the GESP situation the expansive subjects are resistant—negativistic—as is suggested by the fact that five out of six of the sub-groups tested scored not only below the compressive subjects but below "chance expectation." But many hypotheses are possible, and a wide vista of research problems is opened up.

In the meantime, other projective studies are in progress. During her years of ESP research at Harvard, and her more recent work at the A.S.P.R., G. R. Schmeidler made numerous exploratory studies with projective techniques, three of which may be briefly noted here; her own report will be made in due time. (1) She has made use of the series of pictures devised by Morgan and Murray[14] and used at the Harvard Psychological Clinic: the subject is asked to "make up a story" about each picture. Since each story expresses a preoccupation, or *thema,* of the subject, and shows how each such thema molds his apperception, or way of understanding the pictures, it is called the Thematic Apperception Test. Schmeidler has studied the varying moods and attitudes of her subjects in relation to their varying interpretations and has sought to find the relation of such moods and attitudes to ESP scoring levels. The problem of a suitable procedure has not yet been solved. (2) She has also used a picture test devised by Rosenzweig, known as the "Picture Frustration Test," in which cartoons depict a man or woman in a frustrating situation and the subject must indicate what the man or woman said. The test may be scored in terms of the tendency of the subject, as he identifies himself with the person in the difficult situation, (a) to blame the other fellow who caused the trouble, or (b) to take the blame upon himself, or (c) to avoid all reference to blame. The individual's "adjustment status" is the degree of his tendency to respond as other people of the same sex and the same cultural group respond, *i.e.,* his tendency to blame others, or himself, or no one, in those situations in which most people (as discovered in earlier research) blame others, or themselves, or no one. Scored in this way, Schmeidler's preliminary data, published briefly in the course of the report of her ESP work at Harvard,[15] suggest that the taking of the Rosenzweig test results in a rise in ESP scoring level more or less proportional to the adjustment status of the individual; thus

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[14] See Murray, H. A., *et al, Explorations in Personality,* Oxford University Press, New York, 1938, pp. 530-545.

[15] Schmeidler, G. R., and Murphy, Gardner, "The Influence of Belief and Disbelief in ESP upon Individual Scoring Levels," *Journal of Experimental Psychology,* Vol. 36, June, 1946, pp. 271-276.

a well-adjusted person will tend to go up in ESP level in consequence of taking the Rosenzweig test, while a poorly adjusted person will tend to go down. The Rosenzweig test was scored by Schmeidler herself, and was later independently scored by another clinical worker. This result, while suggestive, is based on a small group, and Schmeidler has preferred to draw no confident conclusions until further data are at hand.

By far the most valuable of the existing projective tests, from the writer's viewpoint, is the Rorschach test with ink-blots, published over twenty years ago by the Swiss psychiatrist, Hermann Rorschach. He early noticed how each person sees different things in the same blots. Working experimentally with many kinds of ink-blots, interpreted by hundreds of people, normal and abnormal, he devised a standard set of ten blots, some black on white, some including color, which, in eliciting personally significant responses, have proved extraordinarily useful in psychiatric diagnosis, in evaluation of rate of recovery from mental disorders, in assessment of fatigue, anxiety, and emotional conflict in normal people, and in vocational and scholastic guidance. Ruth Munroe[16] has, for example, recently shown that the Rorschach test can, in general, predict academic achievement better than an intelligence test can, because it shows the emotional blockages which keep a student from effective use of his intelligence.

Now it occurred to Schmeidler as early as 1943 to try to define the role of the Rorschach in ESP research, and the research begun at Harvard has continued on a large scale since she has been with the A.S.P.R. An assistant scores the ESP runs, keeping her in ignorance of them until the Rorschach is scored by Schmeidler. Continuing to separate her subjects into "sheep" (those who accept the theoretical possibility that clairvoyance exists) and "goats" (those who reject this possibility), she has sought to find by means of the Rorschach those personality characteristics which enable the sheep to get their above-chance scores, and those which enable the goats to "miss the target" so as to continue to give sub-chance scores. The results, consistent so far through two large experiments, await the results of a third experiment, after which they can properly be reported in full. In the meantime it is permissible to note that the Rorschach seems to be contributing to an understanding of the problems with which we began our inquiry: it has something to say about the conscious and especially the *unconscious needs* of the subject; it suggests a variety of ways in which he struggles to prevent his unconscious needs and his resulting perceptual and other

[16] Munroe, Ruth L., "Prediction of the Adjustment and Academic Performance of College Students by a Modification of the Rorschach Method," *Applied Psychology Monographs*, No. 7, September, 1945.

activities from breaking into consciousness; and it affords opportunities for a mild degree of splitting of personality, by which material functioning at an unconscious level may force its way into the pattern that the subject sees in the ink-blots, often without his being aware of what is happening.

It is indeed quite possible that a way will be found in which the Rorschach test itself will function, so to speak, as a crystal; *i.e.,* so that paranormally received material may directly influence what is seen in the ink-blots. Even without this use of the test it is possible to get light on the characteristic personal ways in which the subject's unconscious responses fight their way up into his consciousness, and this may prove a highroad to the understanding of the ways in which some individuals circumvent the struggle against their own paranormal activity.

It is quite possible that in time we shall find a way *to combine projective tests routinely with tests of the paranormal.* The studies of Stuart and Humphrey show that one can get satisfactory and scorable free-drawing material while doing a clairvoyance test; the same procedure could be used with many other projective tests. It is even possible that the procedure could be turned the other way around, getting clairvoyance (or telepathy, etc.) material while gathering projective data. It is true that the ESP experiment depends, as a rule, upon volition, so that the subject would, generally, have to know that he was doing an ESP test; but this would simply mean in practice that the "game" one plays in the ESP test would be the "game" of taking a personality test, hoping that some responses would conform to a concealed target, so that it could be scored in both ways, as a personality test and as an ESP test.

This conception of tests for two things at once prompts a suggestion regarding Humphrey's method. She showed, among other things, that the successful hits made by the subjects appeared both when the target item itself was "expansive" and when it was "compressive." But this leaves open the question whether the individual's *responses* were more *expansive* when they were *relatively* correct. It is conceivable that with highly gifted subjects one may, throughout the course of a combined projective-clairvoyance test, watch the ebb and flow of the paranormal as the projective response varies, and thus come much closer to catching its psychological nature. (Why might not this apply to PK as well as to ESP? We might watch the ebb and flow of PK in accordance with changes in mood, volition, etc.) Indeed, at this point one finds oneself thinking of the projective tests as an indicator not only of *personality factors,* but of *favorable states for particular personalities,* along the lines formulated by Schmeidler's first exploratory work with the Thematic Apperception Test.

One of the greatest tasks emerging from such analysis is the study of special sensitives by all those projective methods which offer any promise of making their unusual gifts more intelligible, and of explaining the fluctuations in sensitiveness which characterize even the greatest of this group. In view of the cooperative spirit of those sensitives who have permitted intensive studies to be made of their psychology, it is to be hoped that the newly devised methods will bring a rich harvest.

Summary

A sketch has been offered of a few problems in the study of individual differences which may help us to understand the nature of paranormal gifts; and a few methods of personality appraisal have been mentioned which may help in evaluation of these personality traits. The projective tests, such as free-drawing, picture interpretation, and the Rorschach ink-blot test have been especially stressed. The work of Humphrey and Schmeidler in introducing these approaches has been cited as pioneer research surely destined to bring a large yield in the study of personalities gifted in these respects, and of the most favorable states for their work.

The Psychology of the Psychical Researcher

GERALD HEARD

This may seem a rash title to be used by a layman, and at the same time a rather dull sideline for the reader in search of a quiet quarter of an hour with an experimental paper or report on a good spontaneous case. All that is meant by psychology in this context is what our grandparents meant when they spoke of "temperament"; and, as a matter of fact, no serious student of psychical research can doubt that here, in the psychological make-up of the researcher, lies a vital factor affecting the results we may hope to achieve in this obscure but pre-eminently important subject. Moreover—and this is perhaps even more urgent—in the personality of the investigator lies the clue to the covering hypothesis which in the end we must have if psychical research is to make a large if awkward contribution to the cosmogony of our age.

First we must face the fact that personality factors in the researcher undoubtedly count for much in the results which he obtains in his experiments. A "good sitter" is as needed to get good results with a good medium as to get a good hatching of eggs. This sort of thing, of course, has been brought against psychical research. It is the old bogie of science, subjectivism. It is no use to quote Turner's reply to the lady who remarked that she never saw the sunsets that he did—"Don't you wish you did, Ma'am!" The standard researcher with almost pharisaical self-congratulation thanks heaven that he never sees anything that is not more or less as he expected. The anomalous is almost to be equated with the scandalous. The day seemed to be long past when the dictum "Beware when you find that for which you were looking" could be repeated with acclaim in a classroom. But it is wise to say "seemed" and not "seems." For though we have not discovered a way of ruling out the personal factor in psychical research, we *have* found, on the other hand, that the personal factor, the influence of the observer, has had to be recognized increasingly in the other sciences. The mere act of observing a thing changes the thing. Such a proposition, which in physics sixty years ago would have seemed heretical, is now accepted, if with regret. Biology still holds out against the recognition of inevitable subjectivity in its findings, and this is probably why materialism and mechanism, which have successively retreated from their former fount and fortress, physics, are now finding an insecure shelter among the biologists.

Yet such inquiries as "Life and the Second Law of Thermody-

namics" by Dr. J. A. V. Butler[1] show that biology may not long be able to use the classic mechanics as the basis of its philosophy. This brings us therefore to our second point, or question: Is there such a thing as "Science"? Are there not rather, as Professor Dingle, recently appointed to the Chair of the philosophy and history of science at London University, has said, three great sections of research which can all to some extent, but in different degrees, make use of the scientific methods of linear causality, analysis, and experiment by selection? There would then need to be three kinds of researchers for these three great categories or fields of research. In physics it would seem that the pure analyst is most to be desired. In biology, since when dealing with life processes the power to repeat is much restricted (*e.g.*, we cannot take any of the evolutionary hypotheses and "try them over again"), a thinker who has in him something of the historian is needed—a need supplied in the case of Darwin and Huxley. When we come to psychology this element of irrepeatability, which is another name for uniqueness, is even more in evidence; and therefore the researcher who is to understand the nature of psychological phenomena must not only have the capacity to analyze and the power to be able to perceive the narrational and historical element, but also he must possess something of the creative artist's gift, the power to take an interest in the unique, and from it be able to compose. These categories, taken of course from esthetics, seem to mark the successful investigator in the field of psychical research. Nor do we need to think in the present state of our science that these categories are in any wise a disadvantage. As Whitehead and other students of the beginnings of the great advance in physical science have pointed out, the great physicists of the seventeenth century—the "century of true invention," as it has been called—were discovering "laws" on what today would be thought to be totally insufficient evidence.

The ideal researcher in matters of pure psychology must then be a man who first realizes the position of his science both in its own development and in regard to the other sciences. He must see, as Professor H. H. Price of Oxford has said, that in psychical research we are at the point—as was physics in the seventeenth century—when boldness of hypothesis is demanded. In no other way are we able to compose a frame of reference which will both free thought from categories no longer applicable to present observations and also serve as a canon for further research. Professor Price himself has made some fruitful attempts in this direction when dealing with the type of cosmogony that is required by the existing evidence for precogni-

[1] *Nature*, issue of August 3, 1946.

tion and retrocognition. Our researcher must also understand that psychical research is not only a new science in itself, it is a new branch in the whole tree of science and one that therefore calls for new criteria through which it may express its character and handle its data. For science as a whole is advancing in that vast curve which brings man back from the problem of data through means of the sensa to the basic question of epistemology. The proper study of mankind is man, and the real question for consciousness is the nature not of force or matter but of *consciousness itself*. Toward this problem there is needed an approach very different from that which nineteenth-century scientists called objectivity. The needed approach is one that goes deeper and draws near to what Shankara might have called *discrimination*. It is that profound attitude of mind which can see itself observing and the object observed in their dual and connected relationship. This, of course, is the doctrine of the field rendered in psychological terms or, to use the biological phrase for the same thing, the problem of the ecological whole. To give an example, in a good sitting with a medium there is not simply the medium in a state of dissociation and a detached observer taking notes; there is a situation in which a complex of inter-relationships is being woven so that the source of initiative is hard to discover. The usual distinctions of objective and subjective can actually be hindrances here.

How then is the researcher to conduct himself? Here the kindred subject of anthropology can be of help. It has been found that those anthropologists who made the finest contributions to studies of alien cultures did not go with presuppositions that the people they were studying were "low" or "ignorant," but simply that they were *different*. They did not talk down to them, but spoke across and exchanged. So must it be in this even stranger anthropology which psychical research pursues. As Cushing did with the Zuni when he became naturalized and initiated, and as later Gorer did in Africa, so must the researcher with the medium or sensitive. We do not know the conditions under which these borderland faculties operate, and we must therefore be content at the beginning of the investigation to do what Huxley suggested—but failed egregiously in this respect to practice—to "sit down like a little child before a fact." Something is taking place, and whether it be an anthropological fraud or a new evidence of some psychic faculty it requires study and reservation of judgment—or rather the hospitality of two possible categories for its possible reception. Fraud and evidence of genuine paranormal ability have often come strangely mixed together; and as Fenelon said, He that has never been taken in is a man more wise in his own eyes than in reality.

Psychical research, then, calls for a type of mind more often found

among scientists three hundred years ago, when physical science was making its first giant strides, than it is today. And moreover, because of the present state of all inquiries into natural phenomena, the researcher today has to understand that since all laws are now increasingly seen to be hypotheses and not necessarily wholly objective principles, so the attitude of the researcher must tend to be more that of the artist who composes than that of the analyst who abstracts. Although abstraction and hypothesis are indeed the two great poles of research, psychical research undoubtedly at present has its chief place with hypothesis. Abstraction, while it uses nothing but facts, has to leave unused some data; hypothesis, while it includes all the facts, has to complete its picture, its composition, has to hypothecate facts yet to be found. Abstractionist science accuses this hypothecating method of *suggestio falsi.* It, on the other hand, is just as free to reply that the method of abstraction is guilty of *suppressio veri.* Certainly the abstractionists have turned a very blind eye for far too long to the data of psychical research. But there is no need for resentment and recrimination. We have to recognize that with the conceptions of the field, of ecology, and of Gestalt we have entered an age when hypothecating will again be the spearhead of research; and, in all this research, it is the man who recognizes his personal factor—the contribution which the observer makes to the problem—who is apt now to produce the most fruitful results. When that is said, it is clear that the very difficulties of psychical research are real opportunities, and the position in which the investigator finds himself today is evidence that he is in the forefront of the advance of mankind's thinking and discovery.

Automatic Drawings by "John Alleyne"

WILLIAM OLIVER STEVENS

It is now nearly forty years since Bligh Bond and his friend, John Allen Bartlett, better known by his pseudonym "John Alleyne," began the automatic writing that led to discovering the sites of the lost chapels of Glastonbury Abbey. The story became famous in America as well as in England through the publication by Bligh Bond of *The Gate of Remembrance* and *The Company of Avalon.* It is still an outstanding instance of an extended communication substantiated by subsequent events, in this case by the work of excavation. The method used was a curious dual type of automatic writing, for, while it was done, Bartlett held the pencil and Bond's hand rested lightly on that of his friend.

In this country, however, it seems to be not generally known that Bartlett produced automatically a series of amazing architectural drawings, showing what purported to be restored aspects of Glastonbury, and these were done independently of Bond. They were exhibited in Caxton Hall, London, during the summer of 1925 and at the time excited much popular interest there, but apparently they attracted little notice here in America. The recent death of Bligh Bond suggests that it might be appropriate at this time to recall this unfamiliar phase of his friend's psychic power. The present writer is fortunate in having in his possession several letters from Bartlett to a friend in America and some published material, chief of which is a personal account in the London *Graphic* (November 19, 1921) written by Bartlett himself and illustrated by half-tones made from his pictures. All of this material goes back a matter of twenty to twenty-five years.

There was nothing in the background of John Allen Bartlett to suggest the development of a remarkable sensitive. He was educated at Sandhurst for the army, and served in the first World War with the rank of captain. A photograph of him taken in 1914 shows the eyes of a poet belied by a fierce moustache, which in those days was *de rigeur* for an officer of the British Army. Certainly, professional soldiering was not to his taste, for after the war he turned to the writing of verse, chiefly lyrics for the songs composed by his wife, Maud Wingate, known professionally as "Carlyon de Lyle." A romantic strain in both of them led to their adopting other names than their own for signing their compositions. Bartlett took the name "John Alleyne," and it is under this pseudonym that he appears in the narratives by Bligh Bond of their joint writing in connection with the Glastonbury excavations.

It was, however, long before that partnership took place, about the beginning of the century, that Bartlett began to be conscious of paranormal powers. He says that his friend Florence Marryat used to tell him that he was a "psychic." At the time this amused him greatly, for in those days the word "psychic" was a term associated in his mind only with cranks and gullibles. In fact, that was one subject with which he would have nothing to do. He was not merely indifferent but hostile.

The change in his attitude was brought about by an incident that occurred spontaneously in 1901. In that year his father died. A week or two afterwards, while the young officer was at home on furlough, he was sitting at his father's desk, composing a business letter. Suddenly, he says in the *Graphic* article, "a wave of intense cold enveloped my right side, and my hand immediately wrote of its own volition a message purporting to come from the deceased. It was in his own handwriting, bore his signature, and I did not know its contents until I had read it." Although he was right-handed, this automatic writing was performed by his left hand. The first words were, "I am your father." Bartlett immediately reported the message to his mother but was scoffed at for his pains. However, he continued his "sittings" for the writing.

This initial script "was followed," the narrative continues, "during a period of some two years, by a multitude of communications, bearing the stamp of his individuality and containing details and particulars—often of great value to the family—which could not consciously have been possessed by any other intelligence than his.[1] It convinced both myself and many others that his personality had survived the ordeal of death."

In 1907 Bartlett and two friends, both members of the S.P.R., investigated an alleged haunted house. One of these was Bligh Bond. Automatic writing was resorted to in the hope of obtaining information, and Bartlett says that subsequent inquiry and research among newspapers of the year 1798 corroborated much that was written in this way. Suddenly, in the midst of the writing the "control" interrupted to complain, "Some monks want to speak to you. Why can't they talk English? Surely, it's as easy as talking Latin!" With that began the famous Glastonbury scripts written by the two men together. In regard to these scripts, Bartlett tells us that the automatic writing went on very rapidly indeed and continued even after he had fallen asleep. He says that he represented the "intuitive" and Bond the "intellectual" side of the partnership. Some of the script was

[1] This statement shows that Bond was mistaken in stating (*Gate of Remembrance*, p. 19) that Bartlett had never practiced automatism deliberately before the Glastonbury scripts began.

almost illegible, and Bartlett was nearly always unaware of what the pencil was transcribing.

The next step, and that is the phenomenon about which this article is chiefly concerned, was the appearance before his eyes of passing glimpses of the Abbey as it was alleged to be before its destruction. At first they were blurred, but eventually they became so clear and fixed that he could see them objectively as pictures on a blank sheet of paper. "It remained," he says, "but to trace them over and reproduce their coloring in pastel." Nor were these visions only scenes of the Abbey itself. He received impressions of the faces of the monk Johannes—communicator in *The Gate of Remembrance,* and of Abbot Bere, the friend of Erasmus and the builder of the lost Edgar and Loretto chapels. Bartlett drew their portraits in the same way, by tracing over what he saw on the paper before him.

Just when these drawings began, Bartlett did not say. But in a letter of January 9, 1921, he speaks of being then in the midst of his drawing of the West Front of Glastonbury Abbey; and in another letter of the following May he says, "I do a good many drawings, mostly purporting to be restorations," etc., suggesting by his use of the present tense that he was at that time in the midst of this work. It seems clear, therefore, that these automatic pictures followed by a number of years the Glastonbury scripts that he wrote together with Bligh Bond. It is noteworthy that he made these drawings by himself, that Bond had no share whatever in them. And Bond was the man who had an architectural background; Bartlett had none whatever.

What happened, as nearly as can be ascertained from Bartlett's words, is that he saw the picture on the paper before him, but that the tracing over it was done unconsciously by his left hand at great speed. Then he would finish the drawing consciously with his right hand, adding the colors, usually with pastels, from memory. "The speed and accuracy with which the left hand does its work," he continues, "is in marked contrast to the performances of the right."

For this sort of work Bartlett had no equipment whatever, by talent or training, in either the field of art or architecture. He was an ex-captain of the British professional army, who had turned to writing lyrics and the revival of English folk-songs. Anyone who has ever had the problem of drawing large architectural units, especially the great abbeys and cathedrals of the Gothic age, can well understand the enormous technical difficulties of the task, involving not only the profusion of detail but the problems of perspective. Even such a master of draughtsmanship as Joseph Pennell is said to have often drawn over a photograph taken of a cathedral front in order to help himself over a difficult task. Evidently, Bartlett, with his left

hand, traced automatically at high speed over the picture seen on the paper much the same way as Pennell traced over his architectural photograph. Frequently, though not always, Bartlett added the coloring afterward, from memory, using pastels or water color. Sometimes, as he describes below, he laid the colors on while the vision lasted.

All of these pictures, he says further, "have been drawn against my will for the most part. The visions of portions of the Abbey have come to me when I have been busy writing lyrics for my wife's music."

Apparently, when these visions came before his eyes he went involuntarily into a half trance. "A dreamy state came over me," he explains, "and at first the vision of the Abbey was blurred. Gradually the blur passed and then I could see the lines and colors clear and distinct. I groped for the pastels like a blind man, and as long as the vision remained I could work with them although I had never used a pastel in my life before.

"Once the dreamy state had passed, and with it the vision, I was incapable of drawing anything. The picture of the West Front showing the cross, which has since been excavated on the exact spot I visualized, took twelve months to complete. I could not work at will but had to wait for the dream state.

"The choir occupied six months, off and on. 'Candlemas Eve' came to me when I had no paper at hand and my wife hurriedly obtained some blue sugar paper."

It would seem from these words that the same picture would recur from time to time and by its repeated appearances give Bartlett the opportunity to carry it to completion.

The view of the Abbey's West Front, which took a year to complete, was reproduced in the middle of a double-page spread which the London *Graphic* (in the article quoted above) gave to these drawings. This particular drawing was rendered in water colors instead of pastel, as was true of one of the others reproduced on the same page. Strictly speaking, the west front view is actually a restoration of the whole abbey as seen from the west. It is a matter of regret that this particular drawing cannot be reproduced here. The original is thirty inches by twenty. Perhaps no one can so well appreciate the paranormal character of this work as any artist who has ever tried to sketch the front of a Gothic cathedral. Nor is this merely an architectural rendering. It represents an abbey that exists nowhere in the world. It purports to be a reconstruction of the original shrine at Glastonbury, of which there exists no picture whatever, and the original was destroyed by Henry the Eighth in the year 1539. So it is not only a picture of an abbey, it is a design of exceptional

beauty. Before leaving this drawing we should not forget the detail of the great cross over the west door. This is clearly indicated in the picture and, as Bartlett states above, it was confirmed later when that very cross was excavated.

Ralph Adams Cram, the American authority on Gothic architecture, was much interested in these drawings. He had long talks with Bartlett and declared his faith in the man's sincerity.

Although the west front view holds the place of honor among these illustrations, there were others depicting interior scenes. In the Caxton Hall exhibition there were eight. The *Graphic* article showed a number of these, and in two instances placed the automatic drawing of the restoration alongside the photograph of that part of the Abbey as it looks now.

All of these visions were seen in great detail. The drawing shown by the *Graphic* of the restored refectory reveals an impressive hall filled with minutiae and thronged with small figures of the monks. Bartlett saw it as "abutting on the south cloister, and a great tapestry covered its east wall, and the ridge of its roof was some eighty-five feet from the ground." The interiors show elaborate vaulting, the flowering of stone and wood carving, and tombs on the pavement. All of this came from the hands of a man who declares that out of his "dream state" he is incapable of drawing anything at all.

It would be interesting to know where these drawings are now. On April 11, 1933, John Allen Bartlett died. There seems to be no record of his psychic activities during the last decade of his life; at least nothing that attracted public attention as his scripts and pictures relating to Glastonbury Abbey did. However, his writing seems to have continued. In the letter of January 9, 1921, written to his friend in America, he speaks of having "reams of Automatic Script, including nearly 700 pages of *The Philosophy of the Middle Kingdom,* on which Bond is lecturing, and which he proposes to publish, as I rather prefer to keep in the background in these matters."

Indeed, it would seem that throughout this whole story of an unusual, if not unique, mediumship John Allen Bartlett did very successfully keep in the background. And the most striking phase of his mediumship, one for which there seems to be little recognition, was this rendering of elaborate restorations of a ruined abbey, in drawings produced automatically and with the left hand, by a man who had no technical knowledge of either art or architecture.

In conclusion, attention may be focussed upon two points of interest. First, what was the method of making these drawings? Bartlett testifies that a picture formed on the paper before him, of a face, or more often an architectural scene. This picture could never be produced at will; he had to wait until the phenomenon occurred

spontaneously. When this happened, he says that he was in a "dreamy state" or half trance. Also, although he was right-handed it was almost always his left hand that traced the picture. It did so at great speed, with accuracy and automatically.

Secondly, what paranormal knowledge was revealed by these alleged architectural restorations of Glastonbury? Since nothing whatever is extant as to the appearance of the edifice at the time of its destruction in 1539, these drawings cannot be judged by anything better than that they seem to fit in harmoniously with the ruins as they exist. It will be recalled that in the *Graphic* article some of the drawings were placed alongside photographs of those portions of the Abbey as they look now. It would have been no mean feat for Ralph Adams Cram, with all his expert knowledge of Gothic architecture, to have sketched in his studio a restoration of even one of the aisles of Glastonbury which would not conflict in any particular with the ruins as they stand. However, only one detail can be cited as having in itself weight as evidence of paranormal knowledge and this hangs on Bartlett's testimony. In his drawing of the West Front of the Abbey, he shows a great stone cross over the entrance. According to the *Graphic* article, that identical stone cross was subsequently dug up from the spot on which it had fallen when the building was destroyed.

At all events, it would seem that this little-known pictorial side of the mediumship of John Allen Bartlett is much too interesting to be forgotten.

Note on an Impromptu Experiment in Psychokinesis

EDMOND P. GIBSON

Early in August of this year the present writer constructed a dice chute somewhat similar to the dice chutes used at Duke University and to the chute used in the experiment reported by L. A. Dale in the July issue of this JOURNAL.[1] The writer's chute was made smaller than its predecessors so that it could be easily transported. This small chute stands two feet high to the top of the hopper, the chute slide is seventeen inches long and three and a half inches wide, and is lined with a corrugated paper base, the corrugations set at cross angles to the slope to institute a side twist to the falling dice. The chute itself is placed inside a four-sided baffle which confines the falling dice. This baffle is three and a half inches high, thirty-one inches long, and eighteen inches wide. It was proposed that twenty-four dice be used for each throw, and two dozen white plastic dice, measuring 7/16 inches on the side, were thus obtained from the Parapsychology Laboratory at Duke University. It was also proposed that before each throw the dice were to be shaken in a regular dice cup, and then poured into the hopper of the chute.

The use of this dice chute was planned for an experiment, exploratory in character, in which new subjects for PK experimentation would be sought for, and old subjects taking part in our earlier PK experiments (1935-1937) would be retested so far as they were available.[2] This plan became possible due to our return to the locale of the earlier experimentation, Grand Rapids, Michigan.

It was planned that twelve throws or runs[3] were to be made for each face of the die as target, and that all six faces were to be thrown for by the subject at a session and recorded on a single record sheet. Thus a single task consists of 72 runs (72 throws of 24 dice), the dice being shaken in and poured from a rubber dice cup or a dictaphone record cylinder, thence down the chute to the enclosed area on rug or table.

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[1] Dale, L. A., "The Psychokinetic Effect: The First ASPR Experiment," JOURNAL A.S.P.R., Vol. XL, July, 1946, pp. 123-151.

[2] Gibson, E. P., and Rhine, J. B., "The PK Effect: Some Introductory Series," *Journal of Parapsychology*, Vol. 7, June, 1943, pp. 118-134.

[3] The Duke work has defined a "run" in PK as 24 single die readings: In the experiment under discussion, therefore, a single throw completed a "run."

It is planned to carry this experiment to a total of 1008 runs, dice to be thrown in the sequence of faces from one to six, and an additional total of 1008 runs in which the die faces will be thrown for in reverse order. At the time of writing this informal interim report, the proposed experiment has not been carried out to completion: the experiment is in the latter part of its first phase.

Work with the apparatus had begun only sporadically at our home —only a few "off the record" trials had been made—and the official experiment was not yet under way when we were visited on Sunday afternoon, August 18, 1946, by Mr. Hugh J. Lago (H. J. L.), his wife, Laura (L. L.), and their daughter. (To those readers familiar with our earlier reports on ESP experimentation, it may be stated that H J. L. was subject number four and L. L. was subject number nine in the successful series of ESP tests made during the years 1934-1936 inclusive. They were, in fact, high-scoring subjects who produced independently significant results in various ESP-testing procedures.[4])

H. J. L. noticed the PK apparatus standing at the side of the room, stated that he hadn't done a dice test for a number of years,[5] and said he would like to try the dice since he had a hunch that he would do well. I agreed to score for him.

It was a hot afternoon and we each drank a small bottle of beer before starting the experiment. As previously planned, 24 dice were used for each throw, and twelve throws were made for each die face as target. H. J. L.'s task as recorded on a single record sheet consisted of 72 throws of 24 dice, an equal number for each die face, the dice having been shaken in and poured from a rubber dice cup into the hopper of the chute, from which the dice fell onto the rug where they were confined by the four-sided baffle. H. J. L. completed one record sheet (72 runs), and after a brief intermission completed a second record sheet, 144 runs in all. On both record sheets the subject started out aiming for the one-face, then the two-face, and so on up to the six-face. This experimental session is reported as a unit since the combination of conditions (beer, spontaneity, and a strong hunch as to success) was not repeated in subsequent experimentation with this subject. His results for the entire impromptu session are shown in Table I.

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[4] Gibson, Edmond P., "A Study of Comparative Performance in Several ESP Procedures," *Journal of Parapsychology,* Vol. 1, December, 1937, pp. 264-275.

[5] H. J. L. had taken part in some of the early psychokinetic experiments in Grand Rapids (*circa* 1936), and had achieved interesting results; but his scoring at that time was in no way comparable with that now being reported.

TABLE I

Number of hits and deviation from chance expectation on all faces of the dice as targets, first and second record sheets

Hits on:	one-face	two-face	three-face	four-face	five-face	six-face	Total	Expectation	Dev.	SD	CR
Sheet I	55	56	44	56	47	50	308	288	+20	±15.49	1.28
Sheet II	51	51	51	58	62	67	340	288	+52	±15.49	3.40
Pooled[6] Total	106	107	95	114	109	117	648	576	+72	±21.91	3.29

The deviations on Sheet II and on the Pooled Total are both significant. The P-value associated with the CR of 3.29 is .0005.

I scored the first sheet with H. J. L. who double-checked my counting of the dice. The second sheet was begun with L. L. and Mrs. Gibson checking the scoring of H. J. L. while I acted as witness. The exceptionally high scoring occurred in the last three columns of the second sheet and was witnessed by three observers.

At the close of the experimental session with H. J. L., L. L. threw a series of 72 runs (a single score sheet). Identical experimental conditions obtained except that L. L. did not partake of any beer. Her score sheet showed a deviation from chance expectation of only plus three.

Following upon his significant PK experimentation of August 18th, H. J. L. on two later occasions tried to duplicate his earlier success. The results are shown in Table II.

TABLE II

Number of hits and deviation from chance expectation on all faces of the dice as targets, sessions of Sept. 4 and Oct. 27, 1946

Hits on:	one-face	two-face	three-face	four-face	five-face	six-face	Total	Expectation	Dev.	SD	CR
Sept. 4	45	44	48	41	51	46	275	288	−13	±15.49	−0.84
Oct. 27	52	62	47	48	49	50	308	288	+20	±15.49	1.28
Pooled Total	97	106	95	89	100	96	583	576	+ 7	±21.91	.32

[6] Ninety-six hits are expected by chance on each die-face.

In both later experiments the beer condition was duplicated so as to conform as far as possible to the conditions of the impromptu session of August 18th. In the experiment of September 4th the subject thought that he would succeed, but arrived in the evening and was very tired. In the experiment of October 27th he had no particular "hunch" as to success or non-success. His scoring resembled that of Sheet I of August 18th. He was distinctly more rested when undertaking the experimentation of August 18th and October 27th than during the September experiment.

At the close of H. J. L.'s test of October 27th, L. L. performed 216 runs (three score sheets) under similar conditions. Again her scoring failed to yield an extrachance deviation. Her results are pooled with those of the other miscellaneous subjects who have thus far participated in the experiment.

The results of H. J. L.'s scoring to date, when arranged for the evaluation of vertical salience effects down the columns, and treating the vertical columns of twelve trials (runs) in quarters, show interesting salience in the upper and lower quarters with a center portion giving a non-significant deviation. This salience effect is shown in Table III.

TABLE III

Pooled deviations obtained on each throw position on the record sheet, Subject H. J. L.

Pooled throws	*Sum of deviations*	*Sigma*	*CR*
1st throw: +31 2nd " : ± 0 3rd " : + 3	72 runs: +34	±15.49	2.19
4th " : — 5 5th " : + 1 6th " : + 3	72 runs: — 1	±15.49	.....
7th " : — 2 8th " : + 2 9th " : + 9	72 runs: + 9	±15.49	.....
10th " : + 9 11th " : +17 12th " : +11	72 runs: +37	±15.49	2.38
Total: +79	288 runs: +79	±30.98	2.55 P = .005

The total deviation from chance for the 288 runs made by H. J. L. to date is significant. The initial and terminal salience, as measured by the sum of the first and fourth quarters compared with the sum of the second and third quarters, gives a CR of the difference of 2.03, which is suggestive. Thus it would appear that this high scoring of H. J. L. is not just a "fluke" of a too-short series of data; it carries internal evidence as well as evidence in terms of total deviation of being just what it seemed to be when first scored—an extremely high burst of PK scoring, made evident in a comparatively short series.

A comparison between deviations obtained in the pooled first and last quarters versus those obtained in the two middle quarters may not have been made previously in the evaluation of PK data. This procedure, however, seems to be suggested in the case of the "high-dice" and "low-dice" tests reported by Reeves and Rhine.[7] The table on page 88 of this report shows the total deviation for each trial in the run, pooling both high-dice and low-dice series. The total deviation obtained on the first and fourth quarters of the pooled runs was 308.5 above chance expectation, compared with a positive deviation of only 73.5 for the two middle quarters. This heretofore uncomputed difference between sums of outside and inside quarters yields a CR of 4.23. In spite of many differences in procedure (construction of record sheet, number of dice thrown simultaneously, etc.), a similar initial and terminal salience effect has been noted in the small quantity of data so far supplied by H. J. L. This salience appears in more striking form when we pool in Table IV his data with those of the four other subjects who have taken part in the experiment up to the time of writing.[8]

Four miscellaneous subjects, as noted above, have performed with the new apparatus a total of 576 runs, thus completing eight score sheets to date. Their total deviation is only plus fifteen. When their data are pooled with H. J. L.'s, however, we see the emergence of statistically significant initial and terminal salience, the CR of the difference between scoring rate on outside versus inside quarters increasing to 2.61.

We see in Table IV that the significant salience of the whole experiment to date is contributed to by the scoring of the "chance" subjects and that the salience patterns observed in the scoring of H. J. L. (Table III) are also observable in their data.

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[7] Reeves, M. P., and Rhine, J. B., "The PK Effect: II. A Study in Declines," *Journal of Parapsychology*, Vol. 7, June, 1943, pp. 76-93.

[8] The writer wishes to stress that this is an interim report, and that a full report will be given when the proposed experimental plan has been carried to its completion.

TABLE IV

Pooled deviations obtained on each throw position on the record sheet, subject H. J. L. and four miscellaneous subjects

Pooled throws	*Sum of deviations*	*Sigma*	*CR*
1st throw: +57 2nd " : —11 3rd " : + 5	216 runs: +51	±26.83	1.90
4th " : —23 5th " : + 5 6th " : +18	216 runs: ± 0	±26.83	.....
7th " : — 9 8th " : —12 9th " : — 2	216 runs: —23	±26.83	.....
10th " : +10 11th " : +27 12th " : +29	216 runs: +66	±26.83	2.46
Total: +94	864 runs: +94	±53.63	1.75

CR/d (1st and last versus 2 middle quarters): 2.61. P = .004

Conclusion

An unusually high CR was yielded from the positive deviation obtained in a relatively small number of die throws performed by a subject who scored significantly high in ESP tests ten years ago. Biased dice effect was eliminated by throwing equally for all faces of the die. On the best record sheet (one out of a total of four performed by the subject) 1728 dice were thrown (72 standard runs) and a positive deviation of 52 was obtained. This positive deviation gives a CR of 3.40. After a total of 288 runs, the total deviation from chance expectation obtained by this subject remains significant.

The use of a small amount of beer by the subject before the start of the experiment produced relaxation and this may have contributed to the success of the experiment. Moreover, the impromptu nature of the August 18th experiment may well have been a contributing factor in its success. The planned second and third sessions were not significant in terms of total deviation.

In the column of throws vertically, pooling all columns by vertical position on the score sheet, a strong initial salience and a weaker terminal salience is to be noted in the work of H. J. L. The work of this subject when pooled with the work of four "chance-scoring" subjects shows an increase of salience observable vertically in the data, with a significant difference between the first and last quarters compared with the second and third quarters.

Acknowledgment is made to Dr. Betty M. Humphrey for rechecking the experimental data and for the discovery of the significant difference between the pooled first and fourth quarters versus the pooled second and third quarters.

The following numbers of the JOURNAL are out of print:

January, 1942
April, 1942
April, 1943
January, 1945
July, 1946

The Publications Committee would be glad to receive copies of these numbers from members or other readers. In exchange the donor will be sent, upon request, an equal number of copies of issues still in print (1941-1946 inclusive).

Stewart Edward White: An Obituary

On September 18th last, the novelist Stewart Edward White died at the University of California hospital after undergoing an operation. He was seventy-three years old. The newspaper obituaries spoke at length about his two-score books on outdoor life, ranging in scene from Alaska to Africa, books of wide popularity, and described his adventurous career as a hunter, explorer, and soldier in the first World War. His contribution to the realm of psychic literature, however, was dismissed by a line or two, such as "he embraced spiritualism," in *The New York Times,* and in the *Herald Tribune* the titles of only two of his books in this field. This notice, therefore, will concern itself with only this aspect of his writing.

"Before March 17, 1919," Mr. White says at the opening of the *Betty Book,* "my own 'occult' background might have been called average for a man who had lived an active life . . . I suppose I would have taken my stand on the side of skepticism . . . Spiritualism meant to me either hysteria or clever conjuring or a blend of both." In his explorations he had come upon the phenomenon of telepathy among primitive peoples, but had not been interested enough to try to explain it to himself.

On the date given above, some friends brought a ouija board as a toy to try out. It soon appeared that Mrs. White (Betty) had the best success with it. She was told on the board to "get a pencil." This message was repeated over and over. Later she obeyed and sat with a pencil poised over a sheet of paper. It began to move slowly and formed a long continuous script in which the words had to be deciphered and divided. When the sitting was repeated, in order to make sure that she was not consciously manipulating the writing, she blindfolded her eyes or looked away while her hand moved over the paper. Her husband sat in as the "observer." The phenomenon so excited the curiosity of both Mr. and Mrs. White that they continued experimenting.

In this way began a long series of alleged communications. From writing, "Betty" changed to the method described in *Our Unseen Guest,* of relaxing into a species of "double consciousness," or trance, from which she dictated words which came to her, and which her husband took down.

About a year and a half after this experimenting began, the Whites had assembled over 400 typed pages of MS. Some of this script was personal, such as instruction in technique, but the rest of it expressed a philosophy which, says Mr. White, "had given us a new

outlook and a fresh grip on life." But he waited seventeen years before any of this was offered for publication.

Meanwhile, however, he issued two books that were a departure from the outdoor scene that had made him famous: *Credo* (1925), a book designed to help the man with a scientific background to define his beliefs, and in 1928 a discussion of the problems of everyday living with the singularly unhappy title, *Why be a Mud Turtle?* But neither of these referred to psychic experiences.

In 1937 the accumulated mass of scripts bore fruit in the *Betty Book.* This was followed two years later by *Across the Unknown.* These contained a digest of the purported messages from spirit-communicators called by the Whites "The Invisibles." A third volume, *The Unobstructed Universe,* appeared in 1940 after the death of "Betty," and is a record of her communications received through their friend "Joan," the sensitive who had been the channel for the text of *Our Unseen Guest,* which she and her husband published anonymously a score of years earlier. *The Betty Book, Across the Unknown,* and *The Unobstructed Universe* comprise a trilogy which forms perhaps the most important landmark in communication literature since Stainton Moses' *Spirit Teachings.* These books had a mounting sale; while *The Betty Book* sold over 9,000 copies, *The Unobstructed Universe* topped the score of 32,000.

Subsequently, Mr. White issued *The Road I Know,* further notes on Mrs. White's messages; *Anchors To Windward,* a restatement of the philosophy or religion that the communications had revealed; and finally, *The Stars Are Still There,* in January, 1946. This, his last book, was inspired by the many letters that had flooded upon him in consequence of the "Betty Books," especially *The Unobstructed Universe.* He scrupulously answered every letter that bore a home address. These letters averaged 100 a month over the years. So many of them asked the same questions that Mr. White selected a number of the most typical ones and gave his replies.

As to their significance in psychical research there will always be a difference of opinion. Although Mr. White's character and reputation leave no room for doubting his integrity or intelligence, the habitually skeptical mind will not have the patience to examine the evidence that convinced the Whites, but will shrug these books off as the product of honest but credulous minds. It need hardly be repeated, however, that no one could have started off with an attitude less favorable to "divulgences" than this practical, out-door man, who had the typical agnostic mind of his time.

Finally a word should be added about Mr. White's charm of

personality, his talent for friendship, his generous praise for the books of others, and his patience and tact in answering all the thousands of appealing letters that were inspired by the "Betty Books." It is a matter of deep regret that the distance between his California home and New York deprived our Society of his counsel and inspiration.

In the year 1919, when the Whites began their psychic experiments, as recorded in the *Betty Book,* Mr. White made a trip East from California. He took advantage of the opportunity that journey offered by arranging a consultation with Dr. J. H. Hyslop. Mr. White was not then, apparently, a member of our Society. He was troubled and uncertain about the strange experiences that he and his wife had been having, and wondered whether they had enough validity to justify going on with the experiments. Dr. Hyslop reassured him and encouraged him to continue. According to the records it was not until 1925 that Mr. White actually joined the Society. He became a Voting Member in 1941, a position that he held until his death.

It may be of interest, in concluding this sketch, to tell again a story which is significant of Mr. White's profound belief in survival and of the deference paid to that belief by the editors of *Who's Who in America.* After Mrs. White's death her husband made no alteration of his biographical sketch in the line "m. Elizabeth Grant, of Newport, R. I., April 28, 1904" to indicate that she was no longer living. The editors were about to make the usual notations as to her death, when one of them, to quote *Who's Who in America's* Reference Service, "recalled that he had only a few days previously received a copy of *Across the Unknown* as a gift from a friend. He glanced through it. His eye was caught by the final chapter—'I Bear Witness.' Before he had finished the four pages of the chapter he understood why Mr. White had not himself altered the reference to Betty. And he had reached a decision—he would suggest that the usual notation be not made . . . There results a 'first' among the 433,050 sketches published in *Who's Who in America* since Mr. White's sketch first appeared nearly four decades ago—a waiving of accuracy to make possible a gesture recognizing the beautiful chapter in an unusual book . . ."

WILLIAM OLIVER STEVENS.

Correspondence

[The following letter, received by Mrs. E. W. Allison from Mr. A. T. Baird, author of *One Hundred Cases for Survival After Death,* illustrates the difficulties which an investigator finds in attempting to follow up on reports of "psychic phenomena" appearing in the public press.]

15 Delvin Road
Glasgow, Scotland
October 30, 1946

Dear Mrs. Allison:

For the last three months I have been trying to investigate an account of a "haunted" ship at Grangemouth. A glowing article about it appeared in the Sunday *Post,* written as if the reporter himself had actually witnessed the phenomena. When I tracked him down, however, he confessed that he had learned about the haunting at second hand from one of the ship's crew—a pantryman. I went after this man, and when I found him he admitted he had "seen nor heard no ghost himself," but he said he had gotten the story from his captain, who had seen ghosts on the ship on several occasions. This officer, in turn, said that this was not true; he had been told about the ghosts by a previous skipper, now in retirement. The retired skipper also denied such an honour—he had first heard the story in a public house!

And so exploded an investigation, begun so hopefully with the intention of forwarding it to the A.S.P.R. Of course our critics aver that *all* tales of psychic phenomena fade into nothingness, as this one did, when they are thoroughly investigated. We know that this is true of the majority, but the critics ignore the residual minority, so well founded.

Yours sincerely,

Alexander Baird

THE JOURNAL OF THE AMERICAN SOCIETY FOR PSYCHICAL RESEARCH

Responsibility for the contents of any article appearing in the JOURNAL rests entirely with the contributor and not with the A.S.P.R.

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CONTENTS

Annual Meeting

The Annual Meeting of the Voting Members of the American Society for Psychical Research, Inc., was held on January 28, 1947, at the Rooms of the Society. The President, Dr. George H. Hyslop, presided at the Meeting. The following Members were present: Mrs. E. W. Allison, Mrs. Valentine Bennett, Mr. Arthur Goadby, Mrs. Lea Hudson, Mrs. Lawrence Jacob, Mr. Gerald L. Kaufman, Mrs. Richard L. Kennedy, Jr., Dr. Gardner Murphy, Miss Margaret Naumburg, Mr. William O. Stevens, Miss Gertrude O. Tubby, Mrs. Henry W. Warner, Mrs. E. D. Wenberg, and Mrs. John J. Whitehead, Jr.

The following Trustees were re-elected for a term of three years, ending January, 1950: Dr. George H. Hyslop, Mrs. Lawrence Jacob, Dr. Gardner Murphy, and Mrs. John J. Whitehead, Jr. Mr. Richard L. Kennedy, Jr. and Mr. William Oliver Stevens were elected Trustees of the Society to fill vacancies caused by the resignation of Mr. Lawson Purdy and Mr. H. Addington Bruce.

At the Meeting of the Board of Trustees, which took place immediately after the Annual Meeting, the following officers of the Society were elected for the year 1947: President, Dr. George H. Hyslop; First Vice-President, Dr. Gardner Murphy; Second Vice-President, Mrs. Lawrence Jacob; Treasurer, Mr. Richard L. Kennedy, Jr.; Secretary and Assistant Treasurer, Mrs. E. W. Allison.

At the Meeting of the Board of Trustees held on February 26, 1947, the President appointed the following Chairmen of Committees

to serve for the year 1947 and empowered them to select the members of their respective committees:

RESEARCH COMMITTEE:

Dr. Gardner Murphy, *Chairman*
Dr. Waldemar Kaempffert
Dr. E. J. Kempf
Dr. Margaret Mead
Mr. Seymour Newman
Dr. Adelaide R. Smith
Dr. Montague Ullman
Dr. J. L. Woodruff

FINANCE COMMITTEE:

Mr. Richard L. Kennedy, Jr., *Chairman*
Mr. Gerald L. Kaufman
Mr. Lawson Purdy
Mr. Harold W. Updike

PUBLICATIONS COMMITTEE:

Mrs. E. W. Allison, *Chairman*
Mrs. E. de P. Matthews
Dr. Gardner Murphy
Miss Margaret Naumburg
Dr. J. B. Rhine
Dr. G. R. Schmeidler
Mr. William Oliver Stevens
Miss Signe Toksvig

MEMBERSHIP COMMITTEE:

Mr. William Oliver Stevens, *Chairman*
Mrs. Henry W. Warner (*Chairman of Sub-Committee on Lectures and Teas*)
Mrs. Lawrence Jacob
Mrs. Richard L. Kennedy, Jr.
Miss H. R. Meade
Mrs. John J. Whitehead, Jr.

Obituary

Just before going to press we learned with deep regret of the death of Whately Carington, on March 2, 1947, at his home in Sennen Cove, Cornwall, England. A full appreciation of his work in psychical research will appear in the July issue.

Rorschach Variables in Relation to ESP Scores[1]

GERTRUDE R. SCHMEIDLER

Abstract: Nine ESP runs were conducted in college classrooms, by a group method, with each of 250 subjects. The Rorschach method of personality diagnosis, in the form of a group test, was administered to the same subjects. Using the Munroe Inspection Record as a criterion of adjustment, it was found that the well-adjusted tended to score above chance if they believed in the theoretical possibility of ESP, and below chance if they rejected this possibility, while the poorly adjusted, in both of these categories, scored about at chance. Finer analysis of Rorschach records found more specific indices of adjustment factors related to scoring success.

Introduction

While conducting the ESP experiments that have been reported in this JOURNAL (8, 9, 10, 11), I have often, looking at the data of subjects whom I knew, been tempted to say about a poor score, "Isn't that just like him?" or, "What could you expect?"

Talking to others who have worked in psychical research for many more years than I, and reading the speculations of veteran research workers, have strengthened my feeling that somewhere in the personality of the subject—if we could only put our finger on it!—lay a clue to his capacity for paranormal impressions in any specific situation.

Many examples have been given of psychics who do their best work under special conditions, or with certain sitters. But even with run-of-the-mill subjects, guessing at a half-mechanical task like the order of a deck of ESP cards, there seems to be the same sort of congruence between personality patterns and success.

Let me give some examples of the variety of patterns that may appear. One of my subjects was a gay young woman who had a great zest for anything that was "fun" and that offered a change, but who, at the same time, seemed unsure of herself; she gave the impression of reaching out to adventure and then pulling back to safety. She lived with her stable, highly respectable and domineering mother; and even in her early thirties, seemed still to be in a stage

[1] Dr. Gardner Murphy, Chairman of the A.S.P.R. Research Committee, has discussed with me every step of this procedure; and I should like to acknowledge here both my gratitude to him for his kindness and my great debt for his wise advice. The research reported here was begun in October, 1945, and has continued until the time of writing (February, 1947). Gathering, analyzing and interpreting this material comprised the greatest part of my work when Research Officer (Jan. 1946 to Feb. 1947) of this Society.

of adolescent revolt. During the time that I knew her, she accepted a responsible but poorly paid position and was extremely competent at it; but she left it after a year to take another position which was at least as difficult, and offered even less security. She married impulsively a man she hardly knew; and separated from him after only a few weeks. When I asked her to act as an ESP subject, she was delighted with the idea; arranged an appointment readily; entered into the spirit of the experiment; seemed to enjoy herself; and made an outstandingly high average. A short time later, feeling that perhaps she would be a "good" subject, I asked her to have another try at guessing the cards. This time she agreed with far less enthusiasm, and acted as subject only after repeated requests from me. This second time, her average was poor.

At that time, the drop in score puzzled me. If ESP scores reflected paranormal ability, then one might expect that a person who did very well in one session should do well in the next session too. Now, though perhaps this is only being wise after the event, the low scores in her second session seem, for this subject, a natural sequel to initial high scores. Both in her marriage and in her work, during the two years I knew her, she had entered a new field with great enthusiasm and achieved very quickly the results that she wanted. But she turned away from both as if there were something within her that prevented her from solid, long-term achievements; and in the same way she seemed half-afraid of ESP—after she had done so well at it as to imply that she might have real paranormal ability.

There was another subject who was something of a professional dilettante. In early middle age, he was still studying in a university, as he had been studying in universities for all his adult life. As soon as he approached competence in a field, he shifted to taking courses in another field; he knew enough about a great many subjects for brilliant showy conversation, but not enough for critical or creative scholarship. He had had paranormal experiences and was proud of them; when he heard that an ESP experiment was being conducted, he asked me if he might act as subject "to see if he could make a good score." I was delighted to find someone who actually wanted to be an ESP subject. But as we were discussing procedure, I began to get the impression that he meant literally what he had said: he wanted to show me that he could make a single high score. I suggested firmly that a high *average* was necessary with material like ESP cards; that long-sustained high scores were the only dependable kind. But there was some doubt in my mind as to whether he agreed with me out of conviction or merely out of courtesy. In the experiment, the scores on the first seven runs averaged only 4.4, but on the eighth run he scored twelve hits out of twenty-five. This was

the highest single score any of my subjects had yet obtained; and I made the mistake of telling him so. He was delighted; but his interest in the experiment dwindled visibly; his hits dropped to six out of twenty-five on the ninth run; and I could not persuade him to complete the ten runs of the session. Apparently an unsubstantial, but spectacular showing was enough for him in ESP as in his studies; it seemed as if he did not wish to maintain a high level of achievement, but only to show that he could reach it.

When I conducted group ESP experiments with students whom I had been teaching for several months, there were many whose ESP scores lay in the direction that I had expected. An attractive and competent girl, who contributed to classroom discussion without ever talking too much, whose grades varied between B and A, who was well-liked but never ran for college offices, had an ESP score that was above chance, but not spectacularly high. A thin, intense, serious young man, who was not very bright but worked so hard that he maintained a C average, and who conducted himself quietly and well, classified himself as a "goat"[2] and scored far below chance. There was a colorless, conservative young woman, who never spoke in class unless she was asked a question, who seemed to hold herself aloof from the other students and never showed a spark of interest in the subject matter of her routine, adequate papers. She classified herself as a goat, and scored at chance, with little variation from one run to the next, as if she had beeen holding herself aloof from the experiment, just as she did from other college activities.

Perhaps the most talkative of my students was a tall, fat, boy. Usually his comments were extremely apt; but it was clear that even when he had nothing to say, he would rather raise some irrelevant point or far-fetched criticism than be quiet for a whole class period. His gestures were awkward, and his clothes badly chosen. Most of his reports were outstandingly good, but occasionally he would submit a mediocre one. He was studying to be an accountant, but told me he wanted to be a doctor or a psychologist. He gave the impression of not being at peace with himself. He had too much "push" or drive; and seemed to be unable to use his ability effectively because he was trying too hard to make an impression, or to assert his independence, or to get ahead. When he emphasized to me, before the experiment, that he was very much interested in ESP, I felt sure that he would not be able to score well;

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[2] "Goats" were the subjects who were convinced that ESP could not occur under the conditions of the experiment; "sheep" thought that ESP might occur. It was predicted that goats would score below chance at ESP, but that sheep would score above chance.

and in fact his scores were considerably below chance. A psychologist might call him "ego-involved" instead of "task-involved."

There were many other students whose ESP scores seemed to reflect their personalities; who seemed to handle the ESP situation in the same way that they handled the affairs of everyday life. With these impressions, reinforced by the opinions of so many veteran investigators of psychical phenomena, it seemed worth while to begin a large-scale study of the relationship between personality factors and paranormal ability.

The next question was the technical one of how to bring the problem from a vague personal hunch, supported by case studies (and hostile critics could always say to this, "But how about the cases you *didn't* describe?") to the scientific domain of demonstrable fact. We should have to have two objective tests: one of personality, and one of paranormal ability. If we administered these tests to large groups of subjects and found consistent, repeatable relationships between them, we should be on solid ground.

What tests should be made? Here there was real difficulty. What we wanted was clear enough; a good test should: (1) be easy to administer, so that we could collect enough cases to give a sound basis for generalization; (2) permit easy and objective scoring so that we could be sure of our data; (3) give a fair statement of what it aims to test (not like the earlier "personality tests," which actually showed, in many cases, only the subject's ability to outguess the examiner); (4) give more information about the problem than was needed for the particular point of inquiry, so that when we completed the research, we could have a broader perspective than when we began it.

It will hardly be necessary to say that the perfect test for either personality or paranormal ability has not yet been devised; that in evaluating tests for use in this research, it was necessary to balance weakness in some of these criteria against strength in others. Whenever time allowed, I have tried to give a "battery" of tests instead of a single one. But since there are good tests, even if not perfect ones, for both personality traits and paranormal ability, it seemed wiser to go ahead with the search than to wait for perfection.

The test for the paranormal which was used was the familiar one of guessing ESP symbols. This is not difficult to administer in a classroom; and scoring is objective. The question whether an ESP score affords a fair measure of a subject's telepathic or clairvoyant or precognitive ability is harder to answer. Perhaps no test can. Sometimes a striking report of a spontaneous case comes from a mature person who says that he has never had a similar experience; and subjects who make high scores in one Carington-type experi-

ment may make low scores in the next. Although such observations raise the problem whether any single test of paranormal ability will be valid, two factors argue in favor of continued use of ESP runs. The first is that various subjects have been reported who maintained high ESP scores for long periods of time, and also showed other signs of paranormal ability (2, 6, 7, 12); apparently for these subjects, ESP guesses are a valid test. In the second place, it was from experiments with ESP cards that this investigation emerged; and it seemed logical to continue with the same method.

Can we get any information from an ESP test in addition to the single figure representing a subject's total score? Other possibilities are present but limited, for the rich opportunities for symbolism or qualitative accuracy which other tests offer are almost entirely lacking. But we may look for displacement, as in the case of B. S., the subject who so often seemed to be guessing one ahead of his target (12), and the variation in score from one run (or one part of a run) to the next may be studied. Thus, while a series of ESP runs is not to be considered a completely satisfactory test of paranormal ability, it probably offers, by and large, the best possible test we can use, since its excellence in the first two test criteria overshadows its comparative weakness in the latter two.

In evaluating personality tests, the Rorschach (1, see also discussion in 5) seemed the best for our purpose. Running through the four criteria listed above, we find:

(1) There is an accepted, standard procedure for group administration. And though this procedure is time-consuming, taking about an hour, it is easy to find subjects for it because of widespread interest in the test.

(2) Scoring methods are probably the Rorschach's weakest point, because the scoring is slow, and depends to some (not completely determined) extent on the judgment of the examiner. We felt, however, that the scoring categories were sufficiently objective to make the test usable, especially with the safeguard against bias provided by the rule that scoring a subject's Rorschach must always be completed without knowledge of his ESP scores.

(3) The Rorschach is one of the best tests of personality, as evidenced by its increasingly wide use in clinic and college, and in the armed forces during the war, and by the growing number of psychologists who employ it. It compares favorably with such other valid tests as the Thematic Apperception Test, and is at present far more objective in its scoring.

(4) It essays to show the basic organization of the subject's personality structure, and thus gives extremely rich material for personality research. One of its outstanding characteristics is

that there are no "right" or "wrong" answers. Each subject can make of his responses the unique pattern which represents himself alone; and thus the data are not forced into predetermined and limiting patterns.

The problem of where to find subjects offered little choice. Ideally, we should have chosen two groups to be studied and contrasted: those who show much paranormal ability, and for a control, those who show little. But if we had tried to put this plan into practice, it would have taken many months to find enough subjects who think they have paranormal ability and who would be willing to take part in such an experiment; and then we should have had to take even more time to weed out of this group the ones who deceived themselves into claiming more of such ability than they had. A more practical approach was to make a random selection of subjects who were readily available, on the assumption that some would show considerable paranormal ability.

My own classes were always eager to take part in the experiment, and acted willingly as subjects. The plan of the research helped in enlisting other classes: several instructors in psychology felt that their students would gain enough from taking the Rorschach and having it interpreted to them, to justify the use of class time for research in parapsychology.[3] The number of subjects was limited only by my own capacity for giving these tests, since each individual Rorschach took, on the average, about half an hour to score.

Procedure

The data to be reported here were obtained in eleven group experiments, which gave a total of 303 usable records. One of these experiments was conducted by Gardner Murphy, using the members of his own class as subjects; three were performed by myself on my classes; and I conducted the remaining seven on classes borrowed for the occasion. All subjects were students in psychology in the New York City colleges. All experiments followed the same basic procedure and gave approximately similar results. They will be presented as a unit since it would serve no useful purpose to present them separately.

The procedure of these group experiments was parallel to the method that I have used with individual subjects and have already

[3] I am grateful to Mr. William Triebel for helping make the arrangements, and to Dr. Rudolf Ekstein, Dr. Genevieve Chase, Miss Edith Wladkowsky, Dr. Eugene Hartley, Miss Ruth Berenda and Miss Virginia Staudt, who cooperated so generously by giving me from two to five hours of their classes' time for this research.

reported in this JOURNAL (8, 9). Lists of 25 ESP symbols were made by an assistant, the order of the symbols being obtained by a random method of selection. These lists, used as targets by the subjects, were not known to me until after the guessing was completed. The person who made the lists, who alone had seen the order of the symbols, was never present at the experiment. The lists were concealed from the subjects until their guesses had been made. With all these precautions, there was no possibility of sensory cues to guide the correct guesses. The average number of correct guesses to be expected from each run was five.

The basic procedure, which all the separate experiments had in common, consisted of distribution of record sheets to the members of the class, and of a few introductory comments by the experimenter. The subjects were then told about the nature of the symbols, the number of guesses in a run, and the fact that three runs (75 guesses) should be taken as a unit. We emphasized the impossibility of "figuring out" the correct symbol by any intelligent method. Subjects were told to make guesses in sequence, rather than by skipping; and they were warned against changing a symbol once they had written it.

When they understood what was expected of them, each subject was asked, as in the earlier experiments, to characterize himself as a "sheep" or a "goat." A sheep was defined as a person who thought there might be some possibility of guessing the symbols with better than chance success; the goats were those who were convinced that any relationship between targets and guesses was coincidental. I made it a practice to draw a line on the blackboard, representing the continuum from *belief* that there would be a correspondence between guesses and targets, to *disbelief* in it. Labelling the center with a question mark, as shown below, I went on to say (in effect)

belief————?————disbelief

that of course the subjects to the left of the question mark should be called sheep. But by my definition, the subjects at the center, and even the ones toward the right of the continuum, were also sheep. It was only those *at the extreme right,* who had no reservation, and no shadow of doubt in their minds, who were goats.

For one class, I omitted to draw this continuum, and to give these final instructions. Such an extraordinarily large proportion of these subjects wrote "goat" at the top of the record sheet that I reexamined the procedure, remembered the omission, and in the next class period told them about it. Several of the "goats" said that they would have called themselves sheep under the full instructions. It

therefore seemed best to discard all the "goat" records from this class.

Once the subjects had written either "sheep" or "goat" after their names on the record sheets, they began their ESP guesses. When all had completed the first three runs, the envelope containing the target lists for these runs was opened, and the lists were read to the class. The subjects then guessed the fourth, fifth and sixth runs; and again when they were finished, the targets were read to them. We followed the same procedure for the seventh, eighth and ninth runs; and this completed the ESP experiment proper.

A group Rorschach Test was given (sometimes before, sometimes after, administration of the ESP test) to all these classes by projecting upon a screen ten slides corresponding in order to the ten cards used in the individual test. A booklet was provided each subject to record what he saw in the slides. In addition, various minor tests were given or questions asked of the students. It was hoped that these supplementary tests would serve two purposes: by such slight changes of procedure from one group to the next, the experiment would not become too much a matter of routine; and in addition, the tests might suggest hints about new directions for research. None of these supplementary tests have given sufficient data to justify a report, but some are promising enough to make me expect to continue using them.

All the Rorschach records of the subjects reported here were scored without knowledge of the results of the ESP runs, so that any bias of the experimenter cannot have affected the data. All ESP scores have been double-checked.

Results

The first question we put to the data was whether they confirmed our previous hypothesis about sheep and goats. We may call this:

Hypothesis 1: Sheep will tend to make ESP scores above chance, and goats will tend to make ESP scores below chance.

This hypothesis has been fully discussed in previous articles (8, 9, 10, 11). Six experiments with individual subjects and one group experiment had already been performed and all had tended to confirm it. It is based on four premises: (1) that ESP occurs; (2) that a subject's attitude toward the ESP task (whether it be conscious or unconscious) will affect his scores; (3) that, by and large, subjects who believe that ESP may occur would like to make good scores in an ESP experiment; (4) that subjects who commit them-

selves before the experiment to the unqualified conviction that the idea of the experiment is nonsensical, will, by and large, hope to disprove the ESP hypothesis by their data, and will thus unconsciously aim at making incorrect guesses. (More sophisticated subjects, of course, would aim at the "chance" score of five successes in twenty-five guesses.)

Table I summarizes the data of all the students who participated in the current experiments, and who classified themselves as either sheep or goats. It shows a small difference in the predicted direction. The difference is marginally significant, and tends, as did all our previous research, to confirm the first hypothesis. We may speculate that these deviations are so much smaller than in the individual experiments because of the more impersonal atmosphere of the classroom, which—especially with a visiting experimenter—might make the students feel more remote from the situation; thus motivation for either high or low scores would be lessened, and the average would be nearer chance.

TABLE I

	SHEEP			GOATS		
	Subjects with no Rorschach	Subjects with Rorschach	Total	Subjects with no Rorschach	Subjects with Rorschach	Total
Number of Subjects	29	117	146	24	133	157
Number of Runs*	255	1049	1304	217	1197	1414
Deviation	+9	+111	+120	—21	—127	—148
Mean	5.04	5.11	5.09	4.90	4.89	4.90

* Although all subjects were asked to complete nine runs, there were a few who did not. Their records have been included, and thus the number of runs is not always nine times the number of subjects.

When we come to analysis of the Rorschach records, the richness of the material creates difficulties. One conventional method of listing the scoring categories for the Rorschach gives 66 items (1); and even this has been criticized for omitting some of the most important scores. It has seemed to me that it would be absurd to try to use so many categories when working with hundreds of records; and I have in general followed the much simpler scheme proposed by Dr. Ruth Munroe, which she calls the Rorschach Inspection Record (3).

But in addition to the mechanical listing of high and low scores for various categories, it is possible to approach the Rorschach from a more directed point of view: to put specific questions to a record, such as, "How well-adjusted is this subject?" or: "Does he respond freely to new situations, or does he look for security by restricting himself to a narrowly conventional, 'correct' approach?"

This is, in part, what I have done. On the basis of each group of experiments, new hypotheses presented themselves as to conditions for high or low ESP scores. Subsequent experiments would give an opportunity to test each such theory; and they in turn would give rise to new ones. The Rorschach material will therefore be presented from two points of view: whether it confirms an impression that grew out of previous work (Tables II and III) and whether the cruder listing of items brings new points into prominence (Tables IV and V and Appendices I, II, and III).

The reader may remember that some of the subjects described earlier in this report were students in my classes. These classes, tested in the spring of 1945, were all given Rorschachs as well as ESP tests, and it was from comparison of these records that our second hypothesis arose. (The data are not cited here because I had seen the ESP scores of the students before scoring the Rorschachs, and there might, therefore, have been bias in the Rorschach scoring.) It seemed to me that many of the badly adjusted students made extremely poor ESP scores (*i.e.*, near chance expectation), while most of the well-adjusted youngsters did nicely (*i.e.*, the sheep scored above chance and the goats scored below chance) in the ESP tests. The second hypothesis, therefore, is:

> *Hypothesis II*: Sheep who are well-adjusted will, on the average, make higher ESP scores than sheep who are not; and goats who are well-adjusted will have lower ESP scores than the other goats.

It is fortunate that a single Rorschach score for good or poor adjustment has recently been devised by Dr. Munroe (3, 4). It is derived, essentially, by listing some twenty significant Rorshach categories, and giving one or more checks to each, whenever a subject deviates from "normal" in that category. Adding up a subject's checks gives a summary statement of how deviant he is in these more diagnostic categories; and thus the total of checks is probably as good an indicator of his adjustment as any one figure, obtained from a single test, could be.

But where does good adjustment stop, and bad adjustment begin? Any such boundary is arbitrary and subject to considerable error; but I have set it (following certain implications of Dr. Munroe's

discussion) at a point which divides the group into approximately a ratio of 5:3 for well-adjusted in relation to poorly-adjusted.[4] This may seem rather severe to our college population, and, of course, does not imply that the Rorschach predicts that three out of eight students will be sent to prison or a mental hospital, or otherwise come to a bad end. But perhaps it is not too far out of line to say that at least a third of the students are so mixed up in their own problems that they are likely to approach a new, non-compelling situation like a class ESP experiment from a highly personal point of view. Thus we could not expect them to accept without reservation the simple motivation of the experimenter's "Now try to guess the list!" nor even the implied motivation of "Now prove by your failure that the experiment is a lot of nonsense!" And if the subjects who are preoccupied with their own difficulties do not accept such motivation, we get a clearer differentiation between sheep and goats when those cases are omitted.

TABLE II

Summary of ESP scores according to Hypothesis II: that well-adjusted sheep will make higher ESP scores than poorly-adjusted sheep, and that well-adjusted goats will make lower ESP scores than poorly-adjusted goats.

	SHEEP		GOATS	
	Well-adjusted	Poorly-adjusted	Well-adjusted	Poorly-adjusted
Number of Subjects	74	43	83	50
Number of Runs	665	384	746	451
Deviation	+108	+3	—159	+32
Mean	5.16	5.01	4.79	5.07

When scores of well-adjusted sheep and well-adjusted goats are compared, C.R. diff = 3.55; P = .0002.

This in fact occurs. Table II shows the ESP scores of well-adjusted and poorly-adjusted sheep and goats. Poorly-adjusted sheep scored almost exactly at chance (Mean = 5.01); poorly-adjusted goats were slightly above chance (Mean = 5.07). Thus

[4] The criterion actually used was ten checks or less for good adjustment, and eleven checks or more for poor adjustment. This number was determined on the basis of the subjects from the spring of 1945, and we may therefore take the entire current series of data as a test of the second hypothesis.

the entire negative deviation of the goats of Table I, and virtually the entire positive deviation of the sheep, was contributed by the well-adjusted subjects. The difference between the well-adjusted sheep and the well-adjusted goats is highly significant statistically.

Although it is always gratifying to have an experiment come out as predicted, these data offered no temptation to look at Hypothesis II as a final statement of the problem of ESP. There was a staggering number of individuals whose scores did not follow the general trend. This would, I think, have been predicted by anyone who had studied psychical phenomena. We might say about a sensitive that no one could expect her to give a good sitting during a certain period because at that time she was so disturbed by her personal worries (and this would correspond to the poor ESP scores of the subjects whose Rorshachs showed personality difficulties). But no one would suggest that good psychiatric adjustment should be the crucial test of a successful psychic, and just as surely our data do not mean that all subjects who are well adjusted will score better than chance at ESP and that all maladjusted subjects must score badly.

I suggest, therefore, that we interpret Hypothesis II with due caution, and with proper reservations. We are entitled to say that evidence from a fairly large number of cases suggests that well-adjusted subjects tend to get good (*i.e.*, non-chance) ESP scores. But then we should add that this rule applies to groups, and does not predict that each individual well-adjusted subject will score well. In addition, there is the difficulty that "good adjustment" refers to a complex pattern of behavior which is not easy to define, and which will vary from one situation to the next. Perhaps certain aspects of good adjustment are significant for us, and others are irrelevant. In other words, this hypothesis offers, at best, only a first approximation to a theory of personality in relation to the paranormal.

As for the second aspect of these same data: the average ESP score of the subjects with poor adjustment was not far from chance. Does this mean that subjects with poor personality adjustment show no paranormal ability? Or does it imply that some will do well and others will do badly, making it impossible to predict about the group as a whole?

My own impression is that the latter alternative comes closer to the truth; and that we should be able to separate high-scorers from low-scorers in the poorly adjusted group when we know more about them. Two contrasting cases come to mind to support this point, both relating to subjects who, at the time of the experiment, were college sophomores with severe personality problems.

The one with poor ESP scores was a very quiet youngster, whose voice seldom rose above a whisper and whose handwriting was tiny

and cramped. He dressed neatly; he came to class on time; his grades were good; and his surface adjustment was adequate. But he was an unhappy, dissatisfied boy, unsure of his ability or his choice of a career, whose self-restraint was so excessive that it seemed to hamper him at every turn. Perhaps it would not be too far from the truth if we guessed that the factors of restraint related to his maladjustment, also kept him from the paranormal contacts required for success in ESP.

The other boy was as untidy as the first was neat. He was considered brilliant, and had a talent for poetry that won the respect of his professors. But he seemed at the mercy of his impulses, doing such unpredictable things as emptying a glass of water over a girl who made a joke that annoyed him, or leaving college for a week while classes were in session because he wanted to see an art exhibit in another city. The freedom with which he flung himself into activity, and with which he gave scope to his creative powers, was extraordinary; was it also a condition for the release of paranormal ability? Whatever the reason, in spite of his undoubted poor adjustment, his ESP scores were high and he reported spontaneous telepathic experiences.

But to return to our Rorschach. As the first batch of data was being tabulated, it seemed to me that there was a very large number of exceptions to the rule that good adjustment was tied with good ESP scores; and also that a particularly large number of these exceptions had the personality trait that psychologists call "constriction." This might show itself in a variety of ways, through all of which runs a common thread of extreme self-control; a constricted person might have a colorless personality, or a highly conventional, over-correct approach to problems, or he might be inflexible. Constriction is defined in the Rorschach in terms of an unusually high percentage of responses that rely exclusively on the formal aspects of the material; the usual formula is to say that a record which contains half, or more than half, of such "F" responses is constrictive ($F\% \geqq 50\%$).

This impression of the frequency of poor ESP scores among constricted subjects led to the formulation of a refinement of the previous theory, namely:

> *Hypothesis III*: Well-adjusted and non-constricted sheep will tend to have higher ESP scores than sheep who are poorly-adjusted or constricted; and well-adjusted, non-constricted goats will tend to have lower ESP scores than goats who are poorly-adjusted or constricted.

This hypothesis is based on the data collected before May, 1946.

Table III will present separately the cases obtained before and after the theory was stated.

TABLE III

Summary of ESP scores according to Hypothesis III: that well-adjusted and non-constricted sheep will tend to have higher ESP scores than sheep who are poorly-adjusted or constricted; and that well-adjusted, non-constricted goats will tend to have lower ESP scores than goats who are poorly-adjusted or constricted.

A. Sheep

	Well-adjusted and non-constricted			Poorly adjusted and/or constricted		
Date of Expt.	Oct. '45 to Apr. '46	July '46	Total	Oct. '45 to Apr. 46	July '46	Total
No. of S's	29	29	58	40	19	59
No. of Runs	261	260	521	357	171	528
Dev.	+102	+52	+154	—23	—20	—43
Mean	5.39	5.20	5.30	4.94	4.88	4.92

B. Goats

	Well-adjusted and non-constricted			Poorly adjusted and/or constricted		
Date of Expt.	Oct. '45 to Apr. 46	July '46	Total	Oct. '45 to Apr. '46	July '46	Total
No. of S's	25	49	74	33	26	59
No. of Runs	224	441	665	298	234	532
Dev.	—69	—90	—159	+33	—1	+32
Mean	4.69	4.80	4.76	5.11	5.00	5.06

When ESP scores of the well-adjusted, non-constricted sheep and goats of the July, 1946 series, are compared, C.R. diff = 3.33; P = .0004.

Only ten of the subjects of the July group were both constricted and well-adjusted; and, of course, this number is too small to offer an adequate test of the relation of constriction to ESP success. Out of the ten subjects, the five sheep scored exactly at chance, but the five goats scored below chance (Dev. = —21; M = 4.76). These first data must, therefore, be considered ambiguous in respect to constriction, although they tend to confirm Hypothesis III as it was stated.

There were indications in the July records that the third hypothesis had been stated too narrowly and that two additional factors

should be included as contra-indicators for ESP. One of these factors implies a rigidity or self-restraint in the subject's use of his own creative ability. This score is given if more than half of the M (human movement) responses are like the frozen movement of a statue, or the tensed position of a person who is poised but motionless. Munroe indicates the score as "Mr" where "r" stands for "rigid."

The other factor which seemed prominent in these records was the "quantity ambition" or intellectual ambition of many of the subjects who had low ESP scores. This refers, not to ambition as such, but to *intellectual* ambition, a kind of showing-off, or forcing one's ideas forward. It is indicated by a large number of responses, and I have set the criterion number at 30 or higher in the group record (where time of administration is limited) and at 50 or higher in the individually administered protocol.

When so many factors appeared, in a cursory examination of the Rorschach scores, to have a possible relation to ESP success, it was obvious that a more systematic examination would be needed. I therefore made a table which is summarized in Appendix I, listing each of the Rorschach scores and the ESP score for each of the 250 subjects described in this report. The summary table gives the ESP average and certain other data for the various Rorschach scores.

Glancing down the right-hand column (ESP Mean) in this Appendix table, it is obvious that there is a hodgepodge of high and low ESP scores in which it is difficult to see a pattern. I have attempted to find some order in this confusion by pulling out of the table the subjects whom we expect to have poor scores. In a first attempt, for example, all cases which showed poor adjustment or constriction were excluded, and the table was reconstructed with the records of the remaining subjects. The poor ESP scores of the subjects whose Rorschachs showed either $R \geqq 30$ (thirty or more responses) or Mr (rigid movement) then suggested another reconstruction of the table. I drew it up again with those cases omitted. After several such jugglings, in which various categories were taken out or put back in, the table presented in Appendix II emerged. This table represents my final attempt until more data are available. None of the intermediate forms are presented here.

In this final listing, seven categories have been selected as showing an association with poor ESP scores. They include the three described above, two others which may have a logical association with them, and a pair which seem to represent a contrasting personality pattern. The four new items are:

Total Movement $\geqq 60\%$, *i.e.*, an extremely large percentage of responses describing movement. Such a score is taken to mean an extremely active "inner life." It may carry something of the same significance for our purposes as "quantity ambition." Both have the connotation that a subject may force himself to have ideas even in situations where it would be better for him to be relaxed instead of working so hard.

Absence of both color shock and shading shock (evidence of emotional shock when confronted with color and shading). Under Munroe's generous definition, such absence of shock means that a subject is remarkably passive or unmoved under conditions to which most people will respond more strongly. Like constriction, or rigidity of thinking, it implies a tendency to shut oneself off from experiences instead of being a willing participant in them.

C+ and CF+ (response to color without use of form, and response to color dominant over form), the last two scores, are based on an over-reaction to color at the expense of form. They imply extreme impulsiveness.

These seven categories tend to be associated with poor ESP scores, even in the records where one of these appears without the other six categories (Appendix III).

Decision as to which factors to include in this list depended on two considerations: how many subjects with a certain Rorschach score had poor ESP scores; and how poor the ESP scores were. Thus, when the only subject who showed very marked shading shock also did very badly at ESP, it seemed an inadequate basis for concluding that marked shading shock indicates poor paranormal ability. And again, when the twenty-four subjects with checks for "K,k" (undifferentiated shadings) had ESP averages that were low but in the predicted direction, it seemed safer *not* to draw any conclusion from their low mean scores.

It will be noted that when we exclude the records in which any of these seven scores occur, the remaining poorly-adjusted subjects score about as well in ESP as do the well-adjusted ones (for maladjusted sheep, Mean = 5.39 and for maladjusted goats, Mean = 4.86). This listing of specific factors, relating to *specific maladjustment,* offers therefore an alternative to Hypothesis II, relating to *total maladjustment.*

Two difficulties in the listing will occur to the reader. One is the point already mentioned: the doubtful categories, for which the scores are so few or so ambiguous that we cannot be sure whether the item should be used even provisionally as an indicator of poor ESP scores. The other difficulty is the uncertainty that attends

every research hypothesis: we cannot be sure, until we have more cases, whether any of our apparent relationships are due only to coincidence. With these points in mind, I can offer only as extremely tentative the following hypotheses.

Hypothesis IV: Sheep who are well-adjusted, not constricted, not inclined to impose rigid barriers on their creative thinking and not needing to impress others with their intellectual prowess, will tend to have higher ESP scores than other sheep. Goats with these characteristics will tend to make lower ESP scores than other goats.

TABLE IV

Summary of ESP scores according to Hypothesis IV: that sheep who are well-adjusted, not constricted, not inclined to impose rigid barriers on their creative thinking and not needing to impress others with their intellectual prowess, will have higher average ESP scores than other sheep. Goats with these characteristics will make lower average ESP scores than other goats.

	SHEEP		GOATS	
	Subjects expected to have high ESP scores according to Hypothesis IV	Other subjects	Subjects expected to have low ESP scores according to Hypothesis IV	Other subjects
No. of S's	44	73	62	71
No. of runs	396	653	557	640
Deviation	+167	—56	—173	+46
Mean	5.42	4.91	4.69	5.07

But if we summarize the tables presented in Appendix II and Appendix III, an alternative hypothesis would do as well, namely:

Hypothesis V: Sheep who are not constricted, not inclined to impose rigid barriers on their creative thinking, responsive to change, not needing to impress others with their intellectual prowess, not too overactive in their inner life, and not overimpulsive, will have higher average ESP scores than other sheep; and goats with these characteristics will tend to have lower ESP scores than other goats.

TABLE V

Summary of ESP scores according to Hypothesis V: that sheep who are not constricted, not inclined to impose rigid barriers on their creative thinking, responsive to change, not needing to impress others with their intellectual prowess, not too overactive in their inner life, and not over-impulsive, will have higher average ESP scores than other sheep; and goats with these characteristics will tend to have lower ESP scores than other goats.

	SHEEP		GOATS	
	Subjects expected to have high ESP scores according to Hypothesis V	Other subjects	Subjects expected to have low ESP scores according to Hypothesis V	Other subjects
No. of S's	51	66	71	62
No. of runs	459	590	638	559
Deviation	+204	—93	—175	+48
Mean	5.44	4.84	4.73	5.09

Hypotheses IV and V are equally appropriate at this writing; they make equivalent separations of ESP scores, and only further research can decide between them. But because it is more specific, the latter may, if it is verified, be more fruitful. It indicates that any one of three patterns is unfavorable to good classroom ESP scores: extreme intellectuality, extreme impulsiveness, or extreme reserve. This is susceptible to experimental test, both with the Rorschach and with other methods. Like every other working hypothesis, it is by such tests that it must eventually stand or fall.

Meanwhile, it may find some support from the impressions of mediumistic sittings. Many investigators feel that a psychic sometimes receives an isolated, accurate scrap of information. If she impulsively jumps to a conclusion about its meaning, and reports that conclusion, she may be wrong. Perhaps it was such impulsive over-interpretation of impressions which made some of the CF+ subjects (over-reacting to color) get such poor ESP scores?

The other two terms, "intellectual" and "reserved," must have a curiously familiar ring to many readers. It is well-known that certain mediums find these qualities unwelcome in their sitters; and the uncharitable suggest that it is because the reserved and intellectual person is the one most likely to detect fraud. But if the data of this report are substantiated, the other possibility emerges: that these two traits, carried to an extreme, have the deterrent effect

on paranormal contact that so many psychics have been assigning to them for so long.

Summary

The Rorschach test of personality and ESP tests were given to 250 college students. Relationships between personality patterns and success in the ESP tests were described in the form of five hypotheses. The first three hypotheses proposed have been supported by the later investigations. Further research is needed to provide an independent test of the later hypotheses.

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Appendix I

ESP scores of subjects whose Rorschach records deviated from the average in certain significant categories.

A. Sheep

Rorschach Category*	Rorschach Criterion*	Number of Subjects	Number of ESP Runs	ESP Deviation	ESP Mean
Number of R	≧ 30	21	188	—33	4.82
Refusal	✓	0	—	—	—
"	✓ ✓	3	27	—7	4.74
W	+	29	261	+100	5.38
"	++	7	63	—11	4.83
"	+++	2	18	—19	3.94
"	- or - -	3	27	+3	5.11
"	BV or VB	7	63	+20	5.32
"	V	6	54	+15	5.28
"	B	0	—	—	—
Dd	+	7	63	—3	4.95
"	++	1	9	—3	4.67
S	+	10	90	+18	5.20
"	++	2	18	+3	5.17
Suc	1	3	27	+3	5.11
P	-	11	99	+11	5.11
O	BV or VB	4	36	—21	4.42
"	B	11	99	+11	5.11
"	V	1	9	—1	4.89
At, Sex	+	25	224	+50	5.22
"	++	5	45	—14	4.69
Range	-	26	234	—11	4.95
"	- -	6	54	+12	5.22
"	+	2	18	+5	5.28
"	(a)† -	3	27	—12	4.56
"	(b)† -	7	63	—15	4.76
"	(c)† -	21	189	—6	4.97
"	(c) - -	6	54	+12	5.22
"	(d) -	0	—	—	—

* These listings follow the criteria proposed by Munroe (3) except for R and total number of checks, where Munroe does not give any limiting number.

† (c) overlaps somewhat with (a), (b), and (e). When a subject's scores could be listed in either of two categories, they were included in both categories for this breakdown. In the summary, the subject was, of course, counted as only one case.

Rorschach Category	Rorschach Criterion	Number of Subjects	Number of ESP Runs	ESP Deviation	ESP Mean
Range	(e)†-	2	18	+7	5.39
(H + A):					
(Hd +Ad)	<2:1	8	72	+6	5.08
F%	+	20	180	−27	4.85
"	++	1	9	+3	5.33
"	-	7	63	+33	5.52
"	--	0	—	—	—
F	BV or VB	9	81	+3	5.04
"	B	2	18	+2	5.11
"	V	6	54	+7	5.13
"	E	3	27	+1	5.04
Shading Shock	None	30	270	+46	5.17
"	✓	76	680	+60	5.09
"	✓ ✓	10	90	+18	5.20
"	✓ ✓ ✓	1	9	−13	3.56
"	-	5	45	±0	5.00
"	--	1	9	+7	5.78
"	+	27	239	+35	5.15
"	++	8	72	+10	5.14
Fc	-	17	153	+30	5.20
"	--	8	69	+15	5.22
c	+	6	54	+19	5.35
"	++	1	9	−2	4.78
C'	+	7	63	±0	5.00
"	++	1	9	+1	5.11
K, k	+	19	171	+20	5.12
"	++ or +++	3	24	+12	5.50
M	+	13	117	−17	4.85
"	++ or +++	4	38	+1	5.03
"	-	23	207	+90	5.43
"	--	10	90	−15	4.83
"	B	1	9	+6	5.67
"	rd	2	18	+11	5.61
"	r	10	90	−21	4.77
"	d	1	9	+14	6.56
FM, FM: M	+	18	162	+10	5.06
"	++	6	54	−4	4.93
"	+++	1	9	±0	5.00
"	-	6	54	−31	4.43
"	--	0	—	—	—
m	+	29	261	+70	5.27
"	++	3	27	−3	4.89
Total Movement	+	50	449	+66	5.15
"	++ or +++	8	72	−27	4.62
"	-	1	9	+6	5.67
"	--	0	—	—	—

Rorschach Category	Rorschach Criterion	Number of Subjects	Number of ESP Runs	ESP Deviation	ESP Mean
Color Shock	none	24	216	+44	5.20
"	✓	76	683	−16	4.98
"	✓ ✓	17	150	+83	5.55
"	-	10	90	+14	5.16
"	- -	1	9	+11	6.22
"	+	33	296	−34	4.89
"	++	10	87	+28	5.32
Color Shock and	none	6	54	−8	4.85
Shading Shock	✓	38	342	+93	5.27
"	✓ ✓	51	458	−8	4.98
"	✓ ✓ ✓	19	168	+26	5.15
"	✓ ✓ ✓ ✓	3	27	+8	5.30
FC	-	24	216	+48	5.22
"	- -	19	168	−5	4.97
"	B	7	63	+9	5.14
CF, CF: FC	-	24	216	+34	5.16
"	+	14	123	−51	4.59
"	++ or +++	3	27	+9	5.33
C>1, Cn	+, ++, or +++	3	27	+8	5.30
Total Color	+	11	98	−10	4.90
"	++	3	27	+14	5.52
"	-	18	162	−21	4.87
"	- -	12	108	+39	5.36
Color: Movement	-	28	249	+32	5.13
"	- -	28	252	+21	5.08
"	- - -	14	126	−5	4.96
"	- - - with U	2	18	+11	5.61
"	+	2	18	+16	5.89
"	++	0	—	—	—
"	+++	0	—	—	—
Total Number of Checks	0 - 10	74	665	+116	5.17
"	11 - 24	43	384	−5	4.99
Total		117	1049	+111	5.11
B. Goats					
Number of R	≧ 30	12	108	+7	5.06
Refusal	✓	3	27	−3	4.89
"	✓ ✓	5	45	−10	4.78
W	+	36	325	−9	4.97
"	++	8	72	+8	5.11
"	+++	3	27	+5	5.19
"	- or - -	4	36	+6	5.17
"	BV or VB	7	63	+30	5.48
"	V	4	36	+12	5.33
"	B	1	9	+3	5.33

Rorschach Category	Rorschach Criterion	Number of Subjects	Number of ESP Runs	ESP Deviation	ESP Mean
Dd	+	9	80	−12	4.85
"	++	1	9	+12	6.33
S	+	13	117	−5	4.96
"	++	0	—	—	—
Suc	1	3	27	+7	5.26
P	—	29	262	±0	5.00
O	BV or VB	4	37	+15	5.41
"	B	9	81	−2	4.98
"	V	6	54	+22	5.41
At, Sex	+	26	234	−45	4.81
"	++	9	81	+24	5.30
Range	-	37	334	−46	4.86
"	--	1	8	−11	3.62
"	+	1	9	+11	6.22
"	(a)* -	6	54	−13	4.76
"	(b)* -	5	46	+6	5.13
"	(c)* -	26	235	−33	4.86
"	(c) --	1	8	−11	3.62
"	(d) -	1	9	+7	5.78
"	(e)* -	3	27	−8	4.70
(H + A):					
(Hd + Ad)	<2:1	9	81	−16	4.80
F%	+	17	153	+7	5.05
"	++	0	—	—	—
"	-	8	73	+11	5.15
"	--	1	9	−8	4.11
F	BV or VB	7	63	+13	5.21
"	B	4	36	+6	5.17
"	V	9	90	−1	4.99
"	E	4	36	−5	4.86
Shading Shock	None	48	433	+16	5.04
"	✓	76	683	−137	4.80
"	✓ ✓	9	81	−6	4.93
"	✓ ✓ ✓	0	—	—	—
"	-	7	63	−15	4.76
"	--	2	18	−5	4.72
"	+	41	368	−104	4.72
"	++	4	36	±0	5.00
Fc	-	28	253	−5	4.98
"	--	10	90	+3	5.03
c	+	9	81	+7	5.09
"	++	0	—	—	—

* (c) overlaps somewhat with (a), (b), and (e). When a subject's scores could be listed in either of two categories, they were included in both categories for this breakdown. In the summary, the subject was, of course, counted as only one case.

Rorschach Category	Rorschach Criterion	Number of Subjects	Number of ESP Runs	ESP Deviation	ESP Mean
C′	+	13	117	+4	5.03
"	++	1	9	+6	5.67
K, k	+	22	198	+20	5.10
"	++ or +++	2	18	—2	4.89
M	+	25	225	—26	4.88
"	++ or +++	3	27	—22	4.19
"	-	16	144	—25	4.83
"	- -	14	127	+19	5.15
"	B	2	18	—6	4.67
"	rd	4	36	—1	4.97
"	r	8	72	+25	5.35
"	d	3	27	—7	4.74
FM, FM : M	+	13	117	—18	4.85
"	++	6	54	+6	5.11
"	+++	1	10	+9	5.90
"	-	16	144	—9	4.94
"	- -	7	63	—15	4.76
m	+	27	242	—5	4.98
"	++	4	36	±0	5.00
Total Movement	+	62	557	—57	4.90
"	++ or +++	3	27	—22	4.19
"	-	0	—	—	—
"	- -	1	9	+4	5.44
Color Shock	none	23	207	+16	5.08
"	✓	85	765	—106	4.86
"	✓ ✓	25	225	—37	4.84
"	-	15	135	—62	4.54
"	- -	3	27	—21	4.22
"	+	26	234	—9	4.96
"	++	10	90	—26	4.71
Color Shock and Shading Shock	none	10	91	+14	5.15
"	✓	40	359	+33	5.09
"	✓ ✓	61	549	—154	4.72
"	✓ ✓ ✓	21	189	—26	4.86
"	✓ ✓ ✓ ✓	1	9	+6	5.67
FC	-	19	170	+5	5.03
"	- -	22	198	—26	4.87
"	B	8	72	—8	4.89
CF, CF : FC	-	39	350	—52	4.85
"	+	6	54	—22	4.59
"	++ or +++	4	37	+9	5.24
C>1, Cn	+, ++ or +++	6	54	+20	5.37
Total Color	+	18	163	—8	4.95
"	++	2	18	+14	5.78
"	-	16	144	—30	4.79

Rorschach Category	Rorschach Criterion	Number of Subjects	Number of ESP Runs	ESP Deviation	ESP Mean
Total Color	- -	10	90	+1	5.01
Color : Movement	-	39	350	—68	4.81
"	- -	19	171	—30	4.82
"	- - -	15	135	—12	4.91
"	- - - with U	2	18	+7	5.39
"	+	0	—	—	—
"	++	0	—	—	—
"	+++	1	9	+4	5.44
Total Number of Checks	0 - 10	83	746	—159	4.79
"	11 - 24	50	451	+32	5.07
Total		133	1197	—127	4.89

Appendix II

ESP scores of subjects whose Rorschach records do not show any of the following items: (1) R $\geqq$ 30; (2) F%+; (3) rigid M; (4) no color shock or shading shock; (5) Total Movement++, (6) CF+; (7) C+.

A. Sheep

Rorschach Category	Rorschach Criterion	Number of Subjects	Number of ESP Runs	ESP Deviation	ESP Mean
Refusal	✓	0	—	—	—
"	✓ ✓	3	27	—7	4.74
W	+	19	171	+98	5.57
"	++	2	18	+5	5.28
"	+++	0	—	—	—
"	- or - -	1	9	+6	5.67
"	BV or VB	3	27	+18	5.67
"	V	0	—	—	—
"	B	0	—	—	—
Dd	+	2	18	+12	5.67
"	++	0	—	—	—
S	+	3	27	+24	5.89
"	++	0	—	—	—
Suc	1	1	9	±0	5.00
P	-	1	9	+20	7.22
O	BV or VB	0	—	—	—
"	B	2	18	+10	5.56
"	V	0	—	—	—
At, Sex	+	9	81	+42	5.52
"	++	2	18	±0	5.00
Range	-	11	99	+44	5.44
"	- -	4	36	+18	5.50

Rorschach Category	Rorschach Criterion	Number of Subjects	Number of ESP Runs	ESP Deviation	ESP Mean
Range	+	0	—	—	—
"	(a) -	1	9	−3	4.67
"	(b)* -	3	27	+6	5.22
"	(c)* -	9	81	+41	5.51
"	(c) - -	4	36	+18	5.50
"	(d) -	0	—	—	—
"	(e)* -	1	9	+12	6.33
(H + A):					
(Hd + Ad)	<2:1	1	9	−3	4.67
F%	-	2	18	+25	6.39
"	- -	0	—	—	—
F	BV or VB	2	18	+17	5.94
"	B	1	9	+5	5.56
"	V	1	9	+5	5.56
"	E	0	—	—	—
Shading Shock	none	15	135	+88	5.65
"	✓	32	288	+122	5.42
"	✓ ✓	3	27	+7	5.26
"	✓ ✓ ✓	1	9	−13	3.56
"	-	3	27	+6	5.22
"	- -	1	9	+7	5.78
"	+	18	162	+67	5.41
"	++	2	18	±0	5.00
Fc	-	10	90	+59	5.66
"	- -	2	18	+7	5.39
c	+	1	9	+14	6.56
"	++	1	9	−2	4.78
C'	+	4	36	+7	5.19
"	++	0	—	—	—
K, k	+	8	72	+11	5.15
"	++ or +++	0	—	—	—
M	+	5	45	+31	5.69
"	++ or +++	2	18	+19	6.06
"	-	15	135	+60	5.44
"	- -	2	18	−9	4.50
"	B	1	9	+6	5.67
"	rd	1	9	−2	4.78
"	d	1	9	+14	6.56
FM, FM: M	+	7	63	+23	5.37
"	++	2	18	+8	5.44
"	+++	1	9	±0	5.00
"	-	1	9	+2	5.22
"	- -	0	—	—	—

* (c) overlaps somewhat with (b) and (e). When a subject's scores could be listed in either of two categories, they were included in both categories for this breakdown. In the summary, the subject was, of course, counted as only one case.

Rorschach Category	Rorschach Criterion	Number of Subjects	Number of ESP Runs	ESP Deviation	ESP Mean
m	+	13	117	+76	5.65
"	++	2	18	+5	5.28
Total Movement	+	29	261	+108	5.41
"	-	0	—	—	—
"	--	0	—	—	—
Color Shock	none	12	108	+32	5.30
"	✓	29	261	+117	5.45
"	✓ ✓	10	90	+55	5.61
"	-	5	45	+28	5.62
"	--	1	9	+11	6.22
"	+	8	72	+13	5.18
"	++	4	36	+3	5.08
Color Shock and Shading Shock	✓	25	225	+117	5.52
"	✓ ✓	17	153	+70	5.46
"	✓ ✓ ✓	8	72	+19	5.26
"	✓ ✓ ✓ ✓	1	9	—2	4.78
FC	-	9	81	+47	5.58
"	--	6	54	+14	5.26
"	B	3	27	+29	6.07
CF, CF: FC	-	18	162	+51	5.31
Total Color	+	3	27	—1	4.96
"	++	1	9	+9	6.00
"	-	8	72	+11	5.15
"	--	7	63	+18	5.29
Color: Movement	-	13	117	+70	5.60
"	--	16	144	+62	5.43
"	---	6	54	+24	5.44
"	--- with U	1	9	±0	5.00
"	+	1	9	+10	6.11
"	++	0	—	—	—
"	+++	0	—	—	—
Total Number of Checks	0-10	38	342	+158	5.46
"	11 - 24	13	117	+46	5.39
Total		51	459	+204	5.44
B. Goats					
Refusal	✓	3	27	—3	4.89
"	✓ ✓	3	27	—13	4.52
W	+	19	171	—23	4.87
"	++	2	18	—6	4.67
"	+++	2	18	+2	5.11
"	- or --	0	—	—	—
"	BV or VB	3	27	+20	5.74

Rorschach Category	Rorschach Criterion	Number of Subjects	Number of ESP Runs	ESP Deviation	ESP Mean
W	V	2	18	—4	4.78
"	B	0	—	—	—
Dd	+	4	35	—27	4.23
"	++	0	—	—	—
S	+	6	54	—6	4.89
"	++	0	—	—	—
Suc	1	1	9	—4	4.56
P	-	14	126	—14	4.89
O	BV or VB	2	18	+5	5.28
"	B	6	54	±0	5.00
"	V	3	27	—4	4.85
At, Sex	+	11	99	—39	4.61
"	++	3	27	±0	5.00
Range	-	23	207	—52	4.75
"	--	1	8	—11	3.62
"	+	0	—	—	—
"	(a) -	3	27	—28	3.96
"	(b)* -	4	36	+2	5.06
"	(c)* -	14	126	—32	4.75
"	(c) --	1	8	—11	3.62
"	(d) -	1	9	+7	5.78
"	(e)* -	3	27	—8	4.70
(H + A):					
(Hd + Ad)	<2:1	3	27	—29	3.93
F%	-	5	45	+9	5.20
"	--	0	—	—	—
F	BV or VB	2	18	+9	5.50
"	B	3	27	—1	4.96
"	V	4	36	—9	4.75
"	E	1	9	—9	4.00
Shading Shock	none	21	189	—38	4.80
"	✓	46	413	—120	4.71
"	✓ ✓	4	36	—17	4.53
"	✓ ✓ ✓	0	—	—	—
"	-	3	27	—1	4.96
"	--	2	18	—5	4.72
"	+	29	260	—70	4.73
"	++	1	9	—10	3.89
Fc	-	12	108	—21	4.81
"	--	4	36	—15	4.58
c	+	3	27	+2	5.07
"	++	0	—	—	—
C'	+	7	63	—16	4.75

* (c) overlaps somewhat with (b) and (e). When a subject's scores could be listed in either of two categories, they were included in both categories for this breakdown. In the summary, the subject was, of course, counted as only one case.

Rorschach Category	Rorschach Criterion	Number of Subjects	Number of ESP Runs	ESP Deviation	ESP Mean
C′	++	1	9	+6	5.67
K, k	+	14	126	−16	4.87
"	++ or +++	2	18	−2	4.89
M	+	18	162	−37	4.77
"	++ or +++	2	18	−16	4.11
"	-	9	81	−32	4.60
"	--	3	27	±0	5.00
"	B	0	—	—	—
"	rd	2	18	+3	5.17
"	d	1	9	−9	4.00
FM, FM : M	+	5	45	−5	4.89
"	++	3	27	−3	4.89
"	+++	0	—	—	—
"	-	9	81	−34	4.58
"	--	5	45	−11	4.76
m	+	12	107	−29	4.73
"	++	4	36	±0	5.00
Total Movement	+	43	386	−72	4.81
"	-	0	—	—	—
"	--	1	9	+4	5.44
Color Shock	none	7	62	−18	4.71
"	✓	50	450	−114	4.75
"	✓ ✓	14	126	−43	4.66
"	-	6	54	−11	4.80
"	--	1	9	−5	4.44
"	+	16	144	−52	4.64
"	++	6	54	−25	4.54
Color Shock and Shading Shock	✓	21	188	−26	4.86
"	✓ ✓	39	351	−119	4.66
"	✓ ✓ ✓	11	99	−30	4.70
"	✓ ✓ ✓ ✓	0	—	—	—
FC	-	9	80	−7	4.91
"	--	11	99	−48	4.52
"	B	3	27	−5	4.81
CF, CF: FC	-	24	215	−75	4.65
Total Color	+	9	81	−18	4.78
"	++	0	—	—	—
"	-	10	90	−25	4.72
"	--	3	27	−21	4.22
Color : Movement	-	26	233	−91	4.61
"	--	10	90	−14	4.84
"	---	6	54	−24	4.56
"	--- with U	0	—	—	—
"	+	0	—	—	—

Rorschach Category	Rorschach Criterion	Number of Subjects	Number of ESP Runs	ESP Deviation	ESP Mean
Color : Movement	++	0	—	—	—
"	+++	1	9	+4	5.44
Total Number of Checks	0-10	50	449	—148	4.67
"	11 - 24	21	189	—27	4.86
Total		71	638	—175	4.73

Appendix III

ESP scores of subjects whose Rorschach records show only one of the following items: (1) R $\geqq$ 30; (2) F%+; (3) rigid M; (4) no color shock or shading shock; (5) Total Movement++; (6) CF+; (7) C+.

A. Sheep

Rorschach Item Which is Present	Number of Subjects	Number of ESP Runs	ESP Deviation	ESP Mean
R $\geqq$ 30	12	107	—23	4.79
F% +	9	81	—2	4.98
Rigid M	5	45	—13	4.71
No Color Shock or Shading Shock	4	36	—4	4.89
Total Movement ++	4	36	—6	4.83
CF +	9	78	—20	4.74
C +	1	9	+11	6.22

B. Goats

R $\geqq$ 30	7	63	—7	4.89
F% +	10	90	—6	4.93
Rigid M	4	36	+23	5.64
No Color Shock or Shading Shock	7	63	+5	5.08
Total Movement ++	9	81	+12	5.15
CF +	7	63	—4	4.94
C +	4	36	+18	5.50

The Psychokinetic Effect: Further A.S.P.R. Experiments[1]

L. A. DALE and J. L. WOODRUFF

Abstract: Three series of dice-throwing experiments, designed in the hope of shedding light on the hypothesis that mind may directly influence matter, were carried out at the A.S.P.R. between May and December, 1946. In the first series, which involved 24 subjects and 13,824 single die-throws, apparatus was used which permitted the dice to fall at a distance of approximately 100 feet from subject and experimenters. In the second series, in which 54 subjects took part and in which 31,104 single die-throws were recorded, work was done with the simpler equipment which had yielded positive results in an earlier experiment. In the third series, the principal variable was the introduction of an ESP task at the same session. This series also made use of 54 subjects and in it 31,104 single die-readings were recorded. The results of all the series were close to chance expectation. The purpose of the present report is to put on record *all* the formal PK research that has been carried out at the Society since publication of the first report (1).

Introduction

There appeared in the July, 1946, issue of this JOURNAL a paper by one of the present writers describing an experimental investigation into the problem of the "psychokinetic effect," or, as it is popularly called, "mind over matter" (1). This investigation was an extension (and a verification) of earlier work reported by Dr. J. B. Rhine and his associates in the *Journal of Parapsychology,* beginning with the March, 1943, issue of that publication. Papers on psychokinesis (PK), one of them confirmatory (2), have appeared in the *Proceedings* of the (London) Society for Psychical Research (3, 4, 5).

These investigations have been concerned with the general problem of determining whether the mind can to a measurable degree influence the "behavior" of matter. This is the psychokinetic, or PK, hypothesis. Basically, the method of approaching the problem has been the same for all investigators. Certain inanimate objects—most frequently dice, but in some cases disks or coins—have been placed in motion and an attempt has been made to control by the exertion of "will" on the part of human subjects the position in which these

[1] These experiments were carried out with the approval of Dr. Gardner Murphy, Chairman of the A.S.P.R. Research Committee.

objects will come to rest. Statistics of probability indicate whether these inanimate objects have fallen according to "chance" or whether something more than chance is at work. Since many of the published reports (1, see Bibliography) have yielded results which cannot readily be explained on the chance hypothesis, investigators have been concerned with other alternative hypotheses. Before extrachance results can be taken as evidence of the operation of PK, it is necessary to rule out the possibility that the results obtained can be attributed to such factors as faulty dice or disks, inaccurate observation, or errors in recording the faces of dice or disks. Such factors seem to have been adequately controlled in most of the published investigations.

Beyond the problems of verification and the control of experimental conditions lies another area of experimental interest. If the existence of psychokinesis can be demonstrated, it is likely that it will show up more strikingly under some conditions than under others. One final goal of any research program might well be to attempt a determination of the conditions necessary for the maximum demonstration of the PK effect.

The first A.S.P.R. experiment (hereinafter referred to as "Series I") carried out by Dale was concerned with each of these three areas of experimental interest: (a) the possibility of obtaining evidence for the existence of PK; (b) the careful control of experimental conditions in order to rule out alternative explanations of positive results, and (c) the discovery of certain conditions and relationships necessary for a maximum demonstration of PK.

The present report, which encompasses three separate series of experiments, is also concerned with these problems. All data bearing on the PK hypothesis which have been obtained since the publication of the first report are included in this paper without regard to the question whether or not they offer confirmatory evidence for the existence of PK. The three series are differentiated on the basis of chronology, experimental conditions, and the specific problems being investigated. The results obtained from the three series are essentially of a "chance" nature—that is, they give no clear-cut evidence of the operation of PK—although one or two suggestive trends will be indicated in the discussion of the separate series. These series will be characterized as Series IIa, IIb, and IIc. Because of the chance nature of the results, a lengthy discussion is not called for.

Series IIa

Problem: This series was concerned with the major problem of determining the differential effect produced on PK scoring by the

introduction of the factor of distance into the experimental situation. The only published report dealing with the distance variable in PK testing is that of C. B. Nash (6), who compared scoring levels of subjects at three and thirty feet from the dice. Positive results were obtained under distance conditions, but since the experimenter remained at all times near the dice (and the experimenter may have been the one who was mentally influencing the dice), the test in the opinion of the present writers was not an adequate one. In Series IIa equipment was used which ensured that both subjects and experimenters were at all times at the prescribed distance from the dice.

Methodology and Procedure: Twenty-four female subjects took part in this series. Only female subjects were used because of the findings of the first A.S.P.R. experiment, in which women maintained a higher level of scoring than did men. The women were for the most part college students, and were paid at the rate of a dollar an hour. Prizes were offered for superior scoring. Sixteen of these subjects had participated in the first A.S.P.R. psychokinesis experiment (Series I).

Forty-eight white commercial dice, with scooped-out black spots, 5/8 inches on a side, were used. The randomizing chute used in the first A.S.P.R. experiment was again employed, although certain major alterations and elaborations were made. The chute itself is three feet six inches long, five inches wide, and four inches deep. There are 55 baffles and it is covered over by a glass strip. The original mouth of the chute was removed and a larger one substituted. Within this new mouth a trap door was constructed which could be opened electrically by activating a solenoid by means of pressing a small switch. The switch was fastened to the end of an extension cord approximately 110 feet in length. In this way the trap door (and the dice resting upon it) could be released while experimenters and subject stood at a distance of approximately 100 feet from the chute. A platform 23 inches by 24 inches was constructed 4 feet 8 inches above the dice box. It was supported by four legs nailed to the corners of the dice box. Through an aperture four inches square in the platform a Mercury II camera was mounted. In this way pictures could be taken of the dice after they came to rest. In order to furnish the necessary illumination two flood reflectors were attached to the platform in such a way that light from the Photoflood bulbs was directed on the dice.

Each of the twenty-four subjects was used for one session only. As soon as the dice had been released by the subject's manipulation of the switch which activated the solenoid and released the trap door, the flood lights were turned on and a photograph of the dice

was taken. Immediately after this each of the experimenters (J. L. W. and L. D.), as well as the subject, silently counted the number of dice showing the face that had been "willed for" by the subject. If there was not unanimous agreement as to the number of "hits," one of the experimenters isolated the correct faces from the rest of the dice and another count was made. (The photographs furnished a permanent record for recheck.) The two experimenters and the subject then recorded the score obtained for the throw on their record sheets, all three persons having been provided with a set of such sheets. This recording and photographing procedure was the same for all subjects, whether in "distance" or "same-room" conditions.

A "throw" consisted of the simultaneous release of 48 dice. Each subject made two "throws" for each of the six die faces: One, Two, Three, Four, Five, and Six. Thus ninety-six dice were thrown for each die face. Since the experiment was discontinued after twenty-four subjects had participated (the original plan had called for 54 subjects), the completion of the systematic rotation of die faces used in Series I was not possible. Although each die face was thrown for an equal number of times, fours, fives, and sixes were not thrown for as the *first* target face in the series as was done in the first investigation.

Alternate subjects were used in "distance" and in "same-room" conditions. In the distance experiments the dice were gathered by one of the experimenters, placed in a cup held by the subject, who then poured them into the mouth of the chute on top of the fastened trap door. The two experimenters and the subject then walked approximately 100 feet down a hall and into a room at the end of it. At her discretion the subject then pushed the switch which released the dice. The return trip to the first room was made and the photographing and recording procedure was carried out as described above. The dice were then scooped up by one of the experimenters, placed in the mouth of the chute by the subject, and the walking procedure was repeated. Since each of the six die faces was "thrown for" twice, twelve round trips were made by each subject and by the experimenters. The "same-room" conditions varied from the "distance" conditions only in that the dice were released by the subject while she was sitting next to the apparatus.

Results: The following tables give the essential results. No extra-chance effects are noted. Insofar as possible the analyses carried out were those which yielded significant results in Series I.

TABLE I

Results of PK experiment IIa in terms of total deviation from chance expectation (distance and same-room conditions combined)

N (in single die-throws)	13824
Hits obtained	2299
Hits expected	2304
Deviation	— 5
SD	±43.81

TABLE II

Comparison of scores obtained under distance versus same-room conditions

	Distance conditions	Same-room conditions
N (in single die-throws)	6912	6912
Hits obtained	1137	1162
Hits expected	1152	1152
Deviation	—15	+10
SD	±30.98	±30.98

CR/d (distance versus same-room) negligible

The distribution of hits on the six faces of the dice as targets is shown in Table III. It will be noted that significantly more hits were made on the three higher faces of the dice (CR = 2.9); since there is no other evidence of the operation of PK in the data, this can best be attributed to mechanically biased dice.

TABLE III

Distribution of hits on the six faces of the dice as targets

	One-face	two-face	three-face	four-face	five-face	six-face	Total
No. of hits	375	344	365	418	405	392	2299
Deviation	—9	—40	—19	+34	+21	+8	—5

It was mentioned above that two "throws" (48 dice per throw) were made for each die face, the results of these throws being recorded in two boxes numbered I and II upon a single record sheet. In Series I an orderly decline in rate of scoring occurred from Run I to Run IV on the record page (a "run" there consisting of six throws of four dice). In the experiment now under discussion a comparison was made of hits occurring on Throw I and Throw II

in order to see if similar decline effects were operating. The data of Table IV show that no decline phenomena were present.

TABLE IV

Comparison of scores obtained on Throw I and Throw II (on each of the six record sheets)

	Throw I	Throw II
N (in single die-throws)	6912	6912
Hits obtained	1136	1163
Hits expected	1152	1152
Deviation	—16	+11
SD	±30.98	±30.98

CR/d (Throw I—Throw II) negligible

Discussion: Since the data of Experiment IIa are of a chance nature elaborate discussion is not necessary. It may be well to point out, however, that results which are not statistically significant sometimes suggest the direction in which future research should go. Experiment IIa differs from Series I in a number of important respects, and some of these may have bearing on the difference in the results obtained from the two series. The number of experimenters was increased from one to two; the number of dice simultaneously thrown was increased from four to forty-eight. Because of the number of dice used per throw in Series IIa, elaborate methods of photographing and recording were deemed advisable to insure adequate experimental safeguards. The introduction of the distance variable itself may have been important. In this connection it may be noted that "distance" conditions yielded a deviation of minus fifteen and "same-room" conditions a deviation of plus ten. Since the difference is statistically insignificant, no special importance should be attached to the direction of this difference, but it can be considered in setting up further research plans. These and other factors may have been important in determining the chance nature of our results. (In Appendix I the total scores and deviations obtained by the twenty-four subjects in Series IIa are presented.)

Series IIb

Problem: In this series we were concerned with duplicating, if possible, both the psychological and the physical conditions which had obtained in Series I, and which may have been relevant in producing the extrachance results noted. It was felt that the elaborate mechanical controls and "gadgetry" in Series IIa may have militated against the demonstration of PK. Therefore in Series IIb there

was a trend toward the less elaborate, but still adequately safeguarded conditions of the first published series. Series IIb was set up in such a way that most of the statistical analyses which were made in Series I could be made here too.

Methodology and Procedure: Fifty-four female subjects took part in this experiment. (This is a deviation from the conditions of Series I, in which 29 women and 25 men were used.) The subjects were roughly of college age, most of them being students at Hunter College. Some of them had participated in earlier PK series. The subjects, who took part in one session only, were paid at the rate of a dollar an hour, and prizes were offered for "high" scoring.

Use was made of 6 commercial dice, red in color with scooped-out white spots, and 5/8 inches on a side. (This is also a deviation from Series I, in which only four dice were employed.) The randomizing chute and the dice box were the same as used in Series I. The chute is three feet six inches long, five inches wide, four inches deep, and covered by a glass strip. There are 55 baffles which serve the purpose of disturbing the free fall of the dice. The measurements of the box into which the dice fall from the chute are eighteen and one half inches by twelve inches. The record sheets were the same as used in the first series (1, see p. 127). They allow for the recording of four runs of 24 die-throws each.

The subjects threw six dice simultaneously; four throws of the six dice constituted a run.[2] Four runs were made (and recorded on a single record sheet) for each of the six faces of the die as target; thus each subject when she had completed her task had filled out a set of six record sheets. The order of target faces thrown for was rotated in orderly fashion, and thus each face was not only represented an equal number of times as target, but was also thrown for in a given position an equal number of times.

The dice were scooped up and placed in a dice cup held by the subject by that one of the two experimenters who sat on the floor next to the dice box. The subject then dumped the dice from the cup into the mouth of the chute. After they came to rest in the dice box, the experimenter who sat above the dice box on a divan called out the die faces and the other experimenter and the subject checked on the accuracy of the call. (It was felt that subjects not experienced in working with dice would be unable to handle the problem of accurately recording six dice.) The two experimenters and the subject then recorded the six die faces on individual record

[2] Work at Duke University has standardized a "run" in PK as 24 single die readings.

sheets. At the end of each run of 24 die readings the number of "hits" was checked by the three participants and recorded in the allotted space on the record sheet.

Results: With but one exception the results obtained in Series IIb deviate insignificantly from the results expected by "chance." In Series I one analysis indicated that there was a tendency for positive scoring to concentrate on the last three record sheets of the session regardless of the die face being thrown for (1, p. 137). Although this "improvement through the session" trend was not statistically significant in the first experiment, it interested the experimenters and they were on the watch for it to reoccur in the present experiment. Table VIII shows that the trend did reoccur. The critical ratio of 2.27 is significant at the two per cent level, indicating that a difference of this size is to be expected only twice in a hundred series of comparable length. A glance at the other tables, however, will show the essentially "chance" nature of the results as a whole.

TABLE V

Results of PK experiment IIb in terms of total deviation from chance expectation

N (in single die-throws)	31104
Hits obtained	5123
Hits expected	5184
Deviation	—61
SD	±65.72

TABLE VI

Distribution of hits on the six faces of the dice as targets

	One-face	two-face	three-face	four-face	five-face	six-face	Total
No. of hits	854	837	828	826	877	901	5123
Deviation	—10	—27	—36	—38	+13	+37	—61

Reference to Table VI shows that the total deviation obtained on the three higher faces of the dice was positive, while the total deviation yielded by the three lower faces was negative. This favoring of the higher faces, however, is not statistically significant, as it was in the case of Series IIa. It will be recalled that the dice used in the two experiments were not the same.

TABLE VII

Comparison of scores obtained on Runs I, II, III, and IV (of each of the six record sheets)

	Run I	Run II	Run III	Run IV
N (in runs)	324	324	324	324
Hits obtained	1292	1296	1240	1295
Hits expected	1296	1296	1296	1296
Deviation	—4	±0	—56	—1
SD	±32.86	±32.86	±32.86	±32.86

CR/d (I-II versus III-IV) negligible

In this experiment there is no evidence for the operation of PK in terms of deterioration of scoring rate as the subject progresses along the record page, as there was in Series I.

TABLE VIII

Comparison of scores obtained on pages 1-6 of each subject's work

	Page 1	Page 2	Page 3	Page 4	Page 5	Page 6
Hits obtained	837	827	823	843	888	905
Hits expected	864	864	864	864	864	864
Deviation	—27	—37	—41	—21	+24	+41

CR/d (pp. 1-2-3 versus 4-5-6) 2.27

As in the case of Series I, the "split-half" method of estimating reliability was applied to the data of Series IIb. A non-significant positive correlation was found between scores deriving from subjects' odd-numbered record sheets and those from the even-numbered sheets. Thus no evidence for the operation of PK derives from the results of this test. (In Appendix II the total scores and deviations obtained by the fifty-four subjects are presented.)

Discussion: Series IIb yielded only one result which was suggestively positive. The effect of this positive result, as shown in Table VIII, is minimized when it is realized that it is but one analysis selected from several that were made. The fact, however, that the result is in the predicted direction (that is, in the direction of the result obtained in Series I) must be taken into consideration.

It is of course not possible to explain why positive results comparable to those obtained in the first series were not obtained in the experiment just described. There were, it is true, a number of differences in the objective experimental conditions of the two series: In Series I there was one experimenter, four dice were thrown, both male and female subjects were employed, and it was chronologically the first PK experiment carried out at the A.S.P.R. In Series IIb

there were two experimenters, only female subjects were used, and it followed immediately upon an experiment in which only chance results were obtained (Series IIa described above). It is probable, however, that the important difference between the successful and the unsuccessful series lies not in the objective experimental conditions but in more subtle psychological factors which have yet to be determined. It is clear that an attempt to reproduce *objective* experimental conditions does not necessarily lead to the reproduction of the same *psychological* conditions. As far as the series just described is concerned, the results seem to indicate that we were not successful in reproducing the psychological variables which were important in Series I.

Series IIc

Problem: In this series, which was carried out by only one of the present writers (L. D.), it was hoped that light would be shed on the question of the relation (if any) between psychokinesis and extrasensory perception. It is currently held that PK and ESP are closely related abilities of human personality (*7*), and there is much *indirect* evidence to support this view; but it seemed that a direct experimental approach to the problem was needed. The specific question, then, asked in Series IIc was "Does the subject who tends to do well in an ESP task also tend to do well in a PK task?" It was also hoped that some clarification of the role of the experimenter in possibly bringing about the PK results (1, pp. 142-145) might be forthcoming, since the experimental plan provided for the experimenter's doing the ESP task, and her scores as well as those of the subjects were to be correlated with the PK scores obtained at the same session. Finally, the hypothesis that women are better PK subjects than men was to be tested. An attempt was made to keep objective conditions as much as possible similar to those obtaining in the first experiment, the principal variable being the introduction of an ESP task in the same session.

Methodology and Procedure: The subjects were 54 college students, 32 women and 22 men. They were paid at the rate of a dollar an hour, and none took part in more than one session. Some, however, had taken part in earlier PK series. A cash prize was offered to subjects scoring "high" on the PK task (no rewards were offered for the ESP task).

The dice used were four commercial dice, red with scooped-out white spots. These same four dice, with two additional from the same box, were used in Series IIb discussed above. The randomizing chute and dice box have been fully described in the published account

of Series I, and further described in this paper on p. 71. The record sheet, the same as used in Series I and Series IIb, allowed for the recording of four runs of 24 dice per run.

When he arrived, the subject was shown the PK apparatus and was told that he would be asked to do some dice-throwing later in the session. Then the ESP task was explained. Both the subject and the experimenter (L. D.) guessed the order of six decks of ESP cards which were concealed in six opaque white boxes laid out on a table. Subject and experimenter made their guesses by writing down the supposed order of the concealed symbols on standard ESP record sheets. Scores were not checked until the completion of the PK task; at this time the six boxes were opened and the subject's and experimenter's guesses were compared with the actual card order. (Since neither significant ESP results nor significant correlations between ESP and PK scores were obtained, it is not necessary to describe the many safeguards that were taken in preparing and recording, etc., the ESP material.[3])

The PK task was carried out in exactly the same fashion as in Series I. Four dice were thrown from a dice cup down the randomizing chute. Each subject performed four runs (96 single die-readings) for each die face as target, filling in all six record sheets, one for each die face. As before, the order of target faces was systematically rotated so that each die face was not only thrown for an equal number of times but was also represented with equal frequency as first, second . . . or last target face in the session. Subject and experimenter each kept an independent record of each die face that turned up. In the case of a discrepancy between number of hits recorded by subject and experimenter, the lower score was taken as official. (In this series there were only five such discrepancies.) In all, then, 1296 PK runs (31,104 single die readings) were performed by the subjects and 648 decks of ESP cards were guessed by subjects and experimenter.

Results: Analyses of the PK and ESP data of Series IIc indicate the "chance" nature of the results. Since, as stated above, there was no evidence of significant relationships between PK and ESP scores obtained at the same session, the results of the two tasks will be presented separately in the tables below. The PK material will be dealt with first.

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[3] We wish to thank Miss Dorothy Buck, the Assistant Secretary, for preparing and recording the 648 decks of ESP cards which were used in this experiment.

TABLE IX

Results of experiment IIc (PK task) in terms of total deviation from chance expectation

N (in single die-throws)	31104
Hits obtained	5162
Hits expected	5184
Deviation	—22
SD	±65.72

TABLE X

Distribution of hits on the six faces of the dice as targets

	One-face	two-face	three-face	four-face	five-face	six-face	Total
No. of hits	810	818	906	866	895	867	5162
Deviation	—54	—46	+42	+2	+31	+3	—22

The familiar picture of dice favoring the higher faces again emerges from the data of Table X, although it should be pointed out that the greatest positive deviation was obtained on the three-face.

TABLE XI

Comparison of scores obtained on Runs I, II, III, and IV (of each of the six record sheets)

	Run I	Run II	Run III	Run IV
N (in runs)	324	324	324	324
Hits obtained	1254	1290	1313	1305
Hits expected	1296	1296	1296	1296
Deviation	—42	—6	+17	+9
SD	±32.86	±32.86	±32.86	±32.86

CR/d (I-II versus III-IV) negligible

The data of Table XI show a reversal of the striking decline effect from first to last run on the record sheet found in Series I. The "improvement" of scoring on Runs III and IV in the present data is without statistical significance.

TABLE XII

Comparison of scores obtained on pages 1-6 of each subject's work

	Page 1	Page 2	Page 3	Page 4	Page 5	Page 6
Hits obtained	841	876	866	856	851	872
Hits expected	864	864	864	864	864	864
Deviation	—23	+12	+2	—8	—13	+8

CR/d (pp. 1-2-3 versus 4-5-6) negligible

Reference to Table XII shows that the statistically suggestive tendency for positive scoring to concentrate upon the last three record sheets found in Series IIb (see Table VIII) does not manifest itself in the present series.

The hypothesis that women are better PK subjects than men was not sustained. Table XIII shows that both men and women subjects scored at a chance level.

TABLE XIII

Comparison of scores obtained by male and female subjects

	Female Ss (32)	Male Ss (22)	Total (54)
N (in runs)	768	528	1296
Hits obtained	3062	2100	5162
Hits expected	3072	2112	5184
Deviation	—10	—12	—22
SD	±50.43	±41.81	±65.72

CR/d (F-M) negligible

Application of the "split-half" method of estimating reliability showed a non-significant negative correlation between PK scores deriving from subjects' odd-numbered record sheets and those from the even-numbered sheets. To summarize, then, all the analyses applied to these PK data indicated that they did not differ significantly from chance expectation.

The ESP data will now be briefly described. (In Appendix III are presented the total scores and deviations obtained in the PK task by the 54 subjects, and the total ESP scores and deviations obtained by the subjects and the experimenter.) Pooling subjects' and experimenter's scores on the 648 decks called, the over-all deviation was plus 70. This positive deviation yields a CR of 1.36, which is not significant. Neither the subjects nor the experimenter scored significantly when their data were treated separately. Null results were also obtained from analyses for salience (initial and terminal), for significant grouping of hits (runs of three, four, five, etc., successive hits), and for abnormal run-score distributions (preponderance of extreme scores).

Both the subjects' ESP scores and the experimenter's ESP scores were correlated with the PK score obtained at the same session. In the first case the correlation coefficient was + .12; in the second case it was — .20. Neither is significant.

Before dismissing the ESP results as being entirely without interest, however, it may be permissible to call attention to a "chronological decline effect" which appears in the ESP data. Reference to

Appendix III will show that at the half-way point of the experiment (*i.e.*, when twenty-seven out of the fifty-four sessions had been completed) the total deviation on the 324 decks called was plus 100. This yields a CR of 2.78, which is significant. Of this positive deviation the subjects contributed 48 units and the experimenter 52. At this half-way point the experimenter, yielding to temptation, stopped and added up both the subjects' and her own ESP scores. It happened that Dr. Murphy was ill at the time, and the experimenter wrote him that "if the ESP scores continue at this rate we will have hit the jackpot . . ." or words to that effect.

It has been thought by a number of investigators that it is "bad magic" to do statistical work before the completion of an experimental series ("don't count your chickens before they are hatched"), and this proved to be the case. The second half of the series fell to pieces, yielding a deviation of *minus* 30. The subjects contributed 24 units and the experimenter 6 units to this negative deviation. The CR of the difference between scoring rate in the two sections of the experiment is 2.55 (P= .005), which is significant. On the other hand, this probability value must be taken with a grain of salt in as much as it derives from a division of the data which was not specified in advance. This division, in fact, may seem quite arbitrary and indefensible to some readers, although it does not seem so to the experimenter. Finally, it may be pointed out that the PK data show no "chronological decline effect," nor were the PK scores tallied up, as were the ESP scores, at the end of the 27th session.

Discussion: Despite the fact that *objective* experimental conditions (equipment, number of dice thrown simultaneously, male and female subjects of college age, etc.) were similar in Series I and Series IIc, the results of the latter series did not confirm those of the former. The principal *psychological* variable may have been the attitude of the experimenter. The first series was undertaken in a spirit of adventure; there was intense curiosity to see whether or not the Duke results would be repeated under the conditions specified. Series IIc, on the other hand, was undertaken, as it were, from a sense of duty, and the experimenter's morale was low after the failure of Series IIa. She felt "morally obligated," however, to attempt to repeat the earlier experiment from which positive results had derived.[4] Major interest from her point of view focused upon the ESP task, which provided something new in the test situation. One other factor may also be mentioned—even at the risk of sounding as if an alibi were being put forward! In a three-weeks' period the experi-

[4] It should be noted, however, that in spite of very high morale indeed at the inception of Series IIa, in which the distance factor was tested, the results were of a chance nature.

menter worked with over seventy individual subjects, each session lasting from an hour to an hour and a half. (Series IIc, though chronologically later than IIb, did overlap to a certain extent with IIb.) It is believed by the experimenter that such intensive work, resulting quite naturally in fatigue and strain, is not conducive to the best results.

Summary

Between May and December, 1946, three extensive series of PK tests were undertaken in which 132 subjects performed a total of 76,032 die throws. No clear evidence for the operation of psychokinesis was found in any of the series. It is hoped, however, that this detailed report will be of some value to future investigators in highlighting certain research problems which still remain unsolved.

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Appendix I

PK Series IIa. Total scores and deviations obtained by the twenty-four subjects on their six record sheets. A score of ninety-six is expected by chance. Subjects marked with an asterisk had taken part in the first PK series. "D" indicates the distance condition, "SR" the same-room condition.

Subject Number	Variable	Total Score	Dev.	Total Score, First Throw	Total Score, Second Throw
* 1	D	94	− 2	43	51
* 2	SR	97	+ 1	38	59
* 3	D	101	+ 5	54	47
* 4	SR	91	− 5	44	47
* 5	D	94	− 2	45	49
* 6	SR	102	+ 6	57	45
* 7	D	94	− 2	47	47
8	SR	92	− 4	44	48
9	D	102	+ 6	44	58
*10	SR	113	+17	55	58
*11	D	90	− 6	43	47
*12	SR	90	− 6	45	45
*13	D	102	+ 6	55	47
14	SR	90	− 6	46	44
15	D	91	− 5	43	48
16	SR	105	+ 9	51	54
*17	D	100	+ 4	52	48
18	SR	98	+ 2	51	47
*19	D	89	− 7	50	39
*20	SR	91	− 5	44	47
*21	D	87	− 9	42	45
*22	SR	97	+ 1	42	55
23	D	93	− 3	44	49
24	SR	96	± 0	57	39

Appendix II

PK Series IIb. Total scores and deviations obtained by the fifty-four subjects on their six record sheets (24 runs). A score of ninety-six is expected by chance. All women subjects.

Subject Number	Total Score	Dev.	Subject Number	Total Score	Dev.
1	111	+15	28	98	+ 2
2	97	+ 1	29	107	+11
3	88	— 8	30	110	+14
4	105	+ 9	31	102	+ 6
5	74	—22	32	105	+ 9
6	89	— 7	33	106	+10
7	107	+11	34	96	± 0
8	92	— 4	35	99	+ 3
9	94	— 2	36	90	— 6
10	94	— 2	37	106	+10
11	77	—19	38	86	—10
12	92	— 4	39	78	—18
13	94	— 2	40	99	+ 3
14	93	— 3	41	89	— 7
15	99	+ 3	42	85	—11
16	99	+ 3	43	93	— 3
17	110	+14	44	88	— 8
18	91	— 5	45	84	—12
19	99	+ 3	46	100	+ 4
20	75	—21	47	105	+ 9
21	100	+ 4	48	91	— 5
22	87	— 9	49	96	± 0
23	103	+ 7	50	91	— 5
24	89	— 7	51	87	— 9
25	110	+14	52	90	— 6
26	94	— 2	53	99	+ 3
27	99	+ 3	54	81	—15

Appendix III

PK Series IIc. Total scores and deviations obtained by the fifty-four subjects on their six PK sheets (24 runs), subjects' ESP scores and deviations, experimenter's ESP scores and deviations, and total ESP deviations.

Subject Number	Sex	PK Scores	Dev.	Subjects' ESP Scores	Dev.	Experimenter's ESP Scores	Dev.	Total ESP Deviation
1	F	87	— 9	31	+ 1	31	+ 1	+ 2
2	M	94	— 2	28	— 2	37	+ 7	+ 5
3	M	92	— 4	19	—11	30	± 0	—11
4	F	107	+11	31	+ 1	31	+ 1	+ 2
5	F	99	+ 3	42	+12	36	+ 6	+18
6	F	76	—20	23	— 7	34	+ 4	— 3

Subject Number	Sex	PK Scores	Dev.	Subjects' ESP Scores	Dev.	Experimenter's ESP Scores	Dev.	Total ESP Deviation
7	M	82	−14	27	− 3	23	− 7	−10
8	F	110	+14	36	+ 6	24	− 6	± 0
9	F	87	− 9	33	+ 3	39	+ 9	+12
10	M	103	+ 7	35	+ 5	31	+ 1	+ 6
11	F	91	− 5	31	+ 1	36	+ 6	+ 7
12	F	91	− 5	30	± 0	42	+12	+12
13	F	111	+15	40	+10	23	− 7	+ 3
14	F	102	+ 6	28	− 2	29	− 1	− 3
15	F	100	+ 4	27	− 3	37	+ 7	+ 4
16	F	103	+ 7	28	− 2	28	− 2	− 4
17	M	85	−11	34	+ 4	33	+ 3	+ 7
18	M	100	+ 4	38	+ 8	28	− 2	+ 6
19	M	96	± 0	30	± 0	33	+ 3	+ 3
20	F	87	− 9	36	+ 6	33	+ 3	+ 9
21	F	86	−10	26	− 4	27	− 3	− 7
22	F	86	−10	24	− 6	34	+ 4	− 2
23	M	86	−10	37	+ 7	40	+10	+17
24	F	95	− 1	37	+ 7	34	+ 4	+11
25	F	99	+ 3	35	+ 5	39	+ 9	+14
26	F	93	− 3	37	+ 7	23	− 7	± 0
27	M	106	+10	35	+ 5	27	− 3	+ 2 *
					(+48)		(+52)	(+100; CR:2.77)
28	M	89	− 7	32	+ 2	28	− 2	± 0
29	F	108	+12	24	− 6	28	− 2	− 8
30	F	101	+ 5	30	± 0	35	+ 5	+ 5
31	F	92	− 4	33	+ 3	33	+ 3	+ 6
32	F	104	+ 8	29	− 1	32	+ 2	+ 1
33	F	99	+ 3	23	− 7	31	+ 1	− 6
34	M	113	+17	29	− 1	32	+ 2	+ 1
35	M	86	−10	29	− 1	21	− 9	−10
36	F	98	+ 2	33	+ 3	36	+ 6	+ 9
37	F	98	+ 2	38	+ 8	27	− 3	+ 5
38	M	106	+10	28	− 2	29	− 1	− 3
39	M	94	− 2	23	− 7	26	− 4	−11
40	F	91	− 5	27	− 3	23	− 7	−10
41	F	83	−13	37	+ 7	41	+11	+18
42	M	84	−12	31	+ 1	32	+ 2	+ 3
43	M	93	− 3	30	± 0	24	− 6	− 6
44	F	91	− 5	28	− 2	33	+ 3	+ 1
45	M	103	+ 7	31	+ 1	25	− 5	− 4
46	M	105	+ 9	29	− 1	31	+ 1	± 0
47	F	97	+ 1	32	+ 2	25	− 5	− 3
48	F	106	+10	23	− 7	28	− 2	− 9
49	M	102	+ 6	33	+ 3	38	+ 8	+11
50	M	97	+ 1	17	−13	25	− 5	−18
51	F	94	− 2	28	− 2	32	+ 2	± 0
52	M	93	− 3	30	± 0	31	+ 1	+ 1
53	M	91	− 5	29	− 1	28	− 2	− 3
54	F	90	− 6	30	± 0	30	± 0	± 0
					(−24)		(− 6)	(−30; CR:−.83)

* This division of the data at the halfway point in the experiment is discussed on pp. 77-78.

Unusual Experiences

In the July, 1946, issue of this JOURNAL we presented for the first time a series of "unusual experiences"—experiences which did not lend themselves to publication under the heading of "Cases" either because (a) although giving *prima-facie* evidence of paranormality, they lacked independent corroboration, or because (b) they gave no evidence whatever of paranormality (but were considered worth putting on record because of their unusual psychological interest). The experiences presented in this issue are of the type that give evidence of paranormal factors, but no corroboration is possible.

The first account comes to us from a Member, Mr. MacKinlay Kantor, the well-known author. Mr. Kantor experienced some "haunting" phenomena while spending the night in a London rooming house. The episode occurred during the night of April 20th-21st, 1945. The next day Mr. Kantor made a brief note concerning the experience in his diary. On April 22nd he described the experience in detail to a friend, Captain Francis H. Beaugureau, of the 305th Bomb Group, and on April 27th he again discussed it with two other friends, Colonel Philip Cochrane and Colonel "Micky" McGuire, of the U. S. Army Air Forces. Mr. Kantor made notes of these conversations in his diary, which he kept regularly at the time. He has been good enough to show us this diary, and we have seen all the entries relevant to the experience about to be reported. Finally, during June of the same year, upon his return to America, Mr. Kantor dictated to his secretary a very detailed account of the episode. His account follows:

My first acquaintance with the room came on the afternoon of Tuesday, April 10, 1945. The 364th Squadron of the 305th Bomb Group (H), with which I was flying at the time, had been "stood down" (taken off Operations) for a couple of days, so I decided to go up to London for a brief holiday. Major J. J. Kostal of Chicago went with me. Together we applied for rooms, and together we were directed by the billeting officer to No. 40 Truxley Court, in the south end of Kensington.[1] The lodgings were in fact two houses, No. 40 and No. 42, tan-colored Victorian structures remodeled as a single lodging house. One could enter either at No. 40 or 42; there was a passage cut through on the first floor. The landladly was Mrs. Effie Stuart (pseudonym).

Mrs. Stuart at first seemed doubtful whether she could find rooms for us. She was quite full up, she said, and all the rooms on the

[1] In order to protect the landlady from possible embarrassment, a fictitious address has been used. The true address and the actual name of the landlady have been given to us by Mr. Kantor and are on file at the Society.

third and fourth[2] floors were filled. Finally, however, she assigned Kostal to a single room on the second floor at the side of the house, and then took me around to the front of the house on the same floor, where she hesitated before an open door. (This was in fact the second story and a half. There was a peculiar construction to these houses. The rear portion on every floor, except the first, was half a flight below the front portion of the house.) Mrs. Stuart looked at me inquiringly. "This is the only room I have left," she said. "It's very small, I know."

She was correct. The room was about ten or twelve feet in length and certainly not more than seven feet wide. Actually it was a continuation of the hall itself, apparently walled off by some earlier generation in order to make an extra sleeping-room. Later familiarity with the premises showed that the same plan followed on the first floor, below, and on the third and fourth stories (see footnote 2) above. In each case there was a little hall bedroom walled off at the front—a narrow room with one window looking down on the springtime trees of Truxley Court. I recall thinking it odd that the elderly folk who tenanted the place should prefer to live in third or fourth floor hall bedrooms rather than on the second floor. They would only have had to climb one flight and a half of stairs in order to reach the room where Mrs. Stuart was putting me, yet this room remained untenanted.

A wide hall door, fastened by a modern spring lock, filled about half the north wall of the room; the other half was occupied by the head of a studio couch, with a heavy spread and a lot of daytime cushions. The couch-bed took up a good share of the floor space, its head against the north wall next to the door, its right side against the west wall of the room. Beyond the foot of the couch, on the south wall next to the one big window, was a hall tree where clothing could be hung. A low table stood in front of the window. Well down the east wall was a desk of the secretary type. For the rest of the room's furnishings there were only two chairs.

As it turned out, I did not after all spend the night at No. 40 Truxley Court. I stayed with friends in Hampstead, quite unexpectedly, and returned to the rooming house next day to pick up my bag. I ran into Kostal, and he said that he had spent a comfortable night in his room on the second floor, side. I left London, and Truxley Court slipped from my mind. On the afternoon of April

[2] Mr. Kantor, in discussing his experience with us, said that he was not now entirely certain that there was a "fourth" floor; that is, that there were *two* hall bedrooms above his, although he thinks that this was probably the case. Understandable confusion may have resulted from the "half stories" peculiar to the construction of this house as well as from the English method of numbering floors, which differs from the American system.

20th, however, I found myself back in London. I was working for USSTAF, and they had flown me suddenly over to Britain from France, to consult with an 8th Air Force officer. It was necessary to find lodgings for the night, and Truxley Court came to my mind. Mrs. Stuart said she had but one vacant room and that she would give it to me for the night. It was that same second story front hall-bedroom. How fortunate, said Mrs. Stuart, that it hadn't been taken by someone else!

I had dinner with my friends, and returned to my room before eleven. The maid had not turned down my bed, so I took off the cover and folded it on a chair. The bed was now revealed, completely made up: sheets, one blanket, and a neat comforter of puckered sateen. No one else seemed to be abroad in that house, not even at eleven. I locked the door, turned off the light, opened the window, and looked into the grey night before I got into bed. There was part of a moon, but it was hidden under misty clouds. I slept soundly until the small hours, then awoke, rose, turned on the small light on the desk top, and opened the door. I went into the hall and down the half flight of stairs to the bathroom at the second-floor rear. There were no signs of human activity in the house. On the way back to my room I looked at my wrist watch. It was six minutes after three, and the house was very quiet; so was all of London. I couldn't hear a sound in the tired, hammered city that spread away beyond those walls. In the bedroom, I closed the door and heard the spring lock snap (I distinctly remember that sound of the snapping lock). I turned off the light, raised the window a couple of inches higher, and crawled into bed.

I didn't go back to sleep. About the time I was settled—turned on my right side in the smooth, narrow bed, the way I habitually sleep—I suddenly became aware that someone (or something) was trying to pull the covers off me. For a moment I just lay there and took it. I couldn't believe it. The covers—comforter, blanket, and sheet—were all sliding slowly and steadily toward the foot of the bed. It was a gentle but persistent and sturdy pressure which took them there. For a long moment I thought that in some peculiar manner I must have wound my foot in the bedclothing, and that somehow the weight and pressure of my foot were dragging the covers. It was nonsensical, and in the same moment that I evolved the theory I rejected it. I sat up and looked toward the foot of the bed. There was nothing there.

The room shone soft and grey in light from the wide-open window. I could see the desk, the hall tree, and the chairs; I could make out their dull shapes in the gloom. The covers were still being pulled toward the foot of the bed, and no visible entity was pulling them.

Reaching down with both hands, I got a good grip, tangling my fingers in sheet and blanket and comforter all together, and I hauled the covers back up. Even as I did this, I felt a certain pressure fighting me all the time I drew on the fabric. I think I then began to realize that this was something peculiar to the hour and the place. I had slept in hundreds of beds in many different countries, but never before had I had an experience like this. And the thing which amazed me most was the fact that I wasn't really frightened. I was merely angry. Some force was trying to drag those covers off me, and I resented that force deeply.

Four times, when I relaxed my grip, the covers started down again; each time I think that it was with greater strength and rapidity. Four times I grabbed them and brought the sheet and comforter up around my shoulders again. Then I remember thinking, "This has gone far enough. If you want to get those covers again, you're going to have to take me with them." All this time, I must add, there hadn't been a sound. There wasn't a whisper in that room except for the ripple of fabric, the swishing of cloth as it was drawn down across my rayon pajamas, and the sound of the gasp and grab I made when I pulled the covers up again.

After the fourth onslaught, I dragged the whole mass of bedclothing up against increasing pressure, and locked the stuff around me. I shoved one side of the comforter under my left shoulder, rolled almost over on my face, jammed more bedclothing in behind me, and rolled back again. In addition to this, I hung on to the top margin of all three bed garments. The only way the force could drag the covers down was to drag me with them! But almost as soon as I relaxed my grip or shifted my weight, however slightly, the covers started to go again. The attempts came not in a sly or jerky motion, but with a dull pressure—like the power of inertia. I just lay there and hung on.

Lying in this fashion, on my right side, with my face turned toward the west wall of the room, I became aware that the wall had grown very bright. My first thought was that the moon had broken through the clouds outside and was shining through the window. I lifted my head slightly to observe the window, and found that it was dark; there was no moonlight. I turned over in bed, still holding the covers tightly, and discovered that the *east* wall was intensely bright. The light which I had seen on the west wall was reflected from the opposite wall on the east.

This brightness of the east wall, witnessed by me as I stared from my bed, became (within not more than five minutes after I had first observed it) as white as milk. Then the whiteness seemed to turn to a kind of luminescence. The best way I could describe

the phenomenon is to say that it looked like the mysterious wavery flare of a radium compound: the radium paint which seems to wink and recede and come near again, when you gaze at a luminous wristwatch in the dark. All over that wall, in a rough pattern covering perhaps thirty square feet, the light was flowing and brightening. The brightness occurred from a point about five feet above the floor, down nearly to the floor itself, and in an over-all space probably six or seven feet in length. As the brilliancy increased, so did the *area* of light lessen. It seemed to shrink in, more or less, from all four corners of the patch that was roughly rectangular. There was now (within, as nearly as I can judge, ten minutes from the time I first discerned the light) a kind of misshapen core of whiteness. It was flat. It didn't seem to stand out from the wall—neither the core nor the area of discernible light that surrounded it. But a mass of brilliance was slowly taking shape.

I had begun to hear sounds—perhaps *a sound* would be correct. It was more like a murmur of distant voices than anything else I can think of now or could think of then: voices of a number of human beings. Those distant people seemed to be speaking or wailing or laughing rapidly, but they seemed incredibly far away. Such a babble might be noticed if one stood at the far end of a large house, and heard a party and a funeral simultaneously in progress in rooms on another floor and at the opposite end of the house. Most assuredly I could hear voices. I knew that they were the utterances of human beings, that there were many in number, and that their hullabaloo ran from caterwauling through speechifying and gossip to honest laughter. But I wasn't able to recognize even one word that they were saying. I noted that ***the audible phenomena increased in direct ratio to the increase in the intensity and centralization of the light on the wall.*** The brighter the luminescence, that is, the more high-pitched and sturdy and discernible the voices, the more nearly could I *almost* hear what they were saying.

Almost—but not quite. I could not honestly identify a single word which I heard, yet indeed they were words and I think spoken in English. Certainly there was laughter. Certainly I heard remotely the sound of sobbing; and more than these—lying over them in sound, as frosting covers a cake—was the bickering chatter of little voices, men and women gabbling, interrupting one another, prattling of things which must have been petty in the extreme if they could be recited with such careless rapidity.

As I stared at the core of light on the wall, it began to assume a more specific shape. I am confident that the flow of light—or rather, of luminous material—was casting itself in the mold of a man or a woman. There was one moment when I perceived this: I think

that I saw a head and shoulders and the upper part of a body, but whether of a man or woman I cannot say.

Throughout this whole performance observed by my eyes and my ears, I was resisting mutely. I remember saying to myself: "I won't believe you, I must refuse to look at you, to hear you. I am stronger than you. I will resist! You cannot overcome me. You cannot *make me* hear you or see you." I may not have thought these words, precisely, but certainly such resentful thoughts were flowing through my mind as I gazed and listened.

Then on the instant when I became aware that a head and shoulders and body were taking shape against that empty east wall, I almost spoke the thought aloud—perhaps I did speak it aloud: "Oh, I get the idea! I know what you want. You *want* me to *look* at you and *listen* to you. Well, I won't. You can't make me." With a heave, I rose to a half-sitting position, still wrapped in the covers, still gripping them with my hands. I flung myself on my right side, with my face turned away from the east wall. I lay with my weight pinioning the crumpled bedclothes on my right side, and with my left elbow locking a mat of sheet and blanket and comforter against my body. I was not looking at that wall any longer, and the garbled voices began to fade from my ears. Eventually I fell asleep.

When I awoke in the morning I found that I had pressed the sheet and comforter with such fury that there were hundreds of complex little wrinkles in the cloth—as firmly pushed into the texture of the goods as if an iron had pressed them there. The door was still locked, and the east wall was as empty as very ordinary wallpaper could make it. I do not of course pretend to know what was going on in the room that night; but I do know, however, that I was neither sick, drunk, nor having a dream or nightmare. Although the cumulative effect of my experience while it was going on was to make me feel angry and defiant rather than terror-stricken, I should not care to spend a night alone in that room again.

Finally, at breakfast Mrs. Stuart came up to my table, gave me a long look, and asked me if I had slept well. I told her that I had.

MacKinlay Kantor

Mr. Kantor has been kind enough to give us some additional information relevant to his experience. In answer to a question relating to the length of time that elapsed between his first awareness of the light upon the east wall and the beginning of the auditory effects, he answered: "Unfortunately I cannot relate with admitted accuracy the time sequence of visual and auditory phenomena. It is my impression, however indistinct and confused, that I was aware

of the light upon the wall for several minutes at least before I recognized the activity of auditory occurrences."

When asked for more information as to how long the entire experience lasted, Mr. Kantor replied: "According to the most capable estimation I can offer, both the auditory and visual phenomena (as typified by the distant mutter of voices and the reflected light on the west wall) decreased, within ten or twelve minutes after I turned away from the east wall, to a point where I was no longer aware of them. The pressure on the bedclothes continued for a time; there was a noticeable tension, and a disposition of the bedclothes to slide toward the foot of the bed, which could be discerned whenever I relaxed my grip for more than a few seconds. Therefore I hung on tightly, with my hands squeezed on the fabric. I went to sleep with my hands solidified in a violent clutch; when I slept, it was from pure exhaustion. My assumption is that I retained consciousness for approximately half an hour after I turned over to avoid the seeming appearance of an apparition on the east wall. The phenomena connected with the bedclothing started shortly after three o'clock—I looked at my watch when I returned to my room. I believe, although I did not again look at my watch, that I probably fell asleep between four and four-thirty. So the entire experience lasted, to the best of my knowledge, from an hour to an hour and a half. It may be of interest to note here that when I woke up the following morning my hands were strained, slightly swollen, and gave me considerable discomfort. On one other occasion only have my hands ever felt like that: that was once in Florida when, armed with light bass tackle, I battled a sixteen-pound redfish for forty-seven minutes. That time I immersed my hands in sea water to relieve the ache."

Unfortunately Mr. Kantor left England very shortly after the night which he has described, and he had no opportunity to make inquiries of the landlady or others as to whether or not the room had a prior history of being "haunted." (Mr. Kantor has explained to us that when the landlady asked him next morning if he had slept well, he answered almost automatically that he had. To have said that he had slept very badly indeed would have been to involve himself in giving an account then and there—in a dining room filled with strangers eating their breakfasts—of a very complicated and personal sort of experience.) There seem, however, to be some indications that the room may have had a history: Although London was crowded at the time and rooms were at a premium, this particular room was empty on two occasions; although elderly people were living in the house, they tenanted similar rooms on the floors (or floor) above when Mr. Kantor's room would have

required less stair-climbing; moreover, the history of the bombing-out of upper floors had made rooms on lower floors very desirable; finally, there is the curious attitude of the landlady in her seeming reluctance to rent the room. It is Mr. Kantor's belief, based on his observance of this attitude, that the landlady was *afraid* to let him have the room, but that finally her natural desire to turn an honest penny overcame this fear.

In conclusion, Mr. Kantor has told us that he had never before had a clear-cut psychical experience. He had read one or two books on poltergeist and related phenomena, but did not believe in the reality of such experiences.

The second account of an unusual experience was also sent to us by a Member, Miss Muriel Symington. Miss Symington, who has been a subject in a number of our experiments in extrasensory perception, reported a somewhat similar experience several years ago, and this was published in the JOURNAL, issue of October, 1944, pp. 122-124. Miss Symington writes as follows:

December 15, 1946

About 3:20 P.M. on Saturday afternoon, December 14th [1946] I was waiting for a street car to take me about five blocks to a transfer point. I saw the oncoming car discharging passengers one block away from where I stood. The motorman apparently saw me waiting and ran the block without picking up speed, making it obvious to any motorist that the street car was stopping. As I stepped out from the curb I gave the usual glance up the street, not having any faith in the right of way which is allegedly the pedestrian's in such a situation. No automobile was in sight. I stepped out close to the car track and just as the door opened to admit me an automobile which had apparently turned in from a side street a block away whizzed by me at a good speed—though possibly a perfectly legal speed on a clear road—and so close to where I stood that had I shifted my position an inch or two it would certainly have caught me. I got on the street car without comment and the motorman said, "Gee, Miss, that was a close one," and shook his head. When I say that no automobile was in sight, I mean that at the moment I looked the car had not yet turned into the main street where the trolley line ran.

Luckily the street car didn't have to stop until it was halted by a long red light at my transfer point, a busy thoroughfare. I got out and went over to where the offending car had also been stopped by the light (I had noticed the color and make of this car and there was no possibility of error on my part). I took its license number, MD ——, and then stepped up to the car and tapped on the glass to attract the attention of the driver. He glanced at me and then looked straight ahead. I said, "Did you realize what you did a block or two back?" He made no reply, nor did he look at me again; at that point the lights changed and he drove on. Naturally I was angry, and as the car moved I called out, "Looking for business, doctor?"

I was shocked by my narrow escape and furious with what I assumed to be the doctor's deliberate rudeness on top of his carelessness. I was on my way to dine with a friend and I began to figure what steps I should take to make him "rue the day." Having decided what to do, this sentence flashed into my mind out of the mental sub-cellar of my unconscious: "That will settle *your* hash, Dr. Schultz!"[3] Then I found myself startled and ashamed over endowing the object of my wrath with a foreign name and I cursed myself for being nothing more than a chauvinistic snob. The odd thing is that the driver's appearance was in no way indicative of foreign origin. When I reached my friend's home I told her the story of my narrow escape, but I was too ashamed to tell her that I had endowed the doctor with a name (I did tell her subsequently, but too late as far as evidence is concerned).

This morning [December 15th] I went to a nearby police station to lodge a complaint. The desk sergeant heard my story, took down the license number, and then called up the bureau to obtain the motorist's name. I was stupefied when the sergeant spelled out the name as he received it over the phone: S-C-H-U-L-Z. The only difference was that my mind had yielded a "t" which was not actually in the doctor's name.

Although I should not wish to offer any interpretation of my experience, it does seem that this approximation of the name is difficult to attribute to chance coincidence; hence I am submitting the account to the Society. Perhaps I should add that I have never been acquainted with anyone named either Schultz or Schulz; and what is more, my

[3] For obvious reasons a pseudonym has had to be substituted for the actual name of the doctor, and therefore it was also necessary to change the name which flashed into Miss Symington's mind in relation to the driver of the car. The real name of the doctor is a somewhat less common one than "Schultz," and Miss Symington's impression was correct except for a single vowel.

conscious mind would repudiate the idea of indicting any individual German for the crimes of his country.

MURIEL I. SYMINGTON

Miss Symington has asked us to add here that she wrote Dr. "Schulz" advising him of her intention to take the matter up in Traffic Court and that she subsequently received "a very courteous letter from him apologizing for his carelessness and stating that when I tapped on the glass to attract his attention he had not recognized me as the pedestrian involved, but thought it was a stranger trying to 'bum' a ride."

Lecture and Tea

The final Lecture and Tea of the current season will take place at the Rooms of the Society on Wednesday, April 23rd, at 4 P.M. Dr. Gardner Murphy will be the speaker and his topic is: "Precognition as a Problem for Psychical Research, Psychology, and Philosophy."

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Lecture Series

A series of six lectures and teas took place at the rooms of the Society during the 1946-1947 season. These meetings, which were attended by a large number of Members and their friends, were under the direction of Mrs. Henry W. Warner. The speakers and their topics were as follows:

November 20, 1946—"Psychics and Psychical Research in the United States," by Miss Gertrude Ogden Tubby.

December 18, 1946—"Psychical Research in France," by Mrs. Henry W. Warner.

January 22, 1947—"The Pagenstecher Experiments and their Implications," by Mrs. Richard L. Kennedy, Jr.

February 19, 1947—"Psychical Research in England," by Mr. William O. Stevens.

March 19, 1947—"Swedenborg and Psychical Research," by Miss Signe Toksvig.

April 23, 1947—"Precognition," by Dr. Gardner Murphy.

We wish to announce that the Rooms of the Society will be closed on Saturdays during the months of July and August.

Psychometric Telepathy Across the Atlantic

J. HETTINGER

Abstract: Between February, 1945, and July, 1946, sixteen one-hour experiments in "psychometric telepathy" were carried out between London and New York. In the first series there were ten sessions; in nine of these illustrated magazines were contemplated by an "agent" in New York while a sensitive in London gave her impressions in relation to an object (sheets of handwriting) submitted by the agent. The agent marked on each magazine page the exact time that page was read or pictures thereon were looked at; the experimenter in London similarly timed the exact moment when the sensitive gave each impression. Experimenter's and agent's watches were synchronized. The tenth session was similar to the first nine except for the fact that *two* sensitives (working some miles apart) simultaneously gave impressions while the agent once again perused an illustrated magazine. In the second series, which consisted of six sessions, two sensitives simultaneously acted as percipients while the agent contemplated a set of sixty pictures prepared in a special scrapbook.

In the case of both series, the impressions given by the sensitive(s) were compared with the material which had been simultaneously perused by the distant agent, the hope being that the correspondences between impressions and stimulus items would be so clear-cut and striking as to provide *prima-facie* evidence for telepathy. At the end of the first series, however, the need was felt for a statistical evaluation of the material; difficulties were encountered and in the hope of solving these the second series was set up. Here again, difficulties in evaluation were not surmounted, and therefore neither series can be considered as more than a pilot study. A third series of trans-atlantic experiments is now being planned and it is hoped that in this series the problem of statistical evaluation can be fully solved.—*Ed.*

Introduction

The experiments reported in this article were carried out with a view to ascertaining whether or not the method of psychometric telepathy, using illustrated magazines, as described in my second book, *Exploring the Ultra-Perceptive Faculty*,[1] would yield successful results at a distance such as that between London and New

[1] Hettinger, J., *Exploring the Ultra-Perceptive Faculty*, Rider & Co., London, 1941.

York. That telepathy is not limited by distance is recognized by those who accept the reality of such mental rapport. Hence, in view of the successes attained with the psychometric method at distances of a few miles, they were expected to be repeated across the Atlantic.

After consultation with Dr. Gardner Murphy, Mrs. L. A. Dale, Research Associate at the A.S.P.R., readily agreed to cooperate in these tests, sending me for this purpose several sheets of her handwriting to serve as the articles to be psychometrized. The psychometrists were the two sensitives that I have used in my earlier experiments, Mrs. F. Kingstone and Miss F. Fallows.

We performed in all sixteen one-hour tests, Mrs. Dale acting as subject[2] in New York and the two sensitives as percipients in London. Seven of these tests were carried out simultaneously with the two sensitives (who were miles apart from each other). On these occasions I was kindly assisted by Mr. F. E. Creed, who recorded the impressions given by one sensitive while I was recording those simultaneously expressed by the other. At the synchronized times of the tests, it was noon in New York and early evening in London.

For the benefit of those readers who may not be acquainted with the general method of experimentation I have been using, which dates back to 1938, let me describe it as expressed on page 15 of *Exploring the Ultra-Perceptive Faculty.*

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

"The time factor experiments[3] having shown that a good proportion of the items given by the sensitive corresponded to events experienced by the subject at the actual time the items were given, it was hoped:

"Firstly, that some of the passages read by the subject and pictures

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[2] Dr. Hettinger uses the term "subject" where in this country we would say "agent"—the person who looks at the stimulus material and whose mental states may be apprehended by the percipient.—*Ed.*

[3] See Dr. Hettinger's first book, *The Ultra-Perceptive Faculty,* Rider & Co., London, 1940, pp. 151ff.

contemplated by him would correspond, as to their substance, to the items perceived by the sensitive;

"Secondly, that they would correspond also as regards time; and

"Thirdly, that a series of pictures contemplated by the subject or of the passages read by him and a series of items given by the sensitive would be obtained, each member of which second series would agree with the corresponding member of the first series as regards both substance and time.

"It was thought that if this threefold hope were realized, the results would, in contradistinction to statistical proof, carry conviction *per se,* and that anyone wishing to test the probable existence of the ultra-perceptive faculty for himself could readily do so by acting as subject in the manner indicated, instead of having to rely on lengthy statistical experiments undertaken by others."

Personal Views on Some Pertinent Questions

Before reporting on the results of the transatlantic tests, I feel that I ought to express my views on some pertinent questions:

1. How do we explain that so many of the items given by the sensitives are not applicable? The records of sensitives contain, as a rule, a substantially greater number of impressions which are not applicable, as compared with the number found to apply. We possess no evidence as to the true explanation of the non-applicable items. We can conjecture, however, that they may be attributable to one or more of the following: subconscious associations in the minds of the subjects or sensitives; haphazard statements uttered by the sensitives during complete fading of receptivity, in an endeavor to maintain their mental attitude in the state they find necessary for the exercise of their faculty; interference from another mind not participating in the test; allusions or generalizations which are not always recognized or accepted as such; and, last but not least, imperfect perception distorted beyond recognition.

2. Why are statistics useful in a general respect and quite useless in another more important respect? Opinions on the value of statistics differ: some overrate and others underrate the need for statistical evaluation. In one general respect statistics are undoubtedly very useful; *viz.,* when applied to a comparison between the numerical occurrences of possible alternatives in order to ascertain the probability of one or the other being the correct answer; for if the figure obtained is above the maximum allocated to chance coincidence, we gain confidence in the alternative the occurrence of which proves to be significantly preponderant. Once this confidence is gained, how-

ever, statistics are quite useless for further ascertaining which of the occurrences are chance coincidences and which are non-chance—that is, due to an inter-relation between a fact and its cause—for no statistical procedure can differentiate between the two categories.

3. Why is it difficult to apply statistics to qualitative material? The main difficulty is assessment, in which the personal element plays an important part. If, when a control method is used, the assessor knows which is the actual material and which is the control material, the criticism may reasonably be advanced that he was influenced by a bias one way or the other. If an inexperienced assessor works "blind," and if the impressions of the sensitives are not absolutely concrete hits, he begins to ponder and waver, finally deciding one way or the other, and laying himself open to the criticism that his assessment was of an arbitrary character. If, as has been found to be the case, a large number of the impressions are not concrete hits, but rather distortions, associations, inferences, allusions, relevant interpretations, etc., an assessor not fully conversant with this specific kind of investigation simply ignores them, and the application of statistics thus becomes meaningless. I must confess that I myself, although I have spent hundreds of hours working with this kind of material, often have difficulty in deciding whether or not to accept a given impression as an indisputable hit. As we shall see from the statistical evaluations attempted in connection with these transatlantic tests, we have not as yet succeeded in providing a satisfactory solution to the problem of an adequate statistical handling of psychometric impressions of such great variety as encountered in this type of experimentation.

4. Why do I think that the inductive method is destined to be the next method used in psychic investigations? The answer is that the rich variety of material ultra-perceived by sensitives when analyzed by the inductive method reveals recurrences which enable us, so to say, to "pin down" certain thought-provoking implications. For instance, I have found that pictures showing rising smoke frequently produce the impression: "The breathing is affected." I have also found that "silent speech" is often transmitted and may seem to vitiate the relevance of the sensitive's impression. Thus, in another experimental series, the subject was looking at a picture that depicted, in addition to other constituents, a staircase. In this particular test it had been arranged that the subject should make notes on the salient parts of the pictures he contemplated, and he wrote: "A *flight* of stairs." The sensitive's impression was: "I get *flight,* an airplane." I believe that this incident, typical of a number, speaks for itself. I strongly feel that a large collection of other similar instances ascertained by induction will throw light, if not on the actual *modus*

operandi, at least on the relationship between the psychological make-up of the subject and the sensitive.

5. Why do I think that configuration (Gestalt) plays an important part in the functioning of the ultra-perceptive faculty? One of my earliest observations in experiments with illustrated magazines has been further substantiated by the results of the transatlantic tests reported here, and is becoming crystallized in a point of view which seems to offer an explanation for the character of many of the impressions expressed by the sensitives. They appear to apprehend the situation (stimulus material) in the mind of the subject as a configuration, the impression being found to apply to a portion of the picture or to one of many possible assemblies of its constituents. Configuration (Gestalt) would also shed light on why the sensitive sometimes combines a constituent of one picture with a constituent of another one on the same page, or on two pages facing each other, into a single statement (impression) which, considered as an integer, is not found to be applicable.

The Transatlantic Tests

In none of the tests carried out across the Atlantic did the sensitives know about the exact nature of the experiment in which they were participating, although they may possibly have guessed that it was similar to the earlier experiments because of the use of the stopwatch; but in any case they were completely unaware that a New York-London test was in progress. We performed two series of experiments, the first in 1945 and the second in 1946. Dates of the tests, particulars of the magazines and other picture material used, and the initials of the psychometrists are given in the following lists:

First series:

Number of Test	Date of Test	Magazine Used		Sensitive
1	Feb. 28, 1945	*New Yorker,*	Dec. 30, 1944	Fallows
2	Mar. 1, 1945	" "	Feb. 10, 1945	"
3	Mar. 7, 1945	" "	Jan. 20, 1945	"
4	Mar. 13, 1945	" "	Feb. 24, 1945	"
5	Mar. 15, 1945	" "	Nov. 11, 1944	"
6	Mar. 21, 1945	" "	Jan. 27, 1945	"
7	May 1, 1945	" "	Apr. 7, 1945	Kingstone
8	May 3, 1945	" "	Apr. 14, 1945	"
9	May 7, 1945	" "	Apr. 21, 1945	"
10	May 31, 1945	*Look,*	May 29, 1945	F. and K.

In the second series, which consisted of six "simultaneous tests" using both sensitives, the subject, Mrs. Dale, contemplated a specially prepared scrapbook with 60 illustrations, each page containing a single picture which was viewed for one minute. These pictures were randomly drawn by Miss Wellman, the then Executive Secretary of the A.S.P.R., from a large collection of cuttings from *Pageant* and *Coronet,* were pasted by her in a scrapbook, and presented to Mrs. Dale shortly before the first test.

Second series:

Number of Test	Date of Test	Stimulus Material	Sensitives
1	July 16, 1946	Special scrapbook	F. and K.
2	July 17, 1946	" "	"
3	July 18, 1946	" "	"
4	July 22, 1946	" "	"
5	July 23, 1946	" "	"
6	July 24, 1946	" "	"

The many hits of an exceptional character obtained during these transatlantic tests speak for themselves, I believe, when the sensitives' impressions are compared with the illustrations contemplated at the corresponding times; unfortunately, it is not possible to reproduce here more illustrations than the few which will be found below. As regards the first series, however, readers may be able to compare the selected instances included in the lists hereinafter given with the actual pictures in the magazines, which they can procure or peruse in a library. However impressed they may be by the selection which I report here, I feel confident that they will find the comparison of the sensitives' impressions with the actual pictures much more convincing both individually and by their cumulative effect.

Since chance coincidence is always an alternative hypothesis in all parapsychological investigations, I fully see the point of view of those who insist on the desirability of statistical evaluation. Consequently, in spite of what I stated above in connection with statistical difficulties when dealing with qualitative material, I attempted to consider the results of the transatlantic experiments from a mathematical standpoint. Let me first of all deal with this part of the investigation before I analyze the material from the point of view of its qualitative significance.

Statistical Control

In the statistical analysis of the results of the first series, use was made of the following control method: the issue of each magazine perused was tested against the impressions given in connection therewith and also against the impressions given on another date in connection with another issue of the magazine. Guided by previous experience, the following rules of assessment were established in advance and rigorously applied to both the supposed ultra-perceptive (UP) impressions and control (C) impressions:

1. In addition to instances of complete agreement between an impression and a particular magazine content, acceptance is extended to (a) a particular constituent or constituents of the impression with its or their maximum attributes found to be applicable, and (b) distortions, generalizations, and allusions, provided they are obvious or very pertinent.

2. Correspondence in time has to be exact within stipulated limits, admitting a tolerance of a lag of ± 1 minute.

3. Rule 2 is extended to embrace any time falling within the times marked on two pages facing each other, on either of which the temporal correspondence is found. The reason for this extension is that it often happens that the subject marks the time on one of two pages facing each other and the correspondence in substance is found not on the page with the time mark, but on the page facing it; or the time marking may be made in relation to a certain picture and the impression is found to apply to another picture either on the same page or on the opposite page.

4. Tolerance is extended to a lag of ± 2 minutes in exceptional cases; namely, when an applicable item or particular thereof is very specific and is not found to occur more than once in either the magazine or the record of impressions.

5. A maximum value of 1 is allocated to each statement separately numbered in the record of the sensitive's impressions, or to that part thereof which is found to apply, but where the statement contains two or more disconnected items, they count separately if found to apply.

6. The maximum value of 1 is reduced to ½ if the illustration found to correspond to a particular item reported by the sensitive occurs altogether more than four times in the magazine; if the illustration occurs more than eight times, the value of the "hit" is reduced to 0. Repetitions of the illustration on any one of two pages facing each other, or on both, are counted as one occurrence.

7. The value of 1 or ½ allocated to an item reported by the sensitive is also reduced in proportion to the frequency of its occurrence in the record of the particular test.

8. Items from the record found to apply to a given page or to two pages facing each other are treated as a group with respect to the frequency factor; that is, the individual items retain their allotted values of 1, ½, or 0, as estimated by rule 6, the score of the group being the sum of these values, but the reduction of the total is effected not in proportion to the *individual* frequencies of the items, but in proportion to the frequency of the reoccurrence of the *group*. (It will be seen that the great reduction in values which may be caused by the application of Rule 7, is counterbalanced by this provision, which prevents such a reduction when a number of applicable items given by the sensitive are concentrated in a group at the time that a given page or double-page is being perused by the subject.)

9. Since it is very difficult to do full justice to impressions which seem to correspond to reading matter, especially when the latter is not confined to small paragraphs, such impressions are not scored.

10. Several pages of the magazines perused are regularly devoted to the same topics (sports, music, etc.) in each issue; also to many small advertisements, and now and then to numerous complicated small pictures, as many as thirty per page. For the sake of simplicity, these parts of the magazines have been entirely disregarded.

11. Denoting the total value of an item or group of items, whichever comes into question, by V_t, the frequency of such an item or group of items in the sensitive's record by I_t, and the frequency of the corresponding illustration or group of illustrations in the magazine by M_t, we get for the scoring (Sc) the formula: $Sc = \frac{I_t \times M_t}{V_t}$. This formula is applicable to both ultra-perceptive (UP) and control (C) evaluations.

12. The statistical treatment of the results follows the formulas used in my first psychometric investigation.[4] $p = \frac{S}{N}$ and $q = \frac{C}{N}$ stand for the proportions of the sensitive's (ultra-perceptive) scores and the control scores respectively, wherein $N = S + C$. $\sqrt{\frac{p \times q}{N}}$

[4] *Cf. The Ultra-Perceptive Faculty.*

is the standard deviation (σ) and the probable error (PE) is 2/3 σ. Any result which is four times the PE, for which the odds are 142 to 1, is taken as the figure for and above which the results are considered to be statistically significant.

Here is a specimen of actual scoring, as applied to the fourth experiment in the first series, showing how the assessment was carried out by the application of the above Rules. The first column (No.) gives the number of the impression on the record of the sensitive's utterances; the second column (P) gives the corresponding page in the magazine. All total scores with the fraction ½ and above have been rounded upwards, and those less than ½ have been rounded downwards.

TEST No. 4

Magazine perused: *New Yorker* of February 24, 1945

No.	P	UP Impressions		V	I_f	M_f	Sc
3	C	Rearranging something colored.		1	1	1	1
5	2	Jerking.	V_t	1	1	3	⅓
6	2	It depends on how you work foot or leg.		1			
9	5	Different colors in glass, one brownish and the other much darker.	V_t	1	1	1	2
11	5	Busy cross-road.		1			
12	5	A building.		0			
22	13	Something square and some markings on.		1	1	3	⅓
25	15	Painting; spring flowers.	V_t	2	1	1	2½
26	15	In a room; glass.		½			
49	22	Something is holding me up.		½	1	1	2
51	22	Glass; small bright objects in it; manipulate hand.	V_t	1½			
57	25	I look up to read; something put up high.		1	1	2	½
66	33	Theatre; voice.		½	1	1	1½
67	33	Someone speaking almost in a whisper.	V_t	1			
76	43	Unsteady; holding on with left hand.		1½	1	1	1½
84	45	Lady in bed.		1	1	1	1
92	54	Something I want to press quickly.		1	1	1	3½
94	54	Gentleman in overcoat.	V_t	½			
95	54	Gentleman fumbling with something in front of the coat.		1			
97	54	I want to move my feet, I want to run.		1			

Total UP Score: 17

Summary of the Scores

In carrying out the control, I paired two successive tests; *viz.*, test No. 1 with test No. 2, No. 3 with No. 4, and so on, and compared the magazine actually used in each test, first with the impressions given in connection therewith and then with the impressions given in connection with the magazine used in the paired test. Thus the first comparison provided the ultra-perceptive (UP) score and the second the control (C) score. In the case of the tests with the sensitive K., their number being an odd one (3), I had to pair the last one, No. 9, with No. 8, which was also paired with No. 7. In the case of the simultaneous test in which both F. and K. took part as psychometrists, the impressions they gave were compared, for the purpose of obtaining a control, with another issue of the magazine not used in any test. The following table shows how the ten tests were paired, which issue of the magazine was used in each test, and the UP and C scores respectively obtained.

UP Scores			*C Scores*		
Test	Impressions on *New Yorker*	Sc	Test	Impressions from *New Yorker*	Sc
No. 1	Dec. 30, 1944	15	No. 2	Feb. 10, 1945	4
No. 2	Feb. 10, 1945	22	No. 1	Dec. 30, 1944	11
No. 3	Jan. 20, 1945	13	No. 4	Feb. 24, 1945	7
No. 4	Feb. 24, 1945	17	No. 3	Jan. 20, 1945	9
No. 5	Nov. 11, 1944	16	No. 6	Jan. 27, 1945	9
No. 6	Jan. 27, 1945	19	No. 5	Nov. 11, 1944	10
No. 7	Apr. 7, 1945	10	No. 8	Apr. 14, 1945	3
No. 8	Apr. 14, 1945	8	No. 7	Apr. 7, 1945	3
No. 9	Apr. 21, 1945	10	No. 8	Apr. 14, 1945	3
No. 10	on *Look* May 29, 1945	27	tested on *Look* of	July 10, 1945	20
		157			79

The scores in test No. 10, which was a simultaneous one, are the sums of applicable impressions given by both sensitives.

Applying the formulas given above, we get for the standard deviation:

$$\sigma = \sqrt{\frac{\frac{157}{236} \times \frac{79}{236}}{236}} = 0.03;$$ and for the probable error: $PE = \frac{2}{3}\sigma = 0.02.$

The ratio of the UP impressions was $\frac{157}{236} = 0.66$. The probability

that this ratio will always be greater than the theoretical probability of the tests, *viz.*, one in two (0.5) is: 0.66 — 0.50 = 0.16, or

$$\frac{0.16}{PE = 0.02}$$

. This is 8 × PE, which is significant.

Had the control score been about 25% higher, *viz.*, 100, and thus the UP score only 50% higher than that of the control, the result would have been 5 × PE, that is to say, still above the minimum value of significance, which is 4 × PE. This leads me to advance the following suggestion:

Anyone who wishes to satisfy himself at the personal level of the reality of psychometric telepathy at a distance should, providing that a sensitive is available to him, carry out two 1-hour tests with suitable illustrated magazines and apply the method of mutual control following the rules stipulated. If he gets a 50 per cent higher score for the UP impressions as compared with the control, this should be a personal proof of the reality of telepathy.

Evaluation of the Results by Others

Since, as has already been pointed out, the comparison of impressions and picture material was carried out by me, and I knew which were the "correct" and which were the "control" magazines, I should not wish to present these results as statistical *proof* of psychometric telepathy, in the sense of the results reported on in my first book. Thus, although it is my personal opinion that the results of the two series presented here provide *prima-facie* evidence for telepathy, and although everyone who studied the material from the qualitative point of view was favorably impressed, I fully concurred when the suggestion was made that an independent evaluation of the results should be carried out by someone who did not know which impressions were actually given in relation to the various magazines.

A first attempt in this direction was made by Dr. C. E. Stuart, in whose premature death psychical research has recently lost a most brilliant and capable worker. In connection with this attempt, Mrs. Dale wrote to me in November, 1945, "When I was down at Duke University Dr. Stuart very kindly started to evaluate the fourth sitting by his method of preferential matching, but it soon became apparent that the material as a whole did not lend itself to his technique. The trouble lay in the fact that I had perused some items for as long as five minutes (but the larger proportion for only one minute) and we did not have enough of these five-minute items to make the matching feasible. Moreover, the results would have been

heavily penalized by the fact that, following his method, only pictures actually contemplated could be taken into consideration and marginal material left out of account. When we saw that the technique was not feasible, I showed Dr. Stuart some of the most interesting 'hits.' He expressed the opinion that he was very much impressed by the qualitative evidence."

Another very competent worker, Mr. E. P. Gibson, kindly undertook the laborious work of evaluating the results of the first six tests with the sensitive F. and test No. 10, in which both F. and K. simultaneously acted as percipients. Mr. Gibson used for the purpose of the control issues of the magazines (*New Yorker* and *Look*) which were not used in any of the actual experiments. Here are his figures:

Number of Test	Scores on Expt'l Magazines	Scores on Control Magazines
1	17.10	8.70
2	11.70	2.39
3	3.29	6.12
4	5.59	9.05
5	10.12	3.15
6	4.58	9.08
10 (Simult.)	11.33	5.32
Total:	63.71	43.81

My comments on these results were expressed in a letter to Mrs. Dale, dated March 17, 1946, as follows:

"Although I wish with you that Mr. Gibson's results had been more striking, I consider them nonetheless more or less satisfactory, bearing in mind both the objective and subjective difficulties an evaluator has to cope with in the case of such material, not to speak of the newness of the method of evaluation.

"A difference of three times the probable error is often taken as significant, but for the sake of safety I am following Yule and take 4 × PE as *significant*. I further consider values from slightly below 3 × PE to slightly below 4 × PE as being on the *borderline of significance*, and anything below this as *non-significant* (see p. 44 of *The Ultra-Perceptive Faculty*).

"It is satisfactory to find that Mr. Gibson's scoring yields the result of 3 × PE on such small numbers as 63.71 and 43.81 respectively, and that, as you remark, the difference is in the right direction. Calculation shows that, taking the figures and the direction as

a criterion, the doubles of these numbers for twice the number of tests, say, 128 and 88 for fourteen tests, would yield 4.1 × PE."

I was greatly puzzled by the substantial differences in the UP scores assessed by me and those assessed by Mr. Gibson in tests Nos. 3, 4, and 6. When I received Mr. Gibson's work sheets I came to the conclusion that rule 8, concerning the *evaluation of groups of impressions,* may not have been clearly understood. It is in large part by virtue of these complexes of impressions that I find the scoring of UP greatly to excel over the control scoring.

A further attempt at independent evaluation of these same tests was made by a young student who was entirely unfamiliar with this sort of work. She obtained nearly equal scores for UP and C.

In view of the statistically inconclusive results of, and the difficulties encountered in, the independent scoring of the material, it was suggested by Dr. Murphy and Mrs. Dale that we undertake a new series in which I would be the scorer, but would not know which was the experimental and which was the control material. In this suggestion I fully concurred. In this second series I suggested the use of a specially prepared scrapbook instead of magazines (see p. 99), and the control consisted of a similarly collated scrapbook, prepared immediately after the last test in the series. Each one of the six tests in this second series was a simultaneous one, K. and F. acting as psychometrists. Mrs. Dale was again the subject.

Statistical Results of the Second Series

Here are the results which I obtained on the two books, not knowing which of the two was actually used and which was the control":[5]

Scoring without evaluation: Total number of impressions given by the two sensitives during the six sittings: 1363, of which 264 were accepted as applicable to Book I and 362 as applicable to Book II. (Impressions were accepted as applicable if they corresponded to any one or more of the constituents of the picture—exactly, obviously

[5] A word again about the preparation of these scrapbooks: About 150 pictures were cut from *Pageant* and *Coronet* magazines. This collection was turned over to Miss Wellman, who shuffled them face-down, and then drew at random from the pile 60 pictures which she pasted into a suitable scrapbook. At the end of Test No. 6 the remainder of the pictures were turned over to a student assistant, who shuffled them face-down and drew from this collection another 60 pictures. He then pasted these into a scrapbook identical with the one used in the experiment. These two books were sent in separate packages to Dr. Hettinger in London. It should be stressed that great pains were taken to subject the two books to the same amount of wear and tear, etc., so that there would be no normal way of telling which one had actually been used in the experiment.—*Ed.*

distorted, as an allusion, or as a fully justified inference. The items from the sensitives' records corresponded exactly in time with the contemplation of the picture by the subject, with a maximum tolerance of ± 1 minute.)

Subjecting the two figures to statistical treatment by means of the formulas given on page 101, we get the following results for Book I and Book II respectively:

(I) $p = \frac{264}{626}$, (II) $q = \frac{362}{626}$. *Viz*: (I) 42.10% and (II) 57.83%.

Probable Error: $\frac{2}{3}\sigma = .0133$.

Results for Book II: 5.86 × PE, which is significant.

Scoring with evaluation: The evaluation of the impressions followed the same rules as in the case of the first series; *viz.*, if a picture or part thereof to which an impression was found to apply in substance and time occurred throughout the same book four times or less, that impression was assessed as 1; if between five and eight times, as ½; if more than eight times, as 0. A modification, however, was introduced as regards "integrated scoring." The total value attributed to any *picture* (in either book) was obtained by adding together the individual values allotted to all the impressions found applicable to that picture in substance and time throughout the twelve records of the two sensitives. Since in this series, however, the subject contemplated the same book during each of the six sessions and the repetition of the same or associated impressions in connection with the same picture has an enhancing effect from an evidential point of view, it was thought logical to increase this total value (attributed to a given picture) more quickly than by simple addition if, with respect to one and the same picture, an applicable impression was repeated in the two series of records from the two sensitives, or if these impressions centered around a fundamental factor in the picture. This was carried out only with respect to items valued as 1 or ½, and namely, by taking, instead of the *sum* of the values of the associated items, the *square* of that sum. Thus $1 + 1 = 2$ became $2^2 = 4$; $1 + 1 + ½ = 2½$ became $2½^2 = 6.25$, and so on. Thus evaluated, the pictures in the two books found applicable to the sensitives' impressions lead to the following result:

(I) $p = \frac{151}{531}$, (II) $q = \frac{380}{531}$. *Viz*: (I) 28.5% and (II) 71.5%.

Probable error: $\frac{2}{3}\sigma = .0133.$

Result for Book II: 16.4 × PE, a highly significant figure.

When Mrs. Dale received my analyses of the sensitives' impressions in relation to the pictures in the two books, she cabled me that Book II was the one actually used in the experiment.

A further criticism and suggestion, however, was now conveyed to me in a letter from Mrs. Dale, dated October 13, 1946:

"The hypothesis is (or could be raised) that *you* performed, so to speak, *a single extrasensory act,* and by virtue of this learned which was the correct scrapbook. From then on, scoring of individual items could be colored by this bedrock paranormal knowledge. In other words, the sixty pictures scored are not actually independent items and are therefore not properly evaluated by the CR (or so-and-so-many times the PE) method. It is easy to be wise after the event, but I realize now that I should have sent you the sixty pictures actually used, each paired with a control picture—rather than the series of pictures prepared in scrapbooks. Then there would be no question but what you would have had to make sixty independent judgments. We now suggest that you return the picture material, and that we ask Mr. Gibson to re-evaluate the data when the pictures are removed from the scrapbooks and presented to him in a series of sixty paired items. Whether or not the actual stimulus picture is to be the first or the second of the pair can be determined by a suitable random method. The temporal order of the pictures from one to sixty would of course be retained."

In my reply of October 20th I wrote as follows:

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

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

the second power. You will find such instances in relation to both actual and control books."

I concurred in the suggestion that Mr. Gibson carry out the assessment of the pairs of individual pictures. When returned by him with the evaluated scores, Mrs. Dale separated the latter and grouped them to correspond to the respective series in the two books. Unfortunately the scores obtained—98 for the ultra-perceptive and 126 for the control pictures—now showed a tendency in the wrong direction.

I do not think any useful purpose would be served by detailing in this article the various criticisms I have to offer to this last assessment, since readers do not have before them the material under discussion and are therefore unable to judge the situation. Suffice it to refer to what I have already said above in regard to the difficulties encountered in connection with the evaluation of qualitative material with a view to providing statistical evidence, and to express the hope that in further experiments which we are now contemplating between New York and London we may be able to overcome some of the difficulties, and add statistical evidence to the non-mathematical evidence which I will now proceed to discuss.

Qualitative Analysis

If we analyze the character of the sensitives' impressions and the nature of their correspondence with the contents of the magazine, we find that they belong to different general catagories, *viz.*, some are not at all applicable, some are applicable to the pictures, parts thereof or combination of different pictures, some apply to the reading or circumstances of the test, some are distorted to different degrees, and some are in the form of generalizations, allusions, or pertinent remarks.

Space does not permit of an analysis being made of all the impressions for which correspondence in substance and time was found, but the selection given below provides, in my opinion, substantial *prima-facie* evidence of telepathy across the Atlantic.

The first series:

New Yorker, Dec. 30, 1944.

1. Lejon's advertisement on inside of front cover, showing a small cart with two ponies in front of it, produced the impression (Fig. 1):

Sensitive: "A small pony."

2. Picture of man with monocle on left eye (p. 11).

Sensitive: "Something affecting the left eye."

3. Picture of a man skating, making figures of 8, in progress of completing the 13th one, p. 19 (Fig. 2):

Sensitive: "I want to count numbers rather slowly; I cannot get more than fifteen . . . Watching slow movement of something . . . Standing on something that is rather shaky."

4 Picture of a smoking locomotive (p. 22).

Sensitive: "Oh! Oh! Something is affecting the breathing."

This impression is almost invariably given by sensitive F. whenever smoke is perceived.

5. Picture of a number of people sitting around a table with a minister in the chair praying; the caption is: ". . . and now let us pray silently for a moment that certain long overdue repairs will be made to the rectory" (p. 28).

Sensitive: "I want to join something together, but I have to do it carefully."

This is one of those frequently given impressions which an evaluator new to this kind of material overlooks or disregards, in the absence of something concrete. The fact that the sensitive perceived the joining of "something" and not the specific joining "in prayer" is highly thought-provoking; and investigators who will repeat these experiments will soon be fascinated by similar instances.

6. Elizabeth Arden's advertisement on page 37, including the illustration of one small and four large glass jars, and that of a lady, holding her hand between the 3rd and 4th large ones, produced the impression (Fig. 3):

Sensitive: "Glass jars. I want to take the 4th one."

New Yorker, Feb. 10, 1945

7. Cole of California's advertisement. "Swoon-glo in parachute colors" on page 6, illustrating a lady in brilliant yellow bathing attire, and a large number of miniature parachutes in red, yellow and blue, produced the impression:

Sensitive: "Two colors, red and green . . . Yellow strongest color."

8. Chrysler's advertisement on page 11, showing five men, three of whom are in blue clothing, all inspecting a large blueprint, caused the impression:

Sensitive: "Gentlemen in blue uniform."

These two examples indicate the ultra-perception of colors, although in the first case, green was perceived as blue, and in the second one, the preponderant blue was not a uniform but a large blueprint.

FIG. 1.
Reproduced by permission of the National Distillers Products Corporation.

FIG. 3.
Reproduced by permission of Elizabeth Arden.

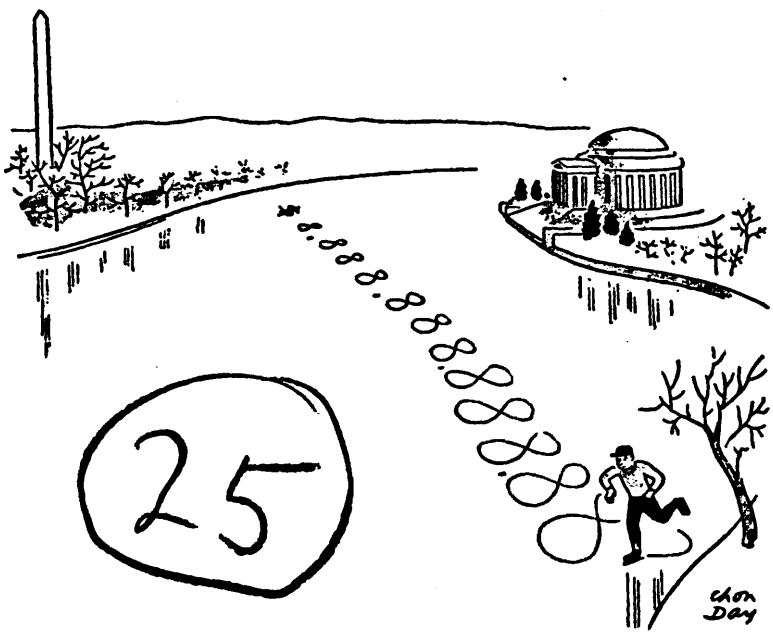


FIG. 2.
Permission Chon Day and *The New Yorker*,
© The F-R. Publishing Corporation.

9. Picture of a calf (National Dairy Products advertisement) railed in by boards with free spaces between them, so that it can look outside (p. 14).

Sensitive: "Something like a window . . . It looks railed up."

A perfect description of the enclosure, yet interesting to note that the calf, the most prominent part of the picture, was not perceived.

10. Picture of a very thin and worried looking bride in the vestry of a church waiting for the bridegroom; the minister is ready for the ceremony; she is telephoning: "Is Mr. Harry Thornton there?"; bouquets of flowers on the desk and in the hands of child bridesmaids (p. 21).

Sensitive: "A thin lady complaining of being very tired . . . Flowers . . . Oh, bother! Oh, bother!"

This example illustrates a perception of feelings and, in addition thereto, of visible items, *viz.*, the thin lady and the flowers.

11. Picture of a man tied up by string to a chair, body and limbs (p. 29).

Sensitive: "A piece of wire or string; I want to take it backwards and forwards."

Here we have again perception of an essential constituent of the picture, *viz.*, the string that has been taken backwards and forwards around the man, but not the other essential—the man himself.

12. Picture of a large ship (Schlitz Beer advertisement) on page 55 produced the impression:

Sensitive: "Someone linked up with ships; I feel I want to go sailing with them."

13. General Motors advertisement on page 65, showing an automobile that has broken down, the driver with spanner in one hand in front of the engine, and freeing his collar from the neck with the other hand, and a boy, in reply to: "Know where I can find a mechanic, sonny?", hollering at him: "Get a horse."

Sensitive: "Something is affecting the back of my neck, as if a collar is irritating me . . . Someone is shouting . . . Intricate, but we must piece it together."

This is a good example of perception of unrelated constituents of a picture and of something contained in the text, *viz.*, "shouting" (in the actual text: "hollering").

14. Kleenex Tissues advertisement showing a hand removing a sheet through the narrow slit in the box, p. 73 (Fig. 4):

Sensitive: "I am trying to fit something in a box; difficult to get it in."

A good example of distortion, in reverse, for the sheet is being removed from, and not inserted into, the box.
New Yorker, Jan. 20, 1945.

15. Picture and text of "At The Movie Houses" on page 9.

Sensitive: "Reflection of light on something white, but I do not see the light, only the reflection."

A thought-provoking impression, because the picture shows only the entrance to the house and not the screen inside.

16. A series of cartoons on page 17 showing a man pursued by a bird of prey, then turning and shooting it, and finally roasting it over a smoky fire.

Sensitive: "I have to hold the breath for something . . . A lot of birds . . . Something humorous."

The bird is shown four times in flight, hence the plural, and the ensemble represents something humorous. Notice again the reference to the breath being affected by the smoke.

FIG. 4.
Reproduced by permission of the International Cellucotton Products Company.

17. Picture of a woman dropping food into a bowl of water with (presumably gold-) fishes (p. 20).

Sensitive: "It's fish or something to do with fish . . . In a place with a lot of water; it is affecting my hands."

The focus in this case was only the bowl of water with the hand over it and the visible fishes: the woman and a low table on which the bowl was resting do not seem to have been perceived.

18. Picture of a waiter holding high up a small round tray with two glasses on it, p. 47 (Fig. 5):

TABLES FOR TWO

Memoirs of a Cellar Dweller

FIG. 5.
Permission the artist and *The New Yorker*, © The F-R. Publishing Corporation.

Sensitive: "I am holding a small round tray with two glasses on."

A perfect hit of something clearly visible.

New Yorker, Feb. 24, 1945.

19. Ford Motor Company's advertisement on page 5 showing a glass ball within which the traffic control indicator is seen at a crossroad, also a building in the background and three intently watching men, the glass ball being held by a brownish-colored hand and the contents inside the ball being in darker colors:

Sensitive: "A busy cross-road and I am hesitating which way to go . . . A building . . . Two different colored liquids in glasses, one brownish, the other much darker."

An interesting example because of the variety of constituents perceived and the distortions, which are self-explanatory.

20. Picture of ice-hockey match on page 33, showing players staring at the goalkeeper with hand on his mouth, the caption being: "Now what? Is there a rule for when the goalie swallows the puck?"

Sensitive: "Someone speaking almost in a whisper."

Many may consider this instance as far-fetched, which is the very reason why I am including it; for, if it is an actual ultra-perceptive impression, it is an interesting interpretation.

21. Rogers Peet Company's advertisement on page 48, showing man in overcoat fumbling with his lighter to light cigarette of lady on skates.

Sensitive: "Something I want to press quickly . . . Gentleman in overcoat . . . Gentleman fumbling with something in front of the coat . . . I want to move my feet, I want to run."

The first three impressions apply to the man and his lighter, and the fourth one to the lady on skates.
New Yorker, Nov. 11, 1944.

22. Cartoon on page 22 with the caption "Say 'Aachen' " showing a medical examination of war prisoners, the attendant examining the open mouth of one of them under a light by holding a small instrument in it in a dentist-like manner (Fig. 6):

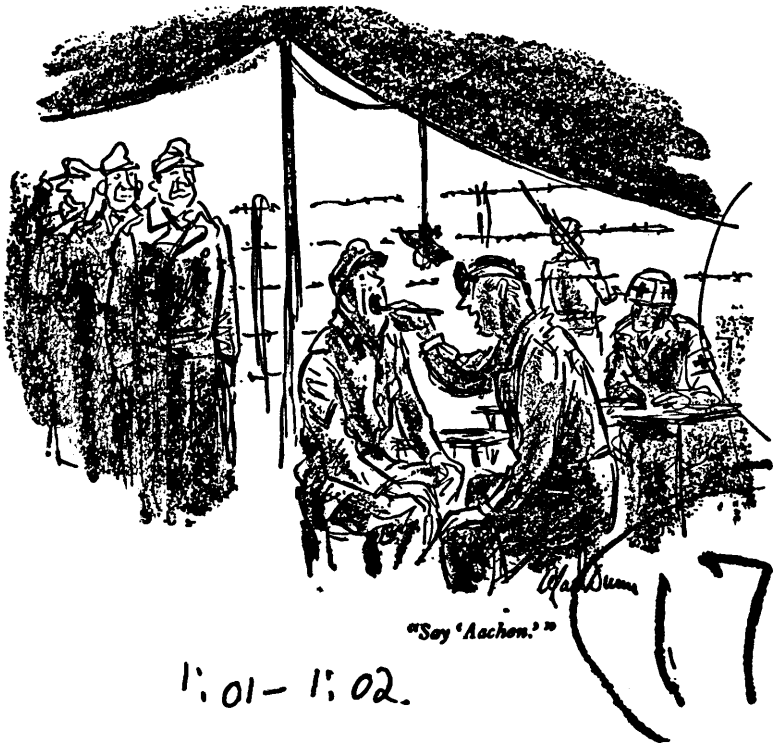


FIG. 6.
Permission Alan Dunn and *The New Yorker,*
© The F-R. Publishing Corporation.

Sensitive: "Something the matter with the mouth; I rather get the impression of a dentist . . . The light is not very good."

This is an excellent impression of the actual situation as depicted in the illustration.

23. Picture of a man emptying a white-looking container of sand into a large bin (p. 28).

Sensitive: "Holding up something that looks like a white cup; hot liquid in it."

This is an instance of numerous cases in which the sensitive distorts the impression by an addition.

24. Picture on page 37 showing two men and two ladies talking around a table covered with papers on the stage of a theatre, the profile of one of the ladies being perceived simultaneously with a most insignificant earring, produced the impression:

Sensitive: "A lady . . . has elaborate earrings."

This example is purposely selected to show that sometimes the least important constituent of a situation is ultra-perceived and exaggerated. This too is thought-provoking.

25. Blue Network advertisement, picture of a boy listening to radio (p. 51).

Sensitive: "I cannot hear very distinctly."

This and similar examples are intriguing, for they are not actual sensations but, according to the sensitives, either words heard or automatic utterings.

26. Pennsylvania Railroad advertisement, smoking chimneys of steelworks (p. 59) produced again:

Sensitive: "Oh, dear! I caught my breath."

Compare this example with the previous ones 4 and 16. I consider the repetition of the same impression with the smoke from different sources a very promising sign for the progress of our study by the use of the inductive method.

New Yorker, Jan. 27, 1945.

27. Cartoon on page 17 showing a man asleep, dreaming of sheep—ten of them in the illustration—running around and around in a circle.

Sensitive: "Something white and fluffy; very soft . . . Something spinning or revolving."

It is once more interesting to note that the revolving performance of the sheep and the color and fluffiness of their wool was perceived, but not the sheep themselves.

28. Cartoon of a man holding small boy upside down by grasping his ice skates (p. 26).

Sensitive: "Walking on something that is rather unsafe."

It is thought-provoking to find that the same impression was given

in example 3, where a man was skating on ice, and that in this case the person who had the skates on was not standing on them.

29. A series of cartoons (pp. 28, 29) of a man with a guitar, in one of which he plays the instrument by using the thumb of one hand and the third finger of the other hand.

Sensitive: "I want to use the thumb and third finger only."

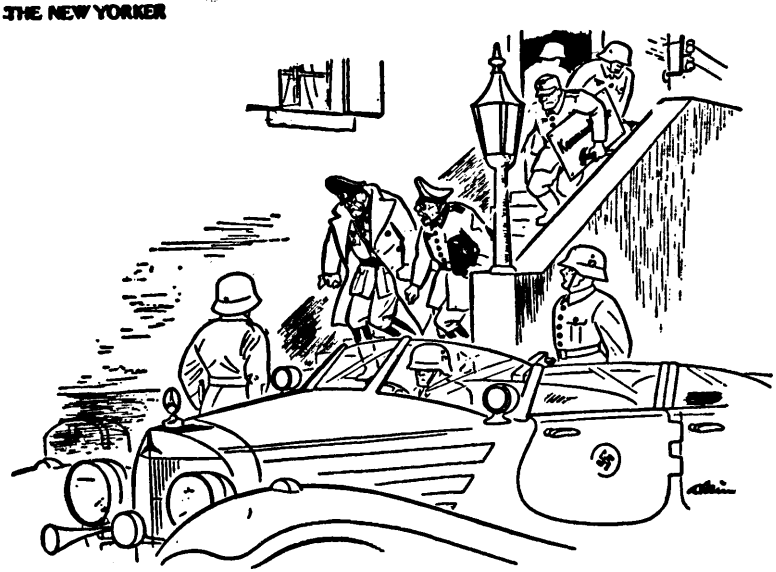
Another excellent example of a hit which is very specific.

30. Allen's Toffee advertisement on page 41 showing two men playing billiards, and smoking, produced quite a series of impressions.

Sensitive: "A lot of movement is going on . . . Something burning, peculiar smell . . . something I want to move the fingers on . . . I am trying to work my body, as if trying to lift my body and legs along a pole . . . I want to move something backwards and forwards."

New Yorker, April 7, 1945.

31. Picture on page 17 showing two German officers followed by soldiers, apparently vacating the offices, and a car ready for their departure, the caption being: "But in a sense we won't be retreating,



"But in a sense we won't be retreating, Herr General. We'll be heading toward the other front."

FIG. 7.

Permission Alain and *The New Yorker,*
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Herr General. We'll be heading toward the other front." Bearing in mind the date of this issue of the *New Yorker,* the sarcasm of the caption is quite clear (Fig. 7):

Sensitive: "Something about making a decision soon of going back."

32. Picture on p. 24 showing a room with stacks and stacks of paper money; caption: "Boy, I'd like to have a nickel for every dollar in this room."

Sensitive: "A small safe with money, papers . . . in it."

33. Cartoon showing a man embraced by a woman (p. 29).

Sensitive: "Close to my face a woman's face."

34. Vermont advertisement showing a coast line along the water (p. 38).

Sensitive: "Looking along the coast line."

New Yorker, April 14, 1945.

35. Picture on cover showing a dust mop, pinkish-red brick wall of house in background, produced the distorted composite impression:

Sensitive: "A lot of red dust in my eyes."

36. Jacqueline Cochran's advertisement on p. 38 showing a row of closed jars, and lower down an open one with fingers inside.

Sensitive: "A row of jars . . . I pick up the one that is mine."

37. Schaefers Beer advertisement on p. 43 showing a man holding a ball and stooping forward ready to throw:

Sensitive: "Someone says: 'Waiting for the kick-off.' "

38. Yardley's advertisement (p. 59) showing bottle of perfume.

Sensitive: "I wished I had some nice perfume."

New Yorker, April 21, 1945.

39. Farnsworth Radio Corporation's advertisement (p. 5) illustrating a moonlit scene; the moon is full. There are many clouds in the sky.

Sensitive: "Sun is shining brightly and then a grey cloud."

Mistaking the moon for the sun is, I am sure, understandable.

40. Lucien Lelong's advertisement (p. 6) in three colored stripes, green, red and yellow.

Sensitive: Something in colors, in stripes.

41. Cartoon on page 26 illustrating corner of a shop with hardly anything on the shelves and notices all over, no this and no that,

the caption being: "Wouldn't it be better just to have a couple of small signs telling what we *do* have?"

Sensitive: "In a room with a dresser in the corner, but nothing on it."

42. Budd's advertisement on page 47, with negro attendants—porters, dining car waiters, etc.—in a series of pictures.

Sensitive: "A lot of little niggers."

43. Kleenex advertisement on page 52: Kleenex repeated at least nine times.

Sensitive: "The letter K signed on different papers."

44. Dictaphone advertisement on page 63 showing man dictating into the microphone part of an electronic dictaphone used as a control center.

Sensitive: "Hearing a message coming through."

Look, May 29, 1945, used in the simultaneous test.

45. Koroseal advertisement (p. 5) showing shoe soles of different colors, including yellow; two men stretching them; yellow venetian blind behind.

Sensitive F: "Something I want to pull and stretch."

Sensitive K: "The color yellow seems to be very much around me."

46. Gem Razor advertisement, page 12. Woman gazing alluringly at officer, touching his tie (Fig. 8):

Sensitive F: "A gentleman in officer's uniform."

Sensitive K: "A woman connected here who does a lot of sighing and wishing."

47. Photograph of George Stirnweiss signing autographs (p. 40).

Sensitive F: ". . . I am writing."

Sensitive K: "I am writing something down."

48. Pepomint Life Savers advertisement (p. 41) showing open old-fashioned treasure box and the annular LIFE SAVERS scattered in and out of it; title: Pleasure Island.

Sensitive K: "I see an old-fashioned box open with a collection of old-fashioned jewelry in it . . . Thinking more of the past than the present."

Sensitive F: "A number of coins but not our coins."

The latter impression seems to apply to the symbolical contents of the box.

It will be seen from these instances in the simultaneous test with F. and K. that they generally perceive different constituents of the

FIG. 8 (*To the Right*)
Reproduced by permission of the American Safety Razor Corporation.

FIG. 9 (*Below*)
Reproduced by permission of the P. Lorillard Company.

picture; in example 47, they perceived the same feature—the writing. In the following case they perceived practically the same thing, which, however, was not at all applicable to the corresponding perusal in the magazine, *viz.*, Swans Floating Soap advertisement on page 31. The impressions were:

Sensitive F: "Examining letters, but I am going back to 43."

Sensitive K: "Looking through some old papers; among them a memorial card; looks like . . . 1858."

The double record shows a number of interesting hits by one of the sensitives in connection with a given picture and by the other one with respect to another picture; within the space available for this article I can give only one example.

49. Advertisement of McQuay-Norris (page 4) showing a man banging an automobile with a long whip produced the impression:

Sensitive F: "Something keeps banging."

50. Advertisement of Old Gold Cigarettes (page 63), showing a young man and lady fishing, produced the excellent composite impression (Fig. 9):

Sensitive K: "I am looking at such a lot of goldfish."

The second series:

As stated above, this series of tests was carried out with a prepared scrapbook of sixty pictures, each contemplated for one minute, which book was used throughout the six tests. The two sensitives F. and K. psychometrized simultaneously Mrs. Dale's writing (prepared in sealed envelopes) at the exact times the pictures in the book were being contemplated.

This last series produced some of the best results for qualitative analysis, because of the cumulative effect of the impressions of the two sensitives with respect to the same pictures during a succession of sittings. Owing to lack of space, I will confine the analysis of this group of tests to a mere recital of those sets of impressions which, in my view, were among the most significant ones. True, there were also a few good hits among the control impressions in both series; but, again in my opinion, many fewer and hardly of the same standard qualitatively.

1. *Picture No. 2.*—A short middle-aged woman who has fallen down the staircase; she lies face downwards, legs still high up on the stairs. A large dollar sign above her shoulder; caption in two lines: "There's money in accidents."

Sensitives: "I feel I want to spring off . . . Elderly lady smaller than myself, something wrong with feet and legs . . . Lady smaller

than myself, excitable at the moment . . . Two lines of writing . . . Using very narrow stairs . . . Talk of money . . . Registering an awful feeling . . . I see coins in a row . . . Someone jumps from one place to another, just getting on the feet again."

It is to be borne in mind that in this and the subsequent examples, impressions which are identical or equivalent belong to different sittings or were given by each one of the sensitives separately.

2. *Picture No. 10.*—Mail Call in a soldiers' camp; the caller has a bunch of letters in one hand and a single one held up in the other one; the other men around look up at him and listen; their only attire is a pair of trousers held up by a belt; the caller has a wrist watch on and the man next to him wears a necklace.

Sensitives: "Strip of paper . . . I am looking at some old autographs . . . I don't want to say anything but listen intently . . . A watch and a necklace . . . A pair of trousers just held up . . . A lot of men, a conference . . . Something about letters—'dead letter office'."

3. *Picture No. 13.*—A weathercock (with the inscription "Lightning Without Fear") lit up by a lightning flash in the form of an *Y*. The cock stands high up on a feathery arrow, below which is the vane, N, S, E, W, above a ball.

Sensitives: "Getting into difficult positions. . . . A springy jump . . . I feel rather giddy from looking down . . . A flight of birds—feathers . . . Lighting arrangements . . . Something large, looks like letter Y, the shape of it . . . Something small keeps changing hands . . . A fine bird, a cockerel . . . A ball game."

4. *Picture No. 16.*—Artist painter laid up in bed, sitting against raised pillow, wrapped and strapped from back to front; he holds in hand a painting of fruit; the whole of the picture is colored, the main colors being blue and green and the blue is of various shades.

Sensitives: "I feel very cold . . . Pain at the bottom of the back; I cannot sit in one position too long . . . In a very cold place and I feel I want to pull things round me . . . I got something strapped on my back . . . A lot of food . . . Someone with artistic touch; I want to describe color effect . . . I have to stand up, but I feel I ought to put my head back and rest . . . A lot of oranges . . . Color blue in all kinds of shades."

In all these examples, obvious distortions are referred to only when quite obvious; on the other hand, details which cannot be described in a few words are left out, as they cannot be easily followed without the pictures.

5. *Picture No. 26.*—A prison cell; white inscription on the wall: "We Who Are About To Die"; square open window with iron bars,

through which comes a bright shaft of light; in the distance, one sees the sky with a number of elongated clouds, and just outside the window, the noose of the rope by which the prisoners will be hung. The words "Condensed Book" appear in a corner.

Sensitives: "A small book which I am studying . . . I want to open and spread out something white . . . Holding on to a rope and I felt I was almost pulled up with it . . . Something affected me rather peculiarly in the head . . . I want to throw my head back . . . I want to hold my head right down . . . Looking at a sky but I see no aircraft."

6. *Picture No. 35.*—A thick green branch of a tree against a blue background, covered by a long train-like Aphis Lion. Inscription: "Aphis Lion *Devouring* Plant Lice," and text describing it as a Wing *Fly.*

Sensitives: "In a train, very near the dining part . . . Interested in flying . . . Just saw a spider. (One of the flies looks like a spider) . . . A beautiful blue . . . Impression of something sticky, glutinous, as if coming from a tree."

7. *Picture No. 37.*—Inside of a church; stained glass windows with small, hardly distinguishable figures; choir in surplice; a few people.

Sensitives: "Something made of glass and I want to do something with it . . . Strong smell of scent . . . Expected a lot of people, only a handful turned up . . . A choir boy's surplice . . . Looking at a lot of special pictures."

8. *Picture No. 38.*—*Three* little girls with their arms close to one another drinking milk from glass bottles through straws; reflection on the table. Also *three* little puppies with a flat milk dish.

Sensitives: "*Three* people walking arm in arm . . . I have *three* to choose from . . . In a room where is a lot of glass; much reflection . . . *Three* magnets."

9. *Picture No. 43.*—Head of Joe Louis, surrounded by stars. The last two of a text of four lines underneath the picture reads: ". . . who became heavyweight boxing champion and 'good-will ambassador' of the colored race."

Sensitives: "Something is irritating the arm . . . As if someone was holding me down to prevent me doing anything . . . As if I wanted to work both my hands up and down the body . . . Want to jump quickly . . . Someone is counting; wants to count movements. (The sensitive moved the index finger up and down vertically) . . . Two men in the attitude of boxing . . . Two men changing places . . . I am not taking sides . . . Impression about famous people."

10. *Picture No. 49.*—Two seals resting. The following impressions given by the sensitives would seem to refer to the seal in action.

Sensitives: "Something very high up, as I have to hold my head back to look at it . . . I do not know whether it is fishes or fishing . . . I want to get up, to jump at something quickly . . . I don't know whether it is water, but I feel wet . . . Something right over the head."

11. *Picture No. 50.*—An antique shop; part of the word *Antiques* is visible on the side of one of the tables, of which there are many, covered with all kinds of things, including a small statue, dolls, a pile of what looks like white sheets of paper or something similar; the shopkeeper has a white coat on and he and the buyer wear glasses; two electric lights with opal shades hang down from the ceiling.

Sensitives: "I have glasses on . . . Some white material . . . Antiques . . . Old-fashioned inn, tables laid, not elaborately . . . Some talk about a dummy . . . Large sheets of paper . . . Series of pretty things . . . Electric lights with opal shades . . . Statue, a small one."

Conclusion

I hope that, in spite of the fact that only a few pictures were reproduced, readers have been able to appreciate the pertinence of the impressions, and that, quite apart from statistical evaluation, the technique of which we hope to be able to improve in connection with future contemplated experiments, they feel, as I do, that the tests as a whole provide substantial *prima-facie* evidence of transatlantic telepathy.

To the American Society for Psychical Research, I hereby wish to express my thanks for the willingness they have shown in cooperating in these tests, and especially to Dr. Murphy, for having encouraged the work, and to Mrs. Dale, for the eagerness, promptitude and thoroughness she displayed throughout—not to mention the large amount of work she carried out—in spite of the difficulties which still existed during the first half of 1945. I also wish to thank Mr. Gibson for the very laborious work he did in connection with the statistical side, on which we hope to improve by some new method of control, and to Mr. F. E. Creed for acting as the second recorder in the simultaneous tests. My gratitude also goes to the two sensitives, Miss F. Fallows and Mrs. F. Kingstone, who on some occasions, while the war was still on, gave their impressions under trying circumstances. Finally, the permission granted by *The New Yorker* (F-R. Publishing Corporation), International Cellucotton Products Company, Elizabeth Arden, National Distillers Products Corporation, American Safety Razor Corporation, and P. Lorillard Company to reproduce the illustrations herein incorporated is also acknowledged with thanks.

W. Whately Carington: In Memoriam

GARDNER MURPHY

In the years immediately after the First World War a young British scientist, W. Whately Smith (later to be known as W. Whately Carington), appeared upon the scene. He was destined to alter very profoundly the structure of psychical research both as an experimental discipline and as a system of ideas for the understanding of man. As early as 1920, Carington (as we prefer to call him) in his publications showed plainly the three inter-related interests which he carried forward with him throughout a singularly productive life—a life all-too-soon terminated on March 2nd of this year. First, he was a devotee of the newer experimental psychology belatedly accepted at Cambridge University, the center of his own earliest intellectual activities; he was especially interested in the experimental use of the "free association" test as a clue to submerged complexes in the manner defined by Jung, and the accompanying studies of the galvanic phenomena of the body which showed the bodily upheavals which accompanied associations of special import to the person tested. His book *The Measurement of Emotion* (1922) reported pioneer investigations into these processes, and in particular of those devices by which memory screens off that which has damaged or threatened the self-esteem of the subject. Secondly, he had already made himself thoroughly familiar with the landmarks of psychical research, publishing in 1920 a readable popularization of research findings under the title of *The Foundations of Spiritualism*. Thirdly, as a mathematician and philosopher, he dug deeply into the problems of mind and body and of the relations of the individual to the cosmos, in such fashion as to formulate a physically and philosophically defensible view of survival and of post-mortem existence. The clarity, energy, and superabundant originality which marked all of his subsequent work were evident even in these earliest publications.

Having the good fortune to visit London and the Society for Psychical Research in those years, when a strange brightness and optimism seemed to pervade the capital of the Empire, and when a sturdy group of scholars were first energetically pursuing the reaches of Mrs. Leonard's mediumship, I was gratified to receive an invitation from Carington to lunch with him at his club. For the most part, it was I who had sought interviews; but with characteristic generosity Carington, who had heard of the young American who was trying to find his way about the world of psychical research, took the first step, drew me out, and let me ask and tell whatever was closest to my heart. The warmth and vitality of his manner, the breezy, almost

"mid-western" joviality of his approach, took me by surprise; but it made so deep a mark on my memory that I can still see him with his twinkling eyes, his very broad smile; I can still hear his chuckling comments on the perversities of psychical researchers. All this made me feel drawn by the hand, as it were, into a junior partnership, and neither time nor distance can dissolve it. On the occasion of the lunch, we were joined for part of the time by Mr. Carl Vett, organizer of the international congresses of psychical research, and the three of us talked about practical steps toward international collaboration in research.

Carington was at this time about to enter upon some experimental researches for the Air Ministry, so that he was, as usual, keeping one foot solidly planted on the "here and now" aspect of science, the other equally firmly planted in that area of bewildering aberrations of human personality which the universities were unwilling to recognize.

As a matter of fact, after 1922 not much more came from his pen regarding the experimental and quantitative approaches to the everyday psychology and physiology with which established science deals. To be sure, many findings relevant to psychology and related disciplines became evident in his numerous psychical research publications; but at huge economic, and even greater personal cost, he was turning more and more to a full preoccupation with psychical research as such. The group of studies with which his name first became solidly established dealt with efforts to use the association test, with which he was so thoroughly familiar, as a device for ascertaining whether or not mediumistic controls and communicators are in fact individualities distinct from the mediums through whom they communicate. By and large, the effort to establish the complete independence of control and communicator had already failed in one case after another, and the penetrating logic of Mrs. Sidgwick's study of the psychology of Mrs. Piper's trance (*Proc.* S.P.R., Vol. XXVIII, 1915) had tended to put on the defensive those who still asserted that one comes face to face with an actual discarnate personality distinct and independent from the personality of the sensitive through whom the communications are received. There were many investigators who maintained that there was a literal invasion of the sensitive's body by the discarnate intelligence; but there were others, like Mrs. Sidgwick, who believed that the discarnate acted telepathically upon a deep substratum of the sensitive's mind. The question arose, however, whether the distinction thus made was actually as sharp as it appeared; whether there might not be a form of interaction between incarnate and discarnate minds which would

make inappropriate the "either-or" way of looking at the question of the reality of independent communicating intelligences.

And beyond this question lay the question whether one could establish by objective methods a kind of personal continuity beyond death manifest not in terms of sheer *memories* or other "mental contents" of the communicator, but rather in terms of the *deeply ingrained habits,* mannerisms, tics, points of view, which are typical of any given personality. In the *word association test* one may perhaps find an answer to this question. Thus persons who are inclined to give opposites—that is, when presented with the word "good" they say "bad," or when presented with "white" they respond with "black"—continue in spite of changes of mood or of experimental situation to cling to such associative habits. Kraepelin had shown that in the profound emotional upheavals of the manic-depressive psychosis, and even in the disintegrative processes of dementia praecox, a basic individuality of word association remains. It occurred to Carington, then, that it might be possible by repeated tests to bring out the fundamental associative habits which lay at the root of a given sensitive such as Mrs. Leonard, and to compare with these fundamental personal habits the habits of controls and communicators operating through her.

With energy and patience, despite many false starts and premature conclusions, admissions of error, and redefinitions of the problem, Carington carried out a series of quantitative studies of trance personalities which at a minimum gave us an hypothesis with which to continue such fundamental investigations. The evidence concerning the nature of mediumistic controls, as a matter of fact, was fairly clear: he found a definite tendency for controls (Mrs. Leonard's Feda and Mrs. Garrett's Uvani) to spend a long time on those associations which were given with special rapidity by the sensitives in their normal states and a short time on those associations which the sensitives had given slowly. In general, that is, with Mrs. Leonard and Mrs. Garrett an inverse relationship was found between association times when their own mental apparatus and that of their controls was compared. The association times were "countersimilar," as he formulated the relation. This points, he held, to the fact that the controls are dissociated fragments of the sensitives' own personalities rather than personalities in their own right. It should be noted, however, that trance personalities (communicators) other than the sensitives' regular controls did not show countersimilarity with the sensitives through whom they communicated. In fact, in his final paper on the problem of trance personalities, Carington describes results which tend to strengthen the claim of autonomy for communicators.

It remains true, of course, that the histrionic skills of trance

sensitives are large and their limits incompletely explored, and it is perhaps possible that by one quirk or another the results might have been achieved through the assumption of attitudes in one mental state which are diametrically opposed to those shown in another state. The problem with which he had grappled was not, even in the final paper on the research, completely solved. He had, however, formulated the problem about which psychical research had speculated for several decades: how may we get beyond the question of content, which might well be telepathically transmitted to the sensitive from any mind, incarnate or discarnate, and cope directly with questions of the fundamental organization of personality? He saw, as so many students of telepathy and clairvoyance have come to see, that questions are unanswerable, and even in large part unstatable, except through the formulation of a clear, quantitative procedure.

At the time of these mediumistic studies, he had just changed his name from Smith to the earlier family surname of Carington, and publications from then on are under this name. Almost all the work in this latter half of his creative period deal with telepathy and closely related problems. The same determination to be systematic, thorough, exact, and quantitative, which had not come to full fruition in his studies of trance mediumship, came to brilliant fruition in his telepathy research—perhaps indeed partly because of errors earlier made, for he had shown a very extraordinary capacity to pick up broken pieces and go forward, making the most of everything learned in the process.

It may be well to recall here how the problem of telepathy research stood in the early and middle 'thirties. For some fifty years, experimental studies of telepathy had been based for the most part upon free drawings "transmitted" from agent to percipient, or upon the capacity of a percipient to pick up impressions about what the agent was doing at the time or had been doing during the preceding day. Quantitative studies, as by the Groningen University group in Holland in 1921, and by G. H. Estabrooks at Harvard in 1925-1926, had been brief and fragmentary, threw rather meager light on the nature of the process, and in no way showed how the experimental procedure could be repeated with new subjects in another laboratory. The brilliant experiments initiated by J. B. Rhine at Duke University had of course been hailed in Britain and in the United States as constituting a very important forward step; but it had become clear that specially gifted subjects of the type found by Rhine could not be picked up under every stone. Indeed, it was difficult for many investigators, American and British, to find subjects capable of scoring significantly in either telepathy or clairvoyance tests. A long series of carefully planned radio experiments by S. G. Soal in

England had apparently revealed that the impressions received by his listeners differed in no respect from a chance distribution of impressions—there was no evidence of telepathy at all. Yet Carington, as a scientist with a conception of the nature of science as a socially shared process and of the vast importance of defining a method which anyone anywhere could repeat with reasonable likelihood of consistent results, was not willing to give up. He was stimulated by this dismal state of affairs to design an original and extraordinarily workable procedure.

Often in science, as in everyday life, it is the thing which is so simple as to be overlooked that constitutes the very core of what needs to be done. For over fifty years psychical research had carried out its experiments in telepathy and one could still argue very plausibly on either side of the following question: are experimental telepathic phenomena limited to a few special sensitives, or are they the common property of the human race capable of being realized in some degree by everyday mortals of both sexes and of every age, degree of endowment and personality make-up? On the whole, the evidence seemed to support the view that the former alternative was correct; that except for a few special sensitives, the rest of us were completely without a capacity for paranormal responses under experimental conditions. Repeatedly inviting large unselected groups of people to take part, Carington put to the test the hypothesis that a telepathic endowment, in a form susceptible of test, is a common property of the human race, and through the remaining years of his life he drove home one blow after another in support of this conception.

In the earliest experiments, the procedure consisted simply of placing a line-drawing in his study, leaving it there over night. Reports were received from scattered percipients who had attempted to duplicate the drawing. He then asked a friend to act as "umpire" to determine the degree of success obtained by these percipients. A control was automatically provided through the fact that the order in which the drawings had been exposed night after night through the course of the experiment was unknown to this umpire. Thus, in stating that a given drawing submitted by a percipient should be counted as a hit in relation to a given target prepared by Carington, the umpire could not know (except perhaps paranormally) whether the agreement was with a true target actually used on the occasion in question, or with a wrong target belonging to some other date. Massing all the material, one could ask whether there was a tendency for the percipient drawings to come in *at the right times* and to agree with the target drawings which were set up at the time when the percipients' responses were made. After some experimentation

with this and with a related method in which part credit for part success was given, encouragement came from the fact that there was clear evidence of a "mass effect" on the part of the percipients, making them send in the right drawings on the right occasions. He then made various fundamental improvements in procedure. The original drawings had at first been based upon the random selection of words from the dictionary, all "drawable" objects being regarded as equally suitable for experimentation. In later work, however, lists of drawable objects were prepared in advance, and from these random selections could be made at the appropriate time. More fundamental, however, was the ingenious stroke by which the use of umpires was altogether eliminated. As more and more material came in, it became obvious that a fair sample of the responses of the British public to this type of experimental situation was being obtained. Among the responses one finds many motor cars and tea cups, a fair number of telephones and blackbirds, very few rhinoceroses or cyclones. One decides, then, that the amount of credit to be given to each hit will depend upon how easy or how difficult it is to make such a hit. The commoner the object upon which hits are made, the less credit they get; the rarer the object, the more credit they get. We shall not here bother with the mathematics of the formula developed by R. A. Fisher, or Carington's use of it; nor with his long and patient development of the formula for repeated experiments. Special emphasis instead will be placed on the systematic development of a "Catalogue" derived from the huge mass of material. It was our privilege at this Society to work with Carington for some months on problems related to the Catalogue construction, and to publish in full in our *Proceedings* this Catalogue which he was finally willing to release in official form. The catalogue method of scoring henceforth consisted simply of defining the credits which all percipient drawings should be given, and of ascertaining whether the day-by-day agreement of such drawings with the intended originals was clearly beyond attribution to chance coincidence.

Now, as to results. Even in the first stages of his work, Carington found clear evidence that subjects were hitting the intended target, but that they were also to some degree hitting the target which came next; that is, a target which would be exposed at the time of the next experiment. So, too, they often hit a target which had been exposed on the occasion just preceding the one officially intended. It was just as if the percipient were a little confused as to the time in which he was functioning, or the time in which Carington was functioning, or both. As early as 1941, Carington had accepted the evidence for precognitive processes which came from these experiments. Control data showed clearly, however, that this tendency to

hit future targets was not an artifact, and that the more remote in the future the target was, the smaller the number of hits upon it. Fortunately S. G. Soal, who stood empty-handed after his arduous radio experiments, was sensitive to a suggestion conveyed to him by Carington that in his current series of experimental tests with individual subjects (as distinguished from his radio tests) he should look for systematic displacements. Sure enough, Soal found in the data of two subjects clear evidence for such displacement. These subjects showed a marked tendency while engaged in tests with cards to hit the card which came next in the series, or, in certain experiments, to hit the card in the position two steps forward. It was this finding which led to the very important experiments in precognitive telepathy which Soal, and later Soal and Goldney in collaboration, carried out with the subject B. S. (*Proc.* S.P.R., Vol. XLVII, 1944).

Since Carington's results with his improved method have already been described on various occasions in the pages of this JOURNAL, we shall content ourselves here with this brief recapitulation, and go forward to a somewhat fuller consideration of the interpretations which he placed upon his findings. It seemed to him clear that the percipients who took part in his experiments were somehow in touch with his own mental operations. This was obviously not due to any intimate or emotional relationship; he was often unknown to individual percipients, and in the inter-university experiments we have the further problem of subjects scoring successfully on targets prepared by persons in no way in rapport with them. On the other hand, it appeared that the distribution of a photograph of his study, showing the location at which the target drawings were to be placed, enhanced the scoring ability of his percipients. He began to wonder if the classical association psychology might not be put to work in the service of the problem. As he prepared for an experiment, he *associated a certain drawing with the "idea of the experiment"*—with the room in which the drawings were to be placed, with the date, etc. There was, then, a network of associations having to do with the experimental situation, a great many of which were thoroughly known to the distant percipients—for they knew the general plan of the experiment, had an opportunity to see the photograph of his study, had in mind the dates for the various tests, and so on. One item connected in Carington's mind with all these things—namely, the specific drawing used—would also become accessible to the percipients *if* it happened that the distinction usually made between different human minds should turn out to be an artifact or philosophical prejudice.

The conception is that all human minds are one mind, arbitrarily

viewed by a sort of prism which artificially separates them. If, consequently, A has in mind a group of inter-related items, and some of these items (or, indeed, only one of them) are supplied to B, the latter will, by association of ideas, come into possession of the missing item or items. This is, of course, a version of the theory of "one big mind," the doctrine developed by the sages of ancient India, by Plotinus, and by many Western mystics; and there is a great deal in F. W. H. Myers, in Bergson, in Grasset, in Warcollier, and in the writings of many other psychical researchers which tends in this direction. It is, however, important to note an essential and important difference between Carington and all other advocates of such doctrines. Carington brought to bear on the problem the discipline of long philosophical and experimental training, and he saw that the theory was of value only in so far as it could be put to work, submitting itself to rigid experimental tests. This he proceeded to do. He showed, for example, that the laws of association as they have been tested under laboratory conditions can in some degree be directly transferred to the telepathic situation. We have, for instance, the law of frequency, which says that the tendency of one idea to arouse another will depend upon the number of times the connection between them is made. Accordingly, Carington was able to show that the more frequently repeated and the more firmly established the associations were between two events in the mind of the agent, the more clearly the first of these events will give rise to the second (*i.e.*, the distant target) in the minds of the percipients. The Carington hypothesis cannot be lightly pushed aside on the ground that one does not like "philosophical theory." The theory has become a guide to experimentation; it will be tested and finally accepted or rejected in terms of empirical results.

The intensity of Carington's philosophical interest became very evident during the years from 1941 to 1947, as the implications of his telepathy theory were more and more systematically developed. In his book *Telepathy,* published first in England, then, a year before his death, in this country under the title of *Thought Transference,* he found room for systematic application of his theory to problems of social psychology, international relations, morals, and the general trend of human life upon this planet, and also for a crisp and vivid formulation of a theory of survival. As he saw it, a cluster of associations belonging to a single mind—a psychon system—is only loosely hung together, as we know from the phenomena of absent-mindedness, sleep, trance, multiple personality, etc. And it is quite possible that just as during our lifetime the system may become more strongly or more weakly organized (or may display the accretion of new elements, or the tendency towards fission or collapse), so the process

of dying may grossly alter the properties of the psychon system. Some individuals may be able to survive the shock of bodily death, others not. Post-mortem existence will not be fed by new sensory impressions, but psychon systems may well continue—and indeed under certain circumstances may in death make progressive consolidation.

It may be of more than historical interest to note through what an evolution Carington's own thinking had gone since he wrote in *The Foundations of Spiritualism* (1920) that ". . . it would be rash to assess the chance of the 'spiritistic' hypothesis proving correct at a value appreciably greater than one half." It was not a process of growing old or soft that led to the gradual consolidation of his conviction of survival. The hardest, toughest, tightest thinking of his life was that which he did during the last half dozen years, and whatever the future may yield by way of new reasons to accept or reject his view, it was empirical material from psychical research and its direct implications that chiefly guided his changing view on the subject of survival.

The concept of a psychon system was in fact for Carington much more than a theory of mind; it was a theory of the cosmos. He carried with him through all the years the interests which had taken shape as early as 1920 in regard to mind-body relationships and the relations of telepathy to clairvoyance. In the recently published discussion by Rhine and various British scholars on the question whether the evidence for clairvoyance is stronger than the evidence for telepathy, we find Carington stoutly maintaining that the mind knows things by incorporating them within itself, and whether the external things thus appropriated are mental or physical is a meaningless question (*Proc.* S.P.R., Vol. XLVIII, 1946). The process by which we cognize what goes on in other minds and by which we read a distant card or page of print is one and the same process. We know nothing except the things which are experienced—for "things-in-themselves" as they exist outside of experience are not things with which science can deal.

During the last few years of his life, Carington's tempo, always a lively one, became more and more eager and intense—even to the point of frenzy; the amount of work he turned out in the course of six years is beyond belief. Some recognition of the value of his labors was evident in the award of the Perrott Studentship to him in 1940, and a Leverhulme Research Grant carried him on for an additional period. The sums, however, were small. He could afford no clerical assistance in his work, nor did he have in his day-by-day living what we should consider the elementary comforts. These deprivations drove him to the point where his nerves became jangled and his

patience worn thin. The correspondence with him during these years is full of warmth, generosity, intensity, excitement, enthusiasm and despair, protest, and phoenix-like renewal of youthful energy. His response to the tiny little things which we were able to do to encourage his work led to more and more direct communication of the real purposes of his life. He wrote us on January 29, 1942: "The position is this: For the last 18 months or so I have been sweating away, by one means and another, at evolving a reasonably repeatable technique not prohibitively cumbrous, etc., in practice. I think that I have got one, though I do not for a moment suppose that it is perfect or that there are no more fences to be cleared; but the strong indications are that it is good enough to go on with. To expound this at all adequately, covering even the most important points, needs a biggish paper, and to use it requires my Catalogue . . ." He thought of himself as laying down once and for all a master key to the repeatable experiment in psychical research. It reminds us of old Champollion, who, on his death-bed, said to the printer's boy who came to take his decipherment of the Rosetta Stone, "be careful of it; it is my *carte de visite* to posterity." He wrote on March 9, 1942: "Please guard the Catalogue with your life, so to say, until the final version is in some sort of print. It contains 'all the Laws and Prophets' and 'everything necessary to salvation'—or at least to conducting experiments in the paranormal cognition of drawings! I hope you may find it interesting to look at if nothing else . . . I believe this to be the first catalogue of its kind ever produced, and it represents about two years of my life, so treat it seriously!"

He guessed perhaps how short his remaining time was to be. He must have known at least that human nerves could not long maintain such a pace, and, moreover, that he had done what was essential to make his method clear and secure. Even the humiliations of poverty, illness, and inadequate recognition of his work never prevented the bubbling-up of a geyser-like humor in which he managed somehow to combine a sardonic laughter at his own expense with an honest protest against the failure of our generation to note the monumental importance of psychical research findings. As he said of the bigoted and narrow-minded, "May their psychon systems painfully disintegrate!"

Fortunately we of the A.S.P.R. were brought very close to Carington in the last months of his life through the opportunity afforded to Miss Signe Toksvig, a member of our group, to visit him in Cornwall at the end of 1946. For hours on end he talked to her of the progress of his thought, and despite many evidences of increasing physical illness, he gave her a glimpse of that indomitable spirit which had become so well-known to us through all our long cor-

respondence. Through Miss Toksvig, and through letters from Mrs. Carington, we have learned of the extraordinary struggle with which he maintained his creative effort even in the dark days at the end, even after he became virtually blind through retinal hemorrhage. Suffering acutely from the bitter winter weather in Cornwall and the absence of even elementary comforts, he struggled to work on the consummation of his life's efforts—a philosophical work to be entitled, we believe, *Matter, Mind and Meaning.* Some portions, we learn, of his posthumous work remain. We feel, however, that, as in the case of F. W. H. Myers, death came approximately at the moment of fulfillment. His was a life of extraordinary richness, variety, nervous trial and error, full of mistakes and contradictions, crowned always by an amazing capacity to recognize error and to go forward to a new vision. To us it seems highly probable that Carington himself detected all the major errors, and that the association interpretation of telepathy, with its accompanying method for a repeatable experiment, will prove to be one of the great permanent landmarks in the history of psychical research, or a cornerstone upon which someday a stable edifice will be built. His own modest evaluation, written in reference to a theory proposed in 1933, might well, except for its excessive modesty, be used regarding his life as a whole:

"And I must confess that, when I view the building in perspective, I am as much struck by the irregularities in execution as by the symmetry of design. I console myself with the reflection that the strong, graceful columns and delicate traceries, which we admire so much today, are never more than rubble for the concrete of tomorrow; so that if I have provided serviceable material or a suggestive plan for the erection of a nobler edifice, I may be well content" (*The Death of Materialism,* 1933, p. 253).

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W. Whately Carington: A Few Personal Impressions

SIGNE TOKSVIG

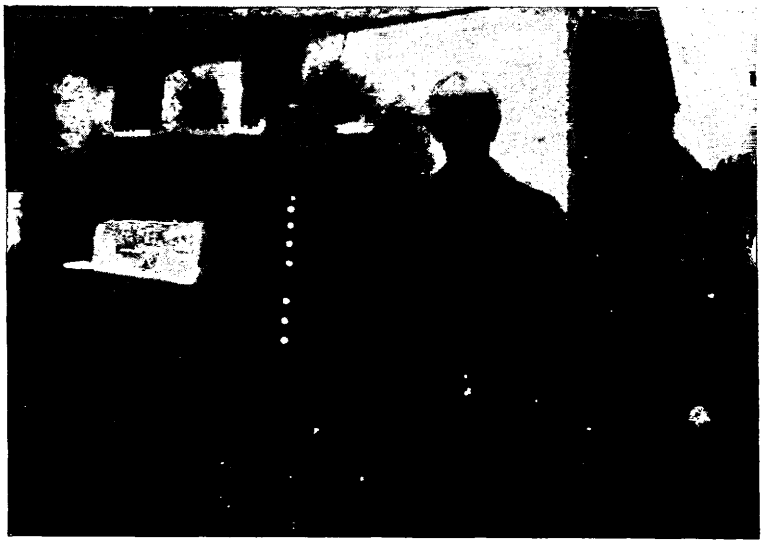
"Whately Carington never comes to London now," I was told by several people, when I was there in 1946, yet I sent him the letter of introduction given me by Mrs. Laura Abbott Dale, his valued correspondent. It did not entice him to London but it procured for me an invitation to come to Sennen Cove, Cornwall, where he lived. Sennen Cove is at Land's End, which sounds formidably far away, especially late in November in an unheated English train, but the eight hours to Penzance, where the Caringtons were to meet me, went quickly enough, through Devon with its warm red rocks and glimpses of sea and through Cornwall, bleak and Celtic with its pale pyramids of sand washed from the China clay. It was dark when I reached Penzance. Carington had written that I should look for a "tall, blond, baldish, discontented professorial type of man with a wife one of whose ancestors had obviously had an affair with a gypsy."

So when through the murk of the platform a tall and in fact rather academic looking man loomed up we made for each other, and presently his charming wife, Hedda Carington, arrived, looking quite like a handsome gypsy. Not being able to tuck me on their motor-cycle, they had hired a car, and in it we bumbled to Sennen Cove, about ten miles away. I was to stay with a villager and eat with them.

We started talking in the car and kept it up till I left two nights later. Sennen Cove curves in from the open Atlantic, and what with the spindrift and mist from the huge rollers and the steady rain the weather favored indoor amusement.

The Caringtons rented a cottage near the beach (£15 a year), one of those whitewashed stone cottages without any modern conveniences that are common in Cornwall, Wales, and Ireland. It had a minimum of comfort, but they had chosen it rather than the far more luxurious life which a man of his brains could easily have procured, because with a low overhead he could have freedom for the kind of work to which he had devoted his life, psychical research. But it meant rough, hard, menial tasks for both of them, and I cannot help thinking that it must have helped to shorten his life. He was obviously under various kinds of strain.

The cottage was big enough, however, to afford him a study, where he worked his intricate calculating machine for me and showed



W. Whately Carington and his wife. Reproduced from a snapshot taken in November, 1946, by Miss Signe Toksvig.

me the long row of cases with the target cards for his telepathy experiments. He meant to give them to the S.P.R. when he went to France. Going to France was his dream. They hesitated between being near Dinant in Brittany or near St. Rémy in Provence, "and you must come and live down the road, about a mile away."

That delighted me! Although he frowned and shook his head at some of my interpretations of his theories, he approved of others. Far from being egocentric, he gave keen attention to my problems also. A couple of chapters in my biography of Swedenborg were largely based on his association-of-ideas theory of telepathy, and he was struck by the way in which Swedenborg's ideas fitted in with his. He knew little of Swedenborg except his name, and for that very reason it interested him, as he repeated to me later in writing, that Swedenborg "quite independently and as the result of an entirely different line of approach" had come to much the same conclusions. "I am almost tempted to suspect there may be some sense in my own conclusions after all."

It was mainly the question of some form of personal survival that he was thinking of here. (After his death his wife wrote to me that he had signed a statement shortly before dying that he believed in survival more or less on the lines expressed in his book *Telepathy*.) He said he felt very strongly that for ethical reasons we must try to escape from our "I-ness" rather than want it to be "immortal"; hence his preference for an ultimately dissolvable psychon-system. His religious philosophy seemed to be the same as that of Krishnamurti, at whose meetings in Ommen, Holland, he had often been, and where he had met his wife, who shared his opinions. Consciousness, he felt, could exist without "self"-consciousness.

While he talked his favorite kitten, Lao-tsu, climbed all over his chair and himself. Lao-tsu mysteriously kept himself a dazzling white little ball of fur, rose-muzzled and blue-eyed, and he was fond of perching on Carington's shoulder and delicately tugging at his ear or even his shaggy eyebrows. Two other kittens and their mother frolicked around. He was very affectionate with them. How to get them to France was on his mind. "But," he smiled, "I have a son who is a gentleman fisherman; you know, he has a schooner and he fishes for a living, so maybe he can move us all."

As he spoke, I thought of the wall in his study, the wall which held his family pictures. There was a colored miniature of an officer who had lost his leg in the Peninsular War. His pegleg stuck out straight in front of him. He refused to have an artificial leg. There was Archbishop Whately of Dublin, splendid in lawn sleeves, a brilliant and a good man who had irritated and delighted his contemporaries. There was his rather melancholy but beautiful mother,

and his father, who had been in Lloyds. But the army and the clergy dominated. Carington had himself been an aviator in the first world war.

If his son were a gentleman fisherman was Carington himself not a kind of gentleman philosopher? But certainly not for a living! He could hardly be classified, and yet as we sat there by the little coal-fire and I was swung both high and low by his challenging talk I did, in my mind, pin a label on him—the very honorable decoration of English eccentric! I doubt if he could have been anything else but English—the kind of Englishman that flames at stupidity, ignorance, injustice and at the unclear thinking that leads to all of these.

Whately Carington was writing a book; it was about two thirds finished before he died. It was to be in three parts, Matter, Mind and Meaning. Much of his conversation referred to it; he was endeavoring to clean up, scour and redefine fundamental terminology before he felt he could go on with experimental work in psychical research.

I was therefore bombarded with a good many "sequence of cognita," the word "cognitum" I gathered having taken the place of our old friend the psychon. In the kitchen while we ate the delicious meals which Mrs. Carington, by means of continental cooking, managed to extract from the scant English rations the discussions continued about how to "clean up this muddle of the relation of mind to matter," and I won't deny that at times I felt I was hanging on to the tail of a fiery comet. But if now and then he could blaze and frown, most of the time he was exquisitely sensitive to other points of view, and courteous without the least touch of remoteness. He was serious without being pedantic, witty without being self-conscious; his touch was light.

Of course, like any man who has evolved a theory that fits a number of phenomena he was keen to make it fit even more, and I soon saw, when the case of Borley Rectory chanced to come up, that he was inclined to deny the reality of poltergeist phenomena because he could not fit them into his theory. But he and his wife, who shared his intellectual work, had satisfied themselves as to the reality of psychokinesis by their experiments with dice that very year. What was the difference? He swerved away into petting Lao-tsu and spinning syllogisms about sequence of cognita.

But he was honest above all. When he saw me off that dark gray November morning, tall, haggard and worn, yet sparkling, I put the question up to him again: "Stop trying to evade the problem and tell me what you think about poltergeist phenomena!"

"You *are* persistent, aren't you!" Then he laughed, "Well, if the recording angel were to press a pistol to my heart and ask if they were genuine, I should be obliged to answer, 'Some of them!'" Then he added very seriously, "But I should be long sorry to think that I couldn't fit them into the theory of the association of ideas."

We corresponded. His last letter was from Penzance Hospital, written in pencil after he was nearly blind, but he pooh-poohed "this almost certainly psychological break-down." He had finished the "Matter" and the "Mind" parts of his new book. The "Meaning" remained before him.

Targets of Carington-Type Experiment, 1947

A ten-day experiment designed to test the hypothesis of repeatability of Whately Carington's Catalogue Experiments was undertaken by Dr. Gertrude R. Schmeidler and Mrs. E. W. Allison in April of this year. Invitations to participate were sent only to recent members of the Society since Mr. Carington's catalogue method requires percipients who have not taken part in such experiments before.

An assessment of the results obtained will appear in a forthcoming issue of this JOURNAL. We append here a list of the target drawings which were used on the ten evenings of the experiment:

April 9	Ostrich	April 14	Chick
April 10	Tree	April 15	Barrel
April 11	Corkscrew	April 16	Vase of flowers
April 12	Thimble	April 17	Rolling Pin
April 13	Mirror	April 18	Egg

Obituary: Charles E. Stuart

J. G. PRATT

The death of Dr. C. E. Stuart of the Duke Parapsychology Laboratory on March 23, 1947, deprived psychical research of one of the most gifted and devoted of its all-too-few full-time workers. His death came as a result of complications growing out of a serious heart ailment which had been diagnosed about fifteen years earlier. Surviving him in his immediate family are: his twin brother, Mr. William Stuart; his wife, Wilma; and two sons: Charles, age 5, and William, age 2 months.

Dr. Stuart and I became close friends while we were undergraduates together at Duke University. Through almost two decades of close association in college and graduate school and in the same profession, he was known to me, as he was to all his friends, as Charlie. To write about him now only in formal terms would give to my words a touch of strangeness and of insincerity which would have been foreign to the personality of the man we knew so well and whose friendship we valued so highly.

Charlie was born in Pennsylvania in 1907. In 1928 he entered Duke University, where he soon won recognition from faculty members and students alike for his brilliance of mind. His undergraduate major was in the field of mathematics, but he showed superior scholastic achievements in every subject. His aptitude for the sciences was demonstrated throughout his college career, and he actually qualified for a second major in psychology by virtue of elective courses which he took in that branch of study. It was typical of the breadth of interest of the man that he chose to record mathematics as his major, in spite of the fact that he had already decided upon a career in psychology. He was graduated with the A.B. degree, *summa cum laude,* in 1932.

Charlie entered the Graduate School of Arts and Sciences of Duke University in the fall of 1932 as an Assistant in Psychology, and he gained the highest respect of his fellow-students and of his teachers. He completed the basic requirements for the Ph.D. degree in the span of two years. He could have done a thesis research upon a problem in some established psychological field, but this would have been for him the "easy way out." His interests had grown beyond those bounds. Already, he was becoming known as a parapsychologist, and a parapsychologist he remained with exemplary singleness of purpose.

While still an undergraduate Charlie had become acquainted with Dr. J. B. Rhine and had developed an interest in the problem of ESP. He conducted some experiments in which he successfully tested

a number of his friends for ESP. He chiefly used himself as subject, however, testing his own clairvoyant ability. The results obtained were quite significant, and they had several interesting psychological features. First, there was a decline in success as the tests continued. Secondly, he again got significant results when he resumed the experiments after a rest of some months, showing a recovery of scoring ability which is perhaps unique in the history of this field. Finally, the test of calling to *miss* the card was tried, and it was quite as successful in giving scores below the level of chance expectation as that of trying to hit the target symbol was in the positive direction.

When Charlie finished his graduate course work, he was awarded a full-time research associateship in the Parapsychology Laboratory, and he continued in this capacity until his death, with the exception of two years as Thomas Welton Stanford Fellow in Psychical Research at Stanford University during the years 1942-43 and 1943-44. Throughout his career, his main research interest continued to develop along the line of the psychological conditions which affect ESP performance, particularly those related to the personality of the subject. He found evidence that each subject has his own personal tempo or rate of response which permits him to score at his best level of success. Forcing the subject to adopt a rate faster or slower than that which he sets for himself reduces the scoring to the expected chance average. Continuing this emphasis upon the relation between ESP and personality factors, Charlie discovered that subjects who are *unaffectable,* as judged from the fact that the score made on a particular run did not influence their level of aspiration (estimate of score) for the next run, were the ones who scored significantly in the test. This research he submitted as a thesis for the Ph.D. degree, and this distinction, the first of its kind for an experimental study in ESP, was granted by Duke University in 1941.

After making a careful study of Mr. Whately Carington's experiments with drawings, Charlie was motivated to turn his attention to a question in which he had long been interested: How may we get more meaningful psychological responses from subjects in ESP tests without sacrificing the technical advantages of the card-calling procedure? He developed his free response method of testing ESP, using pictures as stimuli, and worked out the procedure and the statistics for his preferential matching method of judging and evaluating the results obtained. This contribution to testing methodology made it possible for the people being tested to respond much more freely by making drawings or by writing down their impressions. At the same time, it met the requirements for a strictly impartial and a quantitative assessment of the subject's responses. He worked

out the details of this procedure and published his first results from its use at Duke in 1942.

Much of Charlie's time at Stanford was taken up by teaching duties, but in the time available he carried on further research with his new technique. The results obtained were reported from time to time in the *Journal of Parapsychology* after his return to Duke in the fall of 1944. It was on the basis of these results that Dr. Betty M. Humphrey was able to show that the form-qualities of the subjects' drawings in the tests, particularly the characteristics identified as *expansiveness* and *compressiveness,* were related to the demonstration of ESP in the tests. Charlie, meanwhile, had started upon a line of research to find the relation between ESP performance and the individual subject's pattern of interests in intellectual topics and in all sorts of everyday activities. He had made good progress in this work up to the time of his final illness. I believe that he had gained a degree of personal conviction regarding the statistical significance of his findings which was unusual for one of his careful, even conservative, scientific disposition. These results, together with his realization of their psychological importance as another step toward finding out how psychical capacities integrate with the better-known aspects of personality, went far to sustain him in the last months of failing health.

I have written chiefly of Charlie's contributions to the research literature, for this is where his most important work was done. He is also well known, however, for his other writings in parapsychology. He was co-author of two books: *A Handbook for Testing Extrasensory Perception* and *Extrasensory Perception after Sixty Years.* With Dr. J. A. Greenwood, he wrote upon the statistical methods used in the research; on this topic he was especially well qualified, for through the years he had built upon the solid foundation of mathematical knowledge acquired as an undergraduate. Finally, he became well known indeed to the critical attackers of ESP, for he answered many of them with an effective directness and a dispassionate presentation of the facts which characterized his literary style. Writing was not something which he did easily, but the finished product generally had a literary merit which was envied by many of us who worked beside him.

In his personal relations with his colleagues, one thing about Charlie stands out clearly: he always gave, and gave cheerfully, more than he could possibly receive in return. We constantly sought him out for his keen experimental and psychological insights and good judgment. But particularly did we rely upon him for his knowledge of the deeper mysteries of all matters statistical. His was not a card index type of intellect, but a living force pressing constantly to extend the boundaries of knowledge in his chosen field of research.

Book Review

THE END OF BORLEY RECTORY, by Harry Price. George G. Harrap & Co., London, 1946. 358 pp. 15/—net.

When Mr. Price concluded his narrative of *The Most Haunted House In England* by describing its destruction by fire, he left the reader to infer that there never could be any more to that story. Now, however, he has issued a sequel describing the subsequent phenomena and investigations, for it turned out that even the burned-out ruins of the Rectory were still haunted. This new book is most interesting, and the text is illustrated by excellent photographs. The opening chapter summarizes the earlier volume for the benefit of readers who have not read it. So much interest had been aroused in the Rectory by the first book that there was no difficulty in obtaining volunteers for the subsequent investigations. In all, fifty-eight people spent one or more nights in the ruins or in the near-by summer house.

Chief among these observers was a group, "The Cambridge commission," headed by Dr. A. J. B. Robertson of St. John's College. A summary of his observations appeared in the *Journal* of the (English) Society for Psychical Research, issue of January-February, 1945. Of all those who watched, seventeen noticed nothing out of the ordinary, twenty-two commented on certain doubtful phenomena, but nineteen reported clear manifestations. These were of the same types as had been noted during the earlier years, such as footsteps, slamming doors, knocks and raps, odors, lights, the cold spot (tested by thermometer), hoof-beats and markings on the wall. An important addition to the testimony about the Rectory was received in the form of a letter from Mr. P. Shaw Jeffrey of Capetown, who had been a contemporary of Harry Bull at Oxford, and who spent the Long Vacations with him in 1885 and 1886. He witnessed many phenomena; he saw the nun "several times" and heard the hoof-beats as well as the poltergeist pranks about the house.

Acting on the analysis suggested by Canon W. J. Phythian-Adams, based chiefly on the planchette scripts obtained by the Glanville family, Mr. Price undertook the excavation of the two filled-in wells or tanks in the cellar. According to these planchette messages, the unhappy nun whose ghost had been seen so often, was Marie Lairre, of Le Havre, nineteen years old, who on May 17, 1667, had been strangled by a Waldegrave. "Look in the well in the cellar," said the script, "at the end of the wall." It was inferred from other scraps of communication that the body had been originally interred in the garden and then reburied, at the depth of three feet in the

cellar. Mr. Price ascertained that according to the records, the former Rectory on that site was unoccupied at the time of the alleged murder. The Waldegraves were a prominent Catholic family connected with Borley for three hundred years. Accordingly, Mr. Price had the digging undertaken, and the dramatic climax of the work was the discovery, exactly three feet down, of a large piece of a woman's skull with its jaw bone.

Masses had already been said for the nun of Borley in various Catholic churches in England, and Mr. Price, after having the relics photographed, had them buried with the rites of the Church of England, in May, 1945. During the previous year the ruined Rectory was demolished and the cellar filled in. Only the carriage house remains today.

One last phenomenon is noteworthy because it was accidentally registered by camera as well as witnessed by three persons. In April, 1944, after demolition had been begun, the author, with Mr. Scherman, photographer for *Life* magazine, and Miss Ledsham, researcher for *Time* and *Life* drove to the Rectory for pictures. As all three were looking at the house with the camera leveled, they saw a brick jump up from the floor to the height of about four feet. At that moment the shutter clicked, and in the resulting photograph this brick is seen in mid-air.

It appears that the Rector of Borley, at the time of its liveliest manifestations, Rev. L. A. Foyster, kept a daily record of what he observed. On the basis of this he wrote a manuscript, *Fifteen Months in a Haunted House*. He has since died, and the book and diary notes are as yet unpublished. Certainly this is a record which, if impracticable to print, should at least be preserved somewhere for future study. Mr. Price estimates that during the five years of Mr. Foyster's occupancy there were as many as two thousand poltergeist phenomena. And the Rector was only one of about two hundred witnesses to the strange occurrences at Borley.

No one who has read the two books on this "most haunted house" is likely to challenge Mr. Price when he declares that the Borley phenomena are "outstandingly the best evidenced, the best authenticated and certainly the most documented story of a haunted house in the annals of psychic research. And no other case has been investigated for so long a period (sixteen years) or by so many cultured people of repute or so thoroughly."

WILLIAM OLIVER STEVENS.

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Exploring Telepathy: An Inquiry into Method

JAN EHRENWALD, M.D.

Among the facts assembled by modern psychical research the evidence of telepathic phenomena has received the widest recognition. Can they claim to be the "irreducible and stubborn facts" which in the words of William James, are the first prerequisites of the scientific method? Has psychical research been able to assemble a sufficient number of them so as to make them the basis of valid scientific generalizations? There are two types of telepathic phenomena: those classed as spontaneous occurrences, and those derived from laboratory experiments. They are usually considered under two separate headings, and students engaged in the investigation of either group are only too often inclined to disregard the one outside their frame of reference. Clearly, conclusions derived from such a procedure must fall short of the classical Baconian principle which states that scientific generalizations have to be based on *all* the facts.

It is at this point that psychical research is faced with a decision which every new scientific discipline must make at the beginning of its career: Which part of the evidence can be taken for granted and which is to be discarded as spurious, as a will-o'-the-wisp, that may lead astray for decades to come? The prevailing trend of contemporary psychical research is undoubtedly the quantitative statistical

approach. It has, by repeatable and mathematically assessable laboratory experiments, largely replaced the more or less authenticated anecdotal accounts of the early workers. Such an approach has its indisputable merits. It establishes the factual evidence of the phenomena; it places their investigation on safe scientific foundations. However, the limitations of an exclusively experimental approach should not be overlooked. The laboratory worker focuses his attention on card symbols, diagrammatical drawings, letters of the alphabet, and the like; he takes careful records of the scores made by his subjects and assesses their chance probability by approved statistical methods. In so doing, however, he is apt to lose sight of the psychological factors outside the range of his inquiry. His approach is confined to one single track along which he is bound to proceed; it is apt to narrow his outlook and to yield a one-sided, if not distorted, picture of reality.

Rhine and his followers started from the notion of a possible transmission, in some extra-sensory way, of well defined perceptual images or sensory impressions.[1] In adopting such a working hypothesis, however, the experimenter is easily led to the totally unwarranted conception that paranormal cognition (or whatever it may be called) is really concerned with a quasi-sensory "perception" of distinct sense-data or their psychological representations. This idea has not been borne out by the facts. It is tenable only so long as we confine considerations to artificially simplified and mathematically treatable material. Whenever we abandon the single-track laboratory method, we light upon phenomena of an entirely different complexion, hardly reconcilable with the quantitative experimental approach. The reactions recorded and the materials used in the experimental situation are standardized. The subject must comply with the conditions chosen; he must observe the rules of the game. If "pure telepathy" is being investigated, the percipient has to make the choice between the "mental pictures" of a "circle," or a "wavy line," or a "triangle," etc., to the exclusion of any other ideas that may pass through his mind. If in a statistically significant number of trials his guesses tally with the mental pictures selected by the agent, the experimenter is satisfied that telepathy has taken place.

More recently, however, Dr. Rhine surprised the world with doubts expressed regarding the unequivocal telepathic nature of what was usually considered the standard type of experiments in telepathy.[2] The fact is that foolproof telepathic experiments are very difficult to devise, so much so, that as long as we insist on laboratory experiments (and exclude from our evidence all observations which do not submit to the rules laid down for tests of this kind) we have, indeed, no way

out but to question the very existence of telepathy pure and simple, i.e., of telepathy without the possibility of clairvoyance or clairvoyant precognition as an alternative explanation.

Spontaneous occurrences, however, such as can be observed in dreams, in the psychoanalytical situation, and in neurotic patients, can hardly be expressed in terms other than the telepathy hypothesis. There are the instances in which transmission of mental content seems to occur in such a way, that no contemporary written record of the item concerned can be made responsible. There is transmission of items without any reference to their physical nature or quality, that is, cases in which not a strict congruency or photographic likeness of certain elements or motifs in the minds of an agent and a percipient is suggestive of telepathy, but rather of a peculiar symbolic or metaphorical correlation between the two. In many instances it is the correlation which exists between the whole and the part, between a distinct idea and an apparently far-fetched association connected with it. In these cases telepathy does not amount to a faithful reproduction of a distinct feature of another person's mind. It breaks it up into pieces; it suppresses one of its constituent parts and brings out another, it shifts the emphasis from the essential to the accidental; from the center to the periphery; from the real thing to its symbolic representation, like the dream;[3] from the trivial to the preposterous—if not from the sublime to the ridiculous—like the rhyming slang of the cockney.

Precisely this tendency is guarded against by the provisions of the laboratory experiments. But it is unmistakable even under such semi-experimental conditions as those which obtained in Professor Gilbert Murray's experiments[4] with members of his family, in some of the successful attempts at "telepathic transmission" of drawings carried out by Mr. and Mrs. Upton Sinclair,[5] by R. Warcollier,[6] and more recently, by Whately Carington,[7] to say nothing of the drawing tests of the early workers in psychical research.

Figuratively speaking, one could say that the percipient rarely scores a direct hit on the agent's central idea. His guesses are near misses, as it were, scattered around what the observer, rightly or wrongly, believes is the target idea which the percipient seeks to reproduce in a telepathic way. It will be noted that the same tendency to score within one of the concentric rings surrounding the bull's-eye rather than on the bull's-eye itself, is a characteristic of experiments suggesting the existence of precognitive or postcognitive telepathy (S. G. Soal and K. M. Goldney).[8] It was described as a temporal displacement of the percipient's guess from the target card, to the one which was next to be selected by the agent, after a two or three seconds' interval.

It is readily understood, however, that outside the watertight conditions of the laboratory experiment a similar tendency to displacement—temporal or otherwise—may easily give the impression of vagueness and inaccuracy of the claimed telepathic correspondence. An example may illustrate this point. Elsewhere I described a dream dreamt by a patient of mine suffering from bilious attacks, due to an irritation of the gall-bladder. I was at that time keenly interested in histamine ionization, a method of physio-therapy, which I tried to introduce into the treatment of various neurological disorders. My patient failed to respond to the usual medical treatment, and I was somewhat concerned about her slow progress. One night, thinking over her case, I decided to try out the method of histamine ionization over her gall-bladder. For the sake of the non-medical reader I may mention that the local effect of this treatment consists of producing wheals and reddening on the place of application, very much like the eruption of a nettle-rash. Next morning I entered her room with my small portable apparatus. The patient was apprehensive when I told her that she was going to have "something new." *"I hope no injections, doctor, I am so afraid. I had a dream. You came into this room, as you are coming now, and I had a rash on my abdomen, a red itchy rash, consisting of small spots and wheals. . ."* I asked her to show me the place where she dreamt she had seen the rash. She pointed precisely to the region where I was going to apply the electrode soaked with the solution of histamine.

The telepathic nature of this dream is undoubtedly suggestive, but difficult to prove. I did not, of course, think expressly of the nettle-rash on the same night. Neither did the patient reproduce the notion of histamine ionization as such in her dream. It contained a more or less distinct element or motif which was at that time present in the fringe of my mind. There obviously exists some kind of correspondence between the two sets of mental events, but it is clear that a closer definition of the nature of this correspondence in terms of a strictly scientific statement is hardly possible. It refuses to submit to the standards of mathematical assessment.

Precisely the same problem arises whenever accounts of sporadic telepathic occurrences are subjected to closer scrutiny. In the past two decades an increasing number of accounts of this kind have been reported in the psychoanalytic literature. I need only quote the interesting observations recently reported by Jule Eisenbud[9] and an earlier article of my own on *Telepathy in the Psychoanalytic Situation.*[10] Observations of this kind have the advantage of being recorded by workers trained in subtle psychological observation and self-observation, with special experience in the borderland between sanity and mental disorder from which, for all we know, most of so-called

paranormal phenomena emerge. But on the other hand material derived from such sources is subject to the danger of psychoanalytic over-interpretation, a danger of which the psychoanalyst, Paul Schilder, has warned. The difficulties involved in the analytical approach, however ingenious, may be illustrated by the following observation.

N. Fodor recently described a series of telepathic dreams[11] which he observed in his own family and in his patients. One night the author himself dreamt that he was president of the world. The same night his wife had a dream that she was Queen Alexandra of England. Fodor suggests that there is a telepathic correspondence between these two dreams. He extended his inquiry to a third dream, dreamt by his daughter on the same night. This dream need not be reproduced here in detail. The point is that in Fodor's view its analytical interpretation bore striking resemblance to the former in that it, too, contained a reference to the idea of a God-like father, the Ruler of the World.

Now, the analytical reading of these dreams may be fully justified, and the close resemblance of their latent meanings undeniable. But does this resemblance, or even identity, of meanings warrant their telepathic interpretation? Schilder's critical remarks call attention to the equivocal language of the unconscious. The deeper we descend in the symbolic sphere, he contends, the greater is the similarity between the thoughts of various persons, yet it would be wrong to attribute this to telepathic "correspondence." Sceptics may certainly raise this objection to Fodor's observations.

The examples contained in L. J. Bendit's recent book[12] on paranormal cognition avoid the pitfalls of psychoanalytic over-interpretation, but Case I illustrates another difficulty. He describes how during a session of psychotherapy he himself was seized by a sudden wave of unmotivated anxiety. The patient had just been referring to her own neurotic fear of Hell-fire without, however, showing any evidence of emotion. When questioned, she confessed that she had on her part been in a panic during their conversation. Bendit assumed that in this case he had been affected by his patient's neurotic fear in a telepathic way.

Here too, a telepathic interpretation of the case seems to be suggestive. But, as in the former example, the author would hardly be able to convince a sceptic. How can he prove that his fear did not spring from the same unconscious source as the fear of his patient? And even if we dismiss this objection, how can he ascertain that their common experience was really identical, that is to say, of truly telepathic nature? Indeed, Rhine and his followers hold that the case for telepathy cannot be proved at all on the basis of spontaneous occurrences. This is why they altogether discarded evidence of this

kind and resorted to mass experiments and their statistical evaluation. S. G. Soal, K. M. Goldney and R. H. Thouless, in England, seem to be inclined to hold similar views.

I pointed out, however, in my introductory remarks that a one-sided approach would bar the way to any further progress. Granted that the presence of the telepathic factor in spontaneous occurrences cannot be expressed in mathematical terms, is it, therefore, illegitimate to assert their telepathic nature? If so, how then can the occurrence of telepathy in dreams, in the psychoanalytical situation and in everyday life be established? Shall we give up the hope of throwing more light upon phenomena of this kind and confine ourselves to dealing with quantitative material? This would amount to begging the issue and would result in a vain attempt to press "stubborn and irreducible facts" into the straight jacket of a preconceived theory.

What method, other than quantitative experiments, shall we then resort to? The early workers in psychical research, chiefly concerned, as they were, with the accumulation of facts, obviously failed to devise any. They laid down certain general rules which had to be observed, certain postulates, failing which they dismissed the evidential value of a concrete case as inconclusive. But these rules were not derived from acknowledged scientific principles. They were based on intuition, or at best on common sense judgements, which on the view of Rhine and his co-workers were bound to vary "with the common sense (or bias) of the person making the judgement."

What, then, are the criteria of telepathic correspondence and on what grounds can the occurrence of telepathy in an individual case be established? From such sporadic observations as have been reported in the literature and as have occurred in my own experience, a few fundamental principles emerge. First, the claimed correspondence must refer to things on the purely mental plane. That is to say, to thoughts, ideas, or other elements in the minds of two or more people, and not to things in the outer world. Failing this, the incident cannot legitimately be described as a case of "pure" telepathy.

Secondly, the element which is claimed to be reproduced in a telepathic way must be identifiable as a distinct element. Failing this, the claim can hardly be substantiated and, indeed, no further argument is possible. We have to note at this point that the identification of an element or motif as a well-defined mental event is essentially a matter of subjective interpretation: that is to say, of an intrinsically psychological approach. This might be regarded as an unavoidable weakness of the psychological method. But it may be as well to recall that this problem is not an exclusive characteristic of the mental sphere. Precisely the same task confronts the scientist in the physical

world when he speaks of an "isolated system," as distinct from the "remainder of things in the Universe" (A. N. Whitehead).

Thirdly, the element or motif must exhibit a sufficient number of distinctive features. Failing this, the objection of chance coincidence can hardly be dismissed. Indeed, it has been the great rarity of observations which comply with this requirement that causes students to look for material of an entirely different kind and that has ultimately led to the development of the method of mass experiments and their statistical evaluation. The material used in these experiments consists of well-defined isolated items exhibiting comparatively few "distinctive features," but it will be noted that large numbers of trials necessitated by this method more than make up for its lesser complexity.

Fourthly, the element or motif should possess a distinct emotional relevance to the agent or to the percipient, or to both of them. It may be emphasized that this is a postulate which chiefly refers to spontaneous phenomena. It is suggested by most of the evidence furnished by medical investigators, and it appears that failing some measure of emotional emphasis the telepathic nature of an individual observation is difficult to establish or may altogether escape attention.

The last criterion which we have to consider is tacitly implied in every statement of an assumed telepathic correspondence of two mental events. It is the criterion of their temporal coincidence. In order to define this we cannot avoid making a short digression. The available evidence for spontaneous telepathy shows that two "corresponding" impressions in the minds of an agent and of a percipient are rarely "simultaneous" in the physical sense. More often than not there exists a marked time lag or latency period between the two, or else the percipient's impression may even appear to precede the corresponding impression of the agent. We have already come across this problem when we referred to the tendency to temporal displacement in card-calling tests. Here it confronts us again, although in a different guise. It calls, in any case, for clarification of the way in which the notion of temporal coincidence or simultaneousness is being used in the present context.

The physicist defines temporal coincidence by reference to the position of the hands of two clocks, mechanical or otherwise, provided that the observers are not in motion "relatively to each other." David Copperfield was born and the clock in his mother's bedroom struck nine. Science is satisfied that the two events were simultaneous. But temporal coincidence is an elastic notion on the psychological plane. I thought of the method of histamine ionization and my patient dreamt of a red itchy rash on the same night. Were the two trains

of thought simultaneous? Clearly, this question can only be decided if we are prepared to accept the statement that they contained a certain element in common which was "at that time" of actual interest, of vital significance, to one if not to both of us. That is to say, the emphasis lies not so much on the time factor as on the psychological relevance of the respective element or motif to those concerned. But if so, it is clear that the question of temporal coincidence can be determined by psychological interpretation only. It requires what may be called *comparative analysis* of the two series of mental events, covering as great a range as possible of the mental contents of both agent and percipient, and proceeding along the lines of subjective interpretation familiar to psychoanalysts, individual psychologists, and students of the dynamic schools of psychology in general. In fact, the psychological approach to telepathy thus conceived is nothing else than a modified psychoanalytical approach with reference to two or more persons instead of to one, and with its compass extended so as to comprise a new (the telepathic) factor entering the picture.

We emphasized that such an approach may be subject to the same —if not worse—pitfalls as the orthodox analytical method. However, the example of my patient with the nettle rash, as well as a recent article on telepathic dreams by J. Eisenbud,[13] shows that by selecting occurrences which comply with the above outlined criteria, gross mistakes can well be avoided. But in doing so, we must bear in mind that even when applying all necessary caution the very sensitiveness of the analytic method may turn against us; it may suggest telepathic correspondences where in fact our microscopic high-power magnification, as it were, reveals but the identity of deep psychological texture. We are therefore better advised to keep nearer to superficial levels of the mind, e.g., to consider the manifest dream content rather than meaning detectable in the symbolic sphere.

A further difficulty is more than merely one of method. If we proceed along the lines indicated above, that is, if we compare the mental content of a supposed agent and percipient, we have already committed ourselves to confining our attention to two specific persons and to leave all other potential agents or percipients out of the picture. But it is clear that this procedure is purely arbitrary. If the possibility of telepathy is granted at all, we must allow for its occurrence from whatever outside source. Eisenbud did so to some extent when he extended his inquiry to dreams dreamt by a number of patients. But it goes without saying that, theoretically speaking, any other person ought just as well be taken into account.

This is impossible for obvious reasons. Comparative analysis of more than two persons would confront us with a task of increasing complexity. Indeed, it would lead the comparative method *ad absur-*

dum. It is therefore advisable to confine our comparative approach to a *selected group of subjects*, though we have to be aware of the arbitrary nature of such a limitation.

For similar reasons it is also necessary to confine the notion of what we described as *contemporaneous mental events* to occurrences within a *limited space of time*, e.g., within twenty-four hours. Failing this, we would run the risk of having to compare two or more virtually infinite chains of mental events extending from the remote past to the distant future. This, again, would lead the comparative method *ad absurdum*. Nothing would be easier than picking out apparently telepathic correspondences where the mere multitude of items surveyed may suggest the presence of otherwise unrelated "pairs." For practical reasons comparative analysis should therefore be restricted to an arbitrary unit of time.

Summing up, the method of comparative analysis seeks to ascertain the presence of at least one well-defined element or motif which is common to two independent trains of thought of two separate persons. In doing so, it is guided by the psychological relevance, conscious or unconscious, of the respective item to those concerned within the selected time unit. Temporal coincidence of two identical or closely resembling "pieces of mind" may therefore be assumed whenever they are found to be of topical interest to the agent or the percipient, or to both of them, within a limited time span, irrespective of whether or not they are strictly simultaneous in the physical sense.

The psychological approach to our problem is in this way totally different from the quantitative approach. There is a gulf between the two, such as Henri Bergson has stated to exist between the intuitive approach to psychology, congenial to matters of the mind, and the scientific method, congenial to the spatio-temporal, weighable, and measurable aspects of the physical world. This should not, however, be taken to indicate that research into our problem should be confined to the non-quantitative, psychological approach, to the exclusion of the quantitative method. On the contrary; experience has shown that in the field covered by normal and abnormal psychology, *both* lines of approach have proved their value. Precisely the same is true of the field of paranormal cognition. Here, too, both methods have their specific tasks to fulfill. The method of mass experiments and their statistical evaluation furnishes the bare facts. It provides calculable and measurable data. It makes the occurrence of paranormal cognition an indisputable bedrock truth. But it is also true to say that it gives little insight into the nature of the phenomena and into the mental setting of an individual case. It is at this point where the investigation of spontaneous phenomena, i.e., of observations of otherwise lesser evidential value, has to step in. It throws the facts

established by laboratory methods into the right perspective and fills the gaps which were left between them.

There are, in this way, two alternative methods available for our purpose. Each of them throws light upon different aspects of the identical problem, each of them is able to make a contribution of its own towards the ultimate aim of assembling "all the facts." There can be little doubt that the final reconciliation of these two categories of facts will be no easy task. They confront us, at bottom, with the problem of the relation between physical and psychological experience in general. Dealing, however, with this question goes clearly beyond the scope of specialized research. It falls into the province of the philosopher.

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12. *Paranormal Cognition,* by L. J. Bendit, Faber and Faber, Ltd., London, 1944.
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"The Implications of Psychical Research"

Under the general title "The Implications of Psychical Research," the British Broadcasting Corporation presented a series of talks to its listeners in the spring of 1947. The speakers, four former Presidents of the Society for Psychical Research (London), were Professor H. H. Price, Professor C. D. Broad, Dr. Robert H. Thouless, and Mr. G. N. M. Tyrrell. The talks were published in *The Listener,* an official publication of the B.B.C. We wish to thank the British Broadcasting Corporation for permission to review the talks in this JOURNAL.

Professor Price opened the series with "Philosophical Implications of Telepathy." He said that the very large mass of evidence collected by the Society during the last sixty years, left no reasonable doubt that telepathy occurs. It was the implications of telepathy for the philosophical student of the human mind, that Professor Price wished to discuss. He explained why telepathy is not analogous to radio transmission, and why it appears to be independent of distance, as follows:

"The ordinary man probably thinks of telepathy as a kind of wireless transmission. I believe that this is only a vague and misleading analogy. There is no evidence that any sort of physical radiation passes from one brain to another when telepathy occurs; no evidence of any organ in the brain for transmitting or receiving it; no evidence that any kind of matter can interrupt or reflect or refract this alleged radiation. Again, in any kind of communication by physical means there has to be a pre-arranged code of some kind. First, ideas must be formulated in words. The words in turn must be translated into physical signals, for example, electrical impulses, and this must be done according to an agreed plan. And then at the other end there must be a converse process of 'de-coding.' There is no trace of anything of this sort in telepathy. Moreover, telepathy seems to be independent of distance in space. Agent and percipient may be in the same room, or they may be many miles apart, even hundreds of miles in some cases. Not only so; it appears to be in some degree independent of time as well. There is recent experimental evidence of this. The percipient sometimes receives not what is in the agent's mind now, but what is going to be in his mind a short time afterwards. What kind of wireless apparatus could it be which receives a message before it has been sent off?

"Telepathy, then, seems to be a purely mental process, a direct contact between mind and mind, without any physical intermediaries. If this is true, it has important consequences. It knocks the bottom

out of the materialistic conception of human personality, which holds that every mental event is wholly dependent upon a previous physical event in the brain of its owner. It suggests at least the possibility that the human mind may be able to exist and to have experiences even when its physical organism has been destroyed."

Professor Price does not think, however, that telepathy is a direct knowledge by one mind of another. He said that if there is such knowledge, it requires another name. "If telepathy were a form of knowledge," he said, "it ought to be infallible; for to say we know something which is not in fact the case is absurd." He pointed to the frequent mistakes in telepathic communication and referred to the inconsistency of results. On some occasions the experience between the agent and the percipient corresponds exactly; on other occasions it corresponds partially, and sometimes the correspondence between the agent and the percipient is so slight that it is doubtful whether telepathy has taken place at all. Professor Price then drew attention to another important fact, namely, that the percipient himself cannot tell when he is right and when he is wrong. Summing up the preceding observations Professor Price said, "It would seem, then, that telepathy is a peculiar form of causation in which one mind is directly affected by another; but not a peculiar form of knowledge in which one mind is directly aware of another."

Professor Price next considered the complicated question of how the idea or impression "gets across" from one mind to another. He said that this "getting across" is something which occurs at the unconscious level, and that the percipient's impression is often something of which the agent was not consciously thinking. Moreover, the percipient "is not usually aware that he is in telepathic contact with another mind although he is in fact in contact with it." Professor Price thinks that this unconscious contact is the most important and puzzling part of the process but not the whole of it. He said that in some way the unconsciously received telepathic impression must emerge into consciousness before there can be telepathic communication between the two minds.

"This throws light on an interesting question. Instead of asking ourselves 'why does telepathic communication occur at all?' perhaps we ought rather to ask 'why doesn't it occur more often? Indeed, why doesn't it occur all the time?' The answer to this may be that unconscious telepathic contact between minds does occur all the time. Perhaps there is some repressive mechanism in all of us—something analogous to Freud's 'censor'—which prevents most of these unconsciously received telepathic impressions from emerging into consciousness. We can easily think of good biological reasons for this.

It would be very difficult for us to adapt ourselves to our physical environment, if our consciousness were perpetually thronged by telepathic impressions coming from all and sundry. Imagine they were coming not merely from the minds of human beings and animals in our immediate physical neighborhood, but also from those which are hundreds of miles away; and even from some minds which have existed in the past or will exist in the future but do not exist at the present moment. At any rate it is natural to expect that there would be some such repressive mechanism in beings like ourselves, who have developed sense-organs and intelligence as a means of adapting ourselves to the external world."

The symbolic form, in which the telepathic message so frequently manifests itself to the conscious mind of the percipient, Professor Price explained, may account for the mistakes which occur in telepathic communication. He said that the mistakes may be due to the difficulty of crossing the threshold between the conscious and the unconscious, and that sometimes this difficulty can only be overcome if the idea is masked in symbolic form. As an example, "the idea of death may present itself by a vision of flowers if the percipient associates such flowers with funerals." Professor Price thinks that "perhaps the telepathised idea or impression is always correctly received at the unconscious level but is liable to be distorted or altered in the process of manifesting itself to the percipient's consciousness."

Professor Price then pointed out various ways by which the telepathic idea might manifest itself: by means of a feeling or emotion which suddenly invades the mind of the percipient, or by a mental image, or by means of a telepathic dream, or in the form of a motor automatism, such as automatic speech or writing. He said more interesting possibilities might lie in sensory hallucinations: sometimes in verbal form, when the message appears to be written on a piece of paper, or a hallucinatory voice, or a hallucinatory visible scene.

"Finally, the telepathic impression may manifest itself in the form of a complete and life-size apparition—the 'telepathic phantasm,' as it is called—where the figure of the agent appears to be bodily present in the room, and is seen exactly as if he were really there before the percipient's eyes. This is the most complete and most successful way in which the unconsciously received telepathic impression may manifest itself: I say the most successful, because it ensures that the message will be attended to, and will affect the percipient's thoughts and actions. To the philosopher, apparitions are the most fascinating of all the phenomena of psychical research, at least if one happens to be interested in the philosophy of perception, as I am myself. But I

must not stop to consider them. I will only suggest the possibility that all apparitions, including the haunting apparition (the 'ghost' of popular speech) are in one way or another telepathic in origin."

Professor Price believes that the frequent question of how the telepathic idea or impression "gets across" from one mind to another must be asked in another way, because we cannot assume that the unconscious parts of our minds are separate entities; the fact that telepathy occurs refutes such an assumption. "We must suppose that although our conscious minds are separate entities, there is in some sense a common unconscious, common to all human minds and perhaps to animal minds as well."

In his closing remarks Professor Price suggested the hypothesis that whatever sort of causal connections are found within the individual mind, they may also be operative across its boundaries. He said that at the unconscious level no sharp boundaries can be drawn between the contents of one mind and the contents of another. "If we go deeper into the unconscious, the question whether a given idea or feeling is in your mind or in mine no longer has any clear meaning."

"Philosophical Implications of Precognition" was the subject of the second talk in the series, with Professor C. D. Broad as the speaker. He defined "precognition" in its widest sense, as an experience followed immediately or after an interval by an event which corresponds so closely and uniquely that it would be unreasonable to ascribe the correspondence to chance-coincidence. Precognitions of a perfectly normal kind occur constantly in our ordinary life, said Professor Broad, and he enumerated the main varieties of normal precognition as follows:

1. Expectations founded on deliberate conscious inferences from past and present facts and the laws of nature—for example, when an astronomer calculates that an eclipse will be visible at a certain place at a certain future date.

2. Expectations based on accepting the inferences made by other persons—for example, when an ordinary member of the public expects an eclipse because he finds it predicted in the almanac.

3. Expectations due, not to explicit inference, but to associations formed by frequently repeated regularities in one's past experience—for example, when one expects an object that looks like sugar to taste sweet. A large proportion of this kind of pre-

cognition takes the form, not of explicit expectation, but of automatically adjusting one's body and mind as if one had a certain expectation. This might be called 'prospective behavior' rather than 'precognition.'

4. Expectations based on the fact that one has formed an intention and has already begun to carry it out or has decided to do so at some time in the remoter future—for example, if I decide today to travel to London tomorrow, I expect to find myself travelling tomorrow and such expectations are generally fulfilled. Similarly, if I know that another person has formed a certain intention, I have reason to expect that what he intends will happen in the remoter future.

5. Expectations which tend to bring about their own fulfillment without, or even against, the intention of the person who has them—for example, if from any cause I expect to fall downstairs or to vomit on a certain future occasion, the state of anxiety thus induced may predispose me to trip or to be sick when the time comes.

6. Expectations which are a by-product of an earlier link in a chain of causes, unknown to the person, which is leading up to an event which, when it takes place, will be a fulfillment of the expectation—for example, a person might, without knowing it, be afflicted with a disease which will cause him to die in a certain way, and some of the earlier phases of that pathological process may give rise to feelings of anxiety and even to an expectation of death in the near future, although he is to all appearance in good health.

Professor Broad defined a precognition as "ostensibly supernormal" if it seems *prima facie* not to fall under one of the above heads; ostensibly supernormal precognitions fall under the head of what is usually called "psychic phenomena." Only after an ostensibly supernormal precognition has survived a close examination on several counts and chance-coincidence has been ruled out, can it be considered a genuine case of precognition. To regard the precognition as "supernormal" it must fail to fall under one of Professor Broad's six headings or a combination of several of them. In that case he raised the question, "Could we bring it under one of our six headings if we supposed that the person is capable of acquiring information about certain contemporary or past events by supernormal means —— for example by telepathy or clairvoyance?" If that is possible, Professor Broad describes the case as one of "precognition normally based on supernormal cognition of contemporary or past data." But if no explanation even on these lines can be suggested, he calls it a case of

"irreducibly pre-presentative precognition," the evidence for which he set forth in the following words:

"There is good evidence for the occurrence of irreducibly pre-presentative precognition. One part of the evidence consists of sporadic cases, such as dreams, which have been reported and carefully investigated. Another part consists of the results of carefully devised and controlled experiments with cards and drawings. I do not think that anyone, not hopelessly biased, could feel much doubt of the occurrence of irreducibly pre-presentative precognition after reading, for example, the papers of Mr. Saltmarsh[1] and of Dr. Soal[2] in *Proceedings of the Society for Psychical Research.*"

Professor Broad then pointed out the main difficulties nearly everyone encounters when faced with the "notion" of irreducibly pre-presentative precognition. He said that the "causal" difficulty is genuine and serious and explained it in the following paragraph:

"It is part of the definition of 'precognition' that the correlation between a precognitive experience and the subsequent event which is said to fulfill it shall not be merely fortuitous. Now what does this imply? It implies that either the precognitive experience contributed to cause the event which fulfilled it, or that both were effects arising at different dates from a common cause, or that the fulfilling event contributed to cause the precognitive experience. Suppose, for example, that I dream on Monday night that a certain friend is being run over by a blue car driven by a Negro opposite the National Gallery. And suppose that on Tuesday morning precisely such an accident happens to that friend. It seems clear that the dream did not contribute to cause the accident, and it seems most unlikely that the dream and the accident are both effects of a common cause. Yet the third alternative, that is that the accident contributed to cause the dream, conflicts with an absolutely fundamental principle about causation, namely, that an event cannot have any effects until it has happened and therefore cannot contribute to cause anything that preceded it."

Professor Broad said that so far as he can see there are only three ways out of this difficulty, none of which he considers very attractive:

"One is to hold that, in spite of all appearances to the contrary, the dream and the accident did have a common cause—for example, a volition in the mind of God, or of a magician, or of a spirit. The second is to give up what seems to be the self-evident principle that

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1 "Report on Cases of Apparent Precognition," by H. F. Saltmarsh, *Proc.* S.P.R., Vol. XLII (1934), pp. 49-103.

2 "Experiments in Precognitive Telepathy," by S. G. Soal and K. M. Goldney, *Proc.* S.P.R., Vol. XLVII (1943), pp. 21-150.

an event cannot have any effects until it has happened. The third is to postulate a second dimension of time; and to suppose that the accident precedes the dream in this second dimension, although it follows the dream in the first dimension of time, with which we are familiar."

"Psychical Research — the Next Step" was the subject of the third talk in the series. The speaker was Dr. Robert H. Thouless. In his opening remarks, Dr. Thouless noted the difference between psychical research and scientific psychology and commented on the general attitude of orthodox scientists towards the work of experimenters in psychical research.

"In psychical research one studies such things as thought-transference, clairvoyance, precognition of the future by dreams and other means, psycho-kinesis (or the movement of objects without contact), hauntings, ostensible communications from the spirits of the departed, and so on. In scientific psychology, on the other hand, one studies such things as colour vision, memory, thought, intelligence, character, and so on. There is a curious difference between psychical research and scientific psychology. Scientific psychology is generally considered to be a respectable branch of scientific study; psychical research is not. Those who are interested in psychical research are not generally regarded as serious scientific investigators but rather as somewhat eccentric individuals with peculiar interests and a defective sense of evidence. Even when scientists of high reputation in their own branches (such men as Crookes, Barrett, and Lodge) have turned to psychical research they have not escaped this reproach. I think it is much to the credit of these men that they did apply to psychical research their gifts for critical experimentation and a trained sense of evidence, just as they did to their own branches of physics and chemistry, in spite of the fact that they received honours for their contributions to orthodox science but derision and calumny for their contributions to psychical research. Matters have improved during the sixty years of activity of the Society for Psychical Research. It is no longer generally supposed that anyone interested in telepathy or haunted houses is either a knave or a fool. This idea is not dead yet though it is dying."

Dr. Thouless deplored the absence of a more cordial interest on the part of scientific psychology in the investigations of experimenters in psychical research; he said that this cleavage exists because scientific psychology is based on a theoretical foundation which has no room for such facts as telepathy, clairvoyance, and the rest. "It seeks

everywhere a physical explanation for mental events . . . But in telepathy and clairvoyance there appears to be no such chain of physical causation." Dr. Thouless thinks that psychical research, at the present time, has no better theoretical basis to offer to scientific psychology; this explains in part the lack of cordiality in the scientific psychologist's attitude towards the findings of psychical research. He affirmed that evidence for telepathy, for clairvoyance, and for psychokinesis has been found in an overwhelming amount, and that no just criticism could be made of the methods of the research; a charge, that can be made with more reason by the orthodox scientist against experimenters in psychical research, is that, having proved the phenomena exist, they seem uncertain what to do next.

"The reality of telepathy was sufficiently proved by experiments carried out sixty-five years ago and reported in the first volumes of the *Proceedings of the Society for Psychical Research.* Yet at the present time we are in the extraordinary situation that we are still liable to hear that a psychical researcher has just completed or is just about to carry out some experiments which will prove once and for all the reality of telepathy. Also we are constantly encouraged to accumulate more and more evidence that will at last convince the sceptical scientist. This, I think, shows a failure to understand the psychology of scepticism. The scientist who is not convinced by the evidence we have at present will not be convinced by ten times or a hundred times as much evidence and it is a waste of time to try to collect it for him. It is not lack of evidence but lack of development in psychical research that leaves the scientist sceptical. In science when someone makes an odd and unexpected discovery, other scientists do not go on indefinitely repeating the original experiments. They go on to new experiments which lead to progressively greater understanding until we get to the point that what at first seemed odd and unexpected seems at last to fit into the general body of scientific theory. It seems no longer odd but just what we should expect."

What has retarded development in psychical research, according to Dr. Thouless, is that the path to understanding is probably incomparably more difficult than it has been, for example, in physics. He said that many people regard the facts of psychical research as "mere oddities" that do not fit into an orderly universe.

"It is as if they believed that God created a universe which was orderly on the whole, and that the ordinary facts of science fitted into this orderly universe: facts like the movements of material bodies, radiations, electric currents, material brains, and nerve processes. Then, having created a 99 per cent orderly universe, He

created a few disorderly odds and ends like telepathy, psycho-kinesis, precognitions, hauntings and materialisations, which do not fit in anywhere. If this were right, it would not be unreasonable for serious scientists to study the 99 per cent of the universe that was orderly and to leave the odds and ends to be studied by eccentric individuals whose interest lies in the odd and the exceptional.

"But against this whole idea our minds properly rebel. We feel that somehow it must all fit together, and that there must be a way of looking at the problems of mind and matter in which telepathy will seem no more odd and unexpected than a normal perception, and a haunting apparition no more odd and unexpected than a mouse or a cockroach."

Dr. Thouless said that when the remote goal of understanding is attained, and the facts of psychical research fit into the same body of theory as the rest of the facts of science, scientific psychology will be transformed. That, he feels, is the importance of the facts of psychical research.

"When telepathy, psycho-kinesis, and poltergeists are no longer oddities, when we can see how they fit into the scheme of things, then the barriers between psychical research and other sciences will disappear, and telepathy will be part of scientific psychology and poltergeists will be part of natural history. If that sounds fantastic, this is only because we are looking some distance ahead."

Mr. G. N. M. Tyrrell was the final speaker in the series of talks. He began by summing up the views of the three previous speakers. Professor Price, Mr. Tyrrell said, accepts telepathy as a fact; Professor Broad thinks that no one not "hopelessly biased" can have much doubt of precognition; Dr. Thouless believes that the facts of psychical research "can not in the long run be separate from the facts of science." Mr. Tyrrell thinks that the bewilderment in the minds of many people when confronted with the startling facts of telepathy and precognition "arises from our mistaken assumption that science has drawn a map of the whole universe, and that every human experience must fit into that map. In reality it has drawn a map only of that part of the universe which is in principle open to our bodily senses." He rejected the frequent assumption of a "sixth sense," and referred to the great pioneer work of Frederic Myers in psychology and psychical research:

"So long as we regard telepathy and precognition as a sixth sense, comparable to sight and hearing, we go wrong and fail to notice the

context to which they really belong. Telepathy comes to light as an uprising from the 'unconscious,' for it is in the depths of the unconscious that the telepathic linkage occurs. Here telepathy falls into line with other things which we do not regard as supernormal. Other things besides telepathy rise up from the unconscious and, together with telepathy, make use of the same mechanism. The mechanism is sometimes a dream, sometimes a hallucination of the senses, sometimes automatic writing, and sometimes simply an impulse or an emotion. These are all vehicles by means of which an uprising from the unconscious is effected. Deeply rooted emotions, expectations and memories all make use from time to time of such vehicles; and sometimes the same dream is used to express an emotion originating within the dreamer and a telepathic impulse originating with someone else.

"Frederic Myers, who was a great pioneer in psychology as well as in psychical research, anticipated the term 'unconscious' by his own term, the 'subliminal self.' He saw that far more exists beyond the threshold of consciousness than things which have been repressed from the conscious mind. He regarded this subliminal portion of the self as an opening fanwise toward sublime heights at the higher extremity and towards the animal depths at the lower. It contains, as he put it, a 'gold-mine as well as a rubbish-heap.' I doubt whether most of us realise how true this is or how much evidence there is for the existence of the gold-mine; or, again, how much humanity owes to it. It is from this ultra-conscious region of the self that most of the great achievements of human thought have ultimately sprung — the beauty of art, the inspiration of genius, the visions of the seers. It is, in fact, the one true source of originality."

Mr. Tyrrell then cited examples from the biographies of great writers, poets, artists, and musicians to show how unanimous they are in attributing their achievements to a source outside of themselves. Most of this testimony, he said, had been collected by Dr. Rosamond E. Harding and published in her book *An Anatomy of Inspiration*.[3] Among the geniuses he mentioned, who testified to the inspirational character of their work were Shelley, Blake, Goethe, Keats, R. L. Stevenson, Schubert, Mozart, Coleridge, Max von Weber and Alfred Russel Wallace. Mozart, for example, said the subject "stands almost complete and finished in my mind, so that I can survey it, like a fine picture or a beautiful statue, at a glance. Nor do I hear in my imagination the parts *successively* but I hear them, as it were, all at once." Mr. Tyrrell thinks that this "altogetherness" of inspiration is probably familiar to most intuitive types of

[3] W. Heffer, Cambridge, 1940.

mind. He said that the lightning speed at which the inspired person often works seems to indicate that inspiration proceeds from some timeless source; both artistic inspiration and precognition proceed from a region where timelessness seems to reign.

"Whether in inspiration or telepathy, the content of the subliminal self has clearly much difficulty in getting through to consciousness. We know from psycho-analysis how repressed material passes the 'censor' with difficulty, and in order to do so often has to assume a symbolical form. It may be solely on account of the difficulty of getting through to consciousness that telepathy and precognition are rare. That, too, may be the cause why great inspirations are so rare. George Sand described Chopin's music as miraculous and as coming to him suddenly and complete. 'But afterwards,' she said, 'began the most heart-rending labour I ever saw. It was a series of efforts, of irresolutions, of frettings to seize again certain details of the theme he had heard: he would shut himself up in his room for whole days, weeping, walking, breaking his pens . . .' These are the birth-pangs of inspiration. It is all very well to sit in the stalls as Stevenson did and watch the Brownies at work; but how elusive the inspiration is when one tries to express it! Carlyle spoke a bare half-truth when he said that genius is an infinite capacity for taking pains. Pains without inspiration will not produce a work of genius; but neither will inspiration, unless pains, skill and effort are there to give the timeless idea its space-time form."

The things which have their origin in the deeper levels of the self, according to Mr. Tyrrell, are very different from the things we find in our conscious selves, and it is in these deeper levels that telepathy and precognition have their source. These faculties are "merely uncomprehended items from an uncomprehended region of the self: and I think the lesson they teach us is that the gateway to a deeper knowledge of the nature of things lies within ourselves and not outside us in the external world."

Does Tomorrow Exist?

WHATELY CARINGTON

"What a ridiculous question! If tomorrow existed, it would be today, wouldn't it?"

But although the reader's reaction to my title is natural enough, the question is by no means so silly as it sounds; indeed, it is one which we shall be obliged to answer sooner or later, if we are ever to understand the nature of the human mind and its place in the universe — and until we do that, we shall presumably go on making as big a muddle of our lives as we have done up to date; and this we would, I am sure, all wish to avoid.

The problem, which has a certain fascination of its own apart from this long term view, arises from the fact that recent work has placed the phenomena of "precognition" — that is to say, roughly, the "foreseeing" of future events — on a firm experimental basis, so that it can no longer be brushed aside as all due to coincidence, misremembering, and so forth. But, since you obviously cannot "cognise," or be aware of, or, indeed, have any sort of relation to, anything at all unless it exists, it is clear that the so-called "future" event precognised must, in some sense at least, exist at the moment of precognition. A terrible dilemma, to which I myself see only one possible answer — and that is not to be found by blethering (if I may be allowed the term) about the Unreality of Time (which means nothing at all) or The Eternal Now — which does not, I fancy, mean anything more.

From the earliest times, men have longed (perhaps imprudently) to know the future, and a long line of prophets, seers, oracles, and augurs have catered, more or less honestly, to the demand. On the whole, despite a good deal of inevitable charlatanry, I should say that the history of the subject has been relatively respectable and compares favourably with that of any other "paranormal" activity, such as necromancy, clairvoyance at a distance, and so forth. From classical times onward there has been reported a number of instances of prophecies strikingly fulfilled, and in recent years numerous smaller but not less cogent cases have been collected by the Societies for Psychical Research and private students.[1] To give but a single instance, which has at least the merit of being picturesque: Mrs. Atlay, wife of the Bishop Hereford, dreams that she reads family prayers in the Hall, instead of in the Chapel, as her husband is away, and

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[1] "Report on Cases of Apparent Precognition," by H. F. Saltmarsh, *Proc.* S.P.R., Vol. XLII (1934), pp. 49-103.

that after doing so she finds in the dining room a large pig standing between the table and the sideboard.[2] She comes down and relates her dream *before* reading prayers. It is precisely fulfilled, including the position of the pig. Note that the pig was safely in its sty at the time of dreaming, but escaped while prayers were being read. Pigs are, one may suppose, so seldom found in the episcopal dining rooms that it is difficult to attribute such an occurrence (which is well-authenticated) to mere coincidence; and the difficulty becomes a virtual impossibility when such cases are multiplied by the score.

On a different line, Commander J. W. Dunne created a considerable stir with his book *An Experiment with Time*[3] (first published 1927) describing how he found that many elements in his own dreams were apparently precognitive in character.[4]

But such more or less spontaneous cases, though extremely impressive, are very difficult to assess, and therefore insufficient by themselves to overcome the strong psychological resistance that the idea of precognition not unnaturally encounters in most minds.

More recently, various investigators, notably Rhine, Soal and Goldney, Tyrrell, and the present writer, have obtained significant (i.e., non-chance) precognitive results under strict experimental conditions. The classical work is that of Soal and Goldney,[5] in which, briefly, their subject was asked to "guess" cards drawn at random, under elaborately precautionary conditions, by the experimenters; they found that he scored significantly not on the card known to the experimenter at the moment of guessing, but on the *next* card in the series, i.e., one the nature of which was not known to anyone at that moment. The work was far too careful for the results to be attributed to faulty procedure, and is well supported by other researches; and there can be no doubt that, taking the evidence as a whole, we must accept it as a fact in nature that a future event, such as the observation of a randomly selected card, can be, in some manner, "foreseen" or "precognised," at least by certain people under certain conditions.

But how? In particular, how is it possible for a physical event (i.e., some configuration, so to say, of material objects) to "exist" in such a sense, or to such an extent, that it can be cognised, and yet *not* to "exist" in the sense that, as we say, it has not happened yet? No

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2 "The Subliminal Self," by F. W. H. Myers, *Proc.* S.P.R., Vol. XI (1895), pp. 487 f.

3 The Macmillan Company, New York.

4 These results were supplemented by a formidable theory involving successive "dimensions" of Time, in an infinite regression; but I do not think that this has been accepted by anyone competent to analyse it.

5 "Experiments in Precognitive Telepathy," by S. G. Soal and K. M. Goldney, *Proc.* S.P.R., Vol. XLVII (1943), pp. 21-150.

satisfactory explanation has yet been given, and it will at least be good, clean fun, for those who like this kind of thing, to see whether we can do better than heretofore.

Before we can understand what is implied by "foreseeing," "precognising," or "preperceiving" (if I may coin a term) the form that a future event will take, or the characteristics an object will display, we must examine what actually happens when we "see," or "cognise," or "perceive" an event or object in the ordinary way. If I say "I see a tomato," I am using a highly condensed form of words, which is taken to imply (and usually correctly) much more than the facts actually warrant. More strictly, I ought to say something like "I am aware of a red patch, of such-and-such a shape, hue and shading; and experience of similar red patches in the past leads me to expect that, if I stretch out my hand and grasp it, I shall be aware of certain feelings of smoothness, coolness, firmness, etc., that if I squeeze hard I shall be aware of certain sensations of moisture ("squashing"), etc., that if I put it in my mouth and bite it, I shall be aware of . . . etc., etc." Even this is not a complete expansion, but it will serve our present purpose.

Note, all-importantly, that if the thing does not behave in this way — to put it very colloquially — that is to say, if this sequence of awarenesses, etc., is not followed with reasonable exactitude, it is not a *tomato* that I am seeing; it may be a wax imitation, or maybe I am enjoying a hallucination. A "real" tomato, as we put it, is that which conforms to the accepted specification of a tomato.

But communication would be quite impracticable if we had to use this enormously expanded form of words every time we wished to speak of a material object, and we accordingly use the shorthand symbol or "portmanteau" phrase "a tomato" for all ordinary purposes; but it will be misleading unless it has all the above expansion packed into it, however little we may realize this when talking.

Note, next, as of equal importance, that all we directly know at first hand are the sensations of red, smooth, cool, firm, moist, salty, acid, etc., and of course those of stretching, grasping, biting, etc. The first lot (together I need hardly say, with those corresponding to any other operations we may perform) taken collectively, *are* the tomato; the tomato *is* this collection of these sensations, or awarenesses, etc.

But, you may object, these are only properties, etc., "of" the tomato — what about the tomato *itself?* This is the vulgar error into which pretty well everyone has fallen since the beginning of time — namely, of supposing that because we find it convenient to use the phrase "the tomato" as a time-saving shorthand symbol, there must be a *thing* (tomato-itself) for which the word stands; and that this

"has" the properties, or "causes" the experiences, etc., which are held together in their observed pattern, so to say, by their relationship to *it* — almost as labels might be stuck on a suitcase. No conceivable process of observation, however, will enable you to discover this alleged "tomato-itself"; all you can do is to discover new tomato-properties; and to talk about anything which is inherently unobservable is to talk meaningless nonsense.

The moment we abandon this primitive superstition about "things-themselves," we are free to realise (given a little resolution and fortitude) that there is no compulsive necessity which requires that visual, tactile, gustatory, etc., experiences should be related in the particular way known as constituting a material object. There is no compulsive reason why the various groups should not function and be cognised independently; and as a matter of observed fact they sometimes do and are. When we cognise a visual group, say, not related in the material-object way with the expected tactile group, we say that we are having a visual hallucination; and such experiences are, as is well-known, not very uncommon, while tactile, auditory and other forms of hallucination, though rarer, are by no means unknown. We say that there is an event in the physical world, or that a material object is present, only when the various groups are (I am tempted to say "happen to be") present together, or coincident, or some such phrase, much as we only have a coloured print when the three components of the three-colour process are superimposed.

Now we can clear up the basic difficulty about precognition. I have, say, a vision — sufficiently detailed to exclude coincidence — of some future event; that is to say, an event later occurs of which the visual appearances closely resemble my previous experience. The visual components of that event clearly did exist at the time I had the vision, and that covers the difficulty about the impossibility of cognising something that does not exist. But they had not then been joined by—if I may put it so—or become coincident with the tactile, auditory, etc., components in the relational pattern that constitutes the occurrence of the event, and that deals with the point about an event not existing before it occurs.

It all seems to me perfectly simple and straightforward, provided we are not scared of sticking to the facts, and refuse to be led astray by a lot of verbal balderdash about "things" which "have" properties, and similar pitfalls arising from the uncritical use of language. I think the view suggested also gets us out of the difficulties arising from the view that, if the future can be foreseen, it must be fixed and immutable, so that we cannot avoid it, but are deterministically predestined to endure whatever this now-existing future holds for us. Or,

as Dr. Joad plaintively remarked, "If the future exists, what is to become of Free Will?"

Personally, I have never been able to understand what the term "Free Will" is supposed to mean (and very much doubt whether anyone else does). Determinism I know, and Chance I know — well, more or less — but Free Will seems to me to be wholly meaningless; for whatever deterministic compulsions of external origin may be removed, you must surely still act or decide, either purely by chance or else for reasons of one kind or another, and these reasons will be just as determinative as anything else. Actually, I think it is just another of these word-generated false problems, which are merely nonsensical and mean nothing at all; because I cannot conceive of any possible means of distinguishing a Deterministic world from a Free Will world, and, if you cannot do that, your verbal distinction is just so much empty noise.

But, in any event, it seems quite clear that if the factual occurrence of an event consists in the coming of certain components into a certain relation, or coincidence or the like, then the fact that one of these components has been perceived or cognised does not in the least imply, as a logical necessity, that the coincidence will take place and the event occur. So we need have no qualms, so far as I can see, even on the non-logical ground of our distaste for determinism, to accepting precognition as a fact. And, if we do so and follow the matter up by suitable experiment and reasoning, we shall, I believe, be well on the way to shaking what Professor Lindemann (now Lord Cherwall) once called "the grim pre-eminence accorded by age-long consent to Time." And when we have done that, we may begin to feel a little more at home in the universe than we do at present.

Penkaet Castle

We are indebted to Mr. Alexander Baird, of Glasgow, for the report of an investigation by the Edinburgh Psychic College of certain unusual manifestations at Penkaet Castle, an old Scottish residence near Pencaitland, Haddington, about twenty miles east of Edinburgh. We also wish to thank Mr. Baird for obtaining permission from the College to publish the account in this JOURNAL. The report follows, with some abbreviations because of its length.

Introduction

Penkaet Castle was formerly known as Fountainhall House and has a long and interesting history. It was bought some twenty-five years ago by the late Professor Holbourn. Public attention was attracted by an incident on July 29, 1946, when, during a visit by members of an East Lothian society, a glass globe in the library disintegrated without apparent cause.

It is possible, of course, to suggest some natural cause for this incident; but it was sufficiently peculiar to attract attention to the recent history of the house, and, at the instance of the society, special inquiries were made. These are set out in the report.

They are impressive chiefly because of their continuity and variety. While no single instance, except perhaps the peculiar one of the carved panel seen by a number of witnesses to move outwards from the fireplace and return to its former position at an early stage of the occupancy of the house by the new owner and family, is definitely outstanding and sufficiently witnessed to establish some abnormal agency, the cumulative effect of the testimony of a variety of witnesses gives the Penkaet case some importance.

A statement by Mrs. Holbourn, wife of the late owner, is of special interest. Mrs. Holbourn is an observant lady with an open mind, but with a bias against any supernormal explanation of the things she witnessed. Her son, Mr. Alasdair Holbourn, is also disposed against any supernormal explanation. A notable part of the story relates to the visit in the spring of 1946, of a party of young people from Edinburgh College of Art in connection with the rehearsal of a play. Nearly all of them were seen by a representative of the society and individually interviewed, and they have signed a statement of their experiences. It is a coincidence that the bed used by Charles II, now in Penkaet, and presented to the late Professor Holbourn by his students, comes into the story.

Statement by J. W. Herries, a Trustee of the Edinburgh Psychic College

Attention was attracted to the variety of apparently psychic manifestations which have taken place at the old mansion-house of Fountainhall, otherwise known as Penkaet Castle, by the visit of about one hundred members of the East Lothian Antiquarian and Field Naturalists Society on July 29, 1946, when, in an upper gallery which is used as a library, a glass globe disintegrated, a considerable number of the visitors being in the room, and without anyone being within six feet of it and with no apparent cause. The globe was about two feet high with a base of about twenty inches, and was in the form of a half oval, and protecting a model of the house made by a son of the owner. This peculiar incident led to the recalling of a number of incidents, apparently of psychic origin, which have occurred in recent times in the house.

The mansion-house was acquired by the late Professor Holbourn some twenty-five years ago. It dates to about the beginning of the Sixteenth Century, and, as it is practically in its original condition, of distinctive design and with the rooms undisturbed and to a large extent furnished in harmony with its period, it is one of the most interesting residences in use in Scotland.

On various occasions, noises of footsteps, the moving of furniture, etc., have been heard by occupants in the house which could not be ordinarily accounted for. Traditionally, some of these manifestations are associated with a former owner, John Cockburn. There is a further factor of interest connected with the noises in the fact that the bed of Charles II, a magnificent example of period furniture, stands in one of the upper rooms. It was presented to Professor Holbourn by his students. It is the actual bed used by Charles II; and a feature of its elaborate carving is the mask believed to be the death mask of Charles I in plaster in duplicate on the bottom standards.

Mrs. Holbourn, who has resided in the house before and since her husband's death, recounted a number of manifestations of which she could speak personally. There is a record by Sir Andrew Dick Lauder, a former owner of the house, that when he was a child of nine, he was terrified by seeing what he took to be a ghost standing in front of the fireplace in one of the upper rooms.

Mrs. Holbourn states that from time to time, when occupying the bedroom below the room in which is the bed of King Charles, she and her husband and others have heard noises of movement in the

room above which was believed to be empty at the time. Sometimes the sound was that of furniture being moved; sometimes like someone stumbling and groping about the room. No explanation could ever be found on investigation being made.

About 1924, Professor and Mrs. Holbourn went to visit the island of Foula which Professor Holbourn had purchased in 1901, and a cousin was left in the house. In the course of his stay there, on taking a visitor up to see the King Charles bed, he found the bedclothes disarranged as if the bed had not been made. He drew the attention of the daughter of the gardener to this fact. She is Mrs. Anderson, and lives in an adjoining cottage. It was part of her duty to remake the bed after it had been slept in. She asserted that she had left the bed properly made up. Shortly afterwards, a visitor got permission to take a photograph, and on going up to the room with him, the cousin again found the bedclothes disarranged; and again it was made up by Mrs. Anderson. A day or so later the visitor who had taken the photograph reported that it had been under-exposed and returned to take another photograph, and on going up to the room they again found that the bedclothing had been disarranged. Mrs. Anderson once more made up the bed, and Mr. Holbourn's cousin took the precaution of locking the two doors giving access to the room and seeing that the windows were well secured. Two bricks were also placed against the main door. Next day it was found that the bricks had been displaced and again the bedclothing was disarranged. There was no one staying in the house but himself at the time.

On another occasion, a heavy antique cabinet in the room, very difficult to move, was found six inches away from the wall. A brass ewer and basin which Mrs. Holbourn's grandfather had brought from Turkey had been placed on top of the cabinet, and the ewer was found on its side.

Another important incident, or series of incidents, took place in the spring of 1946. Students of the Edinburgh College of Art were preparing to give a dramatic performance in the college at which Mrs. Holbourn's son was a student, and a party of seven were invited to Penkaet Castle for the purpose of a rehearsal of the play. Mrs. Holbourn arranged for the accommodation of the visitors, and she occupied the music room. During the night she heard somewhere above her what she described as the most appalling noises. This went on till about three o'clock in the morning. She thought to herself that the girls who were in the party were behaving very badly. Her son and her son's wife occupied the dining room, and they reported in the morning that they had hardly slept at all because of the noise, which suggested that the other members of the

party were rehearsing the play and doing so very noisily. They also thought that this was extraordinary behaviour for visitors to a strange house.

When two of the girls, who had been occupying the room above the music room, were asked how they had slept, they said that they had been very much disturbed and had assumed that someone was playing a practical joke and pretending that there was a ghost in the premises. The girls concluded that the best plan was to take no notice whatever. They assured Mrs. Holbourn that they had not moved about at all and kept very quiet throughout the disturbances.

The next two visitors to appear had occupied the room above Mrs. Holbourn's son and his wife. This is the room with the King Charles bed. They said they had had an awful night and practically did not close their eyes at all. They thought that possibly the young man of the party who was sleeping in the study must have been responsible for the noises. They both saw what they described as a most ghastly stain on the wall of their room, and in the morning when they looked for it, they found that there was nothing there to account for what they had seen.

The last to come down was the young man, who had slept in the study. He had slept quite well as he had been tired, and apparently knew nothing of the disturbances.

Mrs. Holbourn said, that sometimes she heard noises in the King Charles room at night. On the night of her husband's funeral, she heard distinctly footsteps coming down the path, and the front door open and shut. Her eldest son went down and searched the ground floor but saw nothing to account for the sounds. When he returned, the household cat came in front of him and appeared to be terrified. She hid herself under the table with her tail lashing from side to side, apparently in a great state of agitation.

I afterward saw Mrs. Anderson, who had seen the disarranged bedclothes, and whose duty it was to make the King Charles bed, and she corroborated, so far as her knowledge went, what has already been stated. She did not sleep in the house, she said, but each morning she saw the disturbed bedclothes and she also noted that the two bricks, which had been placed against the door on the last occasion, had been moved.

The foregoing statement was drawn up by me from information given me at Penkaet Castle by Mrs. Holbourn on October 6, 1946, and has been revised by her.

Edinburgh, October 8, 1946. (signed) J. W. HERRIES

Narrative by Mrs. Holbourn

John Cockburn, a former owner, killed John Seton who was probably connected with him by marriage. It is assumed that the uncanny conscience of the former makes him "walk."

When we first came here in 1923, we were often disturbed by the sound of heavy footsteps going through the house and the sound of something heavy and soft being dragged along. Various people who occupied the house during our visits to Foula complained of hearing shrieks and groans, and that doors which were shut and even locked at night were found open in the morning. One girl was so terrified that she refused to sleep alone.

As time went on the sounds became trivial and even playful. Sometimes when there was a light continuous tapping or rattling my husband would call out, "Now John, that's childish. Stop it!" and the sound invariably ceased at once. We called him "the perfect gentleman."

On Christmas Eve, 1923, we had been singing carols in the music room and were gathered around the fire when a piece of oak, seven by six inches, on which the family crest was carved, leant forward from the wall, paused a moment, and returned to position. We took this as a greeting to the new inhabitants.

About 1925, Avis Dolphin, a survivor of the Lusitania disaster who lived with us for a number of years, was sleeping in the King Charles bed. My husband and I slept in the room beneath. One night Avis came to our door to say that there was someone moving about on the ground floor. My husband got up and they went downstairs to investigate. On returning, when they reached the first floor, they stood on the stairs and listened. From the room above they heard the unmistakable creaking sounds of a person turning over in the Charles bed which the girl had just vacated.

One evening when Avis was coming upstairs in the dark she felt a light touch on her neck like someone gently drawing the tip of a finger across her throat. The same year I sometimes saw faint shimmering lights in the passages.

About 1935, a lady who was recovering from an illness was sleeping in the Charles bed, and my brother slept in the room below. About 5 a.m. he came to me and said, "I think Mrs. R. has fallen out of bed and is knocking for help." I found the lady sleeping soundly in her bed.

I would like to make it clear that although we regard "John" as an amusing, legendary figure, we none of us seriously believe in the

existence of our ghost. I think there must be some material explanation.

Penkaet, October 12, 1946. (Init.) M.C.S.H.

Statements by Weekend Visitors at Penkaet Castle

Statements were made to J. W. Herries in Edinburgh, on December 4, 1946, by members of a party who spent the weekend of March 16 and 17, 1946, at Penkaet, for the purpose of rehearsing a play to be performed at Edinburgh College of Art, the visitors with one exception being students of the College. Those present on December 4, were Mary G. W., Jocelyn L. S., Pat M. T., and Myra B. With them was Mr. L. A. Holbourn, who with his wife, Mrs. Holbourn, Jr., were also at Penkaet that weekend. The other visitors were Miss Rae B. and Wm. H. B., another College student now in the army, neither of whom was present on December 4, and Ishbel W. who arrived on Sunday.

The important part of the evidence of those present was that there was nothing on their part to explain the noise, as of some persons rushing about and speaking or shouting, as described by Mrs. Holbourn, Sr., and Mr. Holbourn. On the positive side, they narrated a number of incidents of interest.

Miss Jocelyn L. S., Miss Pat M. T., and Miss Myra B. arrived at Penkaet at lunch-time on Saturday, along with W. H. B. Miss Rae B. also arrived on Saturday and Miss Mary G. W. arrived on Sunday.

There was a rehearsal on Saturday (of "Ladies in Retirement"), and the party retired early to their rooms after supper.

Miss Pat M. T. and Miss Jocelyn L. S. occupied the room with the King Charles bed, going up to it about 10 p.m. They had two candles in the room, one on either side of the bed, and they also had an oil heater. The room felt very cold. They went to bed but could not sleep, and they lay awake and talked. They noticed, some time after going to bed (about 2 a.m.), a large broad stain on the wall opposite the bed. It was on the right hand side of the fire-place, beginning a little below the cornice and extending halfway down the wall, tapering towards the bottom. It suggested to some extent a section of the paper having come off the wall and hanging down. The walls were light in colour, and this patch was dark—a dark brown color. Next night they suddenly noticed it was gone. They experimented with the candles to see if it could be explained by a natural shadow, but were unable to secure anything like it.

Miss Jocelyn L. S. and Miss Pat M. T. further stated that about midnight they heard an extraordinary sound "like something trundling across the floor" (of the room above the King Charles Room or Library). It was described further as "like something going down a slope." They heard this sound repeated from time to time. They also heard sounds like footsteps in the room above. At the time they put the footsteps down to W. H. Brown, but afterward he stated that at that time he was sound asleep.

Miss Myra B. was in the Long Room (on the same floor as the King Charles Room and behind the latter). She shared it with Miss Rae B. Miss Myra B. stated that she heard the trundling sound overhead. It had a rhythmic character. She heard it only once and she was scared. There were also noises and creaks such as are given out by furniture. One of the glass panes in a window was imperfect, and this might account for some of the sounds. "The room was cold and I had a feeling we never were quite alone."

Miss Joceyln L. S. said she went down one of the stairs during the night—"Nothing happened."

Miss Pat M. T. commented on another matter. "I had with me," she said, "a square clock. It has gone regularly ever since I went to school. I packed it on Saturday morning and it was going then. I took it out at Penkaet and wound it. It never went more than five minutes during our stay. It would not go on the Saturday or the Sunday."

Miss Pat M. T. said she tried to get the clock to go several times. It stopped at midnight definitely.

Mr. Holbourn remarked that no clock would go placed on the wall between the dining room and the next room. Even a watch hung up on that wall would not go.

Miss Jocelyn L. S. said she was extremely ill during her stay. On Sunday, she said, they were accused of having been up all the previous night and engaging in high jinks, but they were all separately in their beds. They were more active on the Sunday night because other people came into their rooms—to discuss matters.

Miss Myra B., who occupied the Long Room with Miss Rae B., said she did not like the room. She disliked it at once. It had a bad effect on her. She also felt ill during the visit.

Mr. Holbourn recalled a previous experience in the "Middle Room." They had a Siamese cat at the time, and one night he heard a scratching at one of the two doors at opposite ends of the room. He rose to open the door and when he was about a yard from it the door was suddenly thrown wide open, and at the same time the other door opened wide and a curtain on the partition wall blew out, al-

though there was no wind to account for this. This occurrence was followed by the sound of footsteps down the passage.

Mr. Holbourn described another incident (date about 1935). He was working in the workshop on the ground floor. It was about 11 p.m. on a summer evening and just getting dark. His grandmother was bed-ridden in one of the rooms on the first floor, and his wife was nursing her. He took the job he was working on outside to look at it when the housekeeper, Betta Leadbetter, spoke from the window, saying that someone was having a bath. Mrs. Holbourn said she had been in bed since 9 o'clock. He went up to investigate and found the bath-room full of steam, the windows and mirror dimmed, but the bath was dry. The housekeeper declared that she heard the water running in and later running out, and someone turning in the bath. They used at that time to get soap by the half hundredweight. It was of a uniform kind and coloured, but in the bathroom there was a piece of square white soap, different from the soap they were using and unlike anything in the house. There was no one in the house unaccounted for who could have been using the bath.

Statement by William H. Brown

William H. Brown, writing from an Army Camp on January 28, 1947, made the following statement:

I am very pleased to make some attempt at a statement about my visit to Penkaet. It is essentially a very negative account, because, as observed in the report, I did actually sleep very soundly on both nights.

I slept directly above the room with the King Charles bed, which was occupied by Miss Jocelyn L. S. and Miss Pat M. T. The room I am informed was the late Professor Holbourn's study and is next to the library. All I did on retiring was to go to the library to look for something to read, come back, undress, and lie in bed reading. You will note that I undressed before beginning to read. On the following morning it was the general belief that I was responsible for the heavy sounds, and if they were not deliberate, they were caused by my undressing at a late hour. In any case, I only read for about half an hour. Being a fairly sound sleeper, it was not long before I became completely unconscious, and in that state I remained until Mrs. Holbourn, Sr., wakened me about seven the next morning. I could not have gone to bed any later than a quarter past eleven and was asleep long before midnight.

The atmosphere in the morning was tense, and as I have suggested, I was thought to be responsible. That, however, is definitely not so. One point that is interesting, and which goes some distance in establishing the authenticity of the statement, is that the account given immediately after the occurrence does not vary in the slightest with the account given nearly ten months later. As you have learned, events on the second night were less spectacular, and in any case, I again slept undisturbed.

I am not particularly sceptical about such occurrences, and I am perfectly willing to believe that they did take place; but as I say, I simply slept and heard or saw absolutely nothing.

I am sorry that this statement is not so constructive as it might have been, although I hope that it serves your purpose.

Comment

The testimony, in support of the unusual happenings at Penkaet Castle by a substantial number of first-hand witnesses of various ages and both sexes, suggests that a further and more intensive investigation would be well worth the time and trouble. It is true that experimental investigations of "haunted houses" present many difficulties. Chief among these is the fact that the disturbances do not, as a rule, occur with any frequency or regularity.

The odd behavior of the cat, coinciding in time with other disturbances in the castle, may be noted, although no great weight can be attached to this single incident. But the literature of psychical research includes other accounts of the peculiar behavior of domestic animals in connection with supernormal phenomena. Among these is the carefully documented report of the spaniel, Fifi, in *"The Psychic in the House,"*[1] by Walter Franklin Prince. Dr. Prince was unable to explain the dog's actions on any normal hypothesis.

Mrs. Holbourn has been described "as an observant lady with an open mind, but with a bias against any supernormal explanation of the things she witnessed." In her own words she "would like to make it clear that although we regard 'John' as an amusing legendary figure we none of us seriously believe in the existence of our ghost." In view of her attitude, as well as on independent grounds, Mrs. Holbourn's account of the "light continuous tapping or rattling" which invariably ceased at once when her husband, Professor Holbourn, called out "Now John, that's childish. Stop it!" is of special interest. The responses to Professor Holbourn's admonitions appeared to be ***prompt and intelligent.***

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[1] B.S.P.R., 1926, pp. 160-173.

Sir William Barrett has drawn attention to the occasionally intelligent responses of *Poltergeists*. He defined a "poltergeist" as a boisterous ghost, a convenient term "to describe those apparently meaningless noises, disturbances, and movements of objects, for which we can discover no assignable cause."

While the manifestations at Penkaet Castle do not seem to conform to the main characteristics usually associated with poltergeists, the intelligent responses of "John" seem comparable to those reported by Sir William in his paper "Poltergeists, Old and New,"[2] from which we quote:

"The phenomena are especially sporadic, breaking out suddenly and unexpectedly, and disappearing as suddenly after a few days, or weeks, or months of annoyance to those concerned. They differ from hauntings, inasmuch as they appear to be attached to an individual, usually a young person, more than to a place, or rather to *a person in a particular place*. Moreover, ghostly forms (except, if we may trust one or two witnesses, a hand and arm) are not seen.

"They [Poltergeists] appear to have some intelligence behind them, for they frequently respond to requests made for a given number of raps; the intelligence is therefore in some way related to our intelligence, and moreover is occasionally in telepathic rapport with our minds. For in one case which I submitted to a long and searching enquiry, I found that when I mentally asked for a given number of raps, no word being spoken, the response was given promptly and correctly, and this four times in succession, a different number being silently asked for in each case."

Mrs. C. Allan, Honorary Principal of the Edinburgh Psychic College and Library, has kindly given us some information about the College which will be of interest to our members. The College was founded in 1932, by the late Mrs. Ethel Miller, a Norwegian lady, as a memorial to her husband. It has about four hundred members. By a Deed dated September 23, 1935, Mrs. Miller conveyed the heritable property at 30 Heriot Row, Edinburgh, to Trustees to be used as the Edinburgh Psychic College and Library for the purposes set forth in the said Deed and in the Constitution of the College dated November 15, 1932. The objects of the Edinburgh Psychic College and Library may be summed up as the study and investigation of psychic phenomena and their implications, and the development of the psychic powers of its Members.

L. W. A.

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[2] *Proc.* S.P.R., Vol. XXV (1911), pp. 377 f.

Book Review

CHALLENGE OF THE UNKNOWN: Exploring the Psychic World, by Louis K. Anspacher, Current Books, Inc., New York, 1947, 327 pp. $3.75. With an Introduction by Waldemar Kaempffert.

Those who attended Dr. Anspacher's lectures on psychic phenomena at the Town Hall two years ago will recall that, in concluding, he announced his intention of making those lectures the basis of a more extended discussion in a book. That book is *Challenge of the Unknown,* which appeared this year, but unhappily, only after the sudden death of the author.

Dr. Anspacher's discussion is sent off to a good start by an admirable Introduction from the pen of a distinguished trustee of our Society, Mr. Waldemar Kaempffert, Science Editor of *The New York Times.* He sets the standard of a high intellectual approach to a subject that has suffered more from its misguided and ignorant enthusiasts than from its enemies. The following passage, coming toward the end of the Introduction, is highly significant, coming as it does from a man of science:

> Because of the limitations that it has imposed on itself physical science can never bring us any closer to reality than we have been. The only reality that we shall ever know must come directly as a spiritual experience and not through a knowledge of particles and fields of energy. The fierce faith of the martyr willing to die at the stake, the sense of communing with something higher than himself that Beethoven must have had when he composed his last sonatas and symphonies, the rare rapture of a poet at one with nature, the exaltation that lifts a mystic out of himself, the intuitions, premonitions, and telepathic messages that compel us to act contrary to all reason, yet correctly, as the event often proves, the vivid dreams that are later verified: these are probably the only reality that we shall ever know.

Dr. Anspacher has treated his subject in five long chapters or sections. These are: "Techniques of Approach"; "Psychic Manifestations in Art and Literature"; "The Present Verdict of Science on Psychic Manifestation"; "What the Great Philosophers Have Said on the Question of Psychics"; and "Religion and Psychics: the Bible as a Psychic Document." These five divisions of a book of 327 closely printed pages, with no breaks except in the opening chapter, require disciplined reading. The chapter on the Philosophers, for example, extends to eighty pages. Dr. Anspacher is frankly limiting his audience to the intellectuals, a fact that is accentuated by his vocabulary,

particularly in that chapter on philosophy just mentioned. The ordinary reader will need a dictionary at his elbow to understand such terms as vertiginous, acedita, detritus, proleptic, hyperesthesia, epistemology, epiphenomenon, paramnesian, and entelechy — to pick out a few at random. Certainly, these words are not in the vocabulary of every-day folk, even those with a college degree.

Sometimes, as in this particular chapter, and in the one on art and literature, the display of erudition becomes so overpowering as to make the forest invisible for the trees. The prolonged discussion of the philosophers amounts to a history of philosophy in which the relation of all the theories to psychical phenomena is in danger of being lost.

The style throughout is florid, suggesting the professional lecturer and old-school dramatist and actor. Sometimes this betrays the author into a mixed metaphor, as when he speaks of "a witch hunt for a foot of clay." Like most lecturers and preachers, too, he slips occasionally into a dogmatic statement which arouses challenge. For example, he says, "Paul was an epileptic." It need hardly be said that while the hypothesis of epilepsy has been repeated many times, there is not the faintest bit of evidence for it beyond his complaining of a "thorn in the flesh." The idea seems to have sprung from the attempts of sceptics to account for the vision on the road to Damascus as being only a hallucination in an epileptic fit, an explanation which Dr. Anspacher specifically repudiates.

These, however, are minor criticisms, and it is high time to speak of the important values of this book and its contribution to the cause of making the study of psychic phenomena take its rightful place in the search for knowledge and in the respect of intelligent people. *Challenge of the Unknown* performs a real service. For one thing, here is a discussion of the phenomena of the supernormal by a man who is exceptionally well read in fields as widely diverse as literature, science, philosophy and religion. In all these realms of thought poets, saints, and sages have tried to explain man, his place in the Cosmos, and the nature of that Cosmos. The author fits the newly recognized psychic phenomena into their place in the universal scheme, showing how their authenticity is buttressed by all these other fields of knowledge and expression, and how these psychic truths come nearer than any of the rest in pointing to Reality. Dr. Anspacher's ideal was Goethe, and it is clear that he tried to deal with this new knowledge in the spirit of that great master.

Probably most readers will agree that the last chapter, "Religion and Psychics," is the climax of the book, as the author intended it to be when he put it at the end. This discussion is easy to read, for it

needs no technical terms or unfamiliar names. It shows how so much of the Bible record, especially the New Testament, deals with "miracles," nearly all of which, at least, are well-known and often repeated psychical phenomena. To Dr. Anspacher Jesus was in the highest degree master of these powers that we call psychic. In a lesser degree they were practiced by the apostles, notably Paul, and they were clearly recognized by the early Church.

It is heartening to see that it is the great spiritual significance of the new truth that inspires the book. In the opening chapter the author declares that "scientific proof for the survival of personality would certainly give the world the greatest, the most pivotal shock it has ever known. It would provide us with an entirely new and august sense of human destiny." This majestic theme is eloquently developed in the concluding chapter, "Religion and Psychics." "Psychical research is providing a new faith in the existence of an unseen and transcendent universe . . . We are trying to prove . . . and in some measure succeeding in proving, that what you call a spiritual world actually exists: a world of independent and abiding reality, not a world that we are obliged to accept in blind faith."

Here the author reveals himself not only as the erudite scholar and lecturer but as a poet and worshiper, standing on the threshold of a temple that he knows to be more vast than the mind of man has ever yet conceived.

WILLIAM OLIVER STEVENS

Program of the Membership Committee

Following the practice of recent years, the Membership Committee is undertaking a program of monthly teas and lectures, beginning in November and continuing through April. Names of speakers cannot be announced at this time, but the dates are as follows:

Wednesday, November 19
Wednesday, December 17
Wednesday, January 21
Wednesday, February 18
Wednesday, March 17
Wednesday, April 21

It will be easy to remember that these teas come on the third Wednesday of each month, at four o'clock.

The committee is anxious to achieve positive results in adding to our membership this year. This should not be difficult because there are so many individuals who are interested in psychical research but who as yet have not joined the Society. To this end present members are urged to cooperate by interesting their friends in joining. The Chairman would appreciate receiving as soon as possible the names of individuals who could be directly approached by letters, and a postcard containing names and addresses is all that is needed.

WILLIAM OLIVER STEVENS,
Chairman

Notice to Members

We wish to announce that Mrs. L. A. Dale resigned as Editor of the JOURNAL on July first of this year in order to devote more of her time to research. Beginning with this issue, the JOURNAL is being edited by the Publications Committee. Contributions submitted for publication and all editorial communications should be addressed to the Publications Committee, Room 916, 40 East 34th Street, New York 16, N. Y.

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