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CONTENTS

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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 records 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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Volume XXXVII JANUARY - 1943 Number 1

CONTENTS

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 26th, 1943, at 4:00 o'clock in the afternoon for the election of Five Trustees and for the transaction of such other business as may properly come before the meeting.

ELEANOR D. WHITEHEAD, *Secretary.*

Concentration Versus Relaxation in Relation to Telepathy

GARDNER MURPHY AND L. A. DALE

In all the years of search for the psychological foundations of telepathy, the investigator has found himself caught between opposing counsels: shall he seek to develop in his cooperating subjects the attitude of concentration—tense and strenuous prosecution of a task—or of relaxed, dissolved, sleep-like passivity? There is a large amount of good evidence that the state of *concentration* is favorable to positive results; there is an equally formidable mass of evidence that positive results spring from *relaxation.* This is no formal academic question. Every step taken in research depends upon the psychological condition one is striving to induce; and what one should strive to induce is dictated chiefly by one's interpretation of the large body of existing data. Scientific work is seldom advanced by beginning each investigation as if all that went before were merely so much history; it is the nature of science to build cooperatively upon foundations laid before.

It may therefore prove valuable for immediate telepathic research if we can bring into sharper focus the critical studies dealing with concentration and relaxation, showing what sort of results have arisen from each condition. Indeed, we may hope, as in the case of most questions which we put to Nature, that the paradox will prove to lie in our conceptions and phraseology, and that behind the apparent contradiction a fairly coherent situation may be discovered.

We will first review some of the evidence that *concentration* is favorable. Just as the Oriental who seeks esoteric wisdom learns by arduous disciplines to concentrate, shutting out the affairs of the world in order to give himself wholly to single-minded contemplation, so the Occidental scholar, executive, physician, or school-child learns to work in a state of concentrated attention to a single absorbing task.

General Habs

Modern science and technology, and the Occidental mind which has shaped and been shaped by them, are the products of focalized, realistic thinking about a series of sharply defined problems. Archimedes was so concentrated on the problem of ascertaining the amount of gold in Hiero's crown that, when the solution came to him as he was stepping into his bath, he ran home unclothed shouting, "I have found it, I have found it!" And when he was drawing geometrical figures in the sand one day he was set upon by Roman soldiers and fatally stabbed, saying to the soldiers as he fell, "Don't spoil my circle." It is said that a few hours after the midday meal Newton could not remember whether he had dined or not, and Gauss would not come down from his attic for forty-eight hours at a stretch when he was completing his mathematical computations. Such concentration was what Dryden had in mind when he wrote that "great wits are sure to madness near allied." Johannes Müller, the German physiologist, comes from his laboratory like a gunner from his turret, saying, "The work draws blood."

Our American intelligence, whetted by three centuries of conquering the soil and exploiting the new technology, has in fact tended to despise every psychological process which is *not* concentrated upon a practical task. "Wake up"; "Put your mind to it"; "Genius is 99 percent perspiration." The familiar phrase, "Nonsense, you're dreaming," illustrates the contemporary attitude as to the utter worthlessness of less concentrated mental states.

At first sight, then, we should expect to find our American parapsychological laboratories concerning themselves almost wholly with problems of the tense, attentive organism, and emphasizing the virtues of concentrated voluntary effort. In Rhine's *Extra-Sensory Perception* (1) we find that most of the experimental subjects are clearly concentrated upon their task; when upset by discordant emotion or by distraction, they fail. Whether voluntarily or by force of habit, they fall into states of intense focalization upon their task.

One thing introduced experimentally to *damage* good performance is a hypnotic drug, sodium amytal, which behaves as expected both in injuring concentration and in depressing the scores; and the thing that brings the subject back to his high scoring level is the stimulant caffeine; he "pulls himself together," frees himself of disorientation. And we find subjects deliberately undertaking to make a specially high score, achieving the result by herculean efforts of concentration—almost like the superhuman concentration reported by the celebrated clairvoyant Ossowiecki in his reading of concealed materials (2).

In the *Journal of Parapsychology* we learn that Riess's high-scoring subject (3) was a hyperthyroid patient, and we know that the thyroid, up to a point at least, is a facilitator, activator, rather than a depressant or relaxing agent. We learn, moreover, that Stuart's subjects scored poorly when the *tempo* of work "distracted" them, and that the flashing of light in their eyes was markedly inhibitory (4 and 5). The good results of children, as in Mrs. Rhine's experiments (6), appear to be attributable in some measure to freedom from self-consciousness, and perhaps to their capacity for undivided attention to the task.

The case is certainly well-documented, broadly founded, and inherently consistent and rational. Why, then, do we find in the history of our subject at least as large a volume of evidence that the task of achieving telepathic contact is the function of a deep, unfocalized unconscious or subliminal self, whose chief enemy and represser is the concentrated conscious self of waking life? We are perennially warned that the practical conscious mind will have no traffic with those processes which seek to slip from the tight hold of our workaday sense of reality, with its sharply defined time and space, its logical rejection of every intimation of a process working by means which transcend the senses; the concentrated conscious mind must be dissolved, drugged, disarmed, obliterated—anything to get it out of the way so that another kind of process may be set free!

Thus, in the first great study of spontaneous telepathy, the classic *Phantasms of the Living,* Gurney, Myers, and Podmore report an extraordinary number of cases in which the percipient is drowsy, delirious, hypnotized, or dreaming; the dream cases are so numerous, indeed, that they require special treatment. In a later analysis of this and other spontaneous material, Warcollier (7) shows that in general the sleeping and near-sleeping states (including the process of going to sleep and that of waking up, along with states of suffocation, coma, and the like) contribute a large proportion of the evidential cases—indeed that the sleep-like condition of *agent,* or *percipient,* and especially of *both,* is a cardinal asset. This hardly looks like "concentration."

Some striking examples of apparent telepathic communication between two sleeping persons may be found in Dr. G. B. Ermacora's paper, "Telepathic Dreams Experimentally Induced." (*Proc.* S.P.R., Vol. XI, 1895, pp. 235-308.) This long and rather complicated series of experiments should be studied in full; briefly, the paper is concerned with the dreams of a four and a half year old child who lived with a cousin possessing unusual mediumistic abilities. The medium's control, "Elvira," with whom Dr. Ermacora was in *rapport,* professed to produce in the child dreams the subject matter of which was suggested by Dr. Ermacora himself. Stringent precautions were taken to prevent any normal communication between the child and her cousin. For instance, on November 9, 1892, Dr. Ermacora, speaking to "Elvira" when the medium was in trance, proposed that the child should have the following dream that night:

> Angelina [the child percipient], with Signorina Maria [the medium], would be at the window of her own room, and would look towards the river. A lamb would be grazing on the bank. A boat loaded with apples would pass, conducted by one boatman. He would stop close to the iron bridge, and get out to drink at the inn. While the boat was unguarded the lamb would jump in and begin to eat the apples, which would make Angelina laugh very much.

How well "Elvira" succeeded may be gathered from Dr. Ermacora's statement of November 10th:

About 4 P.M. I called to discover the results. I saw Signora Annetta [the medium's mother] first, and she was convinced that no communication had been possible between the child and Signorina Maria till the morning. Angelina told her the dream before Signorina Maria was awake. It is useless to repeat it, as it corresponds almost exactly to the programme. I say almost, because there are two points of difference which are precisely what render the results most interesting. One is that Angelina called the animal which ate the apples a "light-coloured dog," instead of a "lamb." Now the child, being a Venetian, had not seen any lambs, and when she saw one [in the dream], she naturally baptised it in her own fashion. The other point of difference is that she did not say where the boatman had gone, and, when questioned on the point on my arrival, she could not explain it, though she remembered the dream perfectly. According to what was said above [that is, if the medium suggests the dream verbally to the child, it is improbable that she will omit the particular about the inn and even less likely that she will call the lamb a "light-coloured dog"], these two particulars favour the hypothesis that the child *sees* the scene, instead of simply hearing it described.

The Groningen investigators found that their highly gifted subject fell naturally into a passive and relaxed state, which was evidently a powerful aid to him; they demonstrated by the galvanic skin reflex (electrical changes in the skin) that the subject could in fact report rather accurately when he achieved this state of relaxation (8). Estabrooks, after a year of hypnotic work with Harvard students, determined to use a *waking,* but *casual* state; not only was there to be no concentration, the subjects were first shown card-tricks and then told simply to *guess* cards (9).

In view of all this, we naturally turn to see what can be learned from the hypnotic state. As usually induced, it is a relaxed, sleep-like state, far indeed from waking, concentrated activity, and seeming to lend itself well to laboratory use in psychical research. There appear to be a number of major pieces of evidence suggesting that the hypnotic state is highly favorable.

Among these, we may refer to the series of hypnotic experiments known as the "Brighton Experiments." These are reported on at length in a paper published in 1889 by

Professor and Mrs. Sidgwick and Mr. G. A. Smith (*Proc.* S.P.R., Vol. VI, pp. 128-170) and in a later one (1892) by Mrs. Sidgwick and Miss Alice Johnson (*Proc.* S.P.R., Vol. VIII, pp. 536-596). The series should be studied in its entirety; we will quote here one typical experiment, as summarized and commented upon by Mrs. Sidgwick in her paper, "On Hindrances and Complications in Telepathic Communication" (*Proc.* S.P.R., Vol. XXXIV, pp. 51-52):

In the one to be now quoted, the elements of the picture* emerged in a piecemeal manner, but each was definite when it came. The subject was *A sandwichman with advertisement of a play.* The percipient—P.—said, "Something like letter A—stroke there, then there." I [Mrs. Sidgwick] said, "Well, perhaps it will become clearer." P. continued, "Something like a head on the top of it; a V upside down—two legs and then a head. A man with two boards—looks like a man that goes about the streets with two boards. I can see a head at the top and the body and legs between the boards. I couldn't see what was written on the boards, because the edges were turned towards me." This case is interesting, because notwithstanding the gradual emergence and tardy understanding of the hallucinatory picture there is strong reason to think that it was the *idea* of a sandwich man, and not a copy of the agent's mental image, that reached the subliminal stratum of the percipient's mind. For the agent—Mr. Smith—stated afterwards that he had pictured to himself the man and one board facing him. And this was the natural way to think of it, since the subject set included the advertisement of a play, which of course would only be seen if the board faced the spectator. The percipient's impression was incomplete.

Moreover, there is the somewhat more spontaneous activity of the hypnotized subject in "traveling clairvoyance"; the subject, upon instruction, imagines himself traveling to a distant point, and reports what he sees, the report being later checked against the distant reality. As an example of traveling clairvoyance, we condense from a paper

* The percipient was hypnotized by the agent, Mr. G. A. Smith, and was then given a blank card to look at. He was told that he would presently see a picture thereon which he would describe. Mr. Smith concentrated his mind on an *imaginary* picture, the subject matter of which had been chosen by Mrs. Sidgwick or Miss Johnson and communicated to Mr. Smith in writing. After hypnosis was achieved, Mrs. Sidgwick talked to the percipient when necessary, Mr. Smith remaining silent.

by Dr. Alfred Backman, of Kalmar, Sweden (*Proc.* S.P.R., Vol. VII, 1891-92, pp. 207-210):

Dr. Backman's subject, Alma, was a fourteen year old girl whom he was treating (in hypnosis) for heart disease. He had made some experiments in thought-transference with her and was encouraged by a number of successes. In April of 1890, he received a letter from a Dr. Kjellman, of Stockholm, suggesting that on the following day Dr. Backman would request Alma, when in hypnotic trance, to "find" a certain Dr. von B., whom she knew, and to describe the room in which he would be sitting, the other persons present, the arrangement of the furniture, and so forth. Dr. Kjellman further added that something not usually there would be hung from the chandelier in order to make the experiment more crucial. Dr. Kjellman's appearance, as well as his apartment, was unknown to both Dr. Backman and his subject. Alma, in trance, "went" to the apartment and, when questioned, gave a description of the room. She said, when asked to look at the chandelier, that there was no "chandelier," but something more like a lamp with something hanging from it. She described it as being long and narrow, of white metal, with some red stuff around it. She did not know what it was, but after being wakened said she thought it probably was a pair of scissors or a paper knife fixed to the chandelier with a handkerchief. She also mentioned many details concerning the furnishings of the room, the people present, and so forth. Dr. Backman sent his notes to Dr. Kjellman, who replied that though in some respects Alma's description of the interior of his apartment had been accurate, it was in other respects incorrect and could only be regarded as inconclusive. "But," he added, "her statement that the object was hanging in a lamp, not a chandelier, was right. It was both a lamp and a chandelier, and the lamp was drawn down a long way under the chandelier . . . There was really hanging in the chandelier a large pair of paper-scissors, fixed by an india-rubber otoscope, and with a tea-rose and some forget-me-nots in one of the handles of the scissors . . . It seems clear to me that she saw something, but she must have been confused by a number of ideas arising from the operation of her own brain."

Lastly, we want to refer to the beautiful series of telepathic experiments, using four experimental subjects, by Dr. Carl Bruck. A report on these experiments was published in German in 1925, and, not being available in English, merits a fuller description here. Bruck systematically compared the achievements of his subjects while awake and

while in hypnosis. The material consisted of drawings and photographs made ready in a room adjoining the experimental room and in many instances placed within a portfolio before being brought into the experimental room. Thus sensory cues as sources of error seem well excluded. Examples* of simple direct hits are the following:

. . . These experiments were done simultaneously with R. and Z., using the drawing of a *ladder* as original. The two subjects were placed about ten feet apart and it was not possible that they could have seen each other's drawings. They were hypnotized simultaneously and were instructed to aim at the original (unknown to both of them, and to the witnesses of the experiment) which had been placed in the portfolio. These simultaneous attempts succeeded extremely well under conditions which forestall all objections. Fig. 7—Original, Fig. 7a—R.'s copy, Fig. 7b—Z.'s copy.

In the results it is to be noted that R. draws well and is better endowed telepathically than Z. He not only reproduces the original in remote details of form and relation to its surroundings—he also occupies himself as somnambulistic "medium," composing a supplementary still-life picture with a bucket in the background. In contrast, Z. got the solution, but was limited to the elementary concept "ladder." Nevertheless, a visual approximation cannot be denied even by one who has not the original for comparison.

Especially instructive is the evidence that real veridical flashes come in the hypnotic state, but that they are often either elaborated by supplementary associations and by efforts to attribute meaning to the impression received, or over-simplified to the point where little but a formal resemblance to the original remains. For example:

This sitting took place on September 4, 1922, at my [Dr. Bruck's] residence. It was a simultaneous experiment with R. and Z. both aiming at the same original, the drawing of a *cup and saucer*. R. was hypnotized by me and Z., sitting at a distance, by Dr. von Rutkowski. Dr. Rutkowski had not seen the drawing, which was kept in the portfolio throughout the experiment. I gave only the brief suggestion that the material would gradually emerge from a dark bank of clouds in such a way as to be copied by both subjects. R. and Z. were so

* The passages in small type have been freely translated and paraphrased from Bruck's original work, *Experimentelle Telepathie*, Julius Püttmann, Stuttgart, 1925.

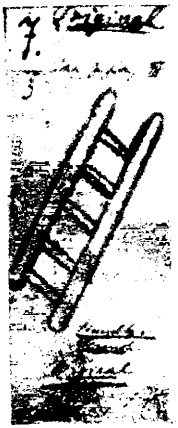
placed that they could not have influenced each other's drawings. Fig. 3—Original, Fig. 3a—R.'s copy, Fig. 3b—Z.'s copy.

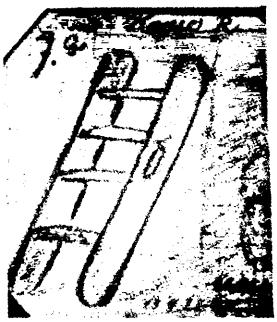
Both these successful results show the possibilities of variation in the telepathic process with respect to numerous details. R.'s drawing shows the familiar auto-suggested motor supplement, namely, a teaspoon. The cup agrees with the original in narrowing towards the top, with the handle on the right side. In addition, however, there is the following, which one might be inclined to apply in developing a general theory of the parapsychic processes: the original is a drawing of one of the cups from my own coffee service (unknown to both R. and Z.). I drew the original rather badly, for the cup is really more delicate and slender. R. has apparently not perceived my rather plump rendering, holding instead to the original form of the cup. It is thus not impossible (although beyond capability of proof) to suppose that "over my head" a direct clairvoyance of the cup occurred. The indistinct line of demarcation between telepathy and clairvoyance, which is frequently pointed out, justifies the raising of this question. No certainty can be reached, however, since one might attribute the result to R.'s superior artistic skill. His sense for style may also have been responsible for the addition of the spoon. In this connection, compare R.'s drawing in the [above] experiment, in which the original was a drawing of a ladder.

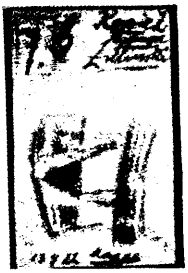
On the other hand, there is too little detail in Z.'s simultaneous drawing. It does show a crudely drawn cup that narrows towards the top, with a handle to the right, but the saucer is lacking altogether. As we found in later experiments, especially with R., this omission of complete elements of form, even to the point of leaving out as much as half of the picture, is due to a tendency towards simplification which may permit us to refer to a systematic conception of "partial telepathy."

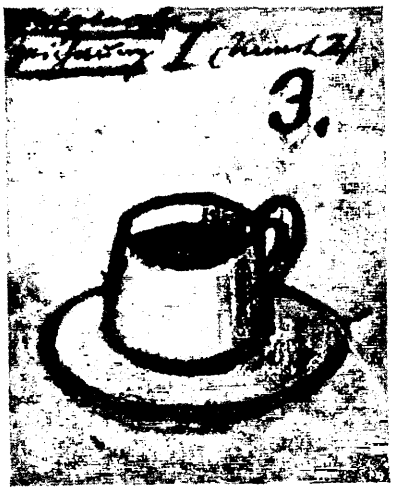
In some instances, the subject draws what the experimenter has prepared for use in the *next* experiment—material previously drawn and lying concealed in the portfolio. An interesting example is as follows:

At the start of the experiment [taking place on September 11, 1922, with R. in hypnosis], one drawing was lying on top of the portfolio and the others [three of them] were contained within it. I had no special methodological purpose in doing this, but to this procedure a surprising and theoretically significant result is due. The picture lying on the portfolio represented a medicine bottle with a stopper, and I looked at it fixedly from time to time during the experiment. Fig. 4—first Original, Fig. 4a—copy, Fig. 4b—second Original.





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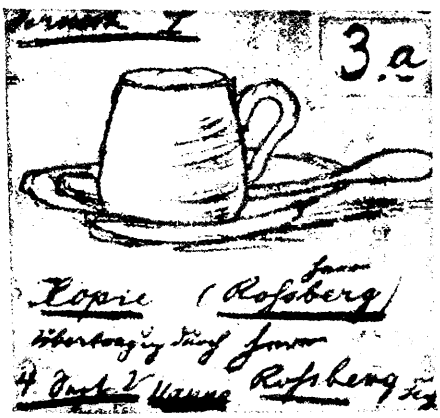
3.a
Kopie (Rosberg)

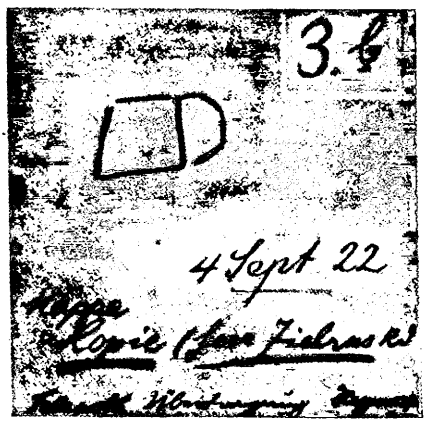
3.b
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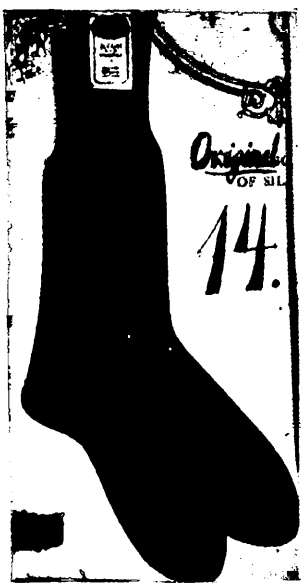
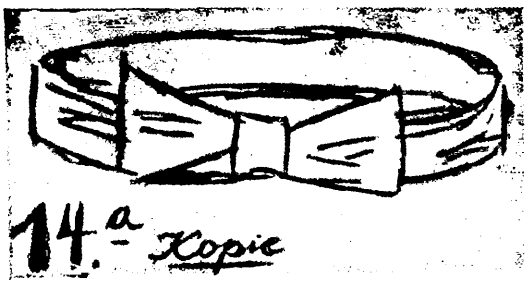
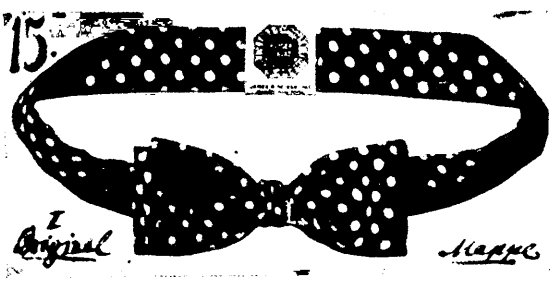
FIG. 4 4 A 4 B







R. now proceeded to draw what was, for me at least, knowing the original, a completely correct response, namely, the upper rim of the bottle neck with the stopper in place; as can be seen in the reproductions of the original and the copy, he even sketched in the shading that appears on the right side of the stopper. Then he ceased drawing and let the pencil drop from his hand as an indication that he had done all he could. Since I, however, had to regard the experiment as incomplete, I insisted that R. should continue to draw if anything further came to him. I admit this is a questionable procedure which, since the bottle had failed to appear in complete form, might end in a suggestive misleading of the subject into a spurious result. To my surprise, however, R. did not follow this lead; he left the first picture unfinished and beneath the neck of the bottle drew a completely new picture. With energetic strokes, indicating a new attitude to the task, he drew, beginning at the top, a sketch of a street lamp. A drawing of a street lamp was in fact the second of the three other drawings which lay in the portfolio. They were entirely out of sight of all those present, including several medical men who had come to witness the experiments.

This new telepathic drawing reveals the exact details of the original. Note that the two enlargements or bulges in the lamp post appear in the correct places relative to each other and to the lamp itself. At the same time, the decorative beams of light which the subject added in his drawing indicate his free creative artistic fantasy.

The experiment yields, so far as we know, a novel result. Instead of the *postponing* of a correct telepathic impression, it presents, under the guise of an apparent partial failure, the solution of a task which has been arranged for a *later* experiment.

In other instances the subject draws the material which had served as experimental material in the preceding experiment, the subject, of course, not having been told what the material was:

The sitting of November 17, 1922, took place at my residence and included experiments 38-45. R. alone was present . . . Experiments 40 and 41 were Portfolio* experiments using the same original (snapshots of two neckties—one a four-in-hand, the other a bow tie). Two failures resulted; nevertheless, the bow tie manifested itself in the next experiment but one (number 43 in the same sitting)—a typical telepathic latency, the more common form of temporal displacement . . .

* *Portfolio experiment*: original concealed within the portfolio.

Experiment 43 is the one which continues the theme of experiments 40 and 41 (same sitting). A snapshot of a pair of socks was used in an Open* hypnotic experiment. The result, negative from a formal viewpoint, displays, to the last detail, an exact reproduction of the bow tie which had been used as original in experiments 40-41. Fig. 14—Original, Fig. 14a—copy, Fig. 15—Original for experiments 40 and 41, bow tie.

Naturally, R. had not been informed of his failure in experiments 40-41. The picture of the bow tie had been carefully put aside and was thus unknown to him.

In spite of all this evidence, however, Bruck will not allow himself to formulate any generalizations regarding the value of the hypnotic state versus the waking state in telepathic experimentation. He compares the whole series of hypnotic results with the whole series of waking results with the same percipient, and indicates that the marked successes are about as frequent in the one state as in the other.

The real value of the hypnotic state may lie, not in the hypnosis *as such,* but in the tendency of hypnosis to simplify consciousness, free it from *self-consciousness,* fixate it upon the experimenter, and hence upon the task which the experimenter assigns. The success of waking telepathic experiments, such as those reported by Bruck, is very possibly due to basic favorable attitudes previously developed in hypnosis. These attitudes appear to be highly focalized, yet occurring in a relaxed state.

If this be so, we may begin to see here the resolution of the paradox which we have discussed. The hypnotic subject is not really less "concentrated," if by concentration one means the unitary motivated orientation towards a task; the very fact that he is relaxed may mean that he is free of practical preoccupations; it is the experimental task alone that is real to him. So in experiments in normal psychology. Ruger (10) found long ago that a *lack of self-awareness,* or, as he says, the development of a pure "problem attitude," was immeasurably superior, in so far as the learning process is concerned, to a competitive, self-conscious, or self-defensive attitude.

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* *Open Experiment*: original lying on top of the portfolio.

In a paper on recent hypnotic experiments, Leuba (11) has offered evidence which suggests that hypnosis is not principally a question of suggestibility as such, but of *simplification* of consciousness, freedom from interference through over-complex stimulation, or freedom from free association; and, we might add, freedom from the conflict which *volition,* as a directive and repressive process, usually involves. Leuba's hypothesis seems so relevant to our present problem that we quote from his paper:

> . . . It is possible, however, that the most fundamental characteristic of the hypnotic state is not suggestibility, though undoubtedly that is usually present, but the limitation of the spontaneous mental life of the subject and the consequent limitation of attention to the stimuli provided by the experimenter . . .
>
> The conditions favorable to the induction of hypnosis—quiet surroundings, muscular relaxation, concentration on some monotonous stimulus—are all conducive to the dissipation of muscular sets and to the narrowing of attention. I venture to suggest the hypothesis that the fundamental characteristic of the hypnotic state is this limitation of a self-initiated mental life, and that suggestibility flows from this limitation as a secondary phenomenon.

In the light of such observations, the state of mind of some of Rhine's subjects, in which there is a slight dissociation, a slight disorientation, a loss of contact with the surroundings, makes sense; and it should be added that as the experimental material becomes more complex, as in the case of the free drawings of Bruck, the dissociation, the freedom from interference, must perforce be longer and more profound than when the task simply consists in guessing one out of five possible symbols (as in Rhine's, or Tyrrell's, procedure). This more profound and more complete dissociation is what Mrs. Sinclair reports (12), and what Warcollier appears to have found in his subjects. Indeed, some of the best telepathic cases seem to involve dissociation, and the limitation of consciousness to a single slowly developing idea, in both agent and percipient. Examples that illustrate this point may be found in Warcollier's book, *Experimental Telepathy.*

The apparent inconsistency, then, between the value of

concentration and the value of relaxation appears to stem from failure to specify *what* is concentrated, *what* is relaxed. The fact seems to be that the everyday attitude of perceiving is directed towards the *immediate world of time and space* to which we are normally adjusted, and unless one learns how to break this attitude, more concentration does nothing but focus the *normal* perception. *Unconsciously,* however, and without effort, one may be oriented towards events with which our normal perception is unable to make contact, and if this orientation is strongly motivated, contact may be established in another way—subconsciously—provided only that the conscious processes do not *interfere.* Some individuals can consciously train themselves to hold their conscious processes in leash, or even to give the deeper processes the "go" signal; in other individuals the training process proceeds unwittingly, perhaps through the deeper satisfaction and self-fulfillment which subconscious processes release.

We are perforce reminded here of Freud's basic distinction between the two ways of thinking: one may think within the framework of practical necessity, or through free, autonomous self-realizing processes which have nothing to do with "reality." We should, of course, have to add to Freud's theory, since it is not *defiance* of reality, but contact with *another aspect* of reality that is involved. Basically, however, we have Freud to thank for the conception of genuine thought which is not directed towards a goal defined in terms of our immediate direct relation to the space and time in which we are immersed; and, even more important, we have him to thank for tools which we may use in attempting an explanation of this little-understood capacity of the unconscious. Perhaps we shall find that the effective drive to paranormal processes—the real orientation to the task which makes paranormal processes possible—stems from some deep personal concern with a means of breaking through the "prison walls" of immediate space and time, and that the ability to succeed in relation to *specific* material (while failing with other material) arises from

the immediate or the symbolic value of the person or thing with which contact must be made. Veridical dreams of events occurring to loved ones would, then, be based on two important factors: a need to make contact, and freedom from conscious interference.

Volition and concentration would thus prove to be utterly irrelevant except in so far as experimenters, psychoanalysts, or educators can guide the individual to voluntary use of his conscious energies in such fashion as to *set free,* and, perhaps in some slight measure, to stimulate the deeper wishes for paranormal contact which lie in the depths of the personality.

This hypothesis, incapable of being verified at present, may none the less be of some immediate use in research since it focusses upon three needs: (1) The need to study the ways in which conscious concentration acts upon deeper processes, (2) the need to study the *general* unconscious attitudes of the experimental subject, and (3) the need to understand the *specific* unconscious attitudes which account for the short-term variations of scoring ability in one experimental subject.

REFERENCES

1. Rhine, J. B., *Extra-Sensory Perception,* B.S.P.R., 1934.
2. Besterman, T., An Experiment in "Clairvoyance" with M. Stefan Ossowiecki, *Proc.* S.R.R., Vol. XLI (1932-33), pp. 345-351.
3. Riess, B. F., A Case of High Scores in Card Guessing at a Distance, *Journal of Parapsychology,* Vol. I, 1937, pp. 260-263.
4. Stuart, C., The Effect of Rate of Movement in Card-matching Tests of ESP, *Journal of Parapsychology,* Vol. II, 1938, pp. 171-183.
5. Stuart, C., and Smith, B., A Second Study of the Effect of Tempo Rates of Matching, *Journal of Parapsychology,* Vol. VI, 1942, pp. 220-231.
6. Rhine, L. E., Some Stimulus Variations in Extra-sensory Perception with Child Subjects, *Journal of Parapsychology,* Vol. I, 1937, pp. 102-113.
7. Warcollier, R., *Experimental Telepathy,* B.S.P.R., 1938.
8. Brugmans, H. J., *L'État Actual des Recherches Psychiques* (2nd International Congress), 1923, pp. 95-125.
9. Estabrooks, G. H., A Contribution to Experimental Telepathy, *Bulletin V,* B.S.P.R., 1927.
10. Ruger, H. A., The Psychology of Efficiency, *Archives of Psychology,* 1915.
11. Leuba, C., The Use of Hypnosis for Controlling Variables in Psychological Experiments, *Journal of Abnormal and Social Psychology,* Vol. 36, 1942, pp. 271-274.
12. Prince, W. F., The Sinclair Experiments Demonstrating Telepathy, *Bulletin XVI,* B.S.P.R., 1932.

The Subject Theory

A New View of the Mind

GEOFFREY ASHE

Author's Note: The following article outlines a theory of psychophysical relations which I believe to contain some novel elements. It seems to offer hints at the solutions of several psychological problems, notably that of extra-sensory perception. The explanation of ESP suggested here is provisional and admittedly not complete, yet it appears to me to go further than any previous hypothesis of which I have heard.

Introduction

Materialistic psychology, as at present taught, takes very much the same stand as Herbert Spencer took. All reality, including *Mind,* is said to be the product of Matter, Motion, and Energy in various forms. What we call mental processes result from electro-chemical action in the sense organs and the nervous system (which includes the brain).

Now at first sight this seems plausible. Our scientific instruments, which are made to deal with Matter, Motion, and Energy, can give us pretty thorough accounts of the world around us, and where they fail it is only because they are not sensitive or accurate enough. We do not find anything that cannot be measured by instruments and described, if not accounted for, in terms of the three above-mentioned factors. And the Behaviorist's argument may be summarized thus: "If we make a fair scientific examination of the human mechanism, we do not find any *mind* or *soul.* We do find a system of physical organs, in terms of which it is possible to give a full account of the so-called mind's workings. And, I suggest, it is plain common sense to be content with this and not look for anything else."

But now let us consider a mental phenomenon. Here is a man seeing a red-colored object. We are given theoretically-perfect scientific instruments. Can we detect inside the man *everything* that goes to the process of seeing the red object?

Well, we can detect a chemical action in the retina of his eye; an electrical impulse in his optic nerve; a series of similar impulses in his brain. That is all. Now we read out the data to a blind man. According to the materialist, we have given a complete account of the mental act of seeing a red object. But can the blind man now picture what "red" is like? No; we have told him nothing. Obviously something has escaped our instruments. What is it? It is the *redness* itself—the subjective, conscious part of the business. And our instruments, which are geared to Matter, Motion, and Energy only, never can get at this. They can never even begin to get at it. The Behaviorist argument quoted above recoils on itself. No "mind" can be detected by our instruments; but we must for that very reason, since conscious phenomena are undeniable,* postulate an Unknown Something to account for such phenomena, a Something in its nature non-physical and not detectable by instruments. We have got to bring in something besides Matter, Motion, and Energy—Soul-stuff, if you like; but I personally prefer the name Subject, as being free from confusing associations. The three ultimates of physical science are everywhere, interacting and passing into each other; hence it is reasonable to suppose that this fourth ultimate is also everywhere (and incidentally in our own brains), and that the physical interactions of Matter, Motion, and Energy in the neuro-muscular system involve also changes in the distribution or form of Subject there, these changes being conscious phenomena.

What I have sought to bring out in this discussion is that Subject is not a mere far-fetched hypothesis, but a virtually necessary assumption. It may be only a descriptive fiction, as some philosophers regard, for instance, the "force" of gravity; but *some such fiction is necessary.* I shall endeavor in a later section of this article to adduce direct evidence from modern physics.

*I have avoided the term "consciousness." Professors Watson and Holt objected to it—justifiably, I think—on the ground that "consciousness" is not an entity in its own right, and that the term, considered apart from conscious phenomena, is meaningless.

The Nature of Events

The main implication of this theory is that every event is basically a redistribution of Matter, Motion, Energy, *and* Subject. Leaving Subject out for the moment, we know that these ultimate factors are, in the nature of things, inseparably linked: a particular change in one, in any system, induces and implies the corresponding changes in the others. This point is important, as we shall see. For the present we must note the corollary that every event whatsoever has both a physical and a psychic aspect—that even molecular motions have what we may call a consciousness value. "Individual" consciousness—which is the fullest, strongest, most vivid—results from the *interlocking* and *piling up* of an immense mass of similar (neuro-muscular) events. The human organism is a centralized system, the most complex in nature; it focuses, as it were, these particular conscious events to such an extent that others are normally crowded out. (Note that dreams, which are said to result from the functioning of *disconnected* chains of neurones, have somehow a much less intense consciousness value than waking experience.)

Extra-Sensory Phenomena

As I said a moment ago, all events, including molecular motions, must involve Subject just as they involve Matter, Motion, and Energy—must, in fact, have a psychic aspect and a consciousness value. We do not participate in the alien consciousness in our environment because our own highly focused and concentrated neural consciousness overwhelms it. (Similarly, "I" have no recollection of consciousness in my own babyhood; my nervous system was not then organized, impressions were not sufficiently piled up, and the feeble and chaotic "consciousness" within the organism was totally alien to the only thing which I now apprehend as consciousness.)

But let us consider a person who is able somehow to set himself in tune with the environmental consciousness—or

who is always in tune with it. His awareness-zone is somehow extended beyond himself. He will not be *conscious* of environmental events, any more than the average person is capable of remembering the greater part of his dreams —the environmental impressions will be submerged. Yet they will reach him, and give rise to corresponding neural activity. Here we find neural activity reflecting Subject activity, not the usual way about; but this is in order (see the second sentence of the previous section).

(*How* these impressions reach him is the real difficulty in the question. Of course the relationship of consciousness to space is very problematical—how large, e.g., is a conscious event? What shape is it? We might suppose that space relations are not essentially involved, we might drag in the Fourth Dimension, or we might work out a process of transmission through the surrounding Subject.)

Now our hypothetical individual keeps receiving these subconscious impressions all the time. And when he repeatedly perceives something both ways at once—in the ordinary way by means of his senses and subliminally by Subject-impression—a conditioned response is built up, as in the normal learning process. For instance, every time he sees an ESP card with a circle on it, as used in the Duke University experiments, he receives at the same time a Subject-impression from the molecular dispositions in the card; and the repeated concomitance of these experiences, the conscious "sensory" and the subliminal "direct," persisting in different environments, will result in their unconscious association. And the experience of life will, in the sensitive individual, teach the distinctive Subject-qualities of all common shapes, actions and so on, just as in the ordinary way it teaches the visual distinctions.

The upshot of it all is that the right individual under the right circumstances—set in tune, no doubt, by thinking along the right lines and avoiding concentration on ordinary conscious events—will be able to respond to the Subject-impression alone. As this has never been con-

sciously experienced, he will not be aware of the impression itself; but the conditioned reflex will cause the impression, or rather the corresponding neural activity, to arouse the corresponding *sensory* image, which is conscious. The circle card will induce a subliminal Subject-event, which will be reflected in consciousness by the visual image of the card. Hence ESP.

It is fair to suggest that in the individual capable of ESP the development of the normal attending mechanisms is accompanied by an unconscious development of the subliminal attending mechanisms (whatever these may be), so that, without knowing how or why or even being aware of the process, he is able to focus his extra-sensory perceptive powers on a particular object or field; he subliminally searches his environment for the pack of cards, and then narrows down the field to the particular card. This is a purely reflex process, and there is no conscious localization. It would be the experimenter's part to find what areas of the brain are involved.

In the ESP experiments conducted at Duke University, two facts were discovered which seemed to confound every theory. First, the position of the object to be perceived—normally an ESP card—seemed to make no difference; the image in the subject's mind was always the same. Second, there seemed to be no clearly-defined space factor; one subject might be very successful at a range of a few feet—and equally so at twice the distance; another might succeed just as well at several hundred yards; another had best results at 250 miles. The "radiation" or brain-wave theory of ESP is at a loss to account for these facts.

Now the Subject theory seems to overcome these difficulties. Given the power of "environmental" consciousness, the rest is easily explicable. There is no question of the position of the card relative to the experimental subject; the card is a psychic phenomenon, and he is not aware of its position or the angle at which it is held, any more than we are aware which way up our neurones are.

As to the second point, the fact that Subject is extra-physical means that in this field ordinary natural laws, and especially the Law of the Inverse Square, need not be supposed to operate. So long as it is perceptible at all, a distant Subject-event may not be "weaker" to the extra-sensory percipient than a close one. And when the laws of environmental consciousness are worked out, it may well be we shall discover how it is that one subject's best distance is, say, twenty feet and another's 250 miles. At any rate we need not be bound by the conceptions of physics.

Second-sight and intuitions experienced at great distances can further be explained by supposing (as appears usually to be the case) that the distant event has some special interest for the individual, so that the subjective aspect of it is singled out from the mass of impressions, with a focusing of subliminal attention following. In the same way a phrase of special interest may catch our eyes when we glance down a printed page.

Telepathy is simply participation in the thoughts of another—that is, in the Subject-impressions of his neural processes—and is a natural result of the power of environmental consciousness. The problem here and elsewhere is how a *distant* Subject-event can have a neural counterpart within the individual himself, which is, I think, implicit in the power to respond to such an event. I refer to the hypotheses stated above: (1) that space relations are simply not involved, or are involved in an entirely non-physical way (which is a radical idea, but not unreasonable, for conscious events *qua* conscious events have no attaching notions of size, shape, or position, and indeed transcend all these); (2) that the Fourth Dimension is involved; or (3) that there is some kind of transmission, analogous to radio, through the Subject which is everywhere.

A Philosophical Approach

Is Subject pure conjecture, it may be asked; or can we find any evidence for its existence in the world of physics?

Let us attempt an indirect approach.

In the first place, the physical world, we know, can be divided and subdivided by the analytic processes of scientific rationalism. As a result of this, it appears that things "as a whole" or "in themselves" are really only ideas, conceptions of the mind; an object or an action can be analyzed into nothing. Thus (to borrow an illustration from the Buddhist scriptures), if we regard a chariot as wheels plus pole plus seat—and so on—the "chariot" disappears; it is a name only. Matter in general is an idea; a material object is a collection of properties—of potentialities of producing events, hence, ultimately, of events—and the "material object" is an idea representing the *organization* of these.

Has the thing-as-a-whole, then, any real extra-mental existence? It would appear that this may be tantamount to saying, Has the *idea* (or something analogous to it) any existence outside the observer's mind? The Subject theory suggests that it may; for the system of events constituting the "thing" has its Subject-reflection, its consciousness value. The conscious phenomena in our own minds are, after all, unified reflections of whole systems of neural events, integrated in some obscure way by Subject.

However, there is a physical factor which must deeply affect our conception of this matter. It is *entropy*. "Things," as I just remarked, are organized systems; but modern physics demonstrates that any organized system involves a measure of *dis*order, of unpredictability, of anarchy. This is its entropy. This curious factor is discussed in Eddington's work *The Nature of the Physical World* (1). He declares that entropy does not fall into any of the "compartments of physics," that the entropy of a system cannot be analyzed or dissected, that the entropy, in fact, seems to represent the whole, and involve an "appreciation of arrangement" (pp. 103-107). He suggests that it is subjective in quality (p. 95), and associates it with beauty and melody rather than with physical phenomena. He indicates that if the thing-as-a-whole has an external reality, we

should seek the unifying factor in its entropy. The passages cited should of course be read in full, but the drift of them seems to show pretty clearly that in this mysterious factor of disorder—by its nature irrational, non-mechanistic, and antithetical to Matter, which is essentially organization—we have in all probability run Subject to earth, or at least found the physical world-element with which to associate it.

As if to confirm this conclusion, Eddington declares in the work quoted that the "direction of Time's arrow" is indicated, or apprehended, by just two things. One is consciousness. The other is entropy. It appears to be the most inexorable law of nature that the amount of entropy in the universe is constantly increasing. Eddington suggests that our time sense is supplied by "entropy clocks" somewhere in the nervous system (pp. 100-101). Thus we find consciousness and entropy intimately associated.

I am not suggesting that Subject *is* entropy exactly. Rather I am pointing out that physical analysis leads, just as our psychic analysis led, to the discovery of an irrational Something, and that the attributes of this irrational Something turn out to be curiously similar whichever approach we adopt.

The ultimate implication of the Subject theory is that the whole universe has a physical aspect and a psychical aspect. It is in the physical sphere that causation operates, and as a rule the psychical seems to reflect the physical; but the reverse process is also possible, indeed I have assumed in this discussion that it does occur. It offers, where necessary, a means of escape from rigid mechanism and the inviolable conservation of energy. As an instance of further possibilities, into which I cannot here enter, I would point out how the theory clears up the age-old problem of volition by allowing for neural processes arising from non-mechanistic and extra-physical causes—that is, reflecting Subject-events, and hence uncaused physically.

If the Subject theory is at all valid, it opens the way for the unified science of the future, in which the dualism of

Mind and Matter will be resolved and psychology united with physics: every phenomenon will be seen as a single event susceptible of interpretation in either physical or psychical terms—in terms of Matter or in terms of Subject. Just as the study of physical processes has thrown light on the workings of the mind, so an expanded psychology and an expanded physics will illuminate each other.

Reference

1. Eddington, Arthur, *The Nature of the Physical World,* Cambridge (England) University Press, 1928.

Some Unusual Experiences

S. COX*

Although the modern practice of applying scientific methods of investigation to paranormal phenomena is a development of the utmost importance, and one that has already yielded valuable results, it has at the same time the unfortunate effect of abstracting attention from certain types of phenomena that do not lend themselves to objective examination. Because spontaneous personal experiences of clairvoyance, telepathy, unusual states of consciousness, and so forth, are almost invariably impossible either to corroborate or assess, it does not follow that they are therefore lacking in interest or significance. Indeed, if we can trust reports of them at all, such experiences should contain much of *general* interest to anyone attempting to form a rough picture of the mind's obscure potentialities.

Accordingly, readers of the JOURNAL may be interested in the following collection of odd personal experiences. They have provided me, at least, with considerable food for thought, and perhaps their appearance here may have the effect of encouraging others to come forward with comparable personal material that they might otherwise hesitate to offer.

Of some interest, possibly, is the manner in which one's attitude to paranormal phenomena appears to change. Until I was about twenty-four I was more of a materialist than later, with little patience for many of the subjects that now interest me considerably. How the change actually started I can hardly say; but I think that this change, when it occurs, generally does so in a characteristic way. To begin with, a mild interest and credence in paranormal phenomena comes to life. Once the interest has appeared, it seems that

*Pseudonym. The author of this paper is well known to the Society through his work in the field of ESP research. For personal reasons, however, he prefers not to use his real name in connection with this account of some of his unusual experiences. *Ed.*

an actual personal experience of some kind may be met with. That, of course, heightens the interest—with the result that further experiences may be encountered; and so on.

In the interests of accuracy I should say that I probably had a few slight paranormal experiences long before any intellectual curiosity in such matters existed at all. Later, of course, when I began thinking about such things, these two or three early experiences appeared in a new light. I remember, for example, one peculiar occurrence. When I was about eighteen, a friend and I often walked down a certain mud path to some tennis courts. It was a path I had walked along literally scores of times. At one place in it was a large rock, around which the path branched. One morning as we passed this rock, for want of something to say I remarked, "Someone should take that rock out of there."

I think I had never *consciously* noticed the rock before, although I had occasionally walked past it for at least five years. Certainly I had never mentioned it before. Yet the next morning, as we went to the tennis courts, two city laborers were digging beside the rock; they dug a large hole beside it and dumped it in. It was a little-used path, not a public thoroughfare, and it mystifies me now that the city authorities should have taken that momentary interest in it.

My friend, it should be added, must be absolved of all suspicion of intervening. He was not at all impressed, and I had to remind him that I had mentioned the rock the day before. He was a serious boy who would not have descended to trickery or practical joking.

Another such trivial incident: For many years a certain old woman vended newspapers on a downtown corner. She was noticeable for her raucous voice, with which she used to call, "Hair-*ald!* Hair-*ald*!" (Herald! Herald!) One day at home, when I had not been downtown for several days, her chant suddenly came to my mind, and I thought, "I wonder if that woman is still there; I certainly haven't noticed her for months." The next day when the *Herald* came out it carried an account of her death. I did not see

the item; it was read out to me by a friend to whom I had voiced the question in my mind the day before.

One of the first books I read on psychic matters was J. W. Dunne's *An Experiment With Time.* In the cases he gives it is to be noticed that when dreams contain details properly belonging to the future, such details are most often encountered in actuality not later than two or three days afterwards. This I have found to be very true indeed in my own experience. Another contention of Dunne's—that the generality of people, if they would go to the trouble of recording their dreams at the very moment of awakening, would find occasional clairvoyant aspects—I think is also largely true. Among a limited acquaintance I know six people whose dreams from time to time appear to be definitely clairvoyant. And those whose interest in such dreams is once aroused generally find enough convincing examples within a few years to make the phenomena appear quite commonplace.

One of the first odd dreams that I had occurred in B——. I was living alone in a small house on a hill in M—— Canyon. The event of the day was the arrival of the postman, who came up the hill every morning in an old car, the sound of which I soon grew to recognize. One morning I was lying on the bed, drowsy, when I heard him come. I was very much tempted to get up at once and see what was in the mail box, but instead dropped off to sleep.

My sleep consciousness was particularly lucid, and I dreamed that I went out to the mail box and looked in it. I saw its contents with utmost clarity. There were three envelopes—one large and squarish, one long and blue, and one ordinary. When I awakened, after about half an hour, I *knew* that I knew what was in the box. I went out to it and found all three envelopes just as I had seen them before, in the dream. Even the order in which they lay was exactly the same. I had no normal reason to expect the arrival of these letters.

Another dream that might be interpreted as clairvoyant was as follows: I dreamed that M—— (a friend of my

sister's) and I were being driven by a chauffeur up Tenth Avenue. We approached Dakin Street. I said, "Shall we turn right or left on Dakin?" (The choice was a matter of some importance to me.)

M—— did not answer me at all, thereby implying that I was to make the decision. So as we came to Dakin I said to the chauffeur, "Turn right," and he did.

I then awoke, about 11:00 A.M. It was a Sunday, and I dressed and went to W——'s for dinner, very unexpectedly meeting M—— there.

I said, "Where were you this morning about 10:45?"
She said, "I was driving with R——."
I: "Did you drive up Tenth?"
She: "Yes."
I: "And turn right at Dakin?"
She: "Yes, we did."

M—— lived at that time about four miles from Tenth and Dakin. I had no reason for thinking that she might have business in that neighborhood. She was not a friend of mine, but of my sister's. I had not seen her for at least a month or two. The city where we both lived, I might add, had a population of several hundred thousand, and consequently an extensive street system.

These and a few other earlier dreams were useful in familiarizing me with the *sensation* of dream clairvoyance. I discovered that when a dream was later to prove itself to some degree true, the dream consciousness was particularly lucid and intense. This fact has a bearing on some more interesting dreams that occurred later, but before relating them I shall mention a few odd experiences that do not fall into quite the same category.

On two occasions events in my own waking life appear to have had a certain impact on the dream consciousness of others. After working for a certain firm about two years, I was promoted. The manager called me into his office and told me of the change. For various reasons of my own, I did not tell anyone. The next morning, between 9:00 and

10:00 A.M., C—— appeared in my office. He is a brusque, rather impulsive man, an acquaintance rather than a close friend, and I had not seen him for at least two months.

He said at once, "Did you get a promotion lately? I dreamed last night that you did, and the dream was so vivid I could hardly disbelieve it."

While living in the B—— house I have mentioned, I received a letter from a friend in another city. One sentence read, "I dreamed of you the other night; you were very much annoyed; it had something to do with gardening."

At first I dismissed the remark almost at once for various good reasons. I am seldom annoyed, and could recall no recent occasion when I had been. Furthermore, I am no gardener and can scarcely be induced to touch a rake or hoe. On top of that, there was no garden; the house was new, and was surrounded by baked brown mud. Later, however, the remark in the letter occurred to me again, and I suddenly connected it with a rather unusual incident.

The floor of the house was on ground level, and I was accustomed to write with my back to a certain window. One afternoon a few days before, a neighbor, almost a stranger to me, had appeared outside the window with a spade. He said, "I've been planting nasturtiums and have a few seeds left over. I'll stick them in along here if you like."

I thanked him and turned to my work again, which was then engrossing me. But the man's presence so very close behind me was disturbing. I knew, for example, that if he raised his eyes he could hardly avoid seeing what I was writing. I wanted to close the window, but that was out of the question. Neither could I, in common courtesy, move my table. I dallied for a few minutes, waiting for him to finish and depart, but he seemed to be tackling the job with some thoroughness. After trying fruitlessly to concentrate again, and a decent interval had passed, I got up briskly as though on business, put on my hat, and went out for a walk.

The incident was more trying than it sounds, and when

I recalled the remark in my friend's letter it seemed to have a certain point.

On two occasions also, I have experienced what, to me, are very definite examples of clairaudience. I was late going to bed one night, alone in the house, and was just getting to that pleasant point when sleep seems likely, when I heard, with perfect distinctness, my name called loudly and intensely, twice. I recognized the voice instantly, and sat up with a start, the sound still ringing in my head. The cry was so intense that the very air in the room seemed to remain alive with it for a second.

Then, as I pulled my wits together, I realized unmistakably a most peculiar thing: the sound had *not* entered consciousness through my ears. How I realized this, and how I could be so certain of it, I do not know. The *effect* was normal in every possible sense; yet somehow I *knew* that my ears had played no part in its reception.

At the time of this experience the owner of the voice was in another city about four hundred miles away (a fact I was well aware of at the time). The next morning I wrote, telling of the incident. In reply I learned that the friend in question, a woman, had been in acute mental distress the night before—so disconcerted, indeed, that, unable to sleep, she had got up, dressed, and left her hotel at about two in the morning to walk through some of the city's deserted streets. She said that during this walk she had been thinking of me, *but had not called my name, even to herself.*

This last aspect of the incident I find intensely puzzling, the more so as I know that, somewhere, I have read of another experience very similar, in which a specific voice was heard to call, while the owner of the voice, although conscious at the time, and in deep trouble, disclaimed all knowledge of the words.

The second clairaudient experience concerns a few bars of music which I heard with perfect distinctness one morning on awakening. It was rather solemn orchestral music, and I was just focusing auditory attention upon it when it

faded into silence in a rather peculiar way, almost as steam from a kettle disappears when it meets the air. This time, whether my ears were playing a part or not I could not tell. The music seemed to be emanating from a certain upper corner of the room. The usual suggested explanations, such as a radio in an adjoining room, I could rule out absolutely.

Let me now attempt to describe a certain type of dream that is really beyond description. There was a period about three years ago when I had the most horrible nightmares, every month or two. I am quite familiar with ordinary nightmares, but these were *different;* I wish I could convey their differentness! They were characterized by a state of consciousness that terrified me beyond expression. When I occasionally mentioned them to one or two intimate friends, I could only describe them as based on a sensation of *uncontrol* that amounted to madness. It was a state of consciousness utterly, utterly removed from all ordinary consciousness, even from the worst nightmare consciousness. In it I was cut off, completely divorced from myself as I knew myself. I was lost in the most terrifying absymal space, and had not even myself to cling to. I was *abandoned* —a rudderless ship on a wild, uncertain sea.

(The fact that from the beginning I could find no other word than "uncontrol" to describe this state of consciousness, later gave me cause for wonder.)

I used to fear these dreams as I feared nothing else, and sometimes was so afraid on waking that I would get up even in the middle of the night rather than risk encountering one again. They were almost invariably followed (and sometimes preceded by) a feeling of pronounced chilliness across the shoulders. I tried to trace this sensation and discovered, oddly enough, that a reduction in cigarette consumption the day before would often induce it. These dreams sometimes contained scraps of material that I would encounter in waking life during the next two or three days.

These nightmare-clairvoyant dreams and simpler clairvoyant dreams used to occur on the average not more than

every month or two. Sometimes several months would pass between them. During one most interesting period of a week, however, I had three such dreams. The reason they came so thick and fast can be attributed, I am pretty sure, to the fact that during that time I was reading the book *Peter Ibbetson* by Du Maurier.

This book has a rather ingenious plot, wherein a man and a woman meet in a dream, carry on a love affair in their common dream, and generally find their dream life more intense and more enjoyable than their waking existence. Their days were dull or painful, merely to be lived through; their real life began in sleep. I was far from being fascinated by the book, but it interested me mildly and I wended my way through it. The period of reading it corresponded very closely to the period during which three unusual dreams occurred in the space of a week.

The first two of these dreams were the simpler. In the first one I dreamed about monkeys—dozens of small ones scampering over the ground. They persisted so long in the dream that I thought about them considerably (in the dream)—recalled my mother's aversion to monkeys, and wondered how it might be accounted for, and so forth.

The next day I was teaching as a substitute in a certain city high school. At noon hour in the auditorium some educational films were shown, which I dropped in to see. Two separate reels pictured monkeys—small ones, dozens of them, running in all directions.

The second dream contained one small clear incident. In it I was talking to someone, I do not know whom, and I said, "Have you read *The Amateur Gentleman?*" Immediately I said this is in the dream, I almost regretted it. I thought, "How unnecessary to bring up such a second-rate book! This person I am talking to may be impressionable, and may read the book simply because I mentioned it—which wouldn't be particularly advisable."

The next day, a Sunday, I went with the M——'s to call on some people who were strangers to me. The host had a

large library, catalogued, and he insisted that Miss M—— and I should examine the catalogue to see if we wanted to borrow any of the books. We went through the typed sheets slowly, making comments about the various authors and texts. We came to Kingsley, and I said, "Have you read *The Water Babies?* I could never finish that book as a child; it creates too eerie an atmosphere."

When I said this I remembered two or three books that had affected me similarly, one being that part in *The Amateur Gentleman* where the hero falls from his horse during a steeplechase and, remounting, finishes the race in a daze. With this train of thought in mind, I said, "Have you read *The Amateur Gentleman*?"

Up to that moment my dream of the night before had been *completely* forgotten; but at the very instant that the words were out of my mouth it flashed up in my memory. I felt quite shocked, and naturally my first thought was, "Could I have avoided pronouncing those six words?"

I should add that *The Amateur Gentleman* was a favorite of my childhood, but that I had not read it or (probably) thought of it for years.

The third dream was more impressive. It was one of the nightmare dreams, and even before I went to bed I experienced the feeling of chilliness across my shoulders—unusually strong and definite. In an attempt to explain it I recalled how many cigarettes I had smoked during the day, but there had been apparently no decrease from the day before. In the dream the terrible sensation of madness and uncontrol descended on me again, and this time there was an added factor.

Previously my state of terror-consciousness had been an inward thing, apparently concealed from observers. But this time there was a physical manifestation of it. My hands twitched continually, the fingers opening and shutting with convulsive movements. This distressed me, the more so as I knew that people about me would now have unmistakable evidence of something seriously wrong with my nervous system.

This part of the dream ended, together with the element of madness, and I found myself on top of a high cliff and faced with the necessity of getting down. But the cliff was of bare gray rock, and there was no way down. However, in my search for a route I happened upon a peculiar ladder-way, made entirely of wire. There was a wire handrail too, and the whole contrivance was neatly made, but looked all too flimsy to bear the weight of a man.

Nevertheless I ventured upon it; but my weight had no sooner rested upon the top step than the whole thing collapsed, or seemed to collapse, beneath me. Actually the wire steps were in the form of narrow stairs, and when I had stepped upon the top one, all of them had slithered downwards, like a Venetian blind closing. The handrail also had slithered downwards, into a plane with the stairs. I was frightened and clung desperately to the thing. My first thought was that it had all become detached at the top, and was falling. But in an instant I was reassured; I knew it was *not* falling because *I* was *swinging*—the ladder, with me upon it, was swinging back and forth.

I made my way downwards cautiously, to the end. But the end was still far from the ground, and I stayed there helplessly, holding tightly in my arms a large, round, metal ball, about the size of a football. Where the ball came from, I do not know; it had nothing to do with the stairs. At this point, this part of the dream ended.

Finally, in the dream, I was walking down the street, and suddenly noticed, on the sidewalk before me, two small severed feet. What sort of creature they had once belonged to, I had no idea. They appeared not repulsive, or even shocking, but merely interesting. There was, I noticed at once, no blood; merely the feet, which were about the size of a man's thumb and waxy red, more like ornamental glass than organic tissue. But the really strange thing about them was that they were *twitching*—twitching and twitching on the sidewalk.

Immediately (still in the dream) I was overcome with interest and amazement. I thought, "How extremely remark-

able! Why, they are the clue to the earlier part of the dream, and my twitching hands, and my madness! How wonderfully clever for someone to have written a story from so small a germ!"

This finishes the third dream, which was very intense from start to finish. I awoke with the feeling of chilliness across the shoulders, and for the first time it persisted somewhat all through the following day. What surprised me most about it was that when I sat down that evening to write out the dream, the sensation of coldness intensified remarkably, and remained unusually strong all the time that I was writing.

At morning recess on the day following the dream, I repaired to the furnace room of the school for a smoke, and while there picked up a copy of *Liberty* belonging to the janitor, and read a story in it. The story was of several young people on a picnic. A disgruntled suitor took a small dog belonging to the heroine to the top of a rock cliff called the Eagle's Nest and left it there. The favored suitor was shown up because he was afraid to climb the cliff and retrieve the dog. Finally the girl herself climbed the cliff, got the dog, and started down; but she got stuck part way down, holding the dog, and the neglected suitor had to rescue her and the dog both.

When I read this story I thought nothing of it, but later it occurred to me that some of the details corresponded to the second part of my dream fairly well—especially as there was with the story a drawing of the cliff, and the girl part way down holding the dog. *I* had descended a cliff part way, and was left in mid air holding on to a round metal ball.

The second day after the dream, during lunch hour at the school, one of the teachers displayed a contrivance that had been made by a Japanese boy in her room. It consisted of a number of flat pieces of wood, about the size of playing cards, fastened together in a row with tape. When one held this chain of small boards by the top piece, and turned the latter over slowly, the whole series of pieces went through

a mysterious cascading motion, almost faster than the eye could follow. I was entranced by the toy at once, examined it attentively, and played with it for some time.

Suddenly it occurred to me that this might be the counterpart of my collapsing wire ladder. So the next time I tripped the mechanism I held it with absolute steadiness to see if the cascading motion also imparted to it a *swinging* motion, such as I had experienced upon the wire ladder. It did. No matter how steadily I held it, when the boards had finished cascading the whole thing was left swinging back and forth.

I began to get somewhat interested in the accumulating evidence; I preserved the story from *Liberty,* and got the Japanese boy to make me one of his contrivances for my own. I thought, "If I find a couple of severed feet on the sidewalk, the dream is going to appear interesting." I encountered nothing further, however, the second day.

The third day was also unproductive of anything interesting—until after school. I was just getting ready to go home when two small girls came into the room. They came in with mischievous expressions, and the one with her hands behind her presented her surprise offering. It was an enormous turkey's foot! As may well be imagined, I was really surprised, but my surprise turned to amazement when the child pulled the tendons in the leg, making the foot twitch convulsively.

Now I must relate a most peculiar dream. I have the record of it, made when I woke up September 18, 1940. In the dream I was walking along a roadway above a beach with several other people. Suddenly I realized, to my consternation, that I was in uniform, a light, unusual, murky blue uniform that fitted badly, especially at the back of the collar. The collar came too high, and cut against my neck.

Let me quote from my original record, made immediately on waking:

> I thought, "Confound this uniform!" (It scarcely seemed to be me —I was taller, straighter, and seemed a quite different personality

from me, simpler. I am vaguely aware now that my face was different, neater, blonder, really another person's face.)

I thought, "Confound this uniform!" And then suddenly I thought, "How on earth did I ever get into it? Did I really join up?" It seemed incredible. Such an important change, too. Now in "the service"—some service. But when did I decide? When? When? Couldn't remember *at all* taking the step.

I was full of consternation. It was obvious that, being in uniform, I must belong somewhere—no doubt I should report somewhere at a certain hour. But I had no slightest clue as to the hour or the place. Those with me were in uniform also, but of a different style, and I knew that no information was to be gleaned from them.

Then there was some action, not essential to the main part of the dream, but I shall include it. I quote again from my original record:

Two young men with me, very close and friendly to each other, but not naturally so. Close because they shared some common interest. The cliff to our left went up to the S—— M—— Mine. One said, "I wish I could get up there. One day I will."

I, rather scornfully, "What would you do if you did get up?"

He: "What do you think?"

I (epigrammatically): "Get ore, I suppose."

He looked at me sharply and curiously, and I sensed that he knew of a secret and valuable vein, very valuable. He guessed my sudden realization and made a quick move to kill me. I got up on a rock and lifted with difficulty a square box, and half-dropped, half-threw it at him. It caught him, but not squarely. Flattened him. His friend, in the secret, bent over him and, I guessed, discovered he was dead. Friend was raging. I ran for my life. End of this.

Then followed a sort of passive dream period during which, suddenly, my perception widened. I became for a fleeting instant *all-aware.* I saw deep; *knew* deep. I was suddenly aware of something overpoweringly impressive—of a vast, intense, crystal-clear *reality.* Everything else in memory and experience was dull, clouded, and on a much smaller scale. Before me appeared with startling clearness the blue, *real* void—a void I instantly knew I was part of, and lived in, but *never remembered;* a reality perfectly familiar, but one that I had never, never been *conscious* of before.

And then, while part of this tremendous reality for an instant, I was suddenly aware of this: that there was *another person* who was trying to be me, a person who wanted to be in a blue uniform, and who got into the uniform *through* me. This shocked and frightened me beyond all measure, especially as I knew it to be true. And I looked straight ahead into the crystal-like silvery blueness, and there, in the very center of it, was a tiny black adult figure—as a man would appear at a distance of two or three blocks.

Seeing this figure I knew at once that it was my brother—a brother who had died at birth when I was a child. And merely, as it were, by looking closer—an effortless attention that did not involve conscious "will" at all—I saw the figure as though it were close by, and noticed the shape of the head, and the features, and the coloring, and was amazed at the subtle similarity to myself. It was even obvious from his appearance that he was several years younger than I, as indeed he would have been. Also I noticed that he was somewhat heavier, especially across the shoulders.

I awoke then, full of utter amazement; and at the very moment of awaking found in my mind thoughts of the Biblical "possessed of an evil spirit," and Christ casting the spirits into swine. And I thought, "That's all absolutely true; and what is more, it's perfectly *natural!*"

So ends this dream, which I am merely relating, without any comment whatsoever. I should add that there was a brother, who died at birth or before. I never knew of this event until about ten years ago, and then heard it mentioned only once. I have never heard it referred to by anyone since, and while it crossed my mind occasionally afterwards, it was an event that scarcely touched me, and I have never dwelt on it for more than a few minutes at any time.

Such dreams as these lose everything in the telling, for it is quite impossible to describe the unearthly *certainty* that accompanies them. The mind seems cut loose from every mundane connection. A new and greater reality, frightening in its intensity, floods in upon it; and *perception* is heightened beyond all recognition.

Correspondence

July 10, 1942

To the Editor of the JOURNAL

Dear Madam:

I have been most interested to read Dr. Hettinger's article, "Exploring the Ultra-Perceptive Faculty," in the July, 1942, issue of the JOURNAL. This article has led me to formulate some opinions concerning the problem of statistical method in the field of psychical research. In writing this letter, I by no means wish to imply that my views are right and that Dr. Hettinger's are in error; as he so correctly says, "General and special education, desires and wishful thinking, ambition and vested interests—all these must be held responsible for creating the differences between us in regard to our approach to the problems of psychical research and its various branches." Undeniably these differences do exist and it may be of some interest to examine them more closely.

There are, in the main, two general approaches to the study of the phenomena which fall into the province of psychical research. On the one hand there are numerous records of spontaneous cases filling the special literature of our subject. These cases yield data which might be characterized as immediate and sporadic. They are immediate inasmuch as they are gathered without benefit of special experimental apparatus or preparation. They are sporadic inasmuch as they are gathered at irregular intervals rather than at the behest of an experimenter who is testing the effect of some experimental variation; these spontaneous experiences come to the individual at odd times, unpredictably, perhaps only once in a lifetime in a moment of great emotional stress.

The alternative approach might be said to yield data which are mediate and controlled. They are mediate inasmuch as the experimenter places limitations on their nature before the actual experiment starts. Thus, the subject must answer in terms of symbols or drawings, and so forth.

There is usually a more or less elaborate experimental set-up connected with the securing of this type of data, a set-up in the main constructed to insure the validity of the data secured. This method is also a controlled method. The subject works according to a definite plan formulated by the experimenter; he makes a certain number of runs, at such and such a time, with certain pre-arranged experimental variables, and so on.

It will be noticed that the question of statistical method in relation to either of these general methods has not yet been raised. This will be done, but another question can be touched upon more logically first. This concerns the relative value of these two general methods—the first *uncontrolled,* in the sense that it deals with spontaneous occurrences which are not subject to the whims of any experimenter, and the second *controlled,* that is, subject to experimental conditions and variations of one type or another.

Both of these approaches would seem to have their place. It may be that the striking successes recorded in the literature of spontaneous cases are due to the very fact that these occurrences *are* uncontrolled. Another feature of these cases, one which has been emphasized by many investigators, is their occurrence in moments that are accompanied by great emotional stress. Thus, many of the published cases concern the psychic apprehension of the death of a loved one. But an emotional atmosphere is markedly difficult to obtain in an experimental situation. It is, of course, possible to vary the situation in such a way that the subject becomes angry or excited, but to impose conditions which might be expected to arouse genuine grief is next to impossible.

Many objections have been offered to the validity of the evidence which seems to be afforded by spontaneous cases. Some of the criticism, of course, bears on the fact that frequently the evidence is dependent only upon the memory of the individuals concerned. We need not dwell on this type

of criticism since each case must stand on its own merits. However, there is a general type of criticism of these cases which is more relevant to this discussion. This general criticism is often presented in two aspects:

1. The individual concerned probably has had many subjective experiences of "something happening" which, on investigation, did not hold up. There is a tendency to forget such unconfirmed experiences; it is only when the experience coincides with an actual "happening" that attention is called to it and memory retains it. In other words, out of thousands of hunches or premonitions, only a few appear to be correct. These are just the instances that are publicized, while all the others are overlooked. But by force of chance coincidence alone a few of these premonitions should coincide with events that come to pass. Thus, these cases cannot be considered as "evidential."

There are several positions from which one can view this type of criticism. Many will prefer to accept it as the true explanation of *all* spontaneous cases. Other individuals take a position which is diametrically opposed to this. For them, it is only necessary to label an occurrence as "spontaneous" and it becomes acceptable as evidence of the highest type. A third view is concerned with the necessity of taking into consideration the question of the number of details in the description which coincide with the actual event, and how startling the details are. In other words, the reliability of the case increases with the number and unusualness of the coincidences between the experience and the event in the objective world to which the experience is supposed to refer. Statistical concepts are implicit in such a view as this.

2. The second aspect of the general criticism is concerned with the point made above. Suppose, for example, I dream that the British are bombing Cologne. In the morning I learn from my newspaper that just such a bombing took place. How much importance should be attributed to this coincidence? Very little, probably, since the British frequently bomb Cologne. Suppose, however, that on a hot

July night in New York I dream that snow is falling in Los Angeles and in the morning my paper brings me the information that there was actually such a storm in Los Angeles. Is such a dream any more significant than our dream of the bombing of Cologne? Based on our knowledge that snowstorms in July in Los Angeles are very rare, we might with considerable cogency answer in the affirmative. Then by checking with the weather bureau we could determine just what the chances were that such a storm would occur. Unfortunately, however, this does not quite solve the problem: it may be my custom to dream of snowstorms in Los Angeles without ever remembering them until the actual storm occurs to facilitate my recall.

Such possibilities as these make a precise evaluation of spontaneous cases exceedingly difficult. Except in unusual circumstances it is impossible to apply explicit statistical methods to such cases. The assertion can, of course, be made that in view of the accuracy of the experience in conforming with the actual happening it is unlikely that chance coincidence can account for the similarity. We cannot, however, make a more precise statement than just that.

The second method of approach to psychical phenomena, as has been pointed out, is the controlled method. By the use of such a method the experimenter makes sure that he knows the conditions under which his results are obtained. That is not to say, of course, that *all* the relevant experimental conditions are clear to the experimenter, or understood by him. Obviously, in spite of his best endeavors, he cannot be sure just how his subjects are reacting to the restrictions of the experimental situation. There are, however, certain advantages in the controlled experimental method which counterbalance the limitations which it undeniably imposes. Thus, the experimenter no longer needs to depend on the memory or the integrity of the subject before deciding upon the reliability of the results obtained since all the judgments of the subject are explicit. He need not be concerned with the possibility, as in the spontaneous cases, that many of the judgments made implicitly by the subject were discarded

by him. In addition, since the experimenter customarily limits the nature of the subject's responses by describing the terms in which they are to be made, he makes possible a more accurate and precise evaluation of the results that the subject obtains.

To this point there is no disagreement with the viewpoint that Dr. Hettinger expresses. I feel, however, that Dr. Hettinger implies or makes directly certain points which can be contested. These points may be enumerated as follows:

1. That the statistical method is *per se* a method for the investigation of psychical phenomena. (The position to be argued here is that the statistical method is a tool which may or may not be employed by an experimenter to evaluate his data.)

2. That more knowledge can be obtained about the *real nature* of psychical phenomena if investigators abandon statistics in the evaluation of their results.

3. As a corollary to the above point, that certainty in regard to a *given* coincidence in a series being the result of a psychic display can be more readily arrived at if a method of evaluation which does not employ statistics is used.

In regard to the first point, it is clear that statistical method is not a "method of investigation" of psychical phenomena. One does not actually "investigate" with statistics. The statistical method is simply a tool for the evaluation of data and it can be used constructively if the experimenter has controlled his method sufficiently. It enables him to say with some assurance that the probability of a given series of events is such and such. Thus, it substitutes for the relatively unprecise evaluation in the uncontrolled spontaneous cases a precise means of evaluation. On the other hand, there may be certain situations in which the use of statistical evaluations would not add much to our certainty. For instance, if an investigator should find an individual who could habitually call correctly twenty out of twenty-five ESP cards intricate statistical analyses of the

data would not add much. Or, if one makes use of the drawing method and finds a subject who invariably reproduces the target drawing correctly, an elaborate matching procedure would no doubt be superfluous. But where success is relatively limited, as is the case in most investigations, the use of statistics to evaluate the results certainly serves a constructive purpose and adds to the authority of the investigation.

I suspect that certain of Dr. Hettinger's statements arise from a belief that statistics cannot be used to real advantage in the evaluation of so-called free stimulus material. When ESP or other card material is used there is a definite probability for each judgment being correct. This is not so obvious when, for instance, drawings are used. I should like to point out, however, that even when this type of material is used, Carington, Pratt, Stuart, and possibly others, have worked out statistical methods for the evaluation of the results obtained. Where this is done the experimenter is enabled to give a precise statement as to the probabilities of the results obtained being due to the operation of chance. If statistics are not used, the experimenter is free to conclude that it is *unlikely* his results are due to chance, but *how* unlikely he is entirely unable to say.

In regard to the second point, I should like to point out that many articles emanating from ESP laboratories are primarily concerned with arriving at conclusions concerning the essential nature of the phenomena under discussion, and are only incidentally concerned with the further establishing of their reality. In any experiment in which conditions are systematically varied it would seem that the experimenter is interested in determining the nature of the phenomena. The fact that he uses statistics to tell him *how great* his differences are adequately illustrates this point. By use of the statistical method he simply makes more certain that he does not overestimate whatever differences he may obtain. Again, to use statistics is simply to use a tool for the purpose of precisely evaluating the results obtained by any controlled method of investigation.

Finally, we must consider Dr. Hettinger's last implication. It is true that statistical method will not tell us which correct judgment is due to a display of psychic ability and which is due to pure chance, but *without* the use of statistics we cannot arrive at any greater certainty in regard to this very real problem. If the material used is of such a nature that chance coincidence seems unlikely, we can say just this, but no more. The difference lies in the fact that by the use of statistical method we can know *just how likely* it is that a given correct judgment is due to chance, whereas without the use of statistics we are unable to arrive at this precise assurance.

JOSEPH L. WOODRUFF
Hodgson Fellow in Psychical Research.
(Now a member of the United States Armed Forces—Ed.)

September 30, 1942

To the Editor of the JOURNAL

Dear Madam:

I am much obliged for the opportunity you have afforded me to reply to Dr. Woodruff's criticisms on my article, "Exploring the Ultra-Perceptive Faculty," published in the July, 1942, issue of the JOURNAL.

The opinions on the two general approaches to the study of psychical phenomena set out in the first portion of Dr. Woodruff's letter call for no comment; I fully concur in his most clearly expressed views. But, as I am anxious not to be misunderstood as to what I am aiming at with my contributions to and views on psychical research, I will attempt to explain my standpoint more clearly in the light of the criticisms raised.

Dr. Woodruff summarizes his comments as follows:

"To this point there is no disagreement with the viewpoint that Dr. Hettinger expresses. I feel, however, that Dr. Hettinger implies or

makes directly certain points which can be contested. These points may be enumerated as follows:

1. "That the statistical method is *per se* a method for the investigation of psychical phenomena. (The position to be argued here is that the statistical method is a tool which may or may not be employed by an experimenter to evaluate his data.)

2. "That more knowledge can be obtained about the *real nature* of psychical phenomena if investigators abandon statistics in the evaluation of their results.

3. "As a corollary to the above point, that certainty in regard to a *given* coincidence in a series being the result of a psychic display can be more readily arrived at if a method of evaluation which does not employ statistics is used."

Let us consider these criticisms.

1. Whether we refer to the statistical method as being a "method of investigation" or a "tool enabling an experimenter to evaluate his data" is, in my opinion, entirely immaterial; what matters is the purpose for which it is used. Both in Rhine's first experiments with Zener cards and my first psychometric experiments, we were searching for evidence for the existence of a transcendental faculty. Therefore, *qua* the aim of such experiments, the reference to the statistical method being a "method of investigation," in contradistinction to the mere enumeration of figures which have not been statistically analysed, is, to say the least, quite appropriate.

Dr. Woodruff states: ". . . where success is relatively limited, as is the case in most investigations, the use of statistics to evaluate the results certainly serves a constructive purpose and adds to the authority of the investigation."

Broadly speaking, I am in agreement with this statement, and nothing that I have said is inconsistent therewith. But apparently we interpret the concept "constructive purpose" differently, and also differ as regards the importance we respectively attribute to the "authority of the investigation."

In a general sense, all investigations may be said to be constructive attempts; but what I had in mind when writing about constructive research was: having admitted the ex-

istence of a transcendental faculty, we ought now to explore that faculty in directions and by methods which might enable us to ascertain some of the facts associated with it, irrespective of any speculative theories—the hope being that the facts ascertained may prove to be so positive as to enable us to start building the science of parapsychology on a definite basis, and possibly also to use these facts for various practical purposes. It will be seen that I am using the adjective "constructive" in a more specific sense.

Constructive research thus directed may or may not make use of statistics, this being dependent on whether or not the material that comes into question lends itself to it. Suppose, for instance, the material is such that, as set out below, some form of adjudication is necessary before the results can be treated by the statistical method. I submit that in such cases the hope of the latter adding anything to "the authority of the investigation" must necessarily be disappointed.

In fact, Dr. Woodruff expresses the suspicion that some of my statements "arise from a belief that statistics cannot be used to real advantage in the evaluation of so-called free stimulus material." He is right; I *do* hold such a belief with respect to material of certain character, especially items which require to be adjudicated as to the values they ought respectively to receive in the statistical calculations. However objective an adjudicator may endeavor to be, his evaluation will always be considered to be affected by an arbitrary factor and in these circumstances statistics, however rigorously applied, could not appreciably add to "the authority of the investigation."

Dr. Woodruff also says: "If statistics are not used, the experimenter is free to conclude that it is *unlikely* his results are due to chance, but *how* unlikely he is entirely unable to say." This statement is, of course, true; but does it really much matter from a practical and constructive point of view whether he does or does not know *to what extent* certain results are *unlikely* to be due to chance, once he has reached the belief that they *are* unlikely to be due to it? In any case,

we must always bear in mind that, however high, a statistical probability cannot be claimed to possess the scientific property of converting a belief based thereon into positive *knowledge*. I submit, therefore, that a strong belief, whether it be based on a highly significant statistical figure or be otherwise reached by a conscientious experimenter, is all that matters in a constructive exploration.

Take, for instance, the two striking examples cited on pages 111 and 134 of my book, *Exploring the Ultra-Perceptive Faculty* (Rider & Co., London, 1941).

Example 137. At the very moment that, in the course of his perusal of an illustrated paper not known either to the sensitive or the experimenter, a subject was contemplating a picture showing the entrance to St. Pancras Station—the latter inscription being only slightly visible—the sensitive, miles away, psychometrising an article belonging to the subject, got the impression "St. Pancras."

Number 9 in the Simultaneous Tests. Two sensitives, miles apart, were psychometrising simultaneously each other's articles submitted to them for that purpose by two experimenters, without the sensitives being aware of the nature of the experiment. At the very moment the one sensitive gave the impression: "I hear a noise like a *heavy chain* being dragged along," the other one stated: "A large, *heavy chain.*"

Let us assume that these were the only cases of perfect agreement, each during a one hour test. What kind of statistics could one use with regard to such results? And assuming free material of this exceptional character did lend itself to statistical treatment, could the highest significant figure carry the same conviction as the agreement in substance and time *per se* in these particular cases?

I venture to suggest that, as a rule, it is not the mathematically ascertained degree of probability, but the confidence the investigator gains from some of his successful results, however far between they may be, that decides him what steps he ought to take next.

2. The implication set out in No. 2 of Dr. Woodruff's summary rather surprises me. I feel almost certain that he did not intend to express what the statement conveys. Its actual meaning will be better perceived from the following paraphrase: Abandon statistics in the evaluation of the results and you will obtain more knowledge about the *real nature* of psychical phenomena. Neither in the two volumes of my published work nor in the article published in the JOURNAL did I say anything warranting such an inference.

The explanation given above under No. 1 is an indication as to where I would draw the line between the truly scientific domain of statistics and the domain where their importance is overrated. Moreover, I have previously stated that I myself am in many ways a protagonist of statistics. May I therefore suggest that in order to remove any misunderstanding, the implication No. 2 ought to be revised to read as follows: "That with certain material, especially free material, more knowledge can be obtained about the *real nature* of psychical phenomena if investigators focussed their attention on a qualitative analysis of the results rather than on statistics."

I trust that on second thought Dr. Woodruff will agree with me on the justification of such views.

3. I thought I had made it quite clear when discussing "chance coincidence" that we lack all criteria for determining which of the results are due to chance and which to a true transcendental psychic ability, whether we use or do not use statistics. I hold that with our present knowledge of the subject a differentiation between the two is an impossibility.

In the final sentence of his letter dealing with this differentiation, Dr. Woodruff says: ". . . by the use of statistical method we can know *just how likely* it is that a given correct judgment is due to chance, whereas without the use of statistics we are unable to arrive at this precise assurance." Does this really amount to anything of value? Just consider the circumstances. You have a long series of apparently

successful results, some of which may admittedly be due to a real ability and some to coincidence, and you do not know which is which. Since the "precise assurance" applies to both categories, viz., to any and every single case of the whole series, what importance is there to be attached to it? The cases are still undifferentiated and yet we are given exactly the same amount of assurance as to the extent the result is likely to be due to chance for the case actually due to it and for the case due to a real ability.

I do not contend that any other method is at present capable of establishing the differentiation. But I do strongly hold that, compared with other analysed examples, instances such as above referred to carry greater conviction that they are not due to chance coincidence; and I also feel that in their case the conviction *per se* tends to be more profound than when imparted by the statistically significant figure.

J. HETTINGER

Case

We are indebted to a personal friend, Mr. Carl Bamberger, for the following case of possible telepathy. On Friday, October 9, 1942, five days after the principal event described here took place, Mr. Bamberger, the percipient, told us about his experience. We asked him if he would be good enough to dictate to us then and there a more complete account. This he did, and several days later we submitted our typescript to him for his approval. For the sake of clarity a few changes were made in the original dictated material; the substance of the first verbal report was retained, however, and appears below. Mr. Bamberger, who is known to music lovers in both this country and in Europe as a conductor, choral director, and teacher of note, has acted as subject for us in some of the experiments in extrasensory perception carried out during the past year at the Rooms of the Society. We would like once more to remind readers of the JOURNAL, and their friends, that reports of experiences that may be significant for psychical research will be most gratefully received by us.

I think it would be best if I immediately went back to my seventeenth year, describing an apparently trivial event that occurred in 1920 when I was a student at the Academische Gymnasium in Vienna. This event has an important bearing on, and is an integral part of, the occurrence that interests you.

One of my classmates at the Gymnasium left our group and entered the Theological Seminary. In the opinion of most of us, this boy was far from being clever and we wondered how he would meet the scholastic requirements of the Seminary. Now I must explain that I had another classmate, a young man by the name of *Friedrich Hauser*.* It would be well to keep in mind that he was in no sense a close personal friend, but simply one of many in our large group. One day in 1920 (needless to say, I no longer remember the exact date) I happened to say something to *Hauser* about my doubts as to the theological student's future. *Hauser* replied, and I remember this exactly, "No, you are wrong; he's so keen he will become Pope one day." *Hauser* had a rather personal intonation and a certain way of

*Pseudonym. The real name is known to the Society.

speaking out of the corner of his mouth that gave his sentences quite a curious flavor. I left the Gymnasium at the end of the year, and to the best of my knowledge I never saw *Hauser* again, or thought of him, until last Sunday afternoon. And this brings me to the contemporary part of my experience.

On Sunday afternoon, October 4, 1942, I was sitting at the bedside of my sister Gertrud, who, as you know, was recovering from an attack of grippe. She was dozing and I was reading the *New York Times*. I know that it was after the midday meal, but I no longer remember the exact moment when I happened to see a picture of Pope Pius XII which illustrated an article appearing in the Magazine Section. I do not now recall the exact title of the article. Looking at the picture in rather a desultory and somnolent fashion, I began to think about the Pope's responsibilities in the world of today. I think that I spoke aloud about this and related problems to Gertrud, but I am not sure whether she heard me because of her drowsy state. Then suddenly my mind went back to that day in Vienna which I have described above, and it was as though I heard *Friedrich Hauser's* voice saying, "No, you are wrong; he's so keen he will become Pope one day." It seemed to me that I recaptured *Hauser's* exact intonation and tone of voice; it echoed in my inner ear, so to speak, like a very recent memory. The impression was so vivid that I am quite sure I spoke the sentence out loud, imitating *Hauser's* odd way of speaking, his habit of hunching up his shoulders, and so forth. Then I began to wonder what had happened to *Hauser,* how things had gone with him since the Nazis had entered Austria, and where he was at present. And I must say again that *Hauser* had been absent from my thoughts for more than twenty years. I had no idea of his present whereabouts.

I come now to the third part of this little narrative. The next morning, Monday, October 5, 1942, I went downstairs to see if there was anything for us in the early mail. In the box was a postcard from *Friedrich Hauser,* dated Sunday, October 4th. I must say that when I saw the signature I was in a sense "shocked" and when I found myself upstairs again my hand was trembling. I called to Lotte [Mr. Bamberger's wife] and with considerable excitement showed her the card. I also showed it to Gertrud. The message said, in effect, that *Hauser* was pretty sure I was his old classmate of long ago and that he would like to see me again. I suppose, therefore, that he was thinking of me on Sunday, at which time he wrote the card. When later I saw *Hauser,* he told me that he had run across my name in a file containing the names of former Austrian citizens.

As I told you, I saw *Hauser* this afternoon [October 9, 1942]

before coming to see you. I told him of my experience, but he was obviously so disinterested in psychical matters that I did not care to question him as to the exact time when he had written the postcard. I regret also that I must have thrown away or mislaid the card. But brief statements from Lotte and Gertrud will verify the arrival of the communication from *Hauser*.

I have been wondering to what extent my experience could best be attributed to the operation of "pure chance." To my mind, one fact speaks against this hypothesis: during the years since 1920 I have seen a very considerable number of pictures of the various popes; never before, however, did a comparable train of thought emerge as the result of viewing such pictures. All memories of *Friedrich Hauser* remained latent until the Sunday when he was presumably engaged in thinking about me.

On October 16, 1942, we obtained the following statements from Mrs. Bamberger and Miss Gertrud Bamberger:

At about nine o'clock on the morning of October 5th I was sitting at the breakfast table. My husband came upstairs with a postcard in his hand, saying, "Look, here's a card from *Friedrich Hauser*!" I could see that he was rather excited. He handed me the card and I read the message, which was substantially as reported above. My husband then told me about the direction which his thoughts had taken when he saw the picture of Pope Pius XII the day before. He said he felt it was very strange.

LOTTE H. BAMBERGER

On Sunday afternoon, October 4th, I was lying in bed, recovering from an attack of grippe. My brother was sitting near me, reading the Sunday *Times*. I have a vague memory that he spoke out loud about the Pope, but I think I dozed off then for I do not recall hearing him repeat *Hauser's* sentence, ". . . he's so keen he will become Pope one day." When the card came next morning my brother showed it to me, saying with excitement, "I can hardly believe this." He has often discussed his old classmates with me, but I do not think he ever mentioned *Friedrich Hauser* before.

GERTRUD BAMBERGER

THE JOURNAL OF THE AMERICAN SOCIETY FOR PSYCHICAL RESEARCH

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Volume XXXVII APRIL - 1943 Number 2

CONTENTS

At the Annual Meeting of the Voting Members of the American Society for Psychical Research, Inc., held on January 26, 1943, the following Trustees were re-elected for a term of three years, ending January, 1946: Mrs. Edward W. Allison, Mr. H. Addington Bruce, Dr. Waldemar Kaempffert, Mrs. Richard L. Kennedy, Jr., and Dr. Edwin G. Zabriskie.

At the Meeting of the Board of Trustees immediately following the Annual Meeting Dr. George H. Hyslop was re-elected President of the Society for the year 1943. Mrs. Richard L. Kennedy, Jr. was elected Secretary, Mr. Lawson Purdy was re-elected Treasurer, and Dr. Gardner Murphy was re-elected First Vice-President.

The President reappointed the following Chairmen of Committees to serve until January, 1944: Dr. Gardner Murphy, Research; Mr. Lawson Purdy, Finance; Mrs. Richard L. Kennedy, Jr., Membership. Mrs. Edward W. Allison was appointed Chairman of the Publications Committee.

At a regular meeting of the Board of Trustees held on February 23, 1943, Mrs. Lawrence Jacob was elected Second Vice-President.

In Memory
of
Thomas Haines Pierson

We regret to record the death, on December 13, 1942, of Mr. Thomas Haines Pierson, for many years an active and highly esteemed Member of the Society. At the Meeting of the Board of Trustees held on December 22, 1942, the following resolution was unanimously adopted:

Be it resolved that the Board of Trustees of the American Society for Psychical Research, Inc. has heard with profound regret the announcement of the death of Thomas H. Pierson. Mr. Pierson was one of the oldest and most valued Members of the Society for which he came to act as Secretary during the years 1925 to 1941, and Trustee from 1925 to January, 1942. On December 23, 1941, he was elected Honorary Vice-President. During the period of his active service to and interest in the aims and purposes of the Society he brought to its support a high degree of intellectual vigor and the discriminating abilities of the highly trained engineer. For many years he was one of the most enthusiastic and industrious supporters of its research. He will long be remembered not only for his loyalty and his activity on behalf of the Society, but for that geniality of temperament and spirit of cooperation which he invariably brought to the labors of this Board.

The Midas Touch In Psychical Research

ERNEST TAVES AND L. A. DALE

We are all familiar with the legend of the Phrygian king, Midas, upon whom the god Dionysus conferred the power of transforming all that he touched into gold. The unhappy king found the gift inconvenient, however, and at last, in danger of starvation, he begged Dionysus to take it back. Now, if *continuing positive results* in psychical research, and especially in what it is convenient to refer to as ESP research, may be considered analogous to gold, we might with reason say that many investigators have been afflicted with the "Midas touch in reverse." The touch of the present writers, for example, when functioning as experimenters, appears likely to transform the gold into dross—and this within a short time-span. The results of our experiments, to put it bluntly, are usually at first positive; there then follows an inevitable decline to chance scoring. Our experience with the various aspects of the Midas touch in reverse is by no means unique. Many references to decline effects in quantitative experiments may be found in the literature of the past sixty years.

Writing in 1884, Richet (1) reports the results of a long series of experiments in card-guessing. He noticed that when a large number of trials were made at one sitting the proportion of hits decreased; that is, his subjects did best when the runs through the deck were short. Rawson (2) said in 1895, in regard to some experiments in guessing playing-cards, that "the fact also that I found myself almost invariably less successful in every series of experiments *after the first** with any agent rather discouraged me, or dulled my interest in persevering." These observations, however, were incidental; we must look to later writers for more detailed treatment of decline phenomena. Before presenting our own data, we want to quote at some

* Italics ours.

length from the work of three earlier experimenters; first, Miss Ina Jephson in England; then G. H. Estabrooks in America, and later, Pratt and Woodruff, of Duke University. It will be seen that our results fit rather neatly into the framework provided by these pieces of research and that particularly interesting comparisons can be made.

In 1925 Miss Jephson (3), later a member of the Council of the S.P.R., undertook some clairvoyance experiments using playing-cards. Encouraged by initial successes of her own, she extended the scope of the experiment to include the work of 240 percipients who sent in five sets of five guesses each—a total of 6000 guesses. The results as a whole, scored by a system devised by R. A. Fisher, were clearly above chance, but we are here especially interested in the Midas touch effects which Miss Jephson found throughout the data. The form taken by her own curve of performance is summarized by the following (pp. 229-230):

"But far more important than the amount of success is a characteristic which shows up when you examine the series carefully, and that characteristic seems to me the most valuable; I mean the highly suggestive fact that there is much more success at the beginning of each trial than at the end, that on each night my second guess or divination was the best and the last the worst . . .

"The significance of this I feel cannot be over-estimated, for if this characteristic of early-success continued and was observable in the efforts of other people also, it would be highly suggestive of some element other than chance at work . . ."

Though the curves for group data do not exactly duplicate Miss Jephson's, it remains true that when the calls of all subjects were analyzed the tendency for hits to cluster at the beginning of each run was just as marked as in her own early calling.

It is especially to be noted that Miss Jephson says, *"As time went on* [she continued her own guessing experiments

for over a year] *I became less and less successful, finally getting results only equal to chance."**

It will be remembered that the percipients guessed five sets of five cards each. After guessing the first set of five cards, the percipients were instructed to wait some hours, preferably a day, before guessing the next set. Thus, Miss Jephson was able to make another interesting observation; viz., that there was a general deterioration of scoring level from first sets of five guesses to last sets of five guesses. She says (p. 245):

"I consider this definite drop from first sets to last sets in the total results got by these 240 people reinforces very strongly the hypothesis put forward, that we are here observing at work, and progressively deteriorating, an unknown faculty of perception. The fact that there is a marked tendency towards first-guess-success in short series of five does not at all necessitate that we should also get this diminution of success from the beginning of the whole series to the end. That both these effects do occur is surely rather noteworthy and suggestive."

This diminution of success from the beginning of an experimental series to the end is just the aspect of the Midas touch that we find most pronounced in our data. This we shall attempt to demonstrate quantitatively in a later section. First, however, let us briefly summarize the results of an experiment that G. H. Estabrooks (4) carried out at Harvard University during the years 1925-26.

In this experiment, which was divided into three temporally separated series (Series 1, 2, and 3), a large number of Harvard undergraduate students undertook to "guess" playing-cards. Some of the subjects, indeed, did not realize that an experiment in telepathy was involved, having been led to believe that their guesses were being used in connection with a statistical study of chance distributions. This extreme naïveté on the part of the subjects may have been a variable of importance; we are inclined

* Italics ours.

to think that it was. In any case, his results were curiously like those of Miss Jephson (and obtained under much better conditions so far as the ruling out of sensory cues was concerned). His subjects guessed twenty playing-cards at a sitting, writing down their calls at a signal from a clockwork apparatus connected with a telegraph key in the experimenter's room. It is especially to be noted that "in the great majority of cases *a subject was used only once for these experiments.*"* The results of 1660 calls were analyzed for hits on color and on suit. The positive deviations in both cases were far in excess of what could reasonably be attributed to chance; but, more interesting, the larger part of the positive deviations came from the first ten cards of the twenty. Now, in connection with some points we hope to make about our own data, it should be said that Estabrooks conducted another series (Series 4) immediately after one of the successful series (Series 1) described above. Here the agent and percipients were placed in rooms about sixty feet distant from each other. The subjects were for the most part the *same men* who had taken part in the first series. The distance series formed a second experiment which was run off at the same sitting; that is, the subjects, having finished their first twenty guesses (Series 1), were immediately asked to call twenty more cards (Series 4), the only difference in procedure being that the agent, Estabrooks, moved to the distant room. The results of this experiment were wholly negative. Estabrooks says he is inclined to ascribe the failure at a distance as being due "largely to the distance factor with perhaps an element of greater insulation entering in or some factor such as adverse autosuggestion."

Examination of Estabrooks' published data, however, indicates that very possibly his interpretation is not correct. The subjects' color-scoring level (in respect of Series 1, a "successful series") had dropped markedly by the time the fifth card was reached. This is shown graphically in

* Italics ours.

Fig. 1, in which scoring levels through the deck for Series 1 are plotted. This curve indicates, we believe, that the insignificant scores reported for Series 4 are attributable, not to the increase in distance, but to the fact that the Midas touch had already begun to operate in Series 1. In Fig. 2 are plotted the deviations at different points through the deck for the total data from Series 1, 2, and 3. Here the operation of the Midas touch is even more evident. Analysis of the suit scores yields essentially the same results.

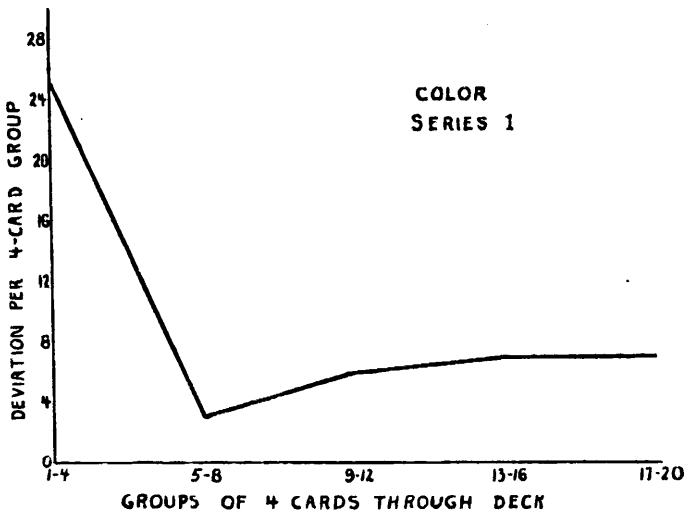

FIG. 1

Estabrooks' Experiment. Deviations from expectation of color scores per four-card group through deck, Series 1.

Lastly, no review of the literature, however cursory, would be adequate without mention of the experimentation of J. G. Pratt and J. L. Woodruff, reported in an article appearing in 1939 in the *Journal of Parapsychology* (5). The entire experiment involved the participation of 66 subjects, who called a total of 3868 decks of ESP cards. The results were highly significant. The purpose of the investigation was to throw light on several specific problems, two of these being, What is the effect of experience in formal ESP tests on rate of scoring? and, What is the relation of "new-

ness" of stimulus material to rate of scoring? In this large-scale experiment, Pratt and Woodruff found that when "new" material was used the scores were significantly higher than when "old" material (material to which the subjects had become accustomed through long usage) was employed. They found that experienced subjects scored as well as naïve subjects *when the stimulus material was new* to them, but noted a decline in the effectiveness of new material throughout successive sessions of its use. Again,

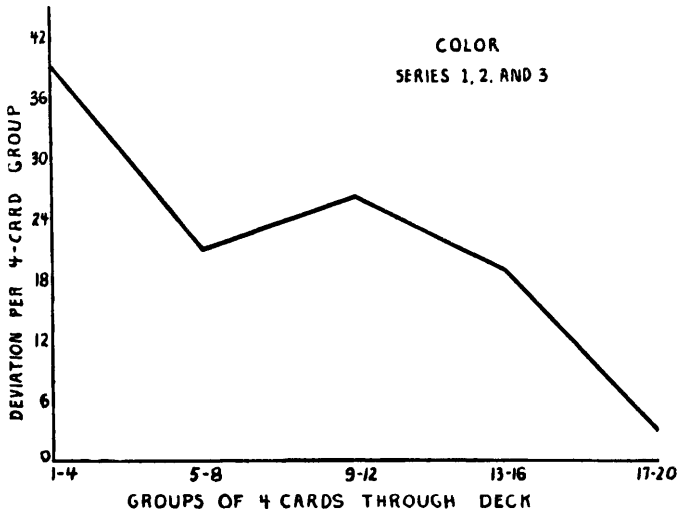

FIG. 2

Estabrooks' experiment. Deviations from expectation of color scores per four-card group through the deck, Series 1, 2, and 3 combined.

we see an aspect of the Midas touch at work. The Duke University writers conclude their paper by saying, "There has been a general clinical impression abroad among ESP workers that a change of conditions helps to keep the subject interested in the tests in a way that favors scoring. Further direct experimental evidence to define the conditions under which this generalization is applicable is obviously needed." Such observations as these have encouraged us to present our own experimental results—results which, if they stood alone, might lack in impressiveness, but which

may also, taken in conjunction with the studies we have mentioned, shed further illumination upon one of the most perplexing problems in parapsychology.

Having presented a brief and necessarily somewhat inadequate historical background, we proceed to a discussion of the Midas touch in reverse as it directly affects our own experimental data. We must make it clear that we use the term to express two different aspects of a situation that seems to obtain in our work. First, there is the Midas touch from the point of view of the experimenter. In this case, a new experiment is begun; the initial total data from all subjects are positive, then the experiment collapses *in toto,* and no subject, experienced or naïve, scores above the chance level. In a later section of this paper we will present suggestive evidence that this actually does happen.

Secondly, we use the term to describe the sequence of events in which an individual subject, or percipient, begins a series of ESP calls under new conditions, scores above chance on the first decks of the given experiment, then, as the experiment progresses, descends to the chance level, or below. This is the Midas touch from the point of view of the subject.

We believe these observations to represent two distinct aspects of one underlying psychological reality. In the first case, we must attribute the Midas touch to the *experimenter.* Events may be as follows: An experimenter devises a new experiment. The initial data are positive. Since an investigator is never, particularly in this field, completely detached from the results, he is elated. The urgency of obtaining a positive result disappears, interest fades perhaps, and this is reflected in the scores his subjects subsequently obtain for him. It is not, of course, necessary to suppose that this complex of events takes place at the conscious level; the role of the subconscious may be as great in the case of the experimenter as it is in the case of the subject. The experimenter may or may not be aware of a slackening of interest in his undertaking; nevertheless, some deep, uncon-

scious need may be perfectly well satisfied by the initial positive result, thus not requiring any further "evidence." At this point, his subjects no longer produce extra-chance scores. In the second case, it is the *subject* who possesses the Midas touch; he can do well at first but not later, when the factors responsible for the decline begin to exert their influence — perhaps self-consciousness, over-anxiousness, fatigue, the crystallization of patterns of calling, or a combination of all these.

As noted previously, we gradually became more and more aware of decline phenomena in our data; they seemed, so to speak, to meet us at every turn. Accordingly, we decided to examine the data in order to determine whether our subjective impressions were supported by quantitative evidence. We selected for examination the total data we had collected since we began our activities, under the direction of Dr. Gardner Murphy, as joint experimenters in the summer of 1941.

Two types of research were in progress during this period: (a) Group experiments, in which a number of subjects simultaneously called various types of ESP stimulus material. The experiments were heterogeneous, and the results were so lacking in interest that group experimentation was finally abandoned. These data, which we have not studied quantitatively in respect of decline effects, are, with one exception, not included in this study. The exception, an experiment involving the use of playing-cards, was included for it represented a long-continued attempt to isolate the effect of one experimental variable. As will be seen later, we present these results as suggestive evidence for the existence of the Midas touch on the part of the experimenter.

(b) Individual experiments, in which subjects came singly, by appointment, to participate in relatively long-term experiments. It is these data which form the greater part of the present study.

The experiments will now be described and the results presented.

Results

I. *Playing-card Experiment.*

During our work with the group in the winter months of 1941-2, a series of experiments was undertaken in which subjects called decks of 26 playing-cards by the GESP method.* The experimental variable employed was the identity of the agent; that is, the two present writers alternated as agents. An immediate difference was noted in the results obtained under the two conditions. When LD was agent, the scores rose above chance; when ET was agent, scores were at chance or below, though not below to a significant degree.

We offer these experimental results primarily to illustrate one aspect of decline phenomena in our data; we do not claim that this particular result is other than suggestive and hence do not present a full statistical analysis—an analysis which could only weary the reader.

The data are plotted in Fig. 3. The curves show the cumulative deviation from chance (in respect of direct hits**) obtained by the two agents. It is apparent that when LD was agent (solid line), a suggestive trend of positive scoring was present at first, whereas when ET was agent (dotted line), the scores were at first slightly below chance, then fluctuated around the expected value.

LD's curve seems to show graphically the operation of the Midas touch from the point of view of the experimenter; that is, at first what seemed to us to be a highly suggestive difference between the two sets of data was obtained. It looked like a promising lead. Experimentation was continued under the same objective conditions as before, but the results deteriorated to an obviously chance level.

*GESP (General extra-sensory perception): an experimental technique which allows of the operation of either telepathy, clairvoyance, or both. In this playing-card experiment agent and subjects were seated in rooms about 40 feet distant from each other. By means of an electrical device the agent signalled to the subjects when a card was exposed and looked at. Thereupon the subjects wrote down their guesses, thus proceeding through the deck. The decks were set up by the use of random numbers and the card order was recorded in advance.

**We must point out that color and suit scores in this experiment were totally without significance.

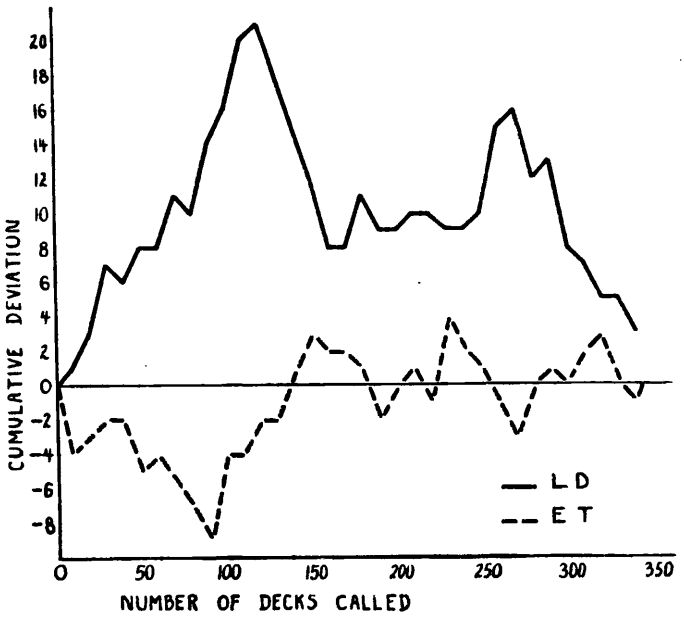

FIG. 3

Comparison of cumulative deviations obtained by LD and ET as agents in Playing-card Experiment.

Two points should be clarified; first, the Midas touch in this experiment would only seem to apply to the agent LD, since ET's data were never of interest—providing merely a "chance" value with which to compare LD's data. Secondly, because of a constant influx of new subjects (and a dropping out of old subjects) we cannot attribute the Midas touch to any individual subject.

A few brief statistics may be mentioned. When LD had been agent for 120 decks (of 26 cards each) 81 direct hits had been obtained—an excess over expectation of 21 hits. Evaluation of this deviation in terms of Deviation/$\sqrt{npq}$ yields a critical ratio of 3.1, indicating that a deviation as large as this one is to be expected, on a chance basis, about once in a thousand such blocks of data. Since, at the time the experiment was terminated (N for LD = 340 decks), the deviation had dropped to an insignificant level, a more precise statistical determination was deemed unnecessary.*

*Determination of the *exact* probabilities would involve, in this case, the computation of the moments of the distribution $(1/52+51/52)^{26}$ and substitution in the Charlier expression of the generalized normal curve.

II. *Mixed-Deck Experiment*

In this experiment a deck containing 25 different symbols was used, the probability of obtaining a hit thus being one in twenty-five. The targets included such things as color-cards, ESP cards, certain specified playing-cards, and cards of alleged affective tone, such as drawings of bombs, flags, snakes, and so forth. Subjects at one sitting called two such decks of twenty-five cards. Each card was inclosed face up in a small white box; these boxes, fifty in number, were laid out in two rows along a table. The subject was brought into the experimental room and shown a sample deck so that he would be familiar with the nature of the stimulus material. He was then allowed to pick up the first box, whereupon he verbally indicated his guess. This the experimenter recorded on a suitable record sheet. Then the subject opened the box and saw whether or not his guess was correct. The same procedure continued until fifty guesses had been made.

The order of the cards set in the boxes was determined by the use of random numbers and was recorded in advance. This record was later compared with the experimenter's recording of the subject's guesses. Needless to say, the experimenter closely watched the subject during the calling process to be sure that the box was not opened before his guess was recorded.

Seventeen subjects called a total of 72 decks, obtaining a total of 70 hits—two below chance expectation. This is a completely insignificant deviation. The cumulative deviation from chance is plotted in Fig. 4. It will be seen that a familiar effect appears again: at first the curve ascends sharply, then tapers off to the chance level. When 11 decks had been called, an excess over expectation of 12 hits had been obtained. Applying, for purposes of approximate assessment of significance, the formula Deviation/$\sqrt{npq}$, we obtain a C.R. of 3.69, indicating that such a deviation is to be expected, if chance alone obtains, about once in ten thousand such blocks of data.

FIG. 4
Cumulative deviation—Mixed-deck Experiment.

It is of interest to note that the mixed deck data were obtained by LD acting as experimenter. This also seems to be a case of the Midas touch in respect of the experimenter; as in the playing-card experiment discussed above, there was a constant influx of new subjects and a dropping out of experienced subjects. It is also interesting to note that when the data are broken down into first and second decks it is seen that of the 70 hits altogether obtained, 41 derived from the *first* deck, 29 from the *second* deck. While this is not a statistically significant difference (C.R.=1.27), we consider it of some importance for reasons that will be seen later.

III. *Experiment AA—GESP versus DT.**

In this experiment, using standard ESP cards, ET and LD acted alternately as subject and experimenter (each

*DT (Down through): An experimental technique in which the subject calls the order of the cards *down through* the deck, this order not being known to the experimenter.

acting as subject for the other). GESP and DT methods were also alternated. Subject and experimenter were seated in rooms 20 feet apart and doors were kept closed. In the GESP trials, the subject wrote down his calls at a signal (electric bell) from the experimenter. Independent records of the target order were kept by the experimenter and compared with the subject's calls. A thoroughly shuffled "master deck," containing 50 of each of the five symbols, was used. Two decks, one for the GESP task and one for the DT task, were dealt out from this master deck. At the completion of the double task, the master deck was thoroughly reshuffled, and so on.

There was a considerable rivalry between ET and LD; in addition to being interested in any differences in scoring which might appear in the GESP versus the DT methods of calling, there was a lively interest to see who could get the higher total scores. No significant differences were found to obtain between the GESP and DT methods.

In Fig. 5 are plotted the cumulative deviations from expectation of the combined GESP-DT tasks from both subjects (LD, solid line; ET, dotted line). A comparison of

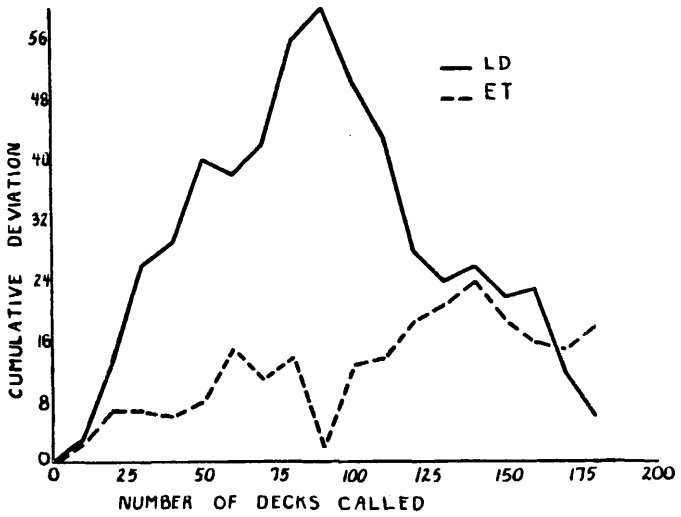

FIG. 5
Cumulative deviations for LD and ET, Experiment AA.

Figs. 3, 4, and 5 reveals certain interesting similarities; namely, LD's data, both as experimenter (Figs. 3 and 4) and as subject (Fig. 5) show a similar positive trend at first, with a subsequent deterioration, while ET's data, both as subject and experimenter, are of little interest. Although perhaps too rigid a dichotomy cannot be drawn between the two aspects of the Midas touch, experiment AA would seem to us to be a case of the Midas touch from the point of view of the subject. LD at first obtained a positive deviation, later descending to the chance level.

When LD had called 92 decks (46 GESP and 46 DT), the positive deviation from chance expectation was 61. Evaluation in terms of Deviation/$\sqrt{npq}$ gives a C.R. of 3.18. This means that such a deviation is expected about once in every 1300 such blocks of data.

IV. *The Midas Touch as a General Tendency in Experimental Subjects.*

Of the 127,050 calls (5082 decks) we are discussing in this section of the paper, 4180 were obtained by methods AE and AJ, shortly to be described. The remaining data are derived from the five lesser experiments enumerated below. In all of these experiments cards were used in which the probability of success was one in five. 72 persons took part as subjects, their ages ranging from eight years to eighty-five years. Nearly all decks were prepared by the use of random numbers, students at City College and Hunter College, for the most part, setting up the material.* The minor experiments involved in the present study were:

1. Experiment AA—discussed from another point of view on pp. 68-70.

2. A "Novelty Experiment"—in which standard ESP cards were used, the variable being that a different method of calling was used for every deck.

3. A DT series using the "Five-symbol Deck." For a reproduction of these cards of supposed affective tone, see

*When the present writers acted as subjects, use was made of a well-shuffled master deck containing 60 of each symbol. After each group of three runs the cards were shuffled back into the master deck.

an article (6) appearing in the January, 1942 issue of this JOURNAL, inset opp. p. 19.

4. "White Box Experiment"—see same article as above, p. 21.

5. Distance GESP calling—the experiment divided into two temporally separated series and involving only one subject.

In the period between August, 1941, and the end of 1942 the largest part of our quantitative research was devoted to collecting data by two methods—methods that we designate by the letters AE and AJ. These methods involve the use of a piece of apparatus we call the KR (knowledge-of-results) machine. Both the machine and methods AE and AJ were described in the article by Murphy and Taves (6) referred to above, pp. 17 ff. We may briefly state here that experiment AE involves immediate knowledge of results as the subject indicates his guess by means of depressing one of five keys, causing the closing of a circuit which rings a bell. In experiment AJ the subject "guesses until he gets it," that is, guesses until he has correctly identified the stimulus card. For the purposes of this analysis, only the *first* guess on each card was considered.

It should be noted that the apparatus has been improved in several respects since the publication of the paper referred to above: (a) A protective circuit has been added which rings a warning bell should the experimenter inadvertently depress two switches simultaneously on his panel. (b) The switches on the experimenter's panel have been changed from those originally used and their operation is practically noiseless. (c) The subject's act of pushing a given key *automatically* brings about the knowledge of results; that is, if the correct key is depressed by the subject a circuit is closed and a bell rings, indicating that the guess has been correct. This effectively rules out any possibility of the experimenter's consciously or unconsciously giving cues as to the target by the length of time between a given call and the signal. (d) An additional counter which auto-

matically records all *wrong* guesses has been added to the experimenter's panel. Thus, when a run through the deck is completed, an entirely objective count is available to compare with the written count made by the experimenter. In view of the safeguards which the apparatus affords, it seems to us that statistically significant scores should be attributed to extra-sensory factors.

The results of these experiments, plus those previously described in this paper, constitute the total quantitative data obtained by the present writers, with the exception of group experiments (abandoned as noted above).

In analyzing these data in respect of the Midas touch effect, *the first three scores obtained by naïve subjects* (naïve, that is, for the particular experiment) *were examined because we had the subjective impression that these scores were significantly above chance.* The scores on the fourth through the sixth decks were noted for purposes of comparison. The main results of this analysis are presented in Table I. It will immediately be seen that our impression as to initial success is borne out by the analysis.

Table I

Comparison of *first* three decks with *second* three decks called by naïve subjects.

Deck	*N (in decks)*	*Dev.*	*Mean*	*N*	$\frac{Dev.}{\sqrt{npq}}$	*P*
1	127	+57	5.45			
2	118	+29	5.25	351	3.36	.0004
3	106	+40	5.38			
4	100	— 3	4.97			
5	90	—16	4.82	273	—.21	.417
6	83	+12	5.14			

First, it will be noted that N, the number of decks, is greatest for Deck 1, decreasing through Deck 6. This is because some subjects called only one deck, some called only two, and so on; for every *first* deck, therefore, there is not a *sixth* deck for comparison.

The *Deviation* column indicates that our hypothesis was verified. The deviations for the first three decks are quite positive. The deviations for the second three decks are insignificant in magnitude, minus in two cases, plus in one. The mean scores produced by these deviations from chance are shown in the *Mean* column. Table I also shows that the total deviation of the first three decks is sufficiently large to yield a P-value of .0004; this means that such a deviation is to be expected only once in 2500 such blocks of data. The table also shows that the scores on the fourth, fifth, and sixth decks lie completely within the limits of conventional chance variability.

Table II

Significance of the difference between mean scores of *first three decks* and all remaining decks.

	First 3 decks	*All other decks*
N (in decks)	351	4731
Deviation	+126	—1
Mean	5.359	4.9998
$Mean_1 - Mean_2$	0.3592	
Sigma of the diff.	0.1106	
C.R.	3.25	
P	.00058	

In Table II are presented the results of a different kind of analysis. Here the *first three decks* are compared with *all other decks*. It is seen that a positive deviation of 126 was obtained on the first three decks, mean=5.359, whereas a negative deviation of *one* was obtained on the remaining 4731 decks—yielding a mean score of 4.9998 as compared with the expected chance mean of 5. In other words, *all* of the positive deviation of the 5082 decks which constitute our total experimental data comes from the first three decks called by naïve subjects. The C.R. of the difference between these two means is 3.25, corresponding to a P-value of .00058. This indicates that the mean score of the first three

decks is significantly different from the mean score of the remaining 4731 decks.

In Fig. 6 these data are plotted graphically in the form of an histogram. Here we see the magnitude of the deviations from expectation obtained on each of the first six decks. It will be noted that the width, as well as the height,

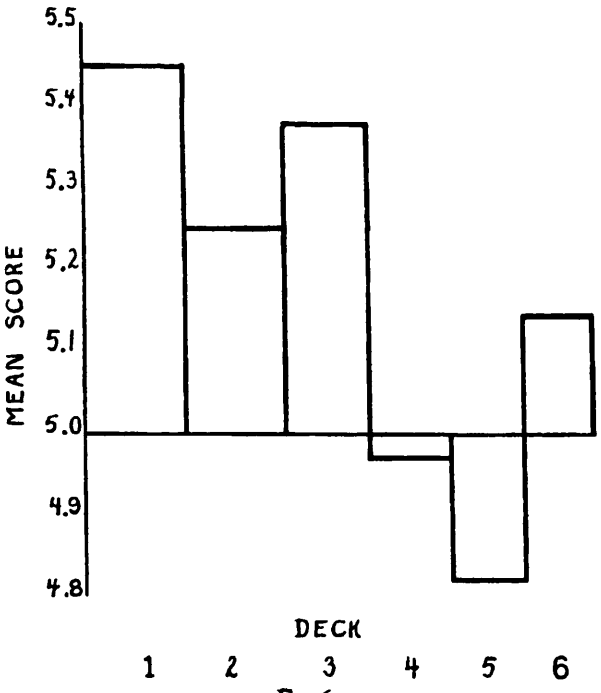

FIG. 6

Histogram showing mean scores for each of first six decks.

of the columns varies; this is to accommodate for the different values of N upon which each of the columns is based. Thus the total area of each column directly corresponds to the deviation for that deck.

Three subsidiary analyses may briefly be mentioned:

(a) At the suggestion of Dr. J. B. Rhine, we examined the first three decks to determine whether the Midas touch was operative within the deck itself. We compared the number of hits in the *first half* of these decks with the number of hits in the *second half,* ignoring hits that occurred on the

thirteenth, or middle, card of the deck. Combining the results from the first three decks, we find that 902 hits were obtained in the first half of these decks, 899 on the second half—a result indicating the homogeneity of the first and second halves. It may be noted, however, that in respect of the *first deck only,* 345 hits were obtained in the first half as compared with 313 on the second half. This difference in favor of hits in the first half of the deck, although statistically not significant, is somewhat suggestive of the Midas touch operating within the short span of the deck itself. The results of the next analysis would also seem to tend in this direction.

(b) At the suggestion of Dr. Murphy, we examined the loci of hits on the *first five calls* of the first three decks. The hypothesis was that if ESP was sufficient to produce significantly high scores on the deck as a whole, it might—due to the operation of the Midas touch within the deck—be exerting its influence most strongly on the first five cards. Here again, the combined results of the first three decks in respect of this analysis are not significant. When the *first deck alone* is examined, however, results as shown in Table III and Fig. 7 are obtained.

Table III

Hits on first five cards, first deck. N (in decks) = 127

	Hits	*Deviation*	$\sqrt{npq}$	*C.R.*
1st Card	36	+10.6	6.375	3.01
2nd Card	34	+ 8.6		
3rd Card	20	— 5.4		
4th Card	26	+ 0.6		
5th Card	26	+ 0.6		

The Table and Graph show that there was present, in the first deck, a tendency towards a "miniature Midas touch." Hits on the first two cards called by naïve subjects are sufficiently in excess of expectation to produce a C.R. of greater than 3.

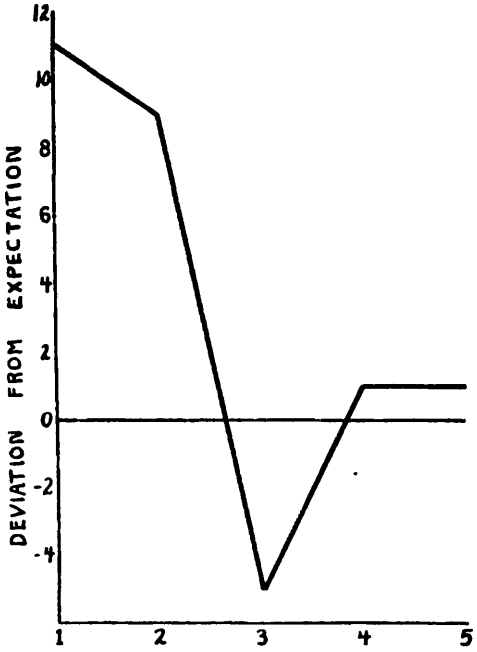

FIG. 7

Deviations from expectation for first five cards of first deck.

(c) In view of these findings in our current data, it was decided to re-examine some data previously published by Murphy and Taves (7). In this experiment 74 subjects called, among other tasks, two decks of ESP cards at one sitting. The results obtained on the *first* and *second* ESP decks are shown in Table IV.

Table IV

Comparison of first and second ESP decks called by 74 subjects.

Murphy and Taves Experiment

	Deck One	*Deck Two*
Hits	419	369
Deviation	+ 49	— 1
Mean	5.662	4.986
C.R.	2.85	.....

The table shows that the scores on the first deck approach independent significance (C.R.=2.85), whereas the scores on the second deck are almost exactly at the chance level.

Discussion

The analyses of our data obtained during the last year and a half have indicated that in our work the calling of material of fixed *p*-value is attended by the phenomenon of initial success. This initial success in every case failed to maintain itself, with the result that the total deviations from chance expectation obtained up to the first of this year (1943) are not significant. The first question, of course, is whether the initial success, or Midas touch effect, resulted from ESP ability, however faint, on the part of our subjects, or whether it was a "chance occurrence"—a statistical artifact without psychological meaning.

It will already have become apparent to the reader that we hold the view that the Midas touch effect in our data is a non-chance phenomenon, and our conviction is strengthened by a comparison of our results with the findings of other experimenters. As we pointed out in our introduction, Midas touch effects very like our own have been noted, if not so called, from the earliest days of quantitative research up to the contemporary work of Rhine and his colleagues at Duke University. In other words, there are *a priori* grounds for suspecting that the effect *does* occur and that it is not due to chance alone. In our opinion, our results add confirmation to those of earlier experimenters. We therefore consider it in order critically to examine the conditions under which our data were obtained—that we may attempt to determine whether the experimental variables responsible for the phenomenon may be subjected to control. It is necessary, in other words, to examine the psychology of both the experimenter and the subject. To vary and control the physical conditions of an experiment is easy, but the psychological variables, to which the Midas touch effect must

be attributed, are not easy of apprehension, still less of control.

One point which requires discussion is this: what happens to the scoring level after the initial success has reached its highest point? Does chance scoring follow, or does a negative trend ensue, the effect of which is to bring the total deviation down to chance expectation? Inspection of Figs. 3, 4, and 5 indicates that in each case the initial success is followed, not by *chance scoring,* but by *sub-chance scoring.* It must be kept in mind that this is not analogous to fluctuations of good and bad hands at bridge, which, if the cards are properly shuffled and dealt, show random fluctuations; the present fluctuations, treated statistically, are orderly and consistent. Although in no case is the negative trend as significant as the positive trend which preceded it, these trends, considered jointly, produce a considerable negative deviation from expectation. This would seem to be a fact of considerable psychological importance.

The positive deviations obtained initially seem to indicate that ESP is operating; the negative trends which follow indicate, not that ESP has failed, but that its effect has somehow been reversed. What are the psychological conditions responsible for this sequence of events? Several factors suggest themselves as being relevant to the discussion:

(a) *Boredom.* A number of investigators have felt that boredom contributes to low scoring, but the introspections of our subjects indicate that negative scoring cannot easily be attributed to such a state. The data of Experiment AA (Fig. 5), in which each of us served alternately as subject and experimenter, are quite revealing in this respect. Interest was never higher on the part of LD than during the calling of the first few decks after the achievement of a C.R. of slightly better than 3 in respect of total deviation. It is important to note that LD was acutely aware of the trend of the experiment at all times, and particularly so during the crucial period when her scores began to decrease. Eliminating boredom, another factor is suggested as being of greater relevance:

(b) *Hypervolition.* By "hypervolition" we mean to indicate what could perhaps be more simply expressed by the phrase "trying too hard." The situation of a subject in an ESP experiment, particularly when that subject's motivations are as pronounced as in the case of LD in Experiment AA, is analogous to that of the horseman in *Macbeth* who leaped upon his steed with such verve that he fell off on the other side. The effort to reach a given goal is so intense that performance suffers as a result, and the goal is further than ever from being reached. The subject tries so hard to get a "good" score that he gets, instead, a low one. Just how this comes about is certainly not clear, but we do feel that an excess of volition, at the conscious level at least, is worthy of consideration as a factor conducive to negative scoring. It is significant to point out in this connection that in the bulk of our work the subject was aware of the degree of success or failure after each individual call, or at least after each run through the deck.

(c) *Hypomotivation.* We have already mentioned on page 63, in connection with the Midas touch in respect of the experimenter, the possibility that once a significant result is obtained an unconscious need may be satisfied, with a resultant decrease in motivation and consequent chance scores. Here we would draw attention to the fact that in Figs. 3, 4, and 5 LD was either subject or experimenter and was, without exception, aware of the trend of each experiment. It is not easy, however, to see how hypomotivation could account for an actual *negative trend.*

It must be emphasized that we are dealing with two problems here. The first involves the negative trends exemplified in Figs. 3, 4, and 5; the second involves the chance trend that appears after the extra-chance scoring in Fig. 6. Our feeling is that the two factors of hypervolition and hypomotivation may possibly have a differential role in producing these two effects; that is, it seems reasonable that hypervolition (a positive force) might be important in the etiology of negative scoring, whereas hypomotivation (a small degree of the positive force, or, indeed, its complete

absence) might provide a psychological background for chance scoring.

A number of other factors which may be of importance in producing low scores should at least be mentioned here—such factors as self-consciousness, fear of doing poorly, self-criticism, and fear of one's own unconscious. Without going too deeply into psychoanalytical concepts, it might be asked, for example, whether most of us do not feel a deep, unconscious fear of losing our everyday orientation in time and space, and of merging our identities with others in the sharing of a paranormal experience. By this token, the "gifted" ESP subject, or sensitive, would be the person who, at a subconscious level, welcomed the dissolution of the watertight bounderies of his own ego and found self-fulfillment in transcending the barriers of time and space which ordinarily separate and isolate us one from the other.

The problem which emerges from this discussion is the question of devising experiments in which at least some of these variables may be systematically controlled. As noted before, this is not an easy problem.

In our experiments subjects had immediate knowledge of results; hypervolition would seem to be dependent upon such knowledge of results. If hypervolition is to be avoided, knowledge of results must be suppressed. But to ask a subject to make a long series of calls in complete ignorance of his scores is to court boredom and chance scoring, as has been shown by Rhine and others. How, then, to avoid this dilemma?

One approach, with which we have just begun to work, involves the presentation of knowledge of results to the subject at a *near-liminal level.* In this experiment, by means of a visual signal of low intensity, the subject is informed as to the success or failure of individual calls. At the conscious level there is a very imperfect perception of the signal light; at the subliminal or near-liminal level there may be presumed to exist a more adequate perception of the stimulus. This methodology would seem to avoid hypervolition (at the conscious level), providing, at the same time, some

level of consciousness with knowledge of results, thus avoiding boredom.

The more difficult problem is concerned with the control of hypomotivation (which we have defined as being operative largely at some subconscious level). Interesting results might be obtained by setting up an experiment in which both experimenter and subject are naïve. This raises problems, however, because an experimenter who is willing to undertake the labor involved in planning and carrying out an experiment, besides having the necessary skills to do so, is automatically excluded from the wholly naïve category. We hope at least partially to avoid hypomotivation on the part of the experimenter by another method. We plan to begin new experiments (with naïve subjects) in the course of which we will refrain from the tabulation, analysis, and discussion of results until a predetermined number of calls has been obtained.

We must state here that we do not claim to have determined the locus of operation of the Midas touch, nor do we claim that it is a universally demonstrable phenomenon (although we suspect that it may be present, unnoticed, in the data of other investigators). We may, nevertheless, devise experiments to study the Midas touch as it appears under our conditions of experimentation.

We may adduce, without discussion, one additional observation which may be relevant in respect of the importance of hypomotivation: it has frequently been noted that persons first sitting with mediums often obtain results that seem impressive from an evidential point of view; the quality of the sittings, however, does not maintain itself throughout a long series. Here again, an unconscious need of the "experimenter" (the sitter) may be satisfied by the evidence initially received, negative results then ensuing.

Two final points remain to be considered:

The question of optional stopping. The critical ratios presented for the data graphed in Figs. 3, 4, and 5 are based arbitrarily on the points of greatest significance in the

individual curve. The P-values given must be interpreted with this fact in mind. The P-values for the data shown in Fig. 6 are presented, however, as based upon the data obtained up to a given point in time. This analysis is not subject to the question of optional stopping since the experiment is continuing, more data are being obtained, and a subsequent report will be presented.

Conditions of experimentation. We may briefly summarize here the conditions under which our data were obtained:

(a) In most of the major work (Experiment AE and AJ), as well as in most of the rest of the work, the subject and experimenter were in different rooms, separated by two closed doors and a hall forty feet long. In a few experimental sessions two other rooms were used, separated by a hall thirty feet long.

(b) Stimulus material used was set up either by the use of random numbers or by the use of a large master deck.

(c) Scoring was completely objective, being effected by the use of mechanical counters.

In conclusion, we would be happy if other experimenters would subject their data to the types of analyses presented in this paper. We think there is a good possibility that data which appear to be without significance in respect of total deviation may have Midas touch effects not easily attributable to chance embedded within them. We believe that Midas touch phenomena are worthy of further investigation and that such investigation may eventually provide us with valuable insight into certain motivational variables of importance in parapsychology.

REFERENCES

1. Richet, Charles, La Suggestion Mentale et le Calcul des Probabilités, *Revue Philosophique*, December, 1884.

2. Rawson, Henry, Experiments in Thought-Transference, *Proc.* S.P.R., Vol. XI (1895), pp. 2-17.

3. Jephson, Ina, Evidence for Clairvoyance in Card-Guessing, *Proc.* S.P.R., Vol. XXXVIII (1928-29), pp. 223-268.

4. Estabrooks, G. H., A Contribution to Experimental Telepathy, *Bulletin* V, B.S.P.R., 1927.

5. Pratt, J. G., and Woodruff, J. L., Size of Stimulus Symbols in Extra-Sensory Perception, *Journal of Parapsychology*, Vol. III, December, 1939, pp. 121-158.

6. Murphy, Gardner, and Taves, Ernest, Current Plans for Investigations in Psychical Research, *Journal* A.S.P.R., Vol. XXXVI, January, 1942, pp. 15-28.

7. ———, Covariance Methods in the Comparison of Extra-Sensory Tasks, *Journal of Parapsychology*, Vol. III, June, 1939, pp. 38-78.

Prediction

For readers of the JOURNAL interested in the mediumship of Mrs. Osborne Leonard, the following prediction concerning the death of the late Dr. John F. Thomas seems worth putting on record. This prediction is not pressed as striking evidence for precognition as it lacks detail and the event which seemed to fulfill it was not a *priori* inconceivable. But it may be added that Dr. Thomas' life expectancy at the time of his death was a little over ten and a half years.

In the course of a proxy sitting with Mrs. Leonard on August 22, 1937, which I recorded for a member of the Boston Society, Feda, Mrs. Leonard's control, paused and in rather an awe-struck manner made some statements which she first asked me not to "write down." These statements concerned Dr. John F. Thomas, for whom I had taken many proxy sittings with Mrs. Leonard. Dr. Thomas' wife died in 1926 and Feda refers to her as "Mrs. Buddy." Over a period of years "Mrs. Buddy" was the principal communicator in an extensive series of proxy sittings with Mrs. Leonard, following two sittings at which Dr. Thomas himself had been present. (See *Case Studies Bearing Upon Survival,* B.S.P.R., 1929, and *Beyond Normal Cognition,* B.S.P.R., 1937, by John F. Thomas.) I had taken three proxy sittings for Dr. Thomas during my 1937 visit to England, but the present sitting was being held for another member of the Boston Society.

Excerpt from Sitting, August 22, 1937

Feda: Mrs. Lyddie, you mustn't write this down.

LWA: Just for myself, Feda.

Feda: Do not let anybody know, just write it down and put it safe. Mrs. Buddy has a feeling she won't have to wait very long for him—not right away, but he will pass over when people will say, 'Oh, he's been cut off.' He will go very suddenly. Peo-

> ple will say, even you will say, 'Fancy his going that way!' But she wants him to go this way; it will be best for him, she says. It will be best for him; it will be something that will come all around him rather quickly. It will be best for him. She wants you to have it in writing.

Mrs. Guinan of the Boston Society and I were quite shocked by this prediction, much more so, in fact, than the scant details would justify. My proxy sittings with Mrs. Leonard during the summers of 1936-37, however, were among the best I ever obtained and it therefore seemed to us that in the midst of so much other veridical material Feda might also be right in this instance. Dr. Thomas, so far as we knew, was in excellent health and very active as the Deputy Superintendent of the Public School System of Detroit. We were in constant correspondence with him and his interest in the B.S.P.R. and his generous help were main factors in the continuance of the Society after Dr. Prince's death.

In November, 1940, a little over three years after Feda's prediction, Dr. Thomas was killed in an automobile accident. As Feda had said, his death did not occur "right away" (but fairly soon, as intimated) and "very suddenly." A copy of the prediction was sent to Miss Isabel Newton in the autumn of 1937, to be filed with the S.P.R.

LYDIA W. ALLISON

Cases

I

We are indebted to Mr. Edmond P. Gibson, of Detroit, Michigan, for the following case of automatism and apparent telepathy. Mr. Gibson is known to our Members for his contributions to ESP research. A number of papers by him have appeared in the *Journal of Parapsychology;* the most recent of these is "Atmospheric Pressure and ESP Score Averages" (with C. E. Stuart, Vol. VI, June, 1942, pp. 95-100). On January 14, 1943, Mr. Gibson wrote us as follows:

In looking over an old file, I have found a case of experimental telepathy through use of the ouija board that may be of some interest to you. The percipients were Hugh J. Lago, a journalist, a friend of his—Miss Laura Barnes*—and Mrs. Gibson. The agent was Mrs. Clifford A. Paige, of Grand Rapids, Michigan, an automatist who has done successful work in calling ESP cards. A case concerned with her automatism was presented in this JOURNAL, Vol. XXXVI, January, 1942, pp. 31-35.

On November 8, 1935, Clifford Paige telephoned us in the evening to say that the control personality of Mrs. Paige, Amelia, had a message for us that we should attempt to get automatically. Mrs. Gibson and I therefore got out an ouija board and tried for the message; we received a broken automatism speaking of an earthquake near San Francisco. I thereupon called Mr. Paige and told him what we had obtained. He said that we had not received the message. In the past, he had furnished me with carbon copies of the Amelia script. During this telephone conversation he stated that he would give me no further copies until we had managed to receive Amelia's message by means of the board.

On November 18th Mr. Lago (hereafter referred to as H. L.) and Miss Barnes were visiting us and they tried to operate the board, producing some blank verse regarding "bowemen hunting the roe-deer." This automatism was similar to other productions of H. L. at the board and we did not bother to report it to the Paiges as we felt that it had no bearing upon the message.

A further attempt was made on December 10th, the same persons

*Now Mrs. Lago.

at the board. They again produced an automatism dealing with early England, as follows:

"Bright is the day
Clear wind is blowing
Files seaward going
Bright flash of metal
Grene flash of sea
Thick on the sands
Swift-marching files
Are lined foemen.
Tall stands Harald, golden heumed
Quick headed . . ."

We decided that this automatism likewise had nothing to do with Amelia's message and we did not report it to the Paiges.

On December 16th, H. L. and Miss Barnes called on us again, and, following a telephone call from Mr. Paige requesting that we continue the experiment, we decided to try for the message once more. Miss Barnes and H. L. at the board. The script follows:

Board—Know arm or foot troubles you but will pass soon.

E.P.G.—Anything further?

Board—You have it.

E.P.G.—Who has it?

Board—Can't get thru. Consider your troubles unimportant. Try to help me.

E.P.G.—Who is communicating?

Board—Where is other board? (This might have referred to the board at the home of the Paiges, but we took it to mean another board stored away.)

E.P.G.—This is the gold-lettered board.

Board—This board is all right.

E.P.G.—Other place asks call now. (At this point, I called Mr. Paige and reported that the trial to get the telepathic message was again in progress, but that apparently we were not being successful. This was at 10:05 P.M. After the call was finished Mrs. Gibson and H. L. sat at the board and the automatism continued):

Board—5 — 6 5 — 6.

Wing northward slim ship, let your dragon prow
Bite thro the greens that lie between me and my homeland.
Bright fires on the shore will beck you in swift keel
And in the byrre the cock will salute your homing.

(At 10:30 P.M. I again called Mr. Paige and told him that no message seemed to be coming through. He stated that he was going to retire for the night. After a short pause, H. L. and Miss Barnes seated themselves at the board and the script continued):

Board—Quccgfrd—Amel(i)a was here nor has progressed un a new level. Was she here?

E.P.G.—Did you write that, Amelia? Who wrote it?

Board—No one will know for some time.

E.P.G.—Will Amelia introduce us?

Board—Norge. (Norge is the name of the personality regularly appearing in the automatic script of H. L.)

Here the script went back to blank verse and early England and had no bearing on Amelia and her message. When we put the board away it was after midnight. We felt sure at this point that some sort of message had been attempted, but I did not get in touch with Mr. Paige until the following noon. At that time I read our script to him and he stated that we had made a "hit." It seems that Amelia's message was to the effect that she had just been "welcomed on level 6." Excerpts from the Paige sittings follow:

—A—

Extract from sitting of Nov. 8, 1935, with Dora Paige (using the ouija board).

Notes by Clifford A. Paige Time, 8:30 P.M.

Amelia—I have something to tell you tonight.

C.A.P.—O. K.

Amelia—This noon I told you that I was better. I must have been for today I was welcomed on level six.

C.A.P.—Isn't that fine.

Amelia—I am so happy. You tell E. P. G. (Edmond P. Gibson) to ask Manley to give him a message from me and see if he can get it through to them. (Note by E. P. G. Manley was a personality who had appeared in automatisms both at the Paige's home and at ours, in connection with the Amelia scripts.)

—B—

Extract from sitting of Dec. 16, 1935, with Dora Paige.

Notes by Clifford A. Paige Time, 7:45 P.M.

Amelia—Call E. P. G. and ask him to go on the board right now

and Manley will give him, or try to give him, my message. (Telephoned Gibson—C. A. P.)

(Later)

Amelia—Write a figure "6" on a piece of paper and concentrate on it for a time. That should help. (We did this—C. A. P.)

The manner in which the apparent extra-sensory transmission took place seems interesting. It came in the midst of some semi-poetic automatism and the first part of the message was received by H. L. and Mrs. Gibson sitting at the board; that is, the disconnected numbers 5-6, 5-6, repeated twice. The balance was received when H. L. and Miss Barnes were at the board a few moments later; that is, the announcement of the progression to a new level. About two years previously Amelia had said that she was on level four, and at a later date she announced that she was on level five. These pronouncements as to "spiritual planes," however, were unknown to H. L. and Miss Barnes. They had no particular meaning or significance for Mrs. Gibson or myself. In any case, we would have no reason to anticipate Amelia's elevation to a higher level, nor any reason to choose this topic from among the thousands of others mentioned in her script as a possible subject for the telepathic experiment. It might also be noted that the significance of the 5-6, 5-6, received by H. L. and Mrs. Gibson at the board, was not appreciated by any of our group until *after* the supplementary message anent the progression to a new level was received, this time with H. L. and Miss Barnes at the board.

The case seems to afford evidence that a rather complex message may be transmitted telepathically. I feel that chance coincidence is an unsatisfactory hypothesis in view of the detailed character of the material received. This did, however, come in a slightly garbled form—which is true of most of the automatisms I have witnessed. The Paiges lived three and a half miles from our home. H. L. and Miss Barnes knew them only casually through their association with us.

It is worth noting that both H. L. and Miss Barnes did extremely well, as had Mrs. Paige and Mrs. Gibson, in card tests for extra-sensory perception. All four of them reached significant anti-chance scoring levels in tests conducted by techniques developed by Dr. Rhine. (Their scoring has been reported in a short monograph in the *Journal of Parapsychology,* Vol. I, December, 1937, pp. 264-275.)

Mr. Clifford A. Paige, of Grand Rapids, Michigan, has been kind enough to send us corroboration for the above incident. His letter is dated March 1, 1943:

Mr. Edmond P. Gibson has submitted a paper to me, dealing with a case of experimental telepathy by use of the ouija board, in which Mrs. Paige was the instrument. This occurred in 1935.

I have gone through my notes for that period and find that Mr. Gibson is entirely correct in all the statements he makes.

I might say that this was the only case where we tried to pass a message, telepathically, from home to home, and we felt that the results were most gratifying.

C. A. PAIGE

II

Mr. Gibson has also sent us an account of what may have been an instance of spontaneous telepathy. He writes, under date of January 14, 1943, as follows:

In the fall of 1933 Mrs. Gibson and I lived in an old, rambling house which had a small back bedroom connected to the front of the house by a narrow hall that led around the side of the bathroom. This was due to the earlier construction of the house, in which the hall, the bath, and the back room had been originally a large bedroom. The back bedroom was not visible from any part of upstairs except to a person who had journeyed along the hall and stood directly in the door. This bedroom was poorly lighted and was principally used as a storeroom for crippled household goods. In it was an old roll-top desk where I would occasionally work when the children's noise was disturbing.

According to my notes made at the time, I find that on the 25th of September, 1933, I had been typing in the back bedroom and had to get into my old army locker trunk for some papers. There was a pile of clothing on the trunk and it must have been in my way when I opened it. Before closing the trunk I tossed the clothes inside—without especially looking at them. I thought no more about the matter. This happened in the evening.

The following day Mrs. Gibson asked me if I had seen some clothing belonging to our daughter Barbara. I replied that I had not. (No memory of what I had done subconsciously the previous evening associated itself with the loss of the clothes, and Barbara, then aged four, did not know where they were.)

Mrs. Gibson went upstairs to search. She looked in Barbara's room and could not find the missing garments anywhere. Then she had an unaccountable urge to go to the back bedroom. Here she went straight

to the old army locker and opened it, without knowing why. There, on the top, lay the missing clothing.

As I said before, the storeroom was generally disused. I seldom typed there and the trunk, which stored old letters, relics of the First World War, shot-gun shells, and the like, was not opened twice a year.

I had no conscious memory of Barbara's having visited me while I was typing in the room the day before, although she might have stopped in, dropped some of her clothing on top of the trunk, and gone on her way. Likewise, I do not remember having carried the clothing into the room. I did later remember, however, dropping a pile of stuff into the trunk.

This may be an instance of extra-sensory perception acting at unconscious levels in a very practical manner. The place where the clothing was found was probably the least likely place in the house—with the exception of the coal bin. Barbara seldom went into the storeroom.

The interesting feature of the episode is this: Mrs. Gibson made a thorough search of Barbara's room for the clothes, after asking me if I had seen them. She then walked into the back storeroom, opened the trunk, and pulled them out. She is certain that she had not been in the room previously, but had found the clothes through following her "hunch."

On February 14, 1943, Mrs. Gibson wrote us about the episode of the missing clothing, as follows:

There does not seem to be much that I can add to Mr. Gibson's report. His notes were written at the time and are substantially correct. I might add, however, that the clothing he mentioned consisted of Barbara's shoes and socks. I hunted for them without any result and then put the matter from my mind. The finding of the clothes was not due to a conscious "hunch," but rather to a sort of automatic action. I walked into the storeroom, opened the trunk, and there were the missing shoes and socks. Apparently I had no *conscious* purpose in going in there and I was not consciously thinking of the clothing when I opened the trunk.

LOTTIE H. GIBSON

III

The following incident, an apparently precognitive dream, has been sent to us by Mr. Forrest White, now a student in his second year at the Medical College of Virginia. The

case is very much weakened by the fact that Mr. White no longer has his contemporary record of the dream and has lost touch with the two students who were concerned with its apparent fulfillment. Nevertheless, partial corroboration has been obtained by Mr. White, and we thus feel that we are justified in presenting the case in the JOURNAL. We suggest that readers come to their own conclusions as to its evidential value. Mr. White has asked us to make it clear in our introduction to his account that he is "at some variance with the general interpretation the Society seems to place on the cause and meaning of psychic experiences, precognitive dreams, and the like." This we gladly do, pointing out, however, that the Society does not hold any corporate view as to the cause or meaning of the cases reported in the JOURNAL. Mr. White first wrote us from the Medical College of Virginia, Richmond, Virginia, under date of January 20, 1943:

At the Richmond City Library this afternoon I happened to pick up the JOURNAL of your Society and, thumbing through it, noticed a reference in an article, "Some Unusual Experiences," to the book *An Experiment with Time* by J. W. Dunne. Since I had read that book with great interest a few years ago, I read the above-mentioned article and several others in the January issue. I noticed that you request readers to send in experiences of their own concerning spontaneous psychic phenomena; therefore perhaps a dream experience of mine, which occurred in February or March of 1940, will interest you.

In accordance with the suggestions of Mr. Dunne in *An Experiment with Time,* I kept a record of my dreams for a period of some weeks. I had occasion to suspect that several of my dreams had a degree of identification with events which occurred in the ensuing days, but one stood out above the others as a clear-cut example of such identification. The dream itself was:

I was adding a column of figures for a friend of mine, leaning over his shoulder as I did the work. I added the column once, getting an answer of 60; I repeated the addition, obtaining 54. Checking it again, I found that neither of these answers was correct. (The correct result, if I actually obtained it in the dream, was forgotten by morning and thus not written down, but I vividly remembered the two incorrect answers.)

The next night the following occurred: I was at the time one of several junior and senior students at Randolph-Macon College who were conducting short coaching classes for freshmen. On the evening in question, I was working with a group of five or six students in algebra. After we had discussed the method of working a particular type of problem, I picked several problems at random for the students to try for themselves. Two of them worked separately on the same problem. The answer was of the type: $ax^4 + bx^3 + cx^2 + dx + e = 0$. The results of the two students agreed except for the term *e*. One had obtained 60, the other 54. On checking the work myself, I found that the correct term was 58. (Neither of these students was the one of whom I had dreamed the night before, nor had I ever had occasion to help the latter with algebra or any other subject.)

My dream of the night before had been forgotten. I was therefore greatly surprised when, following my usual custom, I read over my notes on my dreams of the night before—this just before retiring. There was the record of my helping a friend with a mathematical problem, of two incorrect answers being obtained before a check revealed the correct answer, and, most startling of all, ***the numbers 60 and 54 were the same as the incorrect answers my students had obtained.***

I regret that I no longer have the written record of the dream, and that I did not obtain written statements as to the facts from the two students concerned. I did, however, ask one of them the next day if he remembered the problem and the incorrect answers. He did, and corroborated the fact that they had been 60 and 54. I am quite sure of the details, for I repeated them at the time to several friends and also to my mathematics professor. I have kept the facts fresh in my mind by repeating them to others on several occasions since.

On January 21, 1943, we wrote to Mr. White and asked him if he could obtain statements from one or more of his friends to whom he had repeated the details of the incident. We also asked him if he could tell us something about the quality of the dream, whether it was more vivid than his ordinary dreams, and so forth.

On February 4th Mr. White answered, in part, as follows:

. . . I have written to a friend who I hope will remember the details of the dream, which I discussed with him and which he heard me repeat at a meeting of the *Chi Beta Phi* Scientific Fraternity,

of which I was then (in 1940) president. This meeting took place in March, 1940. I hope to be able to visit my former college within the next two weeks and will then look up the records of *Chi Beta Phi* for specific dates and further corroboration.

On February 21st Mr. White very kindly sent us a further communication.

My attempts to get some sort of evidence concerning my dream have been both successful and unsuccessful. I was unable to find the account of my review of *An Experiment with Time* (in connection with which I related my own "precognitive" dream), in the records of *Chi Beta Phi*—the society before which I gave that review. I did, however, obtain the enclosed statement from a former classmate at Randolph-Macon. He remembered the dream well, having heard me repeat it several times, and having told it several times himself. When I asked him if he remembered it, he could recall all the details except the actual numbers involved, and these he remembered after I told him. I enclose his entire statement.

There was absolutely nothing in the dream itself to distinguish it from any other dream, either in vividness, clarity, or feeling-tone. It was entirely inconsequential, and merely a part of a long, loosely-jointed, and equally inconsequential dream. I am quite sure I would never have remembered it in the morning if I had not at that time been making a special effort to remember my dreams. I did not recall it the next night until I read over the account I had written. Occasionally I have dreams or waking-hour impressions which are rather vivid, and which I think may be significant, but nothing has ever come of them.

Mr. White's friend and former classmate at Randolph-Macon College is Mr. G. Douglas Nelson, of Bethlehem, Pennsylvania. Mr. Nelson's statement, dated February 7, 1943, follows:

I have been acquainted with Forrest White since September, 1937; I was a classmate of his at college (Randolph-Macon College, Ashland, Virginia) from September, 1937 to June, 1941.

During our junior year, September, 1939 to June, 1940, I know that Mr. White read the book, *An Experiment with Time,* and that, experimenting on his own, he recorded his dreams.

He told me the following incident, since it concerned men we both knew. Mr. White said, "I dreamed that I was adding columns of figures, one in particular. I obtained the sum of 60 one time, 54

another, but when I checked them I found neither was right. The next night I was coaching some boys in algebra and gave them some problems. Two of them, Edgar Walker and George Meyer, worked the same problem and got the same answer except for the constant term. One had 60, the other 54*; on checking I found that neither was correct."

Forrest White repeated this incident at a meeting at which I was in attendance. On this occasion he gave a review of *An Experiment with Time*.

I have personal knowledge of the following facts: (a) Forrest White was recording his dreams in 1940, among them the foregoing; (b) he was conducting a coaching class in mathematics during our junior year; (c) he told me, as well as several other students, of his "precognitive" experience; and (d) he repeated it at a meeting of *Chi Beta Phi* National Scientific Fraternity, at which meeting I was in attendance.

G. DOUGLAS NELSON

IV

The following case, a dream that would seem to have been of a typical quasi-precognitive type, is contributed by L. A. Dale, of the Society's staff. Alternative hypotheses to precognition will be briefly discussed after the presentation of the incident. On December 30, 1942, Mrs. Dale wrote, at the request of Dr. E——, an informal account of the dream and its apparent fulfillment. Only minor verbal changes in the original statement have been made here:

Preliminary Note. On Saturday afternoon, December 19, 1942, I (LD) took, under the direction of Dr. Murphy, Dr. Taves, and Dr. E——, one and a quarter grams of chloralose and a hundredth of a grain of scopolamine for experimental purposes. Dissociative phenomena occurred, but on arriving home at about six-thirty P.M. I thought I had returned to a normal state. This may not, however, have been the case; Dr. Taves tells me that I called him up at about eight o'clock to ask him for details about the experimental period. This I would not normally do, and I have no recollection whatever

*Mr. White makes it clear that Mr. Nelson's recollection of these exact numbers is not independent.

of having made the telephone call. I went to bed at about midnight and had the following dream:

I found myself in a large room on the ground floor of a house which I took to be in Florida. I had never seen the room before and I do not know why I assumed it was in Florida. It seemed to be the home of some members of the Society, the Mesirows, with whom I have been in close contact recently. (The Mesirows do not live in Florida, but in Philadelphia, and I have no reason to think of them in connection with the South.) My little dog Myra was lying on a chair towards the side of the room, and there were people present, but I did not see them—only knew they were there. I was pushing a vacuum cleaner along a rug. Then occurred something that filled me with the utmost horror—the vacuum cleaner became *alive* and possessed of a malignant will of its own. It began to heave in and out, or bulge and deflate—I do not know quite how to express it—and I realized I was in great danger, for it was going to explode. I dropped the handle and ran for the door. As I ran I knew I was doing an unspeakable thing—I was leaving Myra to be killed in the explosion. I did not stop to rescue her, however, and just as I reached the door there was a terrible concussion (no sound) and I found myself safe on the lawn. Then followed a somewhat less vivid sequence, the gist of which was this: a man who had been present had attempted to rescue Myra. He had been killed—someone told me this. Then I had Myra in my arms; she was moving, burned, suffering, but at the same time she was dead. At this point I woke up and, looking at my clock, saw that it was five A.M. I was completely disoriented and thought that the dream was real. I went over to the sofa and saw that Myra was all right and sleeping quietly. After a few minutes I realized it was only a dream. I went back to bed and slept until nine o'clock. When I awoke, the memory image of the vacuum cleaner becoming animated, human, and the terrible "heaving in and out motion" that it made, remained utterly real to me.

Later on that Sunday morning the dream was still very much on my mind and I called up Miss Wellman* to tell her about it. I very rarely have a dream as vivid as this one; in fact I can seldom remember my dreams. To the best of my knowledge this was the first time I ever told Miss Wellman a dream of mine; in any case, it certainly was the first time I ever called her up for the express purpose of doing such a thing.

On Tuesday, December 22, 1942, my mother came down from the country and that evening she took me out to dinner. During

* Miss Wellman is the Executive Secretary of the Society.

dinner we decided to go to the movies. It was by that time rather late, so we went to a newsreel theater. I do not remember the name of the theater, but it is on East 42nd street.* As we entered the theater a comic cartoon in color was being shown and the scene was as follows:

A dog salesman was demonstrating a vacuum cleaner to a Scotchman—pushing the cleaner along the rug of a ground floor room in what I later learned was a castle in the country. The room was not particularly like the room of my dream except for the fact that it was very large and on the ground floor. It might also be noted that it was in the country, which was also the case in my dream. Suddenly the vacuum cleaner, which until then had appeared to be a perfectly ordinary one, "came alive" and began to suck up some liquor from bottles under a grill-work in the floor. Then it began to bulge, heave in and out, and finally with an explosive movement it sent the dog salesman and the Scotchman spinning to the other side of the room. I do not recall just what happened after that, nor would it seem to be relevant in the present connection. Neither the dog nor the Scotchman was hurt, and there was nothing horrifying *per se* about the cartoon. The thing that struck me was the vacuum cleaner coming alive and heaving in and out exactly as the cleaner in the dream had done. I do not by any means wish to stress that the *details* of the cartoon scene were like the dream scene. Except that there was in both cases a large room on the ground floor of a place in the country, they were not similar. I might add that I am very much the opposite of a "movie fan" and have not been to the moving pictures more than three times during the last two and a half years.

On the Saturday following my visit to the newsreel theater I told Dr. Murphy and Dr. Taves about the whole episode. They encouraged me to get corroboration from Miss Wellman and from my mother. Miss Wellman wrote, under date of December 30th:

"This account† of the dream is substantially the same as the account that Mrs. Dale gave me over the telephone on Sunday morning, December 20th, 1942."

ADELE WELLMAN

On January 2, 1943, I wrote to my mother, Mrs. Lawrence F. Abbott, of Cornwall, New York, enclosing a copy of my informal account, and asking her if she could testify as to the accuracy of

* I later found out that it was the Embassy Theater, on 42nd Street opposite the Grand Central Station.

† That is, the account written out for Dr. E——.

my recollection of the "vacuum cleaner" scene in the cartoon. On January 5th she answered:

". . . Your letter brings back to me accurately the memory of the animated cartoon we saw the night of December 22nd on East 42nd Street. Every word of the description you give is correct as far as I can say."

It may be profitable to discuss briefly several possible interpretations of the events presented above. The first hypothesis that comes to mind is the chance hypothesis. It is, of course, quite impossible to prove that there is anything more than a chance connection between the dream and the cartoon seen three days later. One point, however, might be kept in mind in this connection; that is, the number of coincidences between the dream scene and the cartoon scene. These coincidences were:

	The dream scene	*The cartoon scene*
1.	A vacuum cleaner coming alive	Vacuum cleaner coming alive
2.	Heaving in and out	Heaving in and out
3.	Dog important in dream	Dog salesman demonstrating cleaner
4.	Action takes place in country	Action takes place in country
5.	Large room	Large room
6.	Ground floor room	Ground floor room

In commenting on these coincidences, it might be relevant to say that I live in a small one-room apartment on the *top floor* of a typical city apartment house. Such house-cleaning as I do takes place in this environment.

Two more possibilities remain to be discussed, both of which, if accepted, would lead to a normal explanation of the incident. The first of these is concerned with the possibility that I might have seen an advertisement, either in the newspapers or displayed in the lobby of some moving-picture theater, depicting the vacuum cleaner in the act of coming alive. Should this have been the case, it might have provided me with the essential material for the dream, the fact of my subsequently seeing the cartoon then being more easily attributable to chance. So far as I know, cuts from cartoons are not used as advertisements in the daily press. I have ascertained that the Embassy Theater does exhibit a poster showing a single scene from its current cartoon; it may be noted, however, that I have no occasion to pass the Embassy, or any other moving-picture theater, on my usual route from home to the office (I live on 38th Street and the offices of the Society are on 34th Street) and, also, that due to nearsightedness I

would be quite incapable of distinguishing the details of such a poster unless I deliberately approached it. This I am sure I have never done.

The more serious criticism against a paranormal interpretation is concerned with the possibility that I may have retained a false memory of the content of the scene in the cartoon; that is, I may have *imagined* that I saw the vacuum cleaner coming alive, and so forth, when in fact it may have done no such thing. My mother's statement does not entirely dispose of this possibility for, as stated above, I sent her my informal account and asked her if she remembered the scene as I described it. It is conceivable that this description brought about pseudo memories. This question could easily have been disposed of if a third person had seen the cartoon, then giving an independent description of the scene under discussion. Unfortunately it was not practicable to trace the whereabouts of the cartoon after it left the Embassy Theater.

If none of the above possibilities recommend themselves, a paranormal "explanation" is left to be considered. Theoretically speaking, precognition is not necessarily demanded by the sequence of events. The cartoon existed *per se,* and was familiar to many persons, long before the dream took place, thus leading to the possibility that its salient features become known to me by extra-sensory means, these features then working themselves into the fabric of the dream. My own introspections (for what they are worth), however, lead me to believe that the process *was* a precognitive one; that is, that in the dream I *anticipated my own future experience.* Although the cartoon already existed, I could not normally have known what my future relationship to it would be. One or two tentative psychological observations may perhaps be made:

Mrs. Kenneth Richmond, in discussing possible motives for a precognitive dream of her own (*Journal* S.P.R., June, 1939), says that most of her psychical experiences seemed to come in response to a *need*—either of her own or of another's. In Mrs. Richmond's case, she had an apparently purposeless precognitive dream on the night she heard Mrs. Lyttelton's Presidential Address, during which address Mrs. Lyttelton urged S. P. R. members to put on record their precognitive dreams. It might be said that Mrs. Richmond's experience was generated by a *need* to "give" Mrs. Lyttelton an example of precognition. I believe that my own dream similarly answered a need—a two-fold need:

(1) It will be remembered that prior to the dream I had been under the influence of a combination of drugs administered in the hope of producing a state favorable to the emergence of paranormal phenomena. During the actual drug session some quantitative experi-

ments were tried, but the resultant scores were totally without interest. In spite of large "blank spots" in my memory of what occurred, I was well aware of the fact that the experiments were negative—that I had "failed," and that I had "let down" the experimenters. I knew that Dr. Murphy and Dr. E—— were especially interested in the problem of precognition; we had often discussed it and I had told them of some earlier dreams of mine. They seemed interested. Perhaps I could make up for my failure in an experiment which had cost them time and trouble by "giving" them an example of precognition. (I speak, naturally, of a hypothetical process—taking place at some subconscious level.)

(2) The drug session, I believe, produced a good deal of uncanalized anxiety; the sleeping personality searched its immediate future for events which would *justify this anxiety,* weaving together such future events and already existing overt fears (the fear of something happening to my dog) into a typical incoherent dream sequence. In support of this theory, it may be said that two other dreams which I believe were precognitive (uncorroborated, unfortunately) occurred at a period when a personal decision to be faced was causing considerable generalized anxiety. The first of these dreams concerned a dog being run over, the second, the mutilation of a woman's face as the result of an automobile accident. The dreams and the objective events which seemed to fulfill them were undoubtedly connected with two of my most deeply-rooted fears.

It might be interesting if other people experiencing precognitive dreams—which at first sight so often appear to be both senseless and purposeless—would analyse them with a "need hypothesis" in mind.

Postscript. Just as this issue of the *Journal* was about to go to press, Mrs. Lydia W. Allison very kindly gave me some information which enabled me to discover the whereabouts of the animated cartoon discussed above. The cartoon, entitled *The Raven,* was being shown at the Winthrop Theater, Brooklyn, on Sunday, March 14th. On that date I took a friend, Miss Ruth Kauffmann, to see it. Her testimony as to the vacuum cleaner scene makes the hypothesis of pseudo memory untenable. Miss Kauffmann dictated the following statement a few hours after seeing the cartoon:

Having read Mrs. Dale's account of the dream and having just seen *The Raven,* I can say that, except for a few details, her memories of the vacuum cleaner scene are essentially correct. Actually the scene was as follows: A mongrel dog and a raven called upon a Scottish terrier in order to demonstrate a vacuum cleaner to him.

While the raven was pushing the cleaner along the rug in a large ground floor room, the mongrel dog went upstairs and attempted to break into the Scottie's safe. The cleaner came alive in the way Mrs. Dale described, sending the Scottie dog spinning to one side of the room, but she forgot that when the cleaner was going through its contortions the mongrel upstairs set off a charge of dynamite—causing a great explosion to shake the room. I should also like to point out that the castle in which all this took place was set against a background of highly colored trees and shrubs, and that the whole scene was bathed in brilliant sunlight, this indicating either that the action took place in the South, or in the North during the summer.

RUTH KAUFFMANN

It will be noted, of course, that my original recollection of the cartoon was wrong in several respects; that is, the "Scotchman" was actually a Scotch terrier (my own little dog Myra is a Boston terrier) and the principal salesman was a raven, not a dog. These errors of memory, however, did not tend spuriously to increase the number of coincidences between dream and fulfilling event. On the other hand, two items in the cartoon which I had forgotten by the time I wrote my original account—the explosion and the background suggestive of the South—would seem to strengthen the case.

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CONTENTS

Predicting Good and Bad Scores in a Clairvoyance Experiment: A Preliminary Report*

GERTRUDE RAFFEL SCHMEIDLER

Many of the experiments from psychological laboratories that are concerned with ESP have given evidence for the existence of telepathy or clairvoyance. But sometimes the results are negative; and often this occurs when the experimenter seems to have been biassed against ESP in the first place. Presumably the experimenter is an honest man, who would not knowingly alter his data in the direction of his bias. Yet it is possible that he unconsciously would select conditions which on the surface were unimportant, but which actually influenced the subjects so that they produced low scores.

The experiment to be described is an attempt to show the importance of such conditions. The subjects were divided into two groups, one of which was expected to get results of chance or lower; the other was expected to score

* This experiment was conducted for Dr. Gardner Murphy, who is Hodgson Fellow at Harvard University. His advice determined both the topic and its present form; and his criticisms and suggestions have guided the course of the research.

above chance. "Objective" conditions were the same throughout, but social atmosphere was not. Most of the differences in procedure between the groups were apparently so trivial that they might not even have been mentioned in another report; and yet here they seem to have determined the presence or absence of ESP.

Procedure

Points Common to Both Groups

Two experimental rooms were used; the subject sat in one and the stimulus cards were in the other. The shortest distance from the subject's chair to the cards was 42 feet, on a line which passed through the walls of two intervening rooms and a corridor. One of the experimental rooms was my office, a corner room with two windows, which was bare but not unpleasant. The other was a "dark room" planned for visual experiments. There were no windows in it, and the walls and ceiling were painted black. The paint was peeling away in places, and it held an accumulation of papers and of dusty bits of apparatus left over from earlier experiments. It was lit by two unshaded electric light bulbs.

The usual ESP cards were used, arranged according to a table of random numbers which is approved by statisticians for such purposes. The cards were prepared by an assistant, who first noted the order on prepared forms, and then made up decks to correspond. Each list was numbered. Thus there is a permanent record of the stimulus cards.

The subject was first shown sample ESP cards, and told that he was to guess a deck of 25 of the cards, arranged in random order. He was seated before a table on which were record forms, a pencil, his instructions, and the sample cards, as well as a telegraph key, which he pressed at the end of a run. I then left him alone, with the door shut, and waited in the other experimental room for the signal that he had completed 25 guesses.

During the time the subject was guessing, the stimulus deck was lying face down in the other office, next to the list which described it. Neither the cards nor the list had

been seen by the subject before his guesses; nor did I know what they were at the time when I saw and spoke to the subject. No one who knew the procedure thought it possible that the subject was guided in his guesses by any sensory cues as to the order of the cards. Further details of the method appear below.

A maximum of one hour was set for the experimental period.

Scoring was done by me, and checked by an assistant. It seems unlikely that many errors escaped us both.*

Points in Which the Two Groups Differed

The most important difference between the groups was in their own expectation of what their scores would be. There are some people (and they seem to be particularly in evidence among psychologists) who are firmly and blindly opposed to the possibility of psychic phenomena. One of them has recently told me, "If it were true, I wouldn't believe it." Now it is intuitively clear that a person who resists ESP so strongly should do badly at it, even though he *says* he is willing to cooperate in the procedure. In many experiments such skeptics would be included among the subjects who were expected to score above chance; and this might reduce the scoring level of the group as a whole. In this experiment the people who "knew" beforehand that ESP could not occur were kept in a class by themselves; and their scores contrasted with those of the more open-minded subjects.

But even the skeptics showed considerable variation in their attitudes. Some were profoundly uninterested in the whole problem, but willing out of simple good will to give me an hour of their time. Others were more emotional; one especially was eager for personal reasons to prove that psychic phenomena did not occur. It is my guess that on the whole such emotional attitudes are apt to result in scores well below the level of chance. Lack of interest will prob-

* A third check is now being made and will be completed before Dr. Schmeidler's final report is presented.—*Ed.*

ably result in chance scoring; and members of the A.S.P.R. would be predicted to score above the chance level.

There are other complicating factors, such as always enter into each session to make it unique. If the subject is self-conscious and trying too hard, if he has a hangover, or a seminar report due for that evening, or a bad cold, his scores may go down. If he has quiet self-confidence and is in a good humor, his scores should go up. But taking, by and large, the subjects who think that ESP may or does occur, as opposed to those who think it cannot possibly happen, I predicted scores above chance for the first, and at or below chance for the second.

Having subjects with favorable or unfavorable attitudes to begin with, I tried to accentuate those attitudes, within normal experimental limits. Both my rooms were generally considered quite suitable for research; so I put the skeptics in the "dark room" and their cards in my office, reversing this for the open-minded group. There was a battered laboratory table in the "dark room"; I left it there for the skeptics to work on—and as often as not, supplied them with the stub of a pencil, not overly sharp. For the other group I used a table with a fairly nice surface and set it under a window so that they could look out at Harvard Yard. Pencils were always freshly sharpened. There were cigarettes and an ashtray available; sometimes I offered them candy. Probably none of these differences mattered very much to the subjects (with the exception of the candy!) and yet they may have served to establish something of the atmosphere of dullness and drudgery on the one hand, and friendly cooperation on the other.

When a skeptic finished a run, he pressed a telegraph key which flashed a small light in the room where I was waiting. I then took away the deck he had been guessing; put the next one on the table; and pressed the corresponding telegraph key, to signal that he could begin another run. The signal in his room was a rather raucous buzzer.

With the more docile group, there was a longer interruption after each run. As soon as a subject signalled that he was done, I would go in to him with a list of the cards

he had been guessing. We would go over the scores together, discuss the methods the subject used, his impressions of success or failure, or anything else we wanted to talk about. This took up so much time that there were never more than ten runs in an hour; whereas in the same period there were fifty runs with the other procedure.

Results

The results came in pretty well as it was predicted they would. Sometimes the skeptics (called the "goats") scored above 5, which is the score expected by chance; and sometimes the open-minded subjects (or "sheep") scored below 5; but after a few weeks of experimentation the mean for the former group was 4.9 and for the latter was 5.4. A statistical test showed that such a difference, small as it may seem, would occur by chance only three times out of a hundred—in other words, that the odds are 97 to 3 that the goats would again do worse than the sheep if the experiment were repeated.

The logical next step was to repeat the experiment, to make sure. When I did, the sheep did less well, averaging only 5.2; readers of this JOURNAL will recognize the Midas touch.* The goats again had an average score of 4.9. The same statistical test applied to the data—now twice as numerous—shows that the odds are somewhat better than 99 to 1 that skeptics would again get poorer ESP scores than open-minded subjects under these conditions. The experiment is now being done still another time. Table I shows the actual figures for sheep and goats.

Describing only the averages omits some of the most interesting points of the research. The following details partake more of the nature of hints than of established facts. But there were dozens of such hints, some weak and some fairly strong; and collectively they may suggest some of the factors which operate in clairvoyance.

Take the subject who vehemently resisted ESP but was consciously trying to cooperate and to guess the cards

* See "The Midas Touch in Psychical Research," appearing in the April, 1943, number of this JOURNAL, and "A Further Report on the Midas Touch," this issue.

Table I

Analysis of scores obtained by skeptical and open-minded subjects.

	Open-Minded Subjects	Subjects Who Expected to Score at Chance
N (in decks)	257	376
Deviation	+84	−35
Mean	5.327	4.907
C. R.	2.62	−.90
P	.01	.18
$Mean_1 - Mean_2$	.420	
Sigma Diff.	.165	
C. R.	2.55	
P	.01	

right. While she was doing the experiment she imagined a deck of cards being flipped over, and wrote down each card as she "saw" it. That is not an uncommon description; many good subjects report similar visual images. Was it only accident that her score for fifty runs averaged 4.46, well below chance?

I think not. The picture of what actually went on is surely a complicated one; and it seems to me that at least these three factors must be somehow involved. (1) Her mother and grandmother seemed to have psychic powers. Perhaps she is potentially a little better at ESP than the average person. (2) She hoped to get low scores, to prove that ESP cannot occur; and therefore directed herself, unconsciously, *not* to get the cards right. As a result she overshot the mark, with scores averaging worse than chance. (3) On the conscious level, she considered it her duty to cooperate as a subject, and to obey whatever instructions an experimenter (no matter how misguided) would give her. Therefore her conscious experiences were the same as those of the other subjects who really were cooperating.

Certain subjects who were classed with the sheep also got low scores. By now I think I can spot one type which

is likely to do badly: the boy or girl who is sincere, shy, and self-conscious—who tries too hard. My guess is that the very good student will do badly too. If he is in the habit of pushing himself to the utmost, his attitude is too intellectual for ESP. And another variation on this theme: the tense person who is so self-critical that he seldom acts spontaneously, but always tries to think a problem out before committing himself on it. He is apt to find himself trying to reason about his guesses; and when he does, the scores usually drop below chance.

One negative finding still piques my curiosity. Previous research gives a great deal of evidence, both direct and inferential, to show the importance of a subject's mood in getting good scores. And yet when my subjects rated their attitude, during each run, as to interest versus boredom and enjoyment versus dislike, there was no relationship at all between their interest (or enjoyment) and success.

Yet even with them I am sure that a relationship exists. Scores on the first run of a session averaged higher than for all the other runs combined; and probably just on that first run a subject's interest and curiosity are keenest (See Table II). The unfortunates with examinations scheduled

Table II

Comparison of first deck called at a session with all other decks.

	First Deck called at a Session, All Subjects	All Remaining Decks
N (in Decks)	37	596
Deviation	+27	+22
Mean	5.73	5.04
C. R.	2.21	.05

for later in the day did not do too well. Some of the boys made their best scores after eating candy; some of the girls after being given a chance to talk about themselves. Can it be that they themselves do not know when they are inter-

ested and contented? Or is there some subtle difference between the surface mood they describe and a deeper emotional readiness to do well or to do badly? The point may seem a slight one; but in my opinion it is worth further study.

The experiment is still under way; and no formal conclusion can be in order. But both the quantitative data and my own impressions from watching and listening to the subjects point in the same direction, implying in the first place that clairvoyance occurs, and in the second that it is related to a variety of psychological factors which may facilitate or hinder it.

A Further Report on the Midas Touch

ERNEST TAVES, L. A. DALE, and GARDNER MURPHY

In the April, 1943, issue of this JOURNAL two of the present writers reported experimental ESP results which were characterized by various decline effects (1). These effects we attributed to a "Midas touch in reverse" on the part of the subject, the experimenter, or both. It was shown that when subjects called a series of decks of ESP cards, scores were significantly above chance for the first three decks, descending to the chance level thereafter. This effect we attributed to the Midas touch in respect of the subject: as the subject becomes less naïve for a given experiment, he loses his ability to do above-chance scoring. We also presented evidence in support of the view that the Midas touch may work in respect of the experimenter; the present paper, however, does not deal with this aspect of the problem. We want to stress the fact here that the decline phenomena mentioned above were not *a priori* expected in the data in which they were found. On the contrary, the experiments reported on in the first paper had been designed to test an entirely different hypothesis. But the Midas touch effect became so obvious that it could not escape detection.

On the other hand, decline effects *were* expected to appear in the data about to be presented, and the entire experiment was specifically planned to facilitate their emergence. The question is, Does the Midas touch hypothesis possess predictive value? Our results lead us to believe that it does. This new experiment will now be described.

Experiment BA

Subjects: It was necessary, of course, to use naïve subjects in this experiment, since it is supposed that the Midas touch makes itself apparent in the first few runs made by a given subject. Accordingly GM* asked Dr. B. F. Riess

* The entire experiment was carried out under GM's direction, and was made possible by moneys available to him from the Hodgson Fund of Harvard University.

of Hunter College if he could make an appeal for volunteers in the latter's psychology classes. Permission was kindly granted and early in March, 1943, GM addressed the classes and asked for volunteers for a "psychological experiment." The parapsychological nature of the experiment was not mentioned since it was desired that the subjects be as naïve as possible. A total of 45 volunteers was accepted and appointments were scheduled. All of the subjects, therefore, were girls of college age. None of them had previously taken part in formal ESP experimentation.

Methodology and Procedure: Three methods of calling were used in the experiment. Two of these are the AE and AJ methods described in another paper appearing in this JOURNAL (2: pp. 17ff. and also 1: pp. 71-72); the third method is similar to method AE, except that the subject *does not* receive knowledge of results after each individual guess. The letters "wkr" are used to signify "without knowledge of results," so that the new method is called AEwkr.

Each subject was used for three sittings, each of which lasted from fifty minutes to an hour and a quarter, depending on the individual subject's preferred speed in calling. At each sitting the subject was required to call nine decks of standard ESP cards: three decks by one method, three decks by another method, and the final three by the third method. Each subject always did a group of three runs without the experimenter's entering the room. At the conclusion of a group of three runs, and before going on to the next method, the experimenter discussed the scores, and other matters of interest, with the subject. The order of methods used was rotated throughout the three sittings; that is, 15 subjects in their first sittings used the methods in this order: AE, AJ, AEwkr; 15 subjects followed the order AJ, AEwkr, AE; finally, the last 15 subjects called in the order AEwkr, AJ, AE. For each subject, in her second and third sitting, these orders were rotated so that each girl began one sitting with each one of the three methods. We planned this rotation of tasks so that a direct comparison of the methods themselves could be

made. Three methods were used, moreover, in order to give the Midas touch ample opportunity to appear. A subject who has called, say, AJ, may still be considered naïve for methods AE and AEwkr. According to the Midas touch hypothesis, we expected that the *first run of all* (first sitting) would give the highest average. We also expected that the first three runs *of each method* would be above chance, and that the first run of these would be higher than the second or third. It would follow from this, therefore, that a subject would do better at her first sitting than at her subsequent sittings. It was for this reason that we had each girl come for three experimental sessions. These sessions took place at approximately weekly intervals.

LD acted as experimenter throughout the series. The objective conditions of experimentation (distance between subject and stimulus material, preparation of stimulus material, mechanical recording, etc.) were exactly the same as described in a former paper (1: p. 82). The subjects were paid on an hourly basis at the rate of seventy-five cents an hour. In addition, rewards were offered as below:

SCALE OF REWARDS

Score	*Reward*
8	.15
9	.25
10	.50
11	.75
12	1.00
13	2.50
14	5.00
15	8.00
16-25	15.00

This scale of rewards was conspicuously posted on the subjects' panel of the machine, and in most cases LD called attention to it before the subject began her first sitting. When the subject appeared at the office of the A.S.P.R., and after the completion of each group of three runs, the experimenter conversed with her on topics of interest. A real effort was made to create a friendly, informal atmos-

phere; cigarettes and candy were offered, and appreciation was shown for "high" scores. It is impossible to measure, of course, to what extent this attempt to create a "favorable" atmosphere was successful. Most of the subjects, however, said that they enjoyed the experiment and expressed eagerness to participate without remuneration in future experiments. Nothing was said, except in a very few cases, about "chance expectation," and the subjects seemed for the most part naïve on this point.

Results: In the entire experiment 1215 decks of ESP cards were called by 45 subjects—a total of 30,375 individual trials. A plus deviation of 51, giving a mean score of 5.04, was obtained. This over-all result is without significance.

The average score of the *first run of the first sitting* (that is, when the subjects were at their most naïve) was 5.42. Since there were, of course, only 45 "first runs" this value is not in itself statistically significant, but it is in line with the hypothesis stated *a priori.*

In Table I are presented the deviations obtained for the *first three* and *second three* runs called by each method (see 1: pp. 72 ff.).

Table I

Deviations of *first three runs* compared with deviations of *second three runs* (by each method) called by naïve subjects, Expt. BA.

Runs	*N (in decks)*	*Dev.*	*Mean*	*Dev.*	*Mean*
1	135	+40	5.30		
2	135	+ 6	5.04	+40	5.098
3	135	— 6	4.95		
4	135	+ 4	5.03		
5	135	—26	4.81	+ 2	5.005
6	135	+24	5.18		

It will be seen from the above table that, as predicted, the average score of the first run for each method is higher (deviation = +40) than the average score for the second (+6) and third (—6) runs.

None of the three methods used yielded a significant mean score; the obtained means for these three methods (N for each method is 405) are shown below:

AE	4.975
AJ	5.044
AEwkr	5.106

The deviations and mean scores obtained on the first, second, and third sittings are as follows:

First Sitting	5.09	(+40)
Second Sitting	5.005	(+ 2)
Third Sitting	5.022	(+ 9)

In Table II we present the *complete* Midas touch material to date. Included in this table are the data shown in Table I above, the original Midas touch material, and all the data collected under the original conditions (that is, exclusive of Experiment BA) subsequent to the publication of the first Midas paper.

Table II

Deviations of *first three runs* compared with deviations of *second three runs* called by naïve subjects, all data (August, 1941, to April, 1943).

Runs	*N (in decks)*	*Dev.*	*Mean*	*N*	$\frac{Dev.}{\sqrt{npq}}$	*P*
1	287	+102	5.35			
2	272	+ 35	5.13	816	3.17	.0008
3	257	+ 44	5.17			
4	242	+ 15	5.06			
5	232	— 35	4.85	699	.32	.37
6	225	+ 37	5.16			

Thus, only eight times in ten thousand similar experiments (each experiment extending over a period of twenty months) would such a result be expected on a chance basis; this result is, therefore, highly significant by standard statistical criteria.

Certain other analyses were made on the data. The following may be mentioned: (a) Salience (in respect of hits on the first card of each deck); preponderance of hits

in the first half of each deck; grouping of hits (runs of three, four, five, etc., successive hits); and abnormal score distributions (preponderance of extreme scores) as tested by Chi-square. Null results were obtained from these analyses.

An isolated case of striking scoring must, however, be noted. One subject, Miss L. L., calling her first deck of AEwkr (seventh deck of her first sitting), obtained a score of 17, the first fifteen cards of the deck being correctly guessed. An unbroken sequence of 15 hits at the beginning of a deck would be expected to occur by chance once in about 30,000,000,000 runs. It will be recalled that all decks used were arranged in accordance with random numbers.

Discussion of results: It is clear that no one method of the three possesses significant advantage over any other—at least under our conditions of experimentation. Further, the mean score obtained by all subjects under all conditions is without statistical significance. It will be noted, however, that the scoring level on Sitting One is higher than on either of the subsequent sittings, and that the average score of the first run of the first sitting was above chance (5.42). These results were predicted on the Midas hypothesis.

We believe that the most important contribution of Experiment BA lies in the fact that a definite type of result was *predicted and was obtained*—namely, a tendency to score high as a function of naïveté, with a subsequent decline to chance scoring. In other words, we suspect it is just possible that we have put the salt on the tail of that *rara avis,* the repeatable experiment in ESP.

In view of the one extreme score obtained by Miss L. L. (as noted above, the odds against correctly calling 15 cards in a row are astronomical), it may be of interest further to discuss the conditions attending this run, as well as the subsequent work of the subject. LD was experimenter throughout the series, but on the occasion of Miss L. L.'s first sitting it happened that ET was present in the room where LD was operating the machine and GM was in another room at the A.S.P.R. office. When the first five

or six guesses registered as correct, LD asked ET closely to observe the rest of the run. This he did. At the termination of the run under discussion GM's advice was sought on how to proceed. He suggested that there should be no change from the usual procedure as described above; that, in other words, Miss L. L. should continue her task of calling the remaining two AEwkr decks without interruption from us. After these decks had been called (yielding scores of 4 and 6) LD entered the room and, without telling the subject of her unusual score, engaged her in conversation. Miss L. L.'s introspections were as follows:

In the "17" run she had started out by looking around the room and certain objects there, which she associated with the five symbols, had successively caught her eye. For instance, when the coils of the radiator seemed to "attract" her she chose the wave; dry cell batteries on a near-by shelf indicated the circle; the corner of the table indicated the star; the square was symbolized by a chair-bottom, and "something she saw in the design of the rug" meant the cross. (Actually there is nothing in the design of the rug, which is of an Oriental type, that even remotely resembles the form of a cross.) Then, as the run progressed, this procedure began to seem too stereotyped, too "automatic," and another approach to the task took its place. She *felt* that she should call, say, a circle, and then she looked around the room until she found an object the outline of which would "justify" her impression. She further stated that after a while (whether towards the end of the "17" run or at the beginning of the next run could not be exactly ascertained) she "got bored with looking around the room and just tried to imagine what symbol Mrs. Dale was looking at, but thought it silly." She was not told at the end of this sitting, as were all the other subjects, what her exact scores were, nor was she paid the reward for her score of 17 until after the last sitting. Her total deviation for Sitting One was +14; Sitting Two, +8; Sitting Three, —4. When Experiment BA was completed, Miss L. L. was kind enough to come on two occasions to take part in further experimentation. The results obtained were of a chance nature.

Summary: An experiment was designed to facilitate the appearance of decline phenomena (Midas touch effects). 45 subjects called, during three sittings each, a total of 1215 decks of ESP cards, using three different methods of calling. Suggestive, but not statistically significant, decline effects were obtained, these effects being similar to those previously noted. When the results of the present experiment are pooled with the results published in the April issue of this JOURNAL, the P-value, based upon the positive deviation of the first three decks called by naïve subjects, is .0008. The Midas trend in the present data is less pronounced than in the previously reported material, but the total result remains statistically significant.

REFERENCES

1. Taves, Ernest, and Dale, L. A., The Midas Touch in Psychical Research, *Journal* A.S.P.R., Vol. XXXVII, April, 1943, pp. 57-83.

2. Murphy, Gardner, and Taves, Ernest, Current Plans for Investigations in Psychical Research, *Journal* A.S.P.R., Vol. XXXVI, January, 1942, pp. 15-28.

Further Consideration of Multiple-Blocking and Unblocking in Normal Subjects

(Constituting a sequel to "Report of Investigations into an Obscure Function of the Subconscious Mind," published in the October, 1942, issue of this JOURNAL.)

HUGH WOODWORTH

[Upon publication of Mr. Woodworth's first paper, we were so impressed with the possibilities of his hypotheses that a repetition of the experiment was initiated at the Society. The results of this experiment are interesting and will be described immediately following Mr. Woodworth's paper. We continue to stand in friendly disagreement, however, with the author on a number of purely statistical points. We have indicated our views in footnotes and in a brief appendix. We shall, of course, grant the author an opportunity to reply.—*Ed.*]

Readers may remember that in "Report of Investigations into an Obscure Function of the Subconscious Mind" a detailed analysis was made of the score results of two subjects, each of whom called 60,000 ESP cards. The purpose of the study was not to present evidence of ESP itself, but rather to demonstrate a peculiar subconscious mechanism in subjects who manifested no ESP ability in the ordinary sense.

To recapitulate briefly, the purpose of the work was to substantiate the following hypotheses:

1. That extra-sensory awareness is a normal ability of the subconscious mind, i.e., that there continually impinges upon the subconscious minds of most (or all) people the very information for which the conscious mind is (at a given moment) seeking. And that this information arrives independently of the ordinary sense channels.

2. That the subconscious mind ordinarily experiences a distaste for such extra-sensory images and concepts impinging upon it, and rejects them, thereby preventing their access to the conscious mind. (This subconscious distaste is called the "blocking" or "rejecting" factor. It operates far below the conscious level.)

3. That the blocking or rejecting factor is variable and accounts for high, zero, and negative ESP results; i.e., that high ESP scoring results from a partial suspension of the blocking factor (or "unblocking"), that zero ESP scoring represents 100 percent rejection of the subconscious information, and that negative or below-chance scoring results from "mutiple-blocking" as explained in the original paper* (pp. 198 ff.).

Now, it has been shown that if a group of scores giving a chance *average,* give at the same time a *distribution* in which there is an excess of both high and low scores at the expense of mean scores, this distribution constitutes supporting evidence of the hypothesis. The scores of H. W. and R——, the two subjects studied in the main paper, gave over-all averages of 5.005 and 5.1075 respectively, the first clearly a chance average, and the second (C.R. = 2.6) indicative of some ESP that had to be taken account of in the analysis.

It remained to be seen, then, whether the *distribution* of scores making up these averages would show an excess of extreme scores at the expense of mean scores. Three types of distribution were investigated, and it was my opinion that the looked-for distortions indeed appeared.

It is here, however, that difficulty arises—the old difficulty of deciding whether a given distortion is a significant one, or can be attributed to the laws of chance. The data in the original paper, as the Editor points out, "are not treated by the currently accepted statistical procedures." This deficiency I now undertake to remedy. In addition I wish to submit the results of a third subject, F——, whose recent 60,000 trials constitute a block of work comparable to that done by R—— and by myself (H. W.).

F——'s experiments were conducted in precisely the same way as were the two other groups of 60,000 trials, but with the added precaution of independent checking of

* Briefly, mutiple-blocking consists of rejection not only of an extra-sensory image impinging upon the subconscious mind, but also of a similar "normal" image, i.e., one that arrived in the mind through the ordinary sense channels.

results. F——'s over-all average is 4.989, satisfactorily non-significant of ESP ability.

It should be noted here that a subject who manifests ESP in the ordinary sense is not a suitable subject for the present study. We are looking for what is in effect a *flattening* of the normal curve (dotted line), with resulting upward thrusts at either end, as shown in Fig. 1. This distortion, by its very nature, can scarcely result from the scoring of any subject whose over-all average is not extremely close to chance. For whenever an ESP factor is at work, the curve of score distribution must show a displacement to the right—a displacement that almost inevitably precludes persistent outcropping above the normal curve of any low-score totals. Fig. 2, showing the graph of a hypothetical slightly clairvoyant subject, illustrates the point.

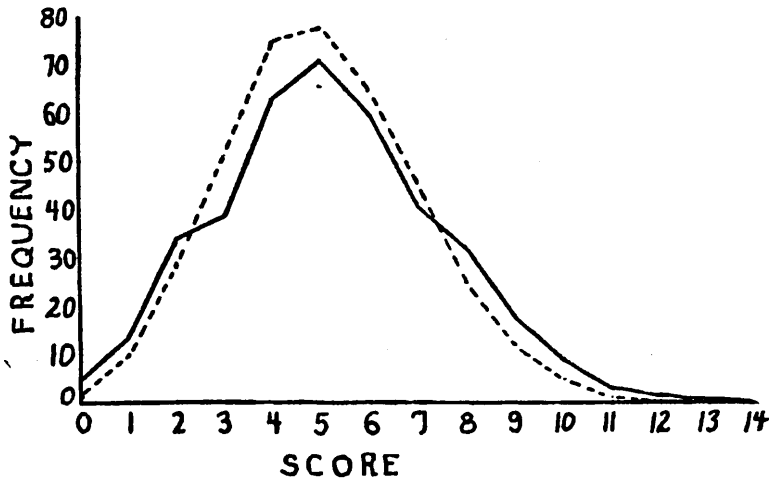

FIG. 1.
Typical multiple-blocking and unblocking graph (Hypothetical).

It is true that the flattening effect may appear even in the scores of a subject manifesting ESP, but it is seldom great enough to overcome the displacement to the right sufficiently to break through the normal curve at its left end. And when it rarely does, the break-through is trivial and unimpressive.

FIG. 2.
Typical ESP distortion (Hypothetical).

Direct ESP, therefore, is a factor in scoring that can only conceal, minimize, or preclude the evidence we are looking for. It follows that R——'s scoring, for our purposes, must be assessed with reservations. The fortunate thing is that with the completion of F——'s work there are now two lots of scores (F——'s and my own) which average extremely close to chance.

Three types of score distribution are investigated:

First there are the individual scores themselves. Each individual score derives from 25 trials, or a single run through the ESP deck. 60,000 trials produce 2400 such scores, ranging (in the results under consideration) from 0 to 14. The expected average score is 5.

Second. In the actual experiments there was a tendency for individual scores to be obtained in groups or "lines" of ten at one sitting. As the sequence of all scores was preserved when they were listed, it was thought that the "line-totals" might reveal incidence of multiple-blocking or unblocking. Accordingly the distribution of such line-totals was studied. The expected average is 50, and the actual totals range from 29 to 74.

Third. In order to discern any evidence of mutiple-blocking or unblocking over periods of time shorter than that required to obtain ten individual scores, the lines-of-ten-scores were halved and the distribution of the resulting "quadrant line-totals" was investigated. The expected average here is 25, and the totals were found to range from 10 to 44.

1. *Distribution of Individual Scores*

Table I gives the mathematical probabilities for each score, and the results obtained by each of the three subjects. It will be seen that in the cases of both H. W. and F—— excesses of both high and low scores appear. F——'s scores of 0, 1, and 2 are 40.2 in excess of expectation, give a C.R.* of 2.76, and a P-value of .003.† F——'s scores of 10 and over are 15.4 in excess of expectation, give a C.R. of 2.41, and a P-value of .008.

My own low and high scores are also in excess of expectation, but to a lesser extent. My 0's, 1's, and 2's give a C.R. of 1.38 and a P-value of .08; my 10's and over give a C.R. of 1.63 and a P-value of .05. From a statistical point of view these values are not significant, but perhaps they may borrow some significance from the fact that they parallel the trend of F——'s results.

If the excesses at either end of the distribution are combined, they give a C.R. considerably greater than that of either end alone. F——'s low and high scores combined give a C.R. of 3.55 and a P-value of .0002. My own combined excesses give a C.R. of 1.95 and a P-value of .026. Table II summarizes all these results.

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* I have calculated the standard deviation from the formula S.D. $= \sqrt{npq}$ or S.D. (in this case) $= \sqrt{2400 \times \frac{235.8}{2400} \times \frac{2164.2}{2400}}$ or 14.58. The actual deviation divided by the S.D. gives the critical ratio (C.R.), from which a P-value can be found.

† The P-value indicates the probability that a given result would occur in purely chance operations. It is conveniently thought of as a fraction, i.e., $.003 = \frac{3}{1000}$, which means that the result under consideration could be expected to occur, in chance operations, 3 times in 1000 repetitions of the entire experiment. Some of the P-values given in this paper, being interpolated, are approximate.

Table I

Distribution of 2400 individual scores.

F——, H. W., and R——.

Score	Expectation	F——		H. W.		R——	
0	9.1	18	dev. +40.2	9	dev. +20.2	6	dev. —68.0
1	56.7	70		61		54	
2	170.0	188		186		165	
3	325.8	317		315		296	
4	448.0	413		466		439	
5	470.4	455		436		452	
6	392.0	385		380		442	
7	266.1	288		259		259	
8	149.6	146		176		156	dev. +25.1
9	70.7	63		60		75	
10	28.3	43	dev. +15.4	34	dev. +10.4	38	
11	9.6	12		11		14	
12	2.8	1		6		4	
13	.7	—		—		—	
14	.2	1		1		—	
	2400.0	2400		2400		2400	

R——'s distribution, as might be expected from the over-all average of 5.1075 (C.R. = 2.6), reveals ESP. The scores from 0 to 5 inclusive are all less than expectation, and those of 8 and over are in excess. This distortion, as has been explained, can generally be counted on to preclude

Table II

C.R.'s and P-Values deriving from Table I.

(Groups of Scores)

	F——				H. W.			
	Dev.	S.D.	C.R.	P	Dev.	S.D.	C.R.	P
Scores of 2 and under	+40.2	14.58	2.76	.003	+20.2	14.58	1.38	.084
Scores of 10 and over	+15.4	6.39	2.41	.008	+10.4	6.39	1.63	.052
Above scores combined	+55.6	15.66	3.55	.0002	+30.6	15.66	1.95	.026

the appearance of that balanced distortion (F——'s and H. W.'s above) which constitutes evidence of multiple-blocking and unblocking. (It is worth noting, however, that the balanced distortion can and sometimes does exist *within* the ESP distortion. But its detection then requires methods we cannot consider here.)

R——'s scores of 8 and over, it might be mentioned, give a C.R. of 2.2 (P = .0139), but this is evidence only of ESP.

I have applied the Chi-square method of appraisal to the distributions of F—— and H. W. in Table I, with the following results:

F——	$\chi^2 = 27.8$	$n' = 12$	$P = .003$
H. W.	$\chi^2 = 15.0$	$n' = 12$	$P = .182$

When the Chi-square of a given distribution turns out to be so low as to give a P-value of .182 or thereabouts, it is the practice among some statisticians to dismiss the entire experiment as non-significant. Such a practice, however, is not always justifiable, for although the Chi-square method is reliable nine times out of ten, it can easily leave undetected results of high significance. Table III, a *hypothetical* distribution to which the Chi-square method has been applied, illustrates the point. Although the P-value of the distribution as a whole is .18, the 0-scores are greatly in excess of expectation, and provide a deviation that, worked out separately, gives a P-value of .0002.

Another weakness of the Chi-square method is that it often necessitates the grouping of extreme scores—scores which may be significant separately but not *en bloc;* and still another weakness is that the method takes no account of any existing pattern in the deviations—a pattern such as mine in Table I, comprising a consistent excess of both high and low scores. Such patterns can constitute highly suggestive clues to ESP functioning, especially when they are found to occur in a number of subjects.

Table IV shows a number of C.R.'s and P-values deriving from individual scores in Table I. For the most part,

Table III

The Chi-Square method applied to a hypothetical distribution.

(Illustrating one of the defects of the method)

Score	Expected		Obtained		Difference	Difference²	Difference²/Expected
0	9.1		20		10.9	118.81	13.05
1	56.7		55		1.7	2.89	.05
2	170.0		171		1.0	1.00	.01
3	325.8		323		2.8	7.84	.02
4	448.0		452		8.0	64.00	.14
5	470.4		465		4.6	21.16	.05
6	392.0		385		7.0	49.00	.12
7	266.1		276		10.0	100.00	.38
8	149.6		145		5.6	31.36	.21
9	70.7		73		2.3	5.29	.08
10	28.3		25		3.3	10.89	.38
11	9.6		7				
12	2.8		2				
13	.7	13.3	1	10	3.3	10.89	.82
14	.2		0				
							15.31

$\chi^2 = 15.31$ $\quad n' = 12$ $\quad P = .18$

When the 0's are worked out separately, however, the standard deviation is $\sqrt{2400 \times \frac{9.1}{2400} \times \frac{2390.9}{2400}}$ or 3.01, the critical ratio is 3.62, and P = .0002.

Table IV

C.R.'s and P-Values deriving from Table I.

(INDIVIDUAL SCORES)

Score	Subject or Subjects	Dev.	S.D.	C.R.	P
0's	F——	8.9	3.08	2.95	.0016
1's	F——	13.3	7.44	1.79	.038
2's	F—— & H. W. combined	34.0	17.75	1.91	.028
5's	H. W.	34.4	19.37	1.77	.039
5's	F—— & H. W. combined	49.8	27.46	1.81	.035
8's	H. W.	26.4	11.88	2.20	.014
10's	F——	14.7	5.28	2.78	.0028
*14's	F—— & H. W. combined	1.7	.547	3.11	.001

*The number of 14 scores mathematically expected in 2400 runs is actually .15, not .2 as shown in Table I which gives only one decimal place.

the C.R.'s are rather low, especially mine, as the Chi-square P-value of my whole distribution would lead us to expect. The fact that a Chi-square result is not, however, always confirmed by an appraisal of its parts, is evident later, in the investigation of quadrant line-totals.

The P-values given in Table IV and elsewhere are taken directly from a Normal Curve of Error, and have not been doubled as perhaps, in some cases, they should be. Whether they should be doubled or not is largely a matter of interpretation, so I am leaving them as they are.

A noteworthy thing about my own score distribution is that the high and low score excesses are confined almost entirely to a certain section of the work. When the six blocks, or sections, of 10,000 trials making up the 60,000 total are considered separately, the third such section from the beginning shows the multiple-blocking and unblocking

Table V

Chi-Square of H. W.'s third block of 10,000 trials.

Score	Expected		Obtained		Difference	Difference²	Difference²/Expected
0	1.5		3		10.1	102.01	9.36
1	9.4		18				
2	28.3		38		9.7	94.09	3.32
3	54.3		33		21.3	453.69	8.36
4	74.7		86		11.3	127.69	1.71
5	78.6		73		5.6	31.36	.40
6	65.3		53		12.3	151.29	2.32
7	44.3		38		6.3	39.69	.90
8	24.9		39		14.1	198.88	7.98
9	11.8		11		.8	.64	.05
10	4.7		6				
11	1.6		1				
12	.5	6.9	0	8	1.1	1.21	.18
13	.1		0				
14	.0		1				
	400.0		400				34.58

$x = 34.58$ $n' = 10$ $P = .00006$ (approx.)

distortion with marked effect. The distribution of this section of the data is graphed in Fig. 11, page 214, of the original report. Its Chi-square, shown here in Table V, gives a P-value of .00006, exceptionally low.

2. *Distribution of Line-Totals*

The line-total extremes are shown in Table VI. F——'s extreme totals are very close to expectation. R——'s work shows a deficiency of low totals and an excess of high ones, again revealing the ESP factor. My own high totals are slightly in excess of expectation, and my low totals considerably in excess—perhaps indicating that over a period of time necessary to obtain ten ordinary scores, the multiple-blocking factor has sometimes showed itself. The C.R. of my low totals is 2.31, giving a P-value of .01.

Table VI

Extreme line-totals and column-totals per 60,000 trials.

EXTREME LINE-TOTALS

	40 and under	60 and over	combined
Expected	15.28	16.52	31.80
F——	13	19	32
H. W.	24	18	42
R——	9	25	34

EXTREME COLUMN-TOTALS

	40 and under	60 and over	combined
Expected	15.28	16.52	31.80
F——	19	18	37
H. W.	24	18	42
R——	12	30	42

When the lines-of-ten-scores are added vertically in groups of ten, "column-totals" are obtained. Any significant variations in these indicate consistent fluctuations of ES factors in the course of the experiments, as explained in the first paper. It will be seen that my column-totals of 40 and under are also in excess, giving the same C.R. of 2.31 and P-value of .01.

When my low line-totals and low column-totals are considered in conjunction, however, the C.R. jumps to 3.23 and the P-value to .0007. It is true that low line-totals and low column-totals indicate somewhat different things, but it is permissible to add them together in our search for statistical significance.*

3. *Distribution of Quadrant Line-Totals*

The distributions of quadrant line-totals for all three subjects are shown in Table VII. They provide a good illustration of the limitations of the Chi-square method, for although the Chi-square P-values are entirely non-significant, it can be shown that the distributions nevertheless possess distortions of considerable significance.

Table VII shows that the flattening effect appears most strongly in my own data. My quadrant totals of 19 and under are in excess; likewise those of 30 and over. Combined, they give a C.R. of 3.03 and a P-value of .001.

R——'s data show the usual displacement to the right, with an excess of high scores and a deficiency of low ones. It may be worth noting, however, that the *combined* high and low scores are in excess, perhaps indicative of the flattening effect *within* the characteristic ESP displacement. This excess, however, is not statistically significant.

F——'s quadrant line-totals are almost exactly equal to expectation.

The examination of totals above and below certain points can reveal multiple-blocking and unblocking only when the evidence is gross. Often, as might be expected, the effects of blocking and unblocking are devious, complicated, or subtle. Sometimes the evidence is seen more easily in the flattening of the center of the graph rather than in the bulging at the ends. In all three sets of data, for example, a significant flattening† is seen in the totals of the 24-scores, which combined yield a C.R. of 2.5 and a P-value of .007.

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* The degree to which line-totals and column-totals are interdependent has not been computed, and the P-value given is probably considerably too low.—*Ed.*

† Most marked in F——'s, where the extreme scores show no significance.

Table VII

Quadrant Line-totals.

Score	Expected	F——	H. W.	R——
7	.002	0	0	0
8	.007	0	0	0
9	.021	0	0	0
10	.062	0	1	0
11	.163	0	0	0
12	.388	0	0	0
13	.843	2	1	0
14	1.686	1	1	1
15	3.119	2	4	5
16	5.361	7	11	2
17	8.593	9	10	10
18	12.889	13	17	12
19	18.147	17	21	15
20	24.048	19	19	16
21	30.056	43	25	28
22	35.529	37	37	43
23	39.768	44	39	37
24	42.253	30	34	37
25	42.667	38	45	35
26	41.026	40	34	46
27	37.617	41	35	35
28	32.922	35	31	38
29	27.523	24	25	24
30	22.018	24	26	26
31	16.866	17	23	24
32	12.387	13	10	19
33	8.726	11	13	6
34	5.903	4	4	5
35	3.840	7	6	5
36	2.399	1	4	4
37	1.443	1	2	0
38	.835	0	1	3
39	.466	0	0	2
40	.250	0	1	0
41	.130	0	0	0
42	.065	0	0	1
43	.031	0	0	0
44	.014	0	0	1
45	.006	0	0	0
46	.003	0	0	0
47	.001	0	0	0
	480.073	480	480	480

One curious characteristic of multiple-blocking and unblocking is that their effect on a graph is never *simple,* as might be thought (see Fig. 10 in the original paper), but *compound.* Instead of a mere flattening at the center and a bulging at the ends, the typical graph is found to possess certain more or less symmetrical (and often significant) dips and peaks (Figs. 11 and 12, original paper). And such dips and peaks, I think, are really to be expected on the grounds of the hypothesis. If multiple-blocking exists, its effect would be to "throw" a certain percentage of all scores to the left of where they might be otherwise expected to occur. Unblocking, contrarily, would throw others to the right. This might account, in the first place, for the peak which often occurs at the center of a graph.

Other effects, however, would be included, for the following reasons:

It must be borne in mind, first, that multiple-blocking and unblocking have their incidence during a certain undetermined *percentage of the time* occupied by the experiment; and second, that without these factors, the subject's scores would approximate a normal graph. It follows from this that although the percentage of distortion affecting each score is the same, the *amount* of the distortion will be greatest in those scores that normally occur most frequently, i.e., in those represented by the central section of the normal graph.

Here the matter becomes complicated. Every point on the graph must be thought of as a center from which two small dislocations are occurring, one to the right and one to the left. The confusing thing is that the dislocations are greatest towards the top of the graph and least towards its ends.

Probably this complex shifting is best thought of as a series of small waves occurring along the whole line of the graph, being greatest at the top, and always moving to both right and left from any given point. Granted this reasoning, it follows that the waves will appear with greatest amplitude at the top of the graph, and that down its sides might occur points where waves, reinforcing each other,

would form nodes or crests, with subsequent dips between them.* Fig. 12 in the original paper seems to be a particularly good illustration of these possible effects.

Still further complications, however, exist. The incidence of multiple-blocking and unblocking no doubt varies somewhat among different "normal" subjects, and in the same subject at different moments. A score of 7, blocked, might become a 6 at one time, and a 5 at another. Taking all this into consideration, it can hardly be expected that the graphs of the scores of any two normal subjects will show much similarity in their dips and peaks. And indeed, such similarity is rare. The curious thing is that it should exist at all—for it does appear to exist to some degree, and with some significance, in the three subjects under consideration. The similarity of the 24's in the quadrant line-totals has been already mentioned; the 20's likewise are similar, each being below expectation; combined, their C.R. is 2.5, giving a P-value of .007. If the 20-scores and the 24-scores of all three subjects are taken in conjunction, the C.R. jumps to 3.3 and the P-value to .0005.

The three subjects likewise have in common a dip at 29, a crest at 30, and a smaller crest at 17—although these are

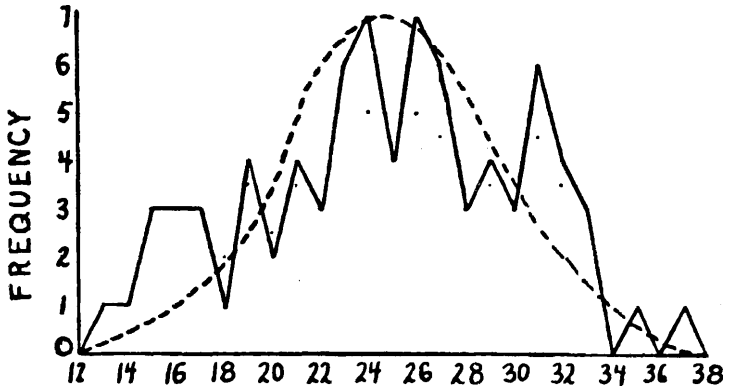

FIG. 3.
Quadrant line-total distribution of H. W.'s third block of ten thousand trials.

* The nodes and dips should be compared with the actual irregularities in more or less normal curves where psychic factors of the type postulated are known to be absent.—*Ed.*

not significant. It is rather unexpected that R——'s graph, having its own ESP distortion, should show these similarities with the others.

Most remarkable of all multiple-blocking and unblocking graphs is that resulting from the quadrant line-totals of my peculiar third block of 10,000 trials. It is shown in Fig. 3. Counting as extreme scores those of 17 and under and 31 and over, the C.R. is 4.3, which has a P-value too small to be included in my tables.*

Appendix

Editorial Comment on Statistical Procedures

In our judgment there is no great purpose in applying the critical ratio method to groups of high and low scores when the standard which determines what is "high" or "low" shifts arbitrarily. This procedure naturally encourages the habit of taking 3, or 2, or some other figure as a standard of "lowness," and 8, or 9, or some other figure as a standard of "highness," depending on which gives the most impressive result. In our opinion it would be sounder procedure to stick to one definition throughout a series of experiments; e.g., on the basis of empirical findings (suggesting where the effect appears) we might set 8 or better as "high," 2 or lower as "low," and if the result is significant, postulate, as an hypothesis to be tested, that in subsequent work the highs and the lows *as defined* will be significantly in excess of chance. But if they are, and if the effect is reasonably consistent, as the author maintains, the Chi-square method, despite the considerations he urges, will probably reveal it. If need be, the critical ratio method may be used, but in terms of standards fixed in advance, not adjusted after the event.

* According to Pearson's Tables, P = .000008.—*Ed.*

Note on an Attempt to Repeat the Woodworth Results

When, in the summer of 1942, we published Hugh Woodworth's first paper, "Report of Investigations into an Obscure Function of the Subconscious Mind," it was decided to have a subject call 60,000 ESP cards; the results of these calls could then be compared with the results of Mr. Woodworth's subjects and independent evidence of multiple-blocking and unblocking might be obtained. Being available at all times, the writer (LD) was chosen to act as subject.

Material: Standard ESP cards were used. A "master-deck" containing 100 of each symbol was thoroughly shuffled before each sitting and dealt out into 20 decks, these decks then being inclosed in 20 individual numbered boxes. There was not, therefore, a fixed number of each symbol in the decks called by the subject. Miss Wellman, the Executive Secretary, prepared the stimulus material and later recorded the card order of each deck on standard record sheets.

Procedure: Each day (from August 31, 1942 to March 2, 1943, with "days off" during vacations, etc.) Miss Wellman presented 20 boxes to LD who then wrote down her guesses as to the order of the cards in each box. A DT (down-through) procedure thus was used (in contra-distinction to Mr. Woodworth's use of the open-matching method). On some days LD's calls were unwitnessed; on other occasions she made her calls in the presence of anyone who happened to be in the office. On completing the day's task, she turned over her record sheets to Miss Wellman, who then opened the boxes and recorded the card order, later comparing this record with LD's. Scores for the day were entered in a special notebook. At the end of the entire experiment, two Hunter College students again compared all record sheets and rechecked for errors in scoring of hits. It is of interest to note that all errors found in the recheck were failures to observe correspondences between target and calls; that is, failure to check hits.

Results: 2400 decks were called (20 decks a day for 120 days) and the average number of hits per deck was 4.95. This over-all result is without statistical significance (C.R., —1.19). Obviously LD displayed no ESP ability in the ordinary sense. According to Mr. Woodworth's hypothesis, a comparison of obtained results with results expected by chance should reveal that extreme scores, high and low, are in excess of expectation—at the expense of scores near the mean (4's, 5's and 6's). (See Fig. 10, p. 213, of Mr. Woodworth's original report, in the October, 1942, issue of this JOURNAL.) The excess of extreme high and low scores is attributed by him to multiple-blocking and unblocking.

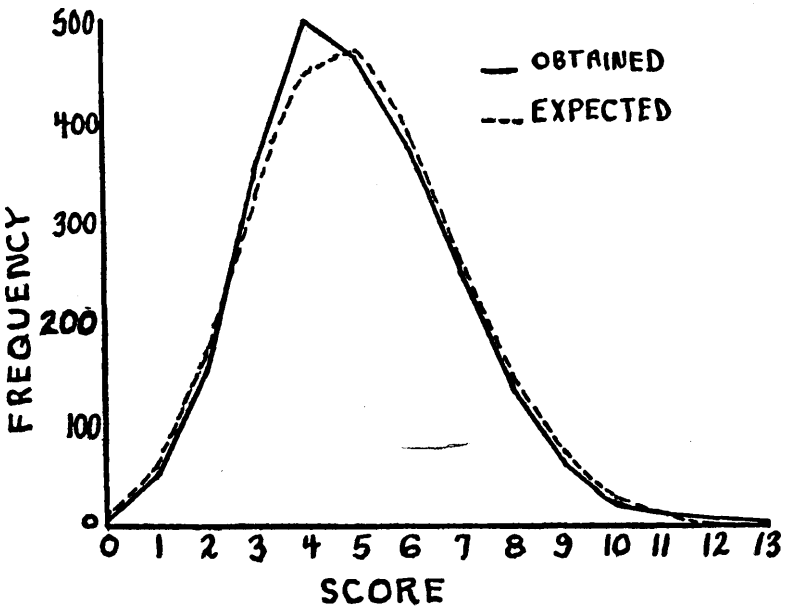

FIG. 1.
Distribution of scores, 2400 decks.

Fig. 1 (of this paper) shows the relation between scores expected by chance and the scores obtained in this experiment. The value of Chi-square is 25.3 and P is .01. It is interesting to compare this figure with Mr. Woodworth's Fig. 10, previously noted, and with Fig. 1 appearing in his article, p. 121, this issue. It will be noted that in Mr.

Woodworth's Fig. 1 scores from 3 to 7 inclusive are below expectation, all other scores being in excess of chance. In LD's data, however, the deviations from chance expectation do not take the same form. If LD's results were in complete agreement with Mr. Woodworth's hypothesis, all scores except those from 3 to 7 inclusive would be above chance expectation. Actually, though, the only scores whose frequencies *exceed* chance expectation are the 3's and 4's and the 12's and 13's. It must especially be noted that scores of 0, 1, and 2 and 8, 9, and 10 are *below* chance expectation. Also, the greatest single contribution to the Chi-square is found in the departure from chance expectation of scores of 4. According to Mr. Woodworth's Fig. 1, scores of 4 should be noticeably less than expected; quite the contrary is the case in LD's data. It is entirely possible, however, that individual differences exist in the locus and extent of the multiple-blocking effect; this is as yet undetermined. As for a possible unblocking effect, it should be pointed out that LD's excess of scores of 12 and 13 contributes nine points to the total Chi-square.

Whether or not these data may be considered as affording evidence in support of the hypothesis of multiple-blocking and unblocking, the fact remains that the departure from chance expectation obtained (in regard to score distribution) approaches statistical significance. It may be remarked that inspection of much of LD's other data reveals the same tendency; namely, to obtain an excess of scores of 4 and of 11 or better. Also, it may be mentioned that the average score of the *first deck* called each day was 5.25. The average score on all remaining decks was 4.93. While this difference is not statistically significant, it is suggestive of the operation of the Midas touch, discussed elsewhere in this issue.

Summary: An experiment was undertaken the purpose of which was to obtain evidence of multiple-blocking and unblocking, as defined by Mr. Woodworth. Frequency distributions comparing obtained and expected results showed effects different from those predicted by the hypothesis.

Either (a) the Woodworth hypothesis requires further clarification and better support or (b) the multiple-blocking and unblocking effect in LD's case is dissimilar from these effects as they appear in the calling of Mr. Woodworth and his subjects. Further data are required to settle the point. It seems to us that any hypothesis attempting to clarify the problem of below-chance scoring is of real importance and it is to be hoped that other interested persons will repeat Mr. Woodworth's experiment.

L. A. DALE

In Memory of Dr. Gustav Pagenstecher

LYDIA W. ALLISON

A letter from Mrs. Gustav Pagenstecher announces the death of her husband, Dr. Pagenstecher, on December 26th, 1942, when he was approaching the eighty-eighth anniversary of his birth. The letter also brings the news of the death of Dr. Pagenstecher's remarkable non-professional medium, Mme. Reyes Zierold, known as Señora Maria Reyes de Z., which occurred last September during a visit to her daughter and grandchildren.

Older members of the Society will recall the widespread interest in the striking evidential statements of Señora de Z. when reports of the experiments were published by Dr. Pagenstecher and by Dr. Walter Franklin Prince more than twenty years ago. Today these reports remain unsurpassed in the special field of psychometry. When in hypnotic trance, Señora de Z. was able to perceive the mental state of her physician and also to give relevant facts beyond the reach of chance concerning a great variety of unknown objects placed in her fingers.

After Dr. Pagenstecher accidentally discovered the apparently supernormal powers of Señora de Z. he began experiments with her in 1919. The results were so surprising that he brought them to the attention of the leading Mexican medical society, which appointed a commission to verify them. The members of the commission witnessed a number of experiments and upheld Dr. Pagenstecher's claims that the observed phenomena were of a supernormal nature.

Dr. Prince's acquaintance with Dr. Pagenstecher had begun with a correspondence and continued until the weight of the accumulating evidence induced him to go to Mexico in 1921, in order to investigate Señora de Z. independently, and also to learn all possible facts concerning the earlier work. The two investigators became great friends and colleagues.

Dr. Prince had seven experimental sittings with Señora de Z. under his own conditions. While the quality of his results did not measure up to the standard of those obtained by Dr. Pagenstecher, Dr. Prince noted that hardly any details were incorrect and he was fully satisfied that the "hits" taken together seemed quite inexplicable on a chance theory. As Dr. Pagenstecher's own manuscript was placed at the disposal of Dr. Prince and edited and exhaustively discussed by him for the English reading public, I find it difficult to speak of one man without including the other, although these lines are written primarily in memory of Dr. Pagenstecher.

Dr. Pagenstecher was descended from a family of scholars and officials and was a graduate of the University of Leipzig. He was a physician of distinction in the City of Mexico, where he had long made his home. He was highly regarded as a public speaker and had twice been invited to deliver addresses on great public occasions in the presence of the President and his cabinet.

Dr. Pagenstecher's reactions to his fortuitous discovery of the powers of Señora de Z. are best described by quoting from Dr. Prince's report (see *Proceedings,* Vol. XV, 1921, p. 190).

"Dr. Pagenstecher had been a materialist for forty years. He was not looking for any change in his philosophy, nor did he have any expectation of strange phenomena when he began to hypnotize Señora de Z. for therapeutic purposes. She herself was not aware that she possessed any peculiar powers. But when she began to manifest knowledge of existing facts supposed to be out of the reach of her normal senses, the hypnotizer, actuated by that curiosity, or interest in matters yet obscure, which is the impelling force of all scientific discovery, began to experiment deliberately . . .

"The letters written to me by Dr. Pagenstecher testify to the strenuosity with which he endeavored to maintain his strict materialistic principles; and none of them, up to the time of my visit to Mexico, distinctly announced relinquishment of these, but it was read between the lines that

he was hard pushed, and even forced over the boundary line. My arrival found him convinced that, as Huxley admitted, there is something in the universe transcending matter and force, and he seemed half amused to see himself in a different camp from that which he had occupied for forty years, led there by conclusions from the facts observed which he did not feel that he could logically and honestly evade."

In his foreword to Dr. Pagenstecher's report Dr. Prince described Señora de Z. as a woman of unusual intelligence, better educated than the average girl in Mexico and the daughter of a former divisional General of the Federal Army, and Governor of the State of Michoacan. Owing to her discovery of fraud during a brief contact with spiritualism as a girl, Señora de Z. had always felt a repugnance for it. For many years her attention had been engrossed by the care of a large family. The phenomena she exhibited were as great a surprise to her as they were to Dr. Pagenstecher, who began hypnotic treatment solely as a means of overcoming insomnia.

Dr. Prince received the impression that Señora de Z., whom he studied in every possible manner, both in the sittings and in her home, as well as on social occasions, regarded her gift in a matter-of-fact fashion. While she was interested in her phenomena, she would not have cared for the experiments, had it not been for her gratitude to the man who by his professional skill had probably saved her life.

The value of Dr. Pagenstecher's contribution to psychical research can hardly be overestimated. "It was evident," and again I quote Dr. Prince, "that great moral courage had been manifested in going before a medical society at least mostly composed of skeptics and demanding that there be placed on record his affirmation that 'in order to hear, to see, to smell, and to taste it is not absolutely necessary to have ears, eyes, tongue and nose,' and that a commission be appointed to test his statements by personally participating in experiments with Señora de Z. Such a claim seriously jeopardized his professional standing and his practice itself,

unless he could substantiate his claims. It was certain that, in spite of their general skepticism of such matters, the commission, including some of the leading physicians of Mexico, did witness to the success of the strange experiments in which they shared." (*Op. cit.*, pp. 191-192.)

It may be added that some leading psychical researchers feel that Dr. Prince's most important studies were made in the field of psychometry and chief among these was his investigation of Señora de Z. Dr. Pagenstecher granted Dr. Prince full liberty to introduce any feature he wished into the experiments, instantly produced any desired original or collateral document and cheerfully answered every question. This attitude on the part of Dr. Pagenstecher enabled Dr. Prince to bring the full powers of his intensely critical mind to the study of the case. It was no small tribute to his colleague that Dr. Prince felt, both while in Mexico and after his return to New York, that Dr. Pagenstecher had understated the facts.

In 1935 nothing had been heard from Dr. Pagenstecher for a long time. Then one day a letter came for Dr. Prince, more than a year after the latter's death in August, 1934. It began, "These lines will be for you almost the greeting of a dead man who has resurged only to give you a great pleasure." The letter was concerned with predictions made fourteen years earlier by Señora de Z. while in the deepest state of cataleptic trance. On November 24, 1921, Dr. Pagenstecher had sent a letter to Dr. Prince enclosing the predictions in three separate envelopes, each sealed with his personal stamp. They were to be held for safe-keeping and opened at Dr. Pagenstecher's death, or earlier if he so authorized. It appeared to the doctor that some of the predictions were beginning to be fulfilled.

Dr. Pagenstecher was deeply distressed when the news of Dr. Prince's untimely death reached him. It was a "real blow." In his reply he wondered what Dr. Prince might have thought of his silence of years. He explained the reasons. He had not wished to make Dr. Prince unhappy by telling him that after the first World War the small fortune he had invested in Germany had been entirely lost

and that he had been obliged to give up his beloved study of psychometry and the reading of psychical research literature to devote himself again to hard work in his practice. There was no hint of complaint.

At about the same time, Dr. Pagenstecher sent me a delightful and reassuring photograph, taken some months earlier. It was an informal picture of himself and his wife in their home made on the occasion of his eightieth birthday. Dr. Pagenstecher still appeared to be a man at the height of his physical and intellectual powers, giving no indication that he had reached a venerable age.

It must have required character and courage for a man in his declining years to begin the struggle for existence anew. But in the friendly correspondence that continued, the general tone and enthusiasm of Dr. Pagenstecher's letters were ample evidence that his spirit was undaunted. The revolutionary experience with Mme. Zierold, when he was well past sixty, had totally changed his sense of values and his outlook on life.

Cases

I

We are indebted to Mrs. Hugh Woodworth, of Nanaimo, British Columbia, for the interesting dream to follow. Mr. Woodworth, who corroborates his wife's account, is known to readers of this JOURNAL for a paper which appeared in the October, 1942, issue—"Report of Investigations into an Obscure Function of the Subconscious Mind." A sequel to this paper appears in this issue. Both Mr. and Mrs. Woodworth have had other spontaneous experiences of a parapsychological nature. Mrs. Woodworth wrote to us under date of May 10, 1943:

I am glad to give you a fuller account of a dream of mine that Mr. Woodworth has already mentioned to you.

The dream was in my mind on waking on the morning of Wednesday, November 11, 1942, and I told Mr. Woodworth about it before either of us got up. I dreamed that the postman had brought two parcels and some letters. One parcel was flattish, suggesting to me a card table folded up—not simply the legs folded, but the top as well. Its dimensions as I dreamed them would be about 12" by 7" by 1½". The flatness was the parcel's most noticeable feature, particularly as I dreamed that it and the letters had been slipped under the front door—although the parcel was not really thin enough to make this reasonably possible. On opening it, I found the contents to be white and flat, but on waking I could remember no other details, or whether, indeed, it turned out to be a table.

When I told Mr. Woodworth about the dream, he expressed surprise that a table should be folded, even in a dream, and in questioning me about it asked, "Like a tablecloth?" To this I assented.

All this was about 10 A.M. as November 11th was a holiday and we had slept late. When we finished discussing the dream, Mr. Woodworth got up, and called to me from the kitchen that there was some mail. This surprised us both, for three reasons: the fact that any mail should have been delivered on a holiday, the fact that it should have come as early as 10 A.M. (since mail *never* had been delivered earlier than 3 P.M.), and the fact that it should have been left at the back door, as it was. (The postman always comes to the front door unless there is a parcel and no one is at home to receive it. In this case he leaves it at the back door for safety's sake.)

Mr. Woodworth brought the mail into the bedroom; there was a letter for him and a parcel for me. (Both of which, incidentally,

had been outside the back door, not slipped under it.) My parcel was very nearly the same size as one of the ones I had dreamed of; it was an inch or two wider, and somewhat flatter—not being a box at all, but a cardboard folder. I opened it and found that it contained a white tablecloth four feet square. It was a totally unexpected gift from a friend in Vancouver. Neither of us had any reason to expect a gift at this time, still less a tablecloth. My birthday was not for two and a half months, Mr. Woodworth's not for four months, and our anniversary was five months away.

FREDA WOODWORTH

Mr. Woodworth inclosed a statement concerning the events recounted above, also dated May 10, 1943:

. . . I am glad to confirm briefly those details of the dream that I knew of at the time. We slept late on November the 11th, waking about 10 A.M. I no longer remember which of us woke first, but I should say we both awoke about the same time. Mrs. Woodworth told me she had dreamed the postman brought a parcel for her containing a table "folded up." I thought this an odd dream and questioned her about it. I said, "You mean the legs were folded?" and she said, "Yes, but the top was folded too." I found this rather incredible, and after a few more remarks asked if it were folded both ways like a tablecloth, to which she agreed.

She did not mention the dimensions of the parcel when she first told me about it, and indeed, I suppose she could hardly have considered these worth mentioning at the time. She did not mention the similarity in size even when I brought the parcel in, but spoke of it somewhat later. The parcel contained a white tablecloth, almost exactly four feet square. As Mrs. Woodworth has explained, it was a totally unexpected gift.

HUGH WOODWORTH

II

The percipient in the following case, which seems to afford evidence for survival, is a Member of the Society, Mildred C. Harris, of Philadelphia, Pennsylvania. Miss Harris, who prefers to use her maiden name in this connection, incloses with her original account corroboratory statements from her husband and from a friend, Mrs. Jean Galer, also a Member of the Society. The real names of the other persons concerned (pseudonyms have been used for obvious reasons) are on file with us. Miss Harris told us of the incident under discussion three or four days after

it occurred. We asked her to let us have a written statement and this she sent us under date of April 20, 1943:

For the past six or eight months, as you know, I have been having various experiences of a psychical nature. From time to time I receive strong impressions of matters normally unknown to me and, upon investigation, these impressions prove to be accurate. The incident you have asked me to record is one such.

On Thursday, April 8, 1943, Mr. *Hoyt** died in Philadelphia at approximately twelve o'clock noon. I learned of his death through normal channels. I must make it clear that, although I am an intimate friend of Mr. *Hoyt's* first wife (they were divorced more than fifteen years ago and Mr. *Hoyt* had married again), I had never, to the best of my knowledge, seen Mr. *Hoyt,* nor had his physical characteristics and appearance ever been described to me.

On the night of April 8th, that of the day on which Mr. *Hoyt* died, before dropping off to sleep about midnight, I mentally inquired if he were all right. At once I grew aware of an individual about medium height, a bit paunchy in girth, round-faced, with piercing eyes.

"Tell *Helen,* tell *Helen!*" he appeared to be saying, mentioning the given name of my friend, his first wife.

"What would you like me to tell her?" I inquired.

"Home! Home!"

"Do you mean that you are at home?"

"No, no! The high mahogany chest of drawers . . . has brass handles or hardware . . ."

A mental view of such a piece of furniture was so clearly impressed upon my consciousness that I could have drawn a picture of it in detail. Then Mr. *Hoyt* seemed to continue:

"The high mahogany chest with brass hardware . . . in the drawer where my handkerchiefs are . . . a black wallet . . . folder . . . tell *Helen!*"

Here I had a strong impression, or mental view, of a small black leather fold-over container which might have been a wallet or folder, or possibly the covers of a black leather notebook. I took it to be a wallet or folder. Then the impressions began to fade.

The next day, April 9th, I reported the entire incident to my friend *Helen Hoyt* (over the telephone) and she verified as accurate the physical characteristics of her former husband as they had been impressed upon me. Later on that same day *Helen* visited the home of the bereaved present wife† and there saw a mahogany chest of

* Pseudonym.

† All the circumstances surrounding the somewhat unusual relationship existing between the first and the second Mrs. *Hoyt* have been made clear to us; it has not been thought necessary, however, to clarify this in the presentation of the case.

drawers with brass handles—just as I described it to her. With some diffidence *Helen* persuaded the second Mrs. *Hoyt* to search in this chest of drawers for the folder indicated. A search was made and a black leather folder was found, as foretold, in one of the drawers with Mr. *Hoyt's* handkerchiefs. This folder contained an old photograph of *Helen*. The present Mrs. *Hoyt* had not known that the photograph existed, nor that there had been such a folder among her husband's things. *Helen* had no idea that the photograph was still in Mr. *Hoyt's* possession. In the same drawer a second black folder was discovered. It contained a codicil to Mr. *Hoyt's* will.*

Perhaps of further interest is the fact that it was generally believed that Mr. *Hoyt's* regard for his first wife had continued through the years. Furthermore, if the impressions I received may be considered as having their source in the surviving personality of Mr. *Hoyt,* the information given, though possibly painful to the second wife, constitutes an added and accurate character trait for the following reasons:

(1) Mr. *Hoyt* was a man of scientific training and would, I think, realize the importance of getting through information that could not easily be attributed to telepathy among the living. The existence of his first wife's photograph in the drawer with his handkerchiefs was just such a piece of information.

(2) I have learned that Mr. *Hoyt* was incisive, brusque, and not always considerate of the feelings of other people, being especially indifferent to them when he believed that they interfered with questions of scientific importance.

On the morning of April 9th I told my husband about my experience of the night before; I also told a friend, Jean Galer. Their testimony is appended. I should say that I told them all the details before there was any question of corroboration from the home of the second Mrs. *Hoyt.*

MILDRED C. HARRIS

Miss Harris inclosed corroboratory statements from Mrs. *Helen Hoyt,* from Mrs. Jean K. Galer, and from her husband. The latter prefers that his name should not appear and therefore signs his statement with initials only.

April 28, 1943

In verification of the episode recounted to you by Mildred Harris, I wish to state that on the afternoon of April 9, 1943, the day after the death of my former husband, Miss Harris reported to me on the

* In further correspondence we have learned that the codicil was of no importance or interest to the first Mrs. *Hoyt.*

telephone several impressions she had received the night previous. She had never, I am sure, seen Mr. *Hoyt,* yet she accurately described to me details of his physical appearance which had been impressed upon her.

As for the mahogany chest with brass pulls, it was only with some difficulty that I was able to ascertain that such a chest of drawers did in fact exist. I saw it, however, exactly as described to me by Miss Harris, when I visited the home of the present Mrs. *Hoyt* later on during the afternoon of the 9th. There was also found, as indicated, in a drawer with Mr. *Hoyt's* handkerchiefs, a black leather folder containing an old photograph of me. I had not known that this photograph was still in the possession of Mr. *Hoyt,* and his present wife did not know of the existence of either the folder or the photograph. In addition, there was discovered in the same drawer, in a similar black leather folder, the codicil to Mr. *Hoyt's* will.

Helen Hoyt

April 20, 1943

This is to state that on the morning of April 9, 1943, my wife reported to me on the telephone the details of an impression she had received the night before concerning Mr. *Hoyt,* who had died that day. Her account of the incident as reported to you is in all details the same as that given to me before the information was passed along to the first Mrs. *Hoyt* and later substantiated in each particular.

N. M.

April 20, 1943

On April 9, 1943, Mildred Harris stopped for a moment at my place of business and, knowing of my interest in the field of extrasensory perception, told me in detail, as in her own account submitted, of her experience of the night before concerning Mr. *Hoyt.* These details were told to me the day before they were corroborated by Mrs. *Helen Hoyt.*

On the evening of April 10th, Miss Harris, her husband, and Mrs. *Helen Hoyt* came to call on me at my home. At this time Mrs. *Hoyt* told us of finding, as a result of the impressions Miss Harris had told her about, the chest of drawers, the folder containing her photograph, and the other folder containing the codicil to her former husband's will. She also confirmed Miss Harris' impressions as to the details of Mr. *Hoyt's* physical appearance, these details normally being unknown to Miss Harris as she had never seen Mr. *Hoyt* during his lifetime.

JEAN K. GALER

Correspondence

Letter from Mr. Whately Carington to Dr. Murphy in regard to the results of the Society's 1941-42 "Long-Distance Telepathy Experiment."

One hundred and sixty-two percipients, many of them Members of the Society, took part in this experiment, which was carried out under the direction of Dr. Murphy. Five simple line drawings chosen by a random method from an already prepared population of 208 drawings were used on twelve experimental evenings (November 25, 1941 to February 24, 1942). Mrs. Dale, of the Staff, acted as agent, retracing and "concentrating" upon each of the five drawings in succession for a period of five minutes. The results of this experiment were sent to Mr. Carington in England for evaluation. Mr. Carington, of course, is well known for his experiments on the paranormal cognition of drawings. See *Proc.* S. P. R., Part 162, June, 1940 and Part 164, June 1941.

. . . I have now completed [January 5, 1943] all the work which at present seems necessary on your big experiment; that is to say, all the obvious tests and one or two less obvious. The results justify my gloomiest prognostications and are completely null, at least from the paracognitive point of view. Despite this null result, however, I do not regard the experiment as a failure, except in the restricted and local sense that it did not do what you hoped it would do. On the contrary, I regard it, oddly enough, as one of my major triumphs, for reasons which I must try to make clear.

I think you will agree, speaking in a general sort of way, that our attitude to any new phenomenon, and our conviction as to the genuineness of its occurrence, will depend largely on the extent to which we "understand" it, and in particular on the extent to which we are able to make predictions about it. I think we should feel suspicious in the presence of a phenomenon which occurred in a wholly sporadic fashion, so that we could never tell when it would "come off" and when it would not. In other words, we shall not feel happy about a new phenomenon until we have excogitated some kind of theory about it which will enable us to make reasonably successful predictions about its occur-

rence. And it is scarcely less important, though much less gratifying, of course, to be able to predict the conditions under which it will *not* occur; at any rate, a theory which predicted that the phenomenon in question would occur under *any* conditions could hardly be considered a theory!

Now, ever since you first told me of the projected experiment I have had doubts as to whether the modifications you introduced might not prove fatal to the occurrence of the effects you are looking for—see, for instance, my letter to you of July 27, 1942—and have felt that the plan of having the 208 potential originals prepared in advance was liable to lead to null results. The null result ensued, and I do not think it could have been predicted on any theory of paranormal cognition other than my own—except, of course, on the "theory" that paranormal cognition does not occur at all! But we know (humanly speaking) that paranormal cognition *does* occur, and that it occurs with drawings. I think that any other theory would say that the preparation of 208 potential originals in advance would make no difference, but that what would matter would be the "concentration" by the agent on the selected original at the appointed time. This, I think, would certainly be true on any "transmissive" theory, such as is commonly held by most people. But on my theory of paranormal cognition (Cf. *Proc.* S.P.R., Part 164, June, 1941, pp. 304 ff.), preparation ahead makes all the difference, as I shall try carefully to explain below.

Before doing this, however, I want to separate clearly two features of your procedure, each of which has had an adverse effect from one point of view or another, though of different kinds. The first of these is the use of a large number of pre-prepared potential originals, as already noted. The second is the fact that percipients were invited to draw up to 60 drawings (5 on each of 12 evenings), and did in fact produce anything from this number downwards. The first of these is, on my theory, liable to prevent anything interesting emerging at all; the second merely precludes the useful application of Catalogue scoring.* As

* Mr. Carington's method of Catalogue scoring will be fully explained in a paper of his to be published by the Society in the Fall of this year.

to the first feature of your procedure—that is, why the 60 originals actually used failed to give a significant result when hits on them are tested in a 12 x 12 contingency table by the ordinary Stevens method:

If my views are approximately correct—and I may say that almost every day brings forth confirmation of them in one way or another—these phenomena can be understood only by dismissing any type of theory which involves "transmission" from one localized mind to another localized mind across the intervening space. It is necessary to think instead in terms of the known facts or "laws" of association working on what is for this purpose no more than a *single mind*—call it the Joint Mind, or the Universal Subterconscious, or anything you like.

When the experimenter (who may or may not be the same person as the agent) draws an original X, the "idea" of X is ipso facto associated in that bit of the joint mind which we call "his" mind with the "idea" of the experiment. The term "idea" is used to denote that aggregation of images which enter or approach the field of consciousness when the object or situation concerned is thought of. This means that whenever the idea of the experiment is again evoked in the experimenter's mind by any future situation, the idea of X will be brought nearer to the focus of consciousness—i.e., will be more likely to enter the field—than it would be if the associative link had not been formed by drawing the original depicting X. That is perfectly straightforward associative psychology, and needs no particular elaboration.

But if we suppose that there is a "subterconscious" common to all individual minds in some degree, then this association of X with the "experiment" will have been formed *for all minds* by this act of drawing the original. Thus X will be more likely than it otherwise would have been to appear in the field of consciousness of any mind to which the idea of the experiment is presented. And this condition is manifestly fulfilled whenever a percipient sits down to do the experiment, with the result that any percipient is somewhat more likely to think of an X in these

circumstances than he would be if the experimenter had not drawn an X as an original.

Note that this theory short-circuits all difficulties about "rapport" and about why telepathy occurs only under certain conditions. Telepathy occurs perpetually under all conditions, and the reason why we are not as a rule conscious of it is simply because there is so much of it that it practically all cancels out—much as if we could hear all radio stations broadcasting at once, which would amount to the same as hearing none. As for "rapport" in the ordinary sense, there is none. But this will have to be discussed in more detail when we come to consider, as some day we must, the extent to which the subterconscious can reasonably be regarded as common to persons of widely different types and experience.

The really important point here is that we cannot think of the telepathic process as starting at the moment when the agent sits down to "concentrate" and the percipient to "receive." What matters is the forming of the associative links, and this, of course, is done when the experimenter draws the originals.

Thus we must think of the 208 potential originals as having been linked with the experiment *when they were drawn,* and the only advantages which the 60 officially used originals have over the other 148 are (a) the fact that the agent "concentrates" on them, and (b) the fact that since the 208 drawings were done some time previous to the start of the experiment, the associative links will have had some chance of fading and weakening.

As regards (a), I think it reasonable to suppose that the act of making an original drawing is likely to be a good deal more effective in impressing it on the mind of the drawer than is the rather vague process known as "concentrating"—though no doubt the concentrating serves to strengthen the links to some extent. As for (b), it seems pretty clear that astronomical time has nothing to do with the process except indirectly; indeed, it would be very odd if the rotation of chunks of rock in the sky had any direct influence on psychical processes. What matters is what I

have termed "relevant psychological time," which is a matter of the extent to which the images in the subterconscious are shuffled about, and the associative linkages and groupings altered. If you associate a group of ideas of any kind together (e.g., X, Y, Z, etc., with Experiment) and then do not think of the experiment for a month, the linkages will remain, one may suppose, substantially unchanged except in so far as they may be somewhat weakened by the strengthening of alternative links between X, Y, Z, etc., with other ideas in the course of everyday experience. Naturally, this last is positively correlated with astronomical time for most people, and that is how astronomical time comes indirectly into the picture; but it is easy to see that its effect may be small, and that there is no sort of guarantee in any particular case that the effect will be proportional to its clock-measured duration.

It follows from all this that, by having potential originals prepared in advance, one is automatically "putting them into circulation," so to say, in the relevant context, and that the 60 subsequently selected for "concentrating" on are likely to have only a small advantage over those not used. And as the paracognitive effect is pretty small anyway, an attempt to show that the 60 originals used are significantly differentiated from the 148 not used, or the 5 used on one evening from the 55 used on the other evenings, is a matter of looking for a kind of second-order small quantity which is very unlikely to succeed in the present state of our knowledge.

In your experiment, the person who made the original drawings may not have had much to do with the matter; Mrs. Dale was evidently the "culprit," so to speak, for there was every opportunity for the 208 potential originals to be firmly associated in her mind with the idea of the experiment*—and that is all that is necessary. The original drawer may have come into it in a minor and indirect way through a linkage with you, but we do not yet know enough

*LD helped to choose the words which were later illustrated by a City College student. She cut the drawings apart, placed them in envelopes, and so forth—thus being entirely familiar with all 208 potential originals.

to say so with certainty; anyway, it matters little in the circumstances.

In other words, if my theory is correct, we could hardly expect, granted the conditions of your experiment, any result of the kind looked for. Again I want to stress that what matters is not the *existence* of the drawings (originals) but the forming of the associations of the ideas of the objects depicted with the *idea of the experimental situation.* The world is full of drawings of objects associated in somebody's mind with something—*but not with the experiment.* If you had happened to pick up on the sidewalk an envelope marked "208 Sketches for Wanamaker's Winter Fashions Booklet," or the like, and had taken your 60 originals from that (and had then done a ten-trial experiment) I do not doubt that you would have had, on Catalogue scoring, a significant result.

Now for the second point—the fact that your methodology prevented the useful application of Catalogue scoring:

At one time I thought it possible that, even if the use of pre-prepared potential originals prevented the 60 selected from differentiating themselves from the remainder, it might still be possible to show that the whole 208 drawings yielded a score significantly above chance expectation. This, however, is not the case, and the reason is easy to see. My own Catalogue embodies the drawings of 741 percipients doing 10 drawings each (more accurately, an average of 11.42 each), and naturally can only be used with assurance to assess the performance of percipients doing an equal, or nearly equal, number of drawings. Your percipients sent in about 28 responses each, on an average. We cannot properly allow for this by arguing that each of your percipients is 2.8 times as likely to draw any given object, X, as a percipient of my Catalogue, and that therefore your expectation of X-drawing percipients is 2.8 times the Catalogue figure, because if the percipient has any sense he will realize that if an X has been used as an original once it will not be so used again, and that there is nothing to be gained by drawing the same thing twice. Of course, a fair number of percipients do not think of this, so that in

practice there is a certain amount of replication. For the most part, however, things are likely to work this way, so that the true expectation for your percipients, for any original—notably common originals liable to be drawn early—will be less (though I do not know how to determine how much less) than the value calculated from the Catalogue on the fiction that you had 2.8 times as many percipients as you actually had. Equally, of course, it would be misleading, in the opposite direction, to treat your percipients as if they had only done an average of 10 originals each.

It is accordingly not surprising to find, as we do, that when the responses to the 208 potential originals are scored by the Catalogue procedure they yield a significant negative result—i.e., when I pretend that there were 2.8 times as many percipients as there actually were. This is, of course, a purely arithmetical matter which has nothing to do with paranormal cognition as such, and it serves only to illustrate the impracticability of using a yard-stick based on data obtained by one method to assess data obtained by another method.

It might, it is true, be argued that, if paranormal cognition had been operative, we ought to obtain a positive result by scoring the *first ten* drawings done by each percipient (where he has done so many as ten, or more) against the 208 potential originals by the Catalogue method; and I think that if such a scoring were to produce a significant positive result it would be admissible. But I do not think that the opposite holds, for the psychological situation of a percipient asked to do 60 drawings and actually proposing to do so (we may reasonably assume), or as many as he can, is by no means the same as that of the percipient asked to do only 10 and knowing that only ten originals will be used. In particular, it seems reasonable to suppose that, with the prospect of time to play with, so to say, he may indulge in a greater degree of picking and choosing—deferring, perhaps, to some future occasion, in the hope of reinforcement or whatever, some of the images that first present themselves. In other and more general words, in

so far as the conditions are different you cannot expect the same things to happen. Consequently I have not thought it worthwhile to undertake the very considerable labor of doing this test. It is rather recondite at best, and, if it gave null results (as inspection and the working of a small sample suggest), it could so easily be explained away on the above lines as to be without meaning.

In conclusion, I repeat that I do not consider the experiment to have been a failure at all, from my point of view, or indeed from any other point of view except the most local. Successfully to predict that a given effect *will not* occur under certain conditions may, I think, quite legitimately be claimed as constituting just as good confirmation, in its own way, of one's guiding theory as successfully to predict that it *will* occur under other conditions. A significantly positive result in your experiment would, at least, logically have required a fairly drastic revision of my theory in order to account for it.

Review

APPARITIONS: being the seventh Frederic W. H. Myers Memorial Lecture, by G. N. M. Tyrrell. Society for Psychical Research, London, 1942. 123 pp. 3s. 6d.

It has been a long time since we have had a mature, scholarly, and systematic study of apparitions; indeed, to find anything comparable to Mr. Tyrrell's paper we would have to go back to Mrs. Sidgwick's article* in the *Proceedings* (1922), and even there the author restricted herself to certain aspects of the present problem. The Tyrrell monograph aims, in accordance with the high design of the Myers Memorial Lectures, to achieve much more than would a mere collection or systematization of recorded cases; it presents a psychological theory of apparitions—visual, auditory, and tactile—and makes pertinent suggestions for additional further inquiries.

The cases cited cover a large number of the old familiar "standbys," but they are chosen, as by a geological or botanical collector, to document the salient *types* of data which have to be reckoned with on any theory. With negligible exceptions, they are S. P. R. cases; they are *waking* cases; and they are—except when, for purposes of comparison, other features are called for—well authenticated veridical cases. Precognitive cases (e.g., those referring to future catastrophes) are excluded. We are dealing almost entirely with two large classes of evidential apparitions: (a) Those suggesting agency of the living or the dying, and (b) those suggesting agency of the deceased. Mr. Tyrrell's own fourfold classification comprises: (1) Experimental cases, as when a man successfully wills that his form appear to a distant friend. (2) Crisis-cases, as when an aviator, shot down in France, appears to his sister in India. (3) Post-mortem cases, as when the form of a man's deceased father appears as if to reproach his son, the son's wife also seeing the apparition. (4) Ghosts, which persistently appear in specific places, and to a variety of persons who "see" them simply by virtue of living in, or visiting, those places.

In the reviewer's judgment, by far the most important original contribution of the monograph lies in its systematic study of the psychological attributes of apparitions. It deals with those common characteristics which make them a distinctive class, and also with the variations which permit establishment of sub-classes. The result is

* Phantasms of the Living. An Examination of Cases of Telepathy between Living Persons printed in the "Journal" of the Society since the publication of the book "Phantasms of the Living," by Gurney, Myers and Podmore, in 1886, *Proc.* S.P.R., Vol. XXXIII, pp. 23-429.

to demonstrate *consistencies* or *uniformities* running through an enormous range of human experiences—a basis for a Linnean system for establishing species. Here an ingenious device is used to classify the general "type." Just as you would have little difficulty in approximately indicating all the characteristic qualities of a rose or a horse, though no *one* rose, or horse, had *all* the qualities, Tyrrell defines (pp. 54-5) the observed qualities of the "perfect apparition," which is a composite of the attributes of the best authenticated examples (any one apparition having several, but not all of these attributes). He compares this "perfect apparition" with a normal human being, and finds many points of resemblance. Some of these may be summarized as follows:

(1) Both figures would stand out in space and would appear equally real and solid. (2) As regards distance and perspective we would detect no difference between the apparition and the living person. (3) Both figures would be seen clearly or otherwise in relation to the amount of light. (4) Both figures would obscure the background. (5) Sounds, such as breathing and rustling of clothes, would be heard on approaching the apparition. (6) The apparition would probably behave as if aware of our presence. If it placed its hand on our shoulders we would feel an ordinary human touch. (7) It might speak to us, but we would not be able to engage it in any long conversation. (8) We would see the apparition reflected in a mirror. (9) Both figures would cast shadows. (10) On shutting our eyes or turning away our head, the apparition would disappear just as a real figure would do; on reopening our eyes, we would see it again. (11) In addition to its clothes, the apparition might carry a cane or other accessories, and it might be accompanied by a dog or by another human being. (12) The apparition might pick up any object in the room, or open and close the door. We would both see and hear these objects moved.

Points of difference between an apparition and a normal human being are brought out, among them the following:

(1) In the presence of the apparition we might feel a sensation of cold. (2) If we tried to take hold of the apparition, our hand would not encounter any resistance. (3) An apparition would not leave footprints, though its steps can be heard. (4) It is inferred that if a photograph were taken of both apparition and living human being, only the human being would come out. And only the sounds made by the human being would be recorded on sound-recording apparatus. (5) The apparition might disappear suddenly, or vanish into the wall. (6) It might become slightly luminous, and small details of it might be visible to us when we were so far away from it that such details could not normally be seen.

After disposing of the physical theory of apparitions, and accepting a psychological interpretation, Mr. Tyrrell proceeds to formulate a comprehensive view based upon (a) a systematic modern statement of the psychology of normal perception, and (b) an hypothesis regarding telepathic rapport between personalities.

The former will be familiar to those of our readers who have seen Mr. Tyrrell's *Science and Psychical Phenomena;* there as well as in the present monograph we find an articulate statement of the view that the "solid objects" with which an observer peoples space are in fact "constructs" based on sense-data which, immaterial as they are, are so ordered as to provide the conviction that they are "out there," parts of a material three-dimensional world. (This is not Berkeleyian idealism, for a real external thing-in-itself is accepted as acting upon our bodies; but the only *properties* we know are sensory properties.) The theory is developed with full benefit of modern experimental psychology, and of psychoanalytic and Gestalt principles, to show that the sense-data are thrown into a form meaningful to the individual perceiver only by virtue of a very complex quasi-automatic process. The unconscious organizing tendencies of the individual, based on his ceaseless need to deal with sense-data as clues to the handling of objects in space, throw each new mass of sense-data into organized relevant form; thus an idea, albeit one not present in consciousness at the time, can clothe sense-data with full external reality, e.g., one sees what one expects to see.

To explain apparitions, the unconscious dynamics of both agent and percipient require analysis. Mr. Tyrrell proceeds to "compare the consciousness of the agent with the author of a play, and that 'something' within him which works out the idea in dramatic form to the 'producer.' Further, the 'something else' within him which expresses this drama in the sensory form of an apparition" is compared "to the 'executor' or 'stage-carpenter' of the play . . . The apparitional drama is quite clearly in most cases a joint effort in which the producers of both agent and percipient take part" (p. 72). Of course, the construction of the drama is conceived to take place beneath the levels of consciousness, and Mr. Tyrrell describes the processes involved as being "very different from anything which takes place in the physical world and different from purely mental processes as well." He says (p. 73), "There is a good deal in the apparition which suggests consciousness and there is a good deal which suggests automatism. The truth is that we are dealing with something between the two extremes of consciousness and mechanism. We are dealing with something which is to a certain extent *like* an idea, and, at the same time, to a certain extent *like* a pattern. I have thought it simplest, therefore, to call it an 'Idea-pattern.' This

idea-pattern is the dramatic production of the agent's idea; while the apparition itself is the sensory expression of the idea-pattern."

An apparition, being a non-physical reality and hallucination, must accordingly be structured by the perceiver as if it were an ordinary external object. Hence it obeys perfectly the psychological laws of perception; it *imitates* ordinary percepts. Evidence is produced to show that the apparition obeys familiar optical laws, disappearing when the eyes are closed, giving rise to an image in a mirror, etc. The apparition, then, is the way in which the perceiver projects an idea-pattern into his ordinary space; and if the hallucination is veridical, the initial impact is telepathic, depending on the unconscious dynamics of both agent and percipient.

The general viewpoint here will not be entirely unfamiliar to those readers of Myers who have followed the psychoanalytic conception of the unconscious structure-giving properties of the mind; but the present statement is probably the most complete formulation of the relation of telepathy to the unconscious which has yet been offered.

Mr. Tyrrell gives a great deal of attention to the contrasting views of Gurney and Myers regarding collective veridical apparitions. Gurney believed that a single percipient telepathically influences the others present so that they share his experience. Myers held that the apparition actually *invades* the space—yet not the physical space —of the percipients. Mr. Tyrrell easily shows that neither view is completely satisfactory, and offers a conception of his own to the effect that other persons present when an apparition is seen are *relevant* to it. He says (p. 79): "Whatever would be the behaviour of the physical figure of the agent under existing circumstances is exactly reproduced. This thorough imitation of the physical event, which forms the *motif* of the theme, accounts, I believe, for collective percipience. Other people standing near the place where the apparition is seen would, if the apparition were a flesh-and-blood person, certainly see him. Therefore, to carry out the scheme thoroughly, they are so psychologically acted upon as to see the apparition and to see it appropriately according to their positions and distances. Their 'producers' and 'stage-carpenters' must, in fact, be got to collaborate with those of the agent and principal percipient. Thus, collective percipience depends on the physical positions of the percipients, *not because physical facts have anything to do with it,* but because their physical positions bring them into the scheme of the idea-pattern. In a word, it may be said that the explanation of collective percipience of apparition lies . . . in the fact that spectators, by their physical presence, become *relevant* to the theme of the apparitional idea-pattern and, because relevant, are drawn into it."

This theory of "relevance" is interesting and is a step in the right direction; another hypothesis, however, is possible. What Mr. Tyrrell *almost* says, again and again, is that apparitions are a part of the *psychological space* of the observer. Now psychological space, *space as we experience it in everyday life,* as has been experimentally shown, is non-symmetrical, fluid, expressive of individual attitudes; it is essentially like all things of the mind. Now, what makes the phantasm of A, thinking intensely of his friend B, appear to B is in part the fact that *A's psychological space*—as he thinks of B and of B's surroundings—*overlaps B's psychological space.* Further, telepathy seems to depend to some extent on *common features* in the minds of agent and percipient, for we are bound together by a network of related associations. Hence, since the psychological space of B, C, and D, who are together, is also somewhat similar, A's thought of B may well provide a collective apparition for all three.

It must in candor be added that neither Mr. Tyrrell's conception of relevance, nor the reviewer's comments on psychological space, does anything much to remove the essential mystery of telepathic contact between persons, except in so far as they emphasize that physical space, having nothing to do with the matter, does not in fact "separate" persons. The reviewer is, moreover, puzzled that Mr. Tyrrell has not referred here to Whately Carington's "one big mind" theory, with which he is presumably fully familiar. This is an ambitious attempt to determine the *modus operandi* of psychological contact between minds. Mr. Carington's theory, we understand, is soon to see the light in fully articulate form; perhaps Mr. Tyrrell felt that it would be ungracious to refer to it before its publication.

Mr. Tyrrell has probably added to his difficulties, rather than lessened them, by excluding precognitive apparitions. Two mysteries are not necessarily worse than one if they throw light upon one another, and Bergson and Einstein alike make it seem probable that we cannot successfully attack the problem of space without facing the problem of time.

On the other hand, Mr. Tyrrell's discussion of clairvoyance is highly pertinent and helpful. He points out that the evidence for clairvoyance (the direct perception of physical objects by other than sensory means) is far from satisfactory; that, indeed, because of the possibility of precognition of the experimenter's mind as he checks the results, there are many difficulties in the way of devising an experiment which would test the hypothesis of pure clairvoyance. He discusses, however, the manner in which his own apparatus* for testing supernormal perception might be adapted to test pure clair-

* For a description of this apparatus, see *Proc.* S.P.R., Vol. XLIV (1936-37), pp. 99-168. Also *Journal of Parapsychology,* Vol. II, June, 1938, pp. 107-118.

voyance. This apparatus might easily be used to score *total* hits, the experimenter never knowing the target for any individual call. The results of such an experimental situation, if extra-chance, could not be accounted for in terms of precognition and would indicate the existence of a true clairvoyant faculty.

As regards the bearing of apparitions upon the survival problem, Mr. Tyrrell's argument is straightforward. Most apparitions of the living emanate from the personalities of those whose forms are seen. In most post-mortem and ghost apparitions, we must, if we are consistent, attribute the *initiation* of the impulse to the deceased, though of course the form which it takes in the percipient's experience is, as usual, due in considerable measure to his own personality. It is, moreover, emphasized that we do not get far by constantly asking ourselves whether a given experience settles the survival question. We do not yet know enough to put our questions to Nature in an answerable form. But apparitions do a great deal to *enlarge our understanding of human personality,* and hence may indirectly yield data which will in time clarify our questions about survival.

Towards the end of the paper there is much trenchant discussion of possible experiments upon hallucinations, normal and supernormal, especially through the aid of hypnosis, with a view to a better understanding of the unconscious dynamics by which an idea gives rise to the externalized, dramatized apparition. In particular, a plea is made for an attempt at the experimental production of *collective* apparitions. It is certainly to be hoped that skilled hypnotic operators with suitable somnambulistic subjects will heed this plea, for such an experimental program is long overdue.

If it be regretted that all the maturity and wisdom brought into focus in this paper, published in 1942, still leaves us not so very far from where we were upon the publication in 1886 of *Phantasms of the Living,* it must be pointed out that from Newton to Einstein was 250 years, that the number of workers in our field is small, that fresh materials different from the old are few, that reluctance to attempt the present type of incisive, systematic thinking still prevails, and that a blind fear of experimental methods, which are somehow conceived to spoil and deflate reality, persists almost unabated today. It is earnestly hoped that the three salient features of the present monograph will become more and more characteristic of psychical research: its direct confrontation of Nature on her own terms; its patient and systematic ordering of the data into intelligible classes with the use of bold, yet flexible, theory, and its specific recommendations for the use of experimental methods by which theories can be tested, and principles finally established.

Gardner Murphy

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CONTENTS

Psychical Phenomena and Human Needs

GARDNER MURPHY

INTRODUCTION

Physical events depend upon energy; and as far as we know psychological events depend upon motives. More and more clearly, as we have learned to observe human nature, we have found that perception does not "just happen"; rather it expresses the dynamics of individual personality. We do not see with our eyes; we see with everything that we have, everything that we are. We manage to exclude to some degree what we do not wish to see; we manage to focus upon, or anchor upon, that aspect of reality which comes to meet our needs. This principle appears to apply to every form of perception, both normal and paranormal. It therefore seems appropriate in psychical research to study, as closely as we are able, the way in which the individual focuses his powers of perceiving, emphasizing what he needs, excluding what he cannot use.

It will never be enough to say that a person perceives paranormally because he is "gifted." Granted that he must

have the gift if he is to see, questions arise as to *why* he sees on some occasions but not on others, *why* he picks out the things he does, *what sort of relation* obtains between the kind of person he is and the things that come through to him. Two men faring through a forest on a murky night will see different things not only because they have different eyes, but also because they have different expectations, different hopes, different fears. And behind the hopes and fears that they can put into words lie *deeper patterns* of hope and fear—a whole system of needs and attitudes—that express different individualities. Perhaps the effort to understand a little more fully the role of motivation will help towards the ultimate goal of understanding all the paranormal processes.

It was, I think, William Mackenzie who first systematically explored the role of such personal dynamics, especially unconscious dynamics, in relation to our field.[1] For him the deepest needs of the sensitive, as well as those of the sitter, constituted centers of energy which sought expression in the phenomena of reverie and trance, never projecting their form upon the upper level of consciousness in a *simple, mechanical* way, but expressing that *plastic, creative* vitality which characterizes all unconscious dynamics. To be sure, Mackenzie seems today to have given psychoanalytic interpretations a wider role than is necessary, in view of other sources of knowledge about motivation; and he stressed the "teleplastic" phenomena of materialization far more than seems warranted. But the fact remains that his work threw into sharp relief the need for a full and patient consideration of all paranormal phenomena in the light of human needs.

In this connection, we cannot emphasize too strongly that the time has passed when one could set paranormal experience in *opposition* to normal experience, regarding the tendencies of the two types of experience as antithetical. There is no place today for that type of analysis which tries

[1] *Metapsichica Moderna*, Rome, 1923.

to force a paranormal dream into a special category where the normal psychology of the dream is no longer operative. As far as we know, the laws of Nature do not suddenly abdicate. Rather, it is through the normal psychology of the dream, or of waking life, that special conditions are sometimes achieved in which the paranormal may emerge. As Mackenzie said, it is the one who delves into the depths of the normal personality who will come out with a clue to paranormal dynamics.

Throughout the history of our subject, spontaneous cases of telepathy, clairvoyance, and precognition have been more and more systematically studied with these considerations in mind. If one surveys, year by year, the cases reported by the Societies for Psychical Research, one finds, as we approach the present time, more and more attention given to dynamics, and an ever-increasing interest in conscious or unconscious motivation. Standards of *evidence* were established at a high level in the 'eighties, but on the psychological side the early cases often seem relatively bleak; almost nothing but the obvious surface wants and interests of the percipients is discussed. The contrast is striking if one looks at recent publications; they are full of good psychological suggestions; the cases "live" in more vivid, dynamic terms.

But here it must be confessed that there is a very, very long way to go; we are only catching glimpses, not seeing vistas. We know but little about the vast world of unconscious needs. What the psychiatrist has picked up is mostly what is forced upon his attention by disturbances in the universal biological tensions connected with reproduction and self-preservation; there has not been time for the recognition of the less obvious, more complex, more highly individualized tensions, nor for the study of the inner characteristics of the creative processes themselves. Consequently, the present paper is an *essay* only. It offers *tentative* hypotheses about human wants in relation to paranormal ways of perceiving, and it may be useful in focus-

ing our study of future spontaneous cases, or even an aid in designing our future experiments; but it is *not* a system of beliefs to be defended. It is an experiment in a way of looking at our phenomena.

Normal Perception, Memory, and Thought

We shall begin by referring to the recent revolution in psychology relative to the role of motivation in our ways of making contact with the outside world. Almost all studies of perception, of memory, and of thought up to the present century emphasized the sense organs and the brain as instruments for mediating reality to us. Philosophers might argue as to the existence of an external world; but in the practical task of trying to explain why a man saw, they followed Newton in studying light and Helmholtz in studying the structure and function of the eye. Similarly, they studied memory as an expression of traces left in the brain by past experience, and thought as an intellectual reorganization of these materials. The problem has been revolutionized by studies which resulted in the formulation of three principles: (1) The principle of figure and ground, (2) the principle of mental set and (3) the principle of autism.

(1) The principle of figure and ground indicates that experience is never in the form of a homogeneous blur or global total, but that a part always stands out against a background; the part which is *figure* is accented by the perceiver or thinker, and is usually the starting-point for other mental processes or for action, the rest being ignored.

(2) But as we confront a new situation, one aspect of it may be the *figure* for one man, another aspect for the other man. What determines the issue? Experimentally it may easily be shown that one is in a state of readiness, or *mental set,* for one aspect, not for another. Give your subject a collection of varicolored objects of different shapes, and ask him to count the number of red ones. When he

has done this, ask him how many of them were triangular. He is lost. Tell another subject to ascertain how many are of triangular shape. He does well; but he cannot tell how many were red. One does not perceive "what is there," and much less does one recall "what is there." *Mental set* determines what is to be figure, and the rest is neglected.

But what kinds of mental sets are there, what is their relative importance, and how are they interrelated? Very broadly, any motive, tension, or need constitutes a mental set. The Arctic explorer dreams of verdant fields; even the wary psychical researcher "sees" a cheque in an opened envelope, admires its pale marbled colors—a cheque which unfortunately was never there![2] The mental sets of the laboratory are temporary affairs, but they operate exactly as the more enduring needs operate. As far as we know, mental sets are temporary tension systems within the organism, and are functionally like the tensions which we recognize when we speak of anxiety or hate as involving tensions. All the human motives which have been closely studied have proved to consist of tensions—regions of high energy concentration. We shall then conclude that there are as many mental sets as there are dispositions of the individual to want, or need, to experience tension; that their relative importance is the relative importance of the underlying needs in that particular person; and that since all parts of the organism intercommunicate with all other parts—tensions flowing and spreading to arouse other regions—there are mixed motives, multiple tensions.

(3) The principle of *autism* is the "tendency of cognitive processes to move in the direction of tension-reduction." The wish is not only father to the thought; it is the father of the perception, and of the act of recall. One sees, recalls, thinks, not only in terms of the structure of the thing actually given, but in terms of the figure-ground relations, the mental sets at work. It should be clear that the term "wish" is insufficient. A man may see not only what he

[2] *Journal* S.P.R., Vol. XXVII, January, 1932, p. 184.

wishes but also what he fears; the real principle is not the wish as such, but the tension. The need may quite literally be the "need" (in the biological sense) to see a burglar when one is in the tension of frightened uncertainty, or to find an insult in an innocent remark. Most autistic processes may give pleasure, because they reduce tension and this is usually pleasant; but this is incidental. The *important* thing is to study the tensions of the individual, and to note how the experience is organized for the individual around these tensions, while for a person with a different system of tensions a different organization of experience is found.

Since the concentration of energy within a given region is not static, but tends to increase or decrease, we have learned to associate motives with the decrease of such energy concentration; in other words, with the *reduction of tension.* Popular speech and common sense coincide in stressing tension as an aspect of motivation. A man is "tense" until he has allayed his fears, vented his rage, cleared himself of an unjust charge. One speaks of getting a thing "off one's chest." Though analogies are dangerous, what happens in the organism when tension is reduced might well be compared to what happens in a chemical reaction (such as a fire or an explosion) when concentrated energy is dissipated in heat or other forms of energy.

It may reasonably be asked whether distressing experiences, such as continuing anxieties or nightmares, really arise from motivation. We have already indicated that the process of motivation is not necessarily pleasant. But we must add that anxiety and other related tension states are typically efforts to defend oneself against threat or danger. They are attempts to be prepared against a surprise; thus fear may be regarded as a device for getting us out of the situation which the danger would bring us were we unafraid.

This, therefore, gives us our hypothesis: Perception occurs in such fashion as to reduce tension, or to meet needs.

Experiments in Autism

Let us look to psychological experiments to see whether this is in fact the case. At Cambridge, England, F. C. Bartlett[3] devised a number of methods for the testing of the autistic tendencies. Narratives presented to a subject and relayed by him to another subject underwent a series of transformations; each person participating in the retelling threw out items which were unacceptable from his point of view and unconsciously reconstructed the story to make it compatible with his own ways of thinking. In the Columbia University laboratory, Sherif[4] showed that judgments of the distance transversed by a point of light in the dark-room consistently indicated the *need of the subject to be in agreement with others,* i.e., "the leveling influence of the group"; and in a later study that a naïve subject systematically adjusted her way of seeing until she saw the light as an expert would see it. At Harvard, R. N. Sanford[5] found that ambiguous pictures were interpreted in terms of food objects with increasing frequency as his subjects went longer and longer without food, and R. Levine[6] found similarly, at City College, that ambiguous pictures shown behind ground glass screens were more and more frequently interpreted as food objects as the hours after eating a meal increased. We may, then, say that in all these cases perception moves in the direction of need satisfaction, tension reduction. To some degree the narrative and the ambiguous picture can be "perceived" in such a way as to relieve tension. The autism operates in a way which permits a satisfying or tension-reducing aspect of the perceived object to be selected as figure, the non-satisfying aspect serving as ground. In R. Levine's experiment, for example, those lines and shadings which sug-

[3] Bartlett, F. C., *Remembering,* Cambridge University Press, 1932.

[4] Sherif, M., *The Psychology of Social Norms,* New York, Harpers, 1936.

[5] Sanford, R. N., "The Effects of Abstinence from Food upon Imaginal Processes," *Journal of Psychology,* 1936, 2, pp. 129-136.

[6] Levine, R., Chein, I., and Murphy, G., "The Relation of the Intensity of a Need to the Amount of Perceptual Distortion: A Preliminary Report," *Journal of Psychology,* 1942, 13, pp. 283-293.

gested food were thrown into relief and organized into food objects; the rest was neglected.

This hypothesis about autism and the figure-ground relationship was directly tested by R. Shafer[7] at City College. He presented to his subjects, during a "training" period, opposite complementary aspects of contour lines, each aspect being capable of being seen as a face in profile. Whenever one aspect of the face was presented he gave his subjects a small money reward, taking away the money whenever the other aspect was shown. As a result of this training period, such firm associations were established that when rewards were discontinued, and the subjects were shown the contour line within a complete circle (in which situation either of the two profiles could be discerned) they saw only that face which had been "rewarded," not the face which had been "punished." The subjects were in general *unable* to see the face except in the way in which it had been rewarded; that face became *figure* for them, the opposite aspect *ground.* They learned, that is, to organize or experience autistically, in a drive-satisfying way.

Motive and Figure-Ground Relations in Spontaneous Telepathy

Though these aspects of tension reduction and of autism seem obvious enough when attention is directed to them, it is, I think, likely to be forgotten that *paranormal* perception usually has been studied in terms of the older view of perception — perception treated without reference to motives, almost as if it were a mirror. In the light of our present approach, it would seem much more likely that paranormal perception, as well as normal perception, is organized in terms of *needs;* in all the vast world, one selects to serve as "figure" that which is most directly related to one's motivation. Since we have found, decade

[7] Shafer, R., and Murphy, G., "The Role of Autism in a Visual Figure-Ground Relationship," *Journal of Experimental Psychology,* Vol. 32, No. 4, April, 1943, pp. 335-343.

by decade, more and more reason to believe that Myers was right in emphasizing the unconscious nature of paranormal processes, it would naturally be to the unconscious, but profound and enduring, motivations that we should turn for our clues.

Suppose, for example, that a mother is busy with her housework or with her baby and has at the time no conscious need to concern herself with her older boy, who is away on a trip with his companions. Suppose, though, that the boy falls into a pond and narrowly escapes drowning. If the mother has a vision in which he is portrayed as sinking and calling for her, we need not regard this as something which emanates exclusively from him, as he knocks, so to speak, at the door of her mind. Rather, it is probable that she has been unconsciously, but continuously, "reaching out" to him in her need to know where and how he is. Regardless of her conscious activity at the time of the crisis, we may regard her as unconsciously needing contact with him, and keeping, as it were, an eye upon him whether she is awake or asleep. We should expect paranormal impressions to come to the individual not simply in terms of the possession of psychic gift but rather in terms of what it is he basically wants to see, wants to hear, wants to know. If we could understand his unconscious motives, we would know what it is that is open to him. And just as we said above, in reference to the often painful character of what is autistically perceived (as deriving from our need to know the worst in order that we may protect ourselves from it), so we can meet the objection that many paranormal impressions, far from being "satisfying," are in fact distressing. They are, I think, distressing because those who experience them love the persons who at the time are undergoing accident or other tragedy. It is not that the mother wants her son to be in danger; it is, rather, that if he must be in danger *she wants to know about it.*

We can now proceed to discuss the more obvious motives which appear as clues to paranormal experiences. Although

sometimes motivation may be so complex and so subtle as to evade analysis, we have nevertheless found that a simple, rule-of-thumb classification, in terms of the more obvious and universal human needs, takes in the great majority of the well-authenticated cases of spontaneous telepathy, clairvoyance, and precognition. The motive of *love,* in the broad sense of the feeling between all those who are dear to one another (including relations between parents and children, brothers and sisters, etc., as well as husbands and wives), is by far the largest single category. The various ego tendencies, such as self-justification and the need to protect oneself from danger, also play a large role. Motivation, in certain types of cases, must also be studied from the point of view of the agent as well as from the point of view of the percipient.

Motives, moreover, as we have seen, are interconnected, and in dynamic relation to one another. Two conflicting motives may at times paralyze the individual. More frequently, one motive may be victorious and the other blocked and held in abeyance; *e.g.,* deep hatred may be covered over by a moral education which enables the individual to believe that he is devoid of all such socially undesirable emotions, yet the hate may express itself indirectly. Or at times a union of motives may be effected by the discovery of objectives which satisfy them both simultaneously.

Before proceeding to the classification proper, I wish to refer at some length to a recent paper[8] by Miss G. M. Bishop, a Member of the S.P.R. Because of her astute recognition of the importance of motivation and of the subtleties displayed in their operation, it is urged that the paper be read in full; the summary below can do justice neither to the dreams themselves as evidence for precognition nor to the author's keen analysis of their psychological and physiological background. Those who read the original will note the *combination of motives,* as in Miss Bishop's fear mingled with love of animals, and the *conflict of*

[8] *Journal* S.P.R., Vol. XXXII, April-May, 1941, pp. 50-59.

motives, as in her sense of guilt overpowered by her desire to maintain an acceptable picture of herself.

Miss Bishop prefaces her tabulation of 14 dreams experienced during a three years' period by explaining that she became interested in watching her own dreams from the point of view of possible precognition after reading J. W. Dunne's book, *An Experiment with Time*. During the autumn of 1935, therefore, she began to keep a file in which she noted a number of dreams which might relate to future events. Her purpose was not to obtain evidence, as such, that precognition occurs (she lived alone and corroboration would not have been feasible); rather her aim was to see if she could throw some light on the dynamics of precognitive dreaming. Most of her dreams seemed to relate to material contained in the next morning's newspaper. She remarks that, living alone and quietly at the time, the paper was the chief event of the day. Quoted below are two such dreams from among the 14 presented in her tabulation:

No. and Date	Summary of Dream	Apparent Reference	Comment
11. Sept. 19, 1937	Was on a mountain-side, afraid to go on, as I was on the edge of a precipice. A man whom I felt to be a good mountaineer, offered to take me roped to him, but I was afraid if I fell I might pull him down too.	*News Chronicle* for Sept. 20 had an article prominently headed "Rope Saves Climber after 200 ft. Mountain Fall. Lowered to Safety by Doctor Companion," and a smaller article below it headed "Londoner's Crash Down Cliff." The first related the rescue of a Cambridge undergraduate.	No memory of any other similar dream. Am very nervous of heights. Am a graduate of Cambridge myself. Possibly a case of clairvoyance in space [but Miss Bishop rejects this hypothesis in later discussion].
13. Feb. 27, 1938	Dreamed I could not find my dog, then heard that she was in a room which was terribly hot. Went to the room and rescued her—it was as hot as a furnace and I pulled her to safety in the open air.	*Telegraph and Post* for Feb. 28 (I had changed from the *News Chronicle* recently and was taking the *Telegraph*) had a story headed "Families Escape. Men go back for Pets," describing the rescue of a dog and cat from a bedroom which was "like a furnace" following a fire.	Have dreamt at intervals that I had lost my dog, to whom I am much attached, but not that I had to rescue her from a fire.

Miss Bishop's first observation of a general nature is that, although her waking interests are mainly intellectual, not one of her dreams related in any way to these interests. The subject-matter of the dreams was of an apparently trivial sort and completely useless as material for action. In almost every case, however, the material had strong emotional significance for her and this she traces in some

detail in all the dreams presented. In regard to the two dreams quoted above, she says, "I have all my life had an instinctive fear of heights, so much so that though I lived in Switzerland for years I never dared to climb. This dream is therefore linked with a subconscious fear," and "I am so attached to my little dog that anything involving or suggesting her is of strong emotional significance to me." Miss Bishop also made some observations of a physiological nature: in the autumn of 1935 she was convalescing from a serious illness (during which there were periods of somnambulism) and was still somewhat nervously exhausted. At this time she collected the greatest number of dreams for her file. Later they became rare; when they did occur she noticed that it was when she was sleeping less well than usual. She says, "In my case, therefore, there seems to be a relation between nervous disturbance and precognitive (or clairvoyant) dreaming. It is as though when my nerves are at rest my dreaming mind is content to chew the cud of past happenings. When I am in a disturbed state, on the other hand, the dreaming mind has a tendency to peer around to see what is ahead."

A Classification of Spontaneous Cases in Terms of Motivation

We shall now give a seven-fold classification of spontaneous cases, in terms of the internal evidence as to their motivation, with typical examples of each. (We wish to thank Mr. W. H. Salter, Hon. Secretary of the Society for Psychical Research, London, for permission to quote from the *Journal* of that Society.)

(1) *Love—the need for contact*: The first case to be quoted under this heading is a remarkable precognitive dream of Samuel Clemens ("Mark Twain"). It is abridged from the report first appearing in the biography, *Mark Twain,* by Albert Bigelow Paine. The dream concerned the death of Mark Twain's brother, Henry Clemens, in the explosion of the steamship *Pennsylvania.* The famous humorist was said to be "lavishly fond and proud" of his young brother.

One night, when the *Pennsylvania* lay in St. Louis, he [Mark Twain] slept at his sister's house and had this vivid dream:

He saw Henry, a corpse, lying in a metallic burial case in the

sitting-room, supported on two chairs. On his breast lay a bouquet of flowers, white, with a single crimson bloom in the center.

When he awoke, it was morning, but the dream was so vivid that he believed it real . . . He told Pamela the dream, then put it out of his mind as quickly as he could . . .

Then came the news, "The *Pennsylvania* is blown up just below Memphis, at Ship Island! One hundred and fifty lives lost!"[9] . . . Many persons had been killed outright; many more had been scalded and crippled and would die. [Henry Clemens was among these.] It was one of those hopeless, wholesale steamboat slaughters which for more than a generation had made the Mississippi a river of death and tears.

. . . He saw the boy taken to the dead room; the coffins provided for the dead were of unpainted wood, but the youth and striking face of Henry Clemens had aroused a special interest. The ladies of Memphis had made up a fund of sixty dollars and bought him a metallic case. Samuel Clemens, entering, saw his brother lying exactly as he had seen him in his dream, lacking only the bouquet of white flowers with its crimson center—a detail made complete while he stood there, for at that moment an elderly lady came in with a large white bouquet and in the center of it was a single red rose.[10]

We have begun with a precognitive, rather than a telepathic case, which may serve to bring out the fact that the dynamics appear to be the same whether telepathy, clairvoyance, or precognition is involved. Our hypothesis would be that at the time of the dream nothing existing in the world meant so much to Mr. Clemens as his young brother, and that in all of time—past, present, or future—nothing was so important to him as his fate.

In the same category as the dream of Samuel Clemens, we may briefly discuss an experience of Miss Lilian Whiting's. It shows the same intensity of relationship between the persons concerned, but without the bond of blood-kinship. The content of the experience was purely emotional rather than sharply defined as in the dream above. Moreover, we are dealing with a waking rather than with a dream experience. Our purpose is to show that motiva-

[9] The original account does not say how much time elapsed between the dream and the accident.

[10] For a somewhat fuller account, see *Noted Witnesses for Psychic Occurrences*, by W. F. Prince, B.S.P.R., 1928.

tion is the key whether the experience be precognitive or telepathic, sleeping or waking, sharply defined as to content or otherwise. In discussing the case Dr. Prince[11] wrote: "Miss Whiting has had many psychical experiences, some of them of impressive evidential character. The one which follows should be considered on the background of Miss Whiting's specially intimate relations with Kate Field . . . We learn that when a girl she adored Miss Field for her literary work, and afterwards had fifteen years of close friendship with her." Miss Whiting describes her experiences in connection with Miss Field's death in her own words:

> The voyage had been an ideal one, full, to me, of a curious uplift of feeling that suddenly changed, the day we landed, to a sadness and desolation inexpressible, and for which no adequate cause could be even faintly conjectured . . . This swift change of feeling from exhilaration of spirits to an unutterable desolation was initiated by an experience for which I can suggest no explanation; but the occurrence was one which leaves an impress that will forever stand as a crisis hour in life. It was this.
>
> On our last night on shipboard we had enjoyed the usual merry time of the "Captain's dinner" with its gala and laughter, and had retired with the happy anticipations of landing at Liverpool in the early morning. I had been asleep for some hours when, suddenly, as if by an electric shock, I found myself standing on the floor of my stateroom with the quiver of a current of electricity pervading me from head to feet as if I grasped a strongly charged battery . . . The words I had just heard—not with the outer ear, but with some inner sense—vibrated in the air. For I had seemed to see standing, I knew not where, three forms, which, by the same inexplicable inner sense, I knew were in the ethereal, not the natural, world; I seemed too, to know that one of these had but just entered that world, and I heard her say, in tones of mingled joy, amazement, incredulity, and triumph, "Is *this* all? It is all over!"
>
> Then I said to myself: "Some one I know has just died, some one whose death will make the greatest difference to me." Yet, strangely, I did not think of *her*—in whose presence or absence the entire world always changed to me—she with whom I constantly lived in thought, whether we were together or whether half the world stretched its space between us . . . On awakening in the

[11] *Ibid*, p. 233.

morning that dread desolation, unanalyzed and unaccountable, settled down over me.

. . . On that June day in Paris, I learned from a cablegram that she [Kate Field] had already been for more than two weeks in the life beyond (and) that her death had occurred the very day that I landed at Liverpool . . .[12]

(2) *Self-protection*: In this category we intend to include those intimations (either dreams or waking impressions) of danger to self or to others—danger which is sometimes averted because of the impression. A straightforward example of an individual protected from death can be found in a case presented by Dr. Prince in our own JOURNAL. As a result of a series of vivid dreams, Thomas King refused to sail on a certain ship. The ship went to sea and the first night out broke to pieces in a storm. Everyone on board perished.[13] Another case illustrating the same factor of self-protection, but with another factor included as well, is that of a medical practitioner who, by driving in a manner altogether alien to his usual habit, protected himself from a smash-up and made possible his effective ministering to persons who were injured. We quote this case nearly in full from the *Journal* of the S.P.R.[14]

We have received the following narrative through the Countess of Balfour, to whom the circumstances were described by Mr. Eames, of whom she is a patient, within a fortnight of the occurrence. He afterwards very kindly wrote out this account, incorporating in it the answers to some questions we asked.

NARRATIVE STATEMENT OF MR. E. G. EAMES.

On Saturday May 24th 1930, I was driving as usual from St. Albans to London, along a road I knew exceedingly well, for I had traversed it hundreds and hundreds of times. My car is a fast one, I drive fastly, for I am always in a hurry, and on this particular

[12] To summarize, Miss Field's death in Honolulu and Miss Whiting's experience on shipboard took place on May 19, 1896. For a full account of this and subsequent experiences, see *After Her Death* (Boston, 1901) by Lilian Whiting.

[13] JOURNAL A.S.P.R., Vol. XVI, 1922, pp. 172-174.

[14] *Journal* S.P.R., Vol. XXVI, October, 1930, pp. 117-118.

morning I had some five operations to perform in London before lunch.

But a preceding car had for long prevented me from accelerating to a really quick speed. It was a Jowett car, being driven carefully and quietly by a typical family man, in a typically leisurely manner . . . In the ordinary way I should have passed by, as I pass by hundreds of similar tourists and forgotten them—miles back. But on this occasion I absolutely could not pass. It was not the car that prevented me. On the contrary it would have been easy to have got by, and I wanted to get by, but something, some exceedingly strong something, insinuated into my subconscious brain that an accident was going to occur . . . It was in no ways concerned with the driving of the man in front. Actually he was driving very well. It certainly wasn't nerves, it was a very real presentiment that a crash was going to occur and a warning not to approach too near. For five miles then I hesitated behind this slow-going old car, blaming my foolishness, but very much aware all the time of a holding back. Soon, however, I was to be very grateful indeed for this intimation for when we reached a point in the Watling St., between Radlett and St. Albans the looked for accident occurred.

A lorry preceded the Jowett which was immediately in front of me. The three of us slowly making our ways to London. On the opposite side of the road, a large saloon car had stopped. Suddenly the door of the stationary car opened, the huge lorry, obliterating the entire view of the road could not quite get by. Without warning he stopped abruptly. The poor little Jowett went on. Turned out, found his road completely blocked in every direction, braked violently, skidded and crashed first into the back of the lorry and then onto a brick wall. And for five miles I had known very distinctly that it was going to happen. So very much so that during that time I had felt very inclined to go up and mention my presentiment to the driver of the little family car.

All that remains to say is that being immediately behind I must in the ordinary way have been smashed up too, but had kept just far enough back to be able to come to a standstill immediately beside the wrecked car. I attended to the little child, who was badly cut, and rushed both he and his mother . . . to the hospital . . . This is the third time in my life that I have felt these strong compelling forces or influences warning me of danger, and always I have been afterwards very deeply grateful that I have been fore-warned.

(3) *Ego, including guilt feelings, need for self-justification,* etc.: We find it worthwhile to make a distinction

between *self-protection* in the simple physical sense, and *self-justification;* that is, the maintenance of one's self-respect and the guarding of one's favorable picture of oneself. These latter we may broadly call the ego-motives. Here we may refer briefly to another medical example, that of a doctor who, while cruising near the West Indies, had a vivid impression that one of his patients in England needed him On returning home the percipient learned that this patient had in fact called at his office at about the time of his impression.[15] Undoubtedly, however, a fusion of many motives may have been involved here, including professional concern for the welfare of the patient, etc. Or we might refer to the very complicated case of the resident of a town near Washington who dreams that he is to become secretary to a congressman, a form of professional advancement of which at the time he had no intimation.[16] Here also should be included Mrs. Dale's dream of the vacuum cleaner, which was reported in a recent issue of this JOURNAL.[17] The dream appears to have been produced in a struggle to achieve a paranormal result after failure in an experiment the day before, but with the additional motive of love for her little dog entering into the picture in a manner reminiscent of some of Miss Bishop's impressions. Like Mrs. Dale, Mrs. Kenneth Richmond in England seems to have had a precognitive dream to "please" somebody specially interested in such dreams. We quote her original account nearly in full, together with her interesting observations on motivation:[18]

> On 27 September 1933 I listened with great interest to Mrs. Lyttelton's Presidential Address, in which she dwelt specially on precognition and asked members to put on record any precognitive dreams that they might have. That night I had the only precognitive dream that has registered itself in my conscious memory.
>
> In this dream I saw four charabancs, all alike, filled with foreign students visiting London. These students were all wearing black

[15] *Journal* S.P.R., Vol. XXXI, March, 1939, pp. 34-37.
[16] *Ibid,* Vol. XXXI, April, 1939, pp. 47-53.
[17] JOURNAL A.S.P.R., Vol. XXXVII, April, 1943, pp. 95-101.
[18] *Journal* S.P.R., Vol. XXXI, June, 1939, pp. 83-85.

astrakhan caps. The dream struck me as so curious and senseless that I mentioned it to my husband on waking. On the same morning I took an unusual route to my office . . . in order to change some foreign money for a child who was staying with us . . . As I was about to enter the bureau, four charabancs passed, filled with people all wearing black berets. Large labels on the windscreens announced that the party belonged to a foreign student organization. The sight of them gave me a distinctly uncomfortable sensation, a sense of strangeness at the close resemblance of the scene to the one of which I had dreamt.

The dream showed no labels on the windscreens, but the information conveyed by these labels appears to have been represented by the dream-impression that the occupants of the charabancs were foreign students. The only complete discrepancy was the appearance of black astrakhan caps in the dream and black berets in the reality . . .

I have generally found that any psychical experiences that I have had were in response to some need that could be traced, either my own or another's. In this case, it seems that the need was that impressed upon my mind by Mrs. Lyttelton in her Presidential Address, for more recorded cases of precognitive dreaming. As I have noted, the dream appeared at first sight to be particularly senseless, and was put on record for that reason.

Guilt-feelings may be important from the point of view of motivation in paranormal experiences coinciding with death or disaster, as in the case of the man who, when his neighbor committed suicide, heard footsteps and then a "most appalling shriek—a prolonged wail of horror."[19] The unfortunate young man had begged the percipient to spend the evening with him and the latter had refused, pleading a prior engagement which he in fact did not have. The percipient's wife, sitting nearby, heard nothing. It is not difficult to see that the tragedy might have been *figure* for the percipient, *ground* for his wife.

Finally, the ego cases should include simple self-gratification, or gratification of vanity, as in a striking case in which the well-known sensitive, Mrs. Warren Elliott, was the percipient. This case has the additional merit of showing the role that humor may have played as a secondary

[19] *Phantasms of the Living*, Vol. I, pp. 222-225.

motivation. (Whether the communicator was in this instance an independent entity or a part of Mrs. Elliott's own psychology need not be discussed here.) The case is summarized from the *Journal* of the S.P.R.[20]

One morning Mrs. Elliott received a letter from one of her regular sitters, a Mrs. *Hammond* (pseudonym), saying that she was sending her a birthday gift from Selfridge's and that she thought Mrs. Elliott would be "amused" at the contents of the package. Later on in the morning, while reading another letter, Mrs. Elliott became aware of the presence of Mr. *Hammond* (the deceased husband of her sitter), who said, "They are nightdresses Mrs. *Hammond* is sending you." He then showed Mrs. Elliott a white one—"and there are two other shades," he said. Mrs. Elliott thereupon called up Mrs. Hammond, who confirmed that the gift package, which arrived several hours later, contained three nightgowns. One was white and the other two were of different shades. [The incident is well corroborated and normal explanations seem unlikely.]

(4) *Money, or other material gain*: In this category we wish to discuss the motive of material gain, specifically the money motive. Though we are aware that from the psychoanalytic point of view money as such is not a truly basic interest, we must at least recognize that the paranormal cases exhibiting the money factor are numerous. First we quote from the *Journal* of the S.P.R. a case which apparently arose from a man's concern with his failure to receive money which was rightfully his. In February, 1938, the percipient, Mr. E. J. Harris, wrote as follows:[21]

. . . Some few years ago [Mr. Harris states that the dream first occurred in 1925 or 1926] I was constantly having a recurrent dream. In this dream, I went to my mother's old desk and took from a spring drawer, which I knew of, a sum of money which I counted, and then put back. Gradually the amount increased, until on my last dream visit it amounted to sixty pounds . . . Recently [October, 1937] my sister, whom I had not seen for some time, came to visit us, and I repeated the story of my dream to her . . . the following day she told my wife and myself that she had passed a sleepless night, and felt constrained to tell us that just before my mother

[20] *Journal* S.P.R., Vol. XXVI, October, 1929, pp. 143-145.
[21] *Ibid.*, Vol. XXX, April, 1938, pp. 197-198.

died, she told my sister that she had saved a sum of money to be given to me, which was then in the Post Office. She signed an authorization for my sister to get the money, and shortly after passed away. When my sister drew this money from the Post Office, she found it was exactly sixty pounds. As I was at that time in receipt of a good income, my sister kept the matter to herself, and used the money to help pay for a trip . . . [Corroboratory statements from the percipient's wife, sister, and son are appended. It is to be noted that Mr. Harris' mother died nearly 20 years before the first occurrence of his dream.]

We may also quote the striking case of an hallucination experienced in the gaming-rooms at Monte Carlo, as a result of which the percipient won a sum of money. We summarize from the original report appearing in the *Journal* of the S.P.R.:[22]

Mr. and Mrs. Boustead had been playing roulette at Monte Carlo. As they had not been winning, they decided to return to their hotel for tea. Passing through one of the game rooms, Mr. Boustead happened to glance at a roulette table where, to his surprise, he saw two balls in the wheel. He went closer to see what was the matter, and immediately realized that one ball was real while the other, though exactly like the real one, was blurred and would not come into focus. The "phantom" ball was lying either in number 34 or number 6 (these numbers are next to each other on a roulette wheel); as the wheel was still moving he could not be certain which, although he felt it was number 34. After Mr. Boustead had stared at the ball for at least five seconds it suddenly vanished. He thereupon walked over to his wife, who was standing about ten yards away, and told her what he had seen. He then went back to the gaming table and placed some money on both number 34 and number 6. At the next spin of the wheel the real ball fell into number 34 and Mr. Boustead won.

Finally, in this category should be included an even more striking example, a presentiment relating to the winner of the Derby, which came not to a professional devotee of the race-track, but to an elderly Quaker who was strongly opposed to betting and who had been, in fact, an officer in an anti-betting society![23] As Miss Bishop pointed out,

[22] *Journal* S.P.R., Vol. XXIII, June, 1925, pp. 88-91.
[23] *Ibid.*, Vol. XXVIII, April, 1934, pp. 216-218.

we have here an example of the operation of unconscious motivation. It is not to a person who is indifferent, simply bored with betting, that this experience comes; it is to a person who has taken a determined position *against* it, a fact which suggests the operation of unconscious motives, perhaps wishes and needs which have been effectively repressed by activity in a contrary direction.

(5) *Curiosity*: By "curiosity" we mean the active desire to follow up clues and to resolve an incomplete picture of a situation; and we shall have to make the category broad enough to include so-called "maudlin curiosity" and that whole group of motives which impel certain individuals to read horror stories and, whether in war or peace, the more gruesome details of human suffering and catastrophe. Much of the content of gossip is of the "isn't it terrible?" variety, and publicists and dramatists alike have always known that people "want" or "need" a certain amount of the horrible and the sensational. Sheer idle curiosity is probably of some significance in itself, but it appears to be the kind of curiosity that feeds on "murder stories" which figures most prominently in the cases which have come under our present survey. We are not referring, of course, to cases of catastrophe to one's own loved ones, which are included in the first category; we refer rather to cases of sheer avid curiosity in relation to unknown persons.

A striking example of an experience coming within this fifth category is Dr. Prince's own precognitive dream relating to a woman's head severed from its body, the jaws of which opened and closed upon his *hand,* filling him with horror at the thought of a head which still lived apart from its body. This dream coincided closely with the decapitation of a Mrs. *Hand,* among whose effects was found an insane letter indicating that her head would continue to live after it had been severed from her body.[24] Many of J. W. Dunne's precognitive dreams appear to be dreams of catastrophe to

[24] JOURNAL A.S.P.R., Vol. XVII, 1923, pp. 89-101.

persons unknown to him, such as his dream of the Mont Pelee volcano disaster, in which thousands of persons lost their lives, and a later dream concerning a terrible fire in a French rubber factory, where many working girls were suffocated by the fumes of the burning rubber.[25] The criterion by which the sleeping mind seeks *something exciting* with which to make contact seems to be much the same as the criterion of a tabloid journalist: "Find something exciting and preferably something terrible." It should be mentioned that a secondary ego factor may be involved when it concerns investigators in our field, such as Dr. Prince and Mr. Dunne, who in a sense want cases because they enhance the percipients as not only investigating the experiences of others but also having genuine experiences of their own.

(6) *Agent's need to make contact*: Up to this point, we have primarily concerned ourselves with motivation from the point of view of the percipient, and have said nothing about the motives of the agent. Sometimes, however, we have clear facts regarding the agent's motives, as when, in semi-experimental cases, a person successfully projects an image of himself to an unsuspecting percipient. (See *Phantasms of the Living,* Vol. I, pp. 104 ff.) Indeed, in many reported cases of a different type, agency would seem to involve a "self-expression" motive—the desire or need on the part of the agent to enhance or justify or explain himself. And we must, I think, likewise note the great frequency of cases in which there is strong evidence for the active love of the agent for the percipient. Thus in our sixth category we may refer the readers to another S.P.R. case in which a woman in Scotland is awakened from sound sleep by feeling someone press a kiss on her forehead. Looking up, she sees an Austrian friend of hers standing by her bed. "She looked as though she desired to say something . . . but I was so startled, not to say, afraid,

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[25] *An Experiment with Time,* New York, Macmillan, 1927, pp. 34-37 and 39-40.

I was speechless . . . Her expression was so sad and enquiring I cannot forget it."[26] The percipient's friend had died in Vienna at about the time of the experience. The most striking aspect of this case is the pathetic need of the phantasm to make itself understood. It seemed to want to convey something which it could not express. No compelling activity or need on the part of the percipient, who was "awfully frightened," is evident. This case can be compared with another "phantasm" case, in which the percipient looks up from her book to see her husband standing by the foot of her bed. After gazing at the apparition for a few seconds, she unconcernedly put out the light and went to sleep. She stated that she "was neither alarmed nor excited *nor even particularly interested.*"[27] Her husband had died twenty-four hours before. We always have, I suppose, some degree of activity on the part of the percipient, but it is the compelling motivation of the agent which stands out as the primary clue in cases like these.

Especially striking is a case reported in full in the *Journal* of the S.P.R., in which a group of percipients do indeed have a need to receive a message, but in which the longing of the agent is demonstrably the major clue to the content of the message received. Indeed, the boy's longing was so intense that he seems to have "forced his way," uninvited, into the midst of the group of percipients. The case does not easily lend itself to abridgment, and, again, readers are urged to refer to original material; the summary below, however, may give an idea of the salient points involved:[28]

A spiritualistic circle of six persons at Flushing, Holland, met on the evening of July 23rd, 1922. Of these persons, two had once known English, but had not kept it up; the remaining four were unacquainted with English. By use of a simple wooden device held over the letters of the alphabet, messages were spelt out. On the

[26] *Journal* S.P.R., Vol. XXV-XXVI, Nov., 1930, pp. 133-135.

[27] *Ibid,* Vol. XXII-XXIII, July, 1925, pp. 98-104.

[28] *Ibid,* Vol. XXI, Dec., 1923, pp. 170-175.

evening in question, a "communicator" appeared, stated that he was an Englishman, and indicated that he would write a song for them, see 1a below:

1a		*Evening Song*		*Evening Song*
THE SUN HAS SET	1b	The sun has set,	1c	The sun has set,
AND NOW A new		And now a new		and now anew
WITH F A LL END EW		With fallen dew		With fallen dew
THE GRASS IS WET		The grass is wet.		The grass is wet.
FIrSt parT		And little burd		Each little bird
each little bird		is sing to rest		Has sunk to rest
Has sunk storest		Within his nest.		Within its nest
witH ts netstn		No song is heard.		No song is heard.
O Sng is hear				

Now, a fifteen year old boy lived opposite to the house where the séance took place; he longed to attend one of these mysterious meetings, but was not allowed to. On the evening of July 23rd, he watched the persons entering the house across the way, then, bored and vexed at not being invited, he got out an old school-book which contained the stanzas of an English poem he had once learned at school (see 1c above). He drowsed over this poem for about a half hour, during which time the sitting across the street was continuing. On the following day one of the sitters learned what the boy had been doing, and asked him to *type out from memory* the poem he had been reading. The boy's version is found in 1b above.

(7) *Motive strong from point of view of both agent and percipient*: Finally, we have to deal with a number of cases in which motives seem quite clear both on behalf of the agent and on behalf of the percipient, but which do not easily fit into our first category. These may be divided into two sub-categories: (a) Those cases in which the agent is still alive or at the point of death (cases which Gurney, Myers, and Podmore called "Phantasms of the Living") and (b) cases in which we must say that the agent is unknown *unless* it be in fact a deceased person; that is, cases in which the impression, taken at its face value, would indicate survival and post-mortem agency.

Under the first subheading we might include a case collected by Dr. Prince,[29] in which a missionary sees the apparition of his wife, from whom he had been separated for ten years, on the night of her death. The phantasm

[29] Prince, W. F., *Human Experiences*, B.S.P.R., 1931, pp. 145-146.

seemed to want to indicate that their marital difficulties had been her fault. Then there is the striking case of the "dollar bills," fully reported by the S.P.R.[30] Here a young Roman Catholic woman fell into a swoon upon hearing of her father's sudden death while away from home. On recovering consciousness, she stated that he had appeared to her and told her that some money had been sewn inside the shirt he was wearing when he died. When his clothes were finally recovered, 35 one-dollar bills were found sewed in the pocket of the shirt, as described in detail by the percipient. In this case, although the apparent agent had been dead for about twenty-four hours at the time of the percipient's experience, the hypothesis of "latency" might still be held to apply. This hypothesis, however, can hardly be invoked in regard to the "Chaffin Will" case,[31] where the ostensible agent had been dead for four years. This leads us to our second subheading, and two cases, both from the S.P.R., are quoted at some length to illustrate the type. The first of these is essentially spontaneous; the second involves the mediumship of Mrs. Leonard. But in each of the cases the motives, I think, are quite clear from the point of view of both percipient and agent. (In the mediumistic case we must regard the sitter as the "percipient.") The incident of the "damaged cross" is summarized from the fully corroborated report appearing in the *Journal* of the S.P.R. In March, 1930, the percipient, Mrs. Sparkes, wrote in part as follows:[32]

"My late husband died in May 1929 . . . I ordered a firm of Monumental Masons to erect a Cross over the grave. They wrote last January that this had been done, and sent their bill. My custom is to pay all bills at once. My impulse was, accordingly, to, then and there, send a cheque. A communicator purporting to be my late husband has been in the habit of sending me messages since his death. I scribble them down on bits of paper or in a note-book. While I contemplated sending the cheque—I was holding a pencil not waiting for any message or thinking of anything in particular, when

[30] *Proc.* S.P.R., Vol. VIII (1892), pp. 200-205.
[31] *Ibid,* Vol. XXXVI (1926-27), pp. 517-524.
[32] *Journal* S.P.R., Vol. XXVI, June, 1930, pp. 93-95.

it wrote: 'See to my grave at Shamley Green. I see a crack at the base of the right side of the cross . . . on the right side standing on the path. Do not go on with the payment until it is attended to, or it will not be done.' . . . On at last seeing the monument I found a gash three inches long, one inch wide, and one inch deep on the lowest block on the right hand side facing the path."

Miss Newton interviewed Mrs. Sparkes and ascertained that she had had no news of the progress of the stone between ordering it and hearing that it was completed. Mrs. Sparkes also stated that she never went to cemeteries as she disliked them. Two independent witnesses confirmed "that on the right side of the stone, facing it from the path, there is a defect in the granite . . . filled up with cement . . . from the outline of this defect it has the appearance of a tool mark rather than a crack."

Our final episode, considerably abridged, is taken from Mrs. Salter's paper, "A Report on Some Recent Sittings with Mrs. Leonard."[33] The sitter was Mrs. Dawson-Smith and the communicator purported to be her son who was killed in January, 1920.

EXTRACT FROM SITTING WITH MRS. LEONARD, JANUARY 10, 1921

Feda [*Mrs. Leonard's control*]: And he says, "Have you got my little key? You were touching it the other day. As you moved about, you touched the key. And there was an old purse with a receipt in it, a tiny paper. It feels old. I wish you could find it, old, worn and soiled, mixed up with a lot of other things." He doesn't think you have it. Do find it. He calls it a counterfoil. Try and unearth it. He will be so interested. He knows you have it, a long narrow strap close to it. "I noticed that accidentally." He says this is important.

In November, 1924, Mrs. Dawson-Smith wrote to Sir Oliver Lodge as follows:

"I think you will be interested to hear of another fulfilled prophecy. [Mrs. Dawson-Smith quotes from above extract.] I hunted for this paper, and over a big trunk in our box-room I noticed a long strap hanging. I opened the box and hunted through the contents and came across a worn old leather purse, and in it a worn old counterfoil of a money order. I carefully took it out and put it in my desk, thinking the importance might be found later. And *true!* The following explains:

"I had a letter from the 'Enemy Debt Clearing Office' demanding

[33] *Proc.* S.P.R., Vol. XXXVI (1926-27), pp. 187-332. The episode of the "missing counterfoil" is on pp. 303 ff.

a sum of money said to be owing to a Hamburg firm, which was incurred in July, 1914—before the war. I *knew* my boy had paid it . . . so I wrote to the Controller and explained that the account had been paid ten years ago. Then the Hamburg people said they had not received it, and if I had no proof other than my memory, it would be pressed (the claim). *Then* I remembered my boy's message at Mrs. Leonard's and I hastened to look at the counterfoil and found it the identical paper needed to prove the account had been paid. Needless to say I sent a complete account of the transaction to the Controller, and he of course verified it in every particular, and wrote to apologise for having caused me so much worry and trouble, and to say no further action would be taken in the matter . . ." [The existence of the counterfoil and the correspondence with the Enemy Debt Clearing Office was verified by Mrs. Sidgwick and Sir Oliver Lodge.]

Motivation in Experimental Cases

The reader may at this point inquire as to our reason for emphasizing spontaneous cases rather than experimental cases. The chief reason has been the richness of our evidence regarding motives in the spontaneous material. With the experimental material we know very little about motives except that the subject is trying, at least consciously, to make high scores. We do, however, have some clues which indicate that the motives operating in the experimental cases are similar to those in the spontaneous cases. The brilliant subject, Mr. C. J., used by Martin and Stribic[34] assimilated his experimental experiences to his previous spontaneous experiences. He not only wanted to do well in the experiments; he was, as it were, recapitulating the spontaneous experiences which had meant so much to him. The desire to please a loved one is clearly important, as shown in experimental cases where engaged couples have successfully acted as agent and percipient.[35] Self-justification seems to have played a role when the experimenter has defied the subject to do well and he has accepted the chal-

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[34] Martin, Dorothy, and Stribic, Frances, "Studies in Extra-Sensory Perception: III., A Review of all University of Colorado Experiments," *Journal of Parapsychology,* Vol. IV, 1940, *cf.* especially p. 173.

[35] Rhine, J. B., *New Frontiers of the Mind,* New York, Farrar and Rinehart, 1937, pp. 104 and 164-165.

lenge,[36] while resistance or antagonism to the experimenter has, in several instances, been clearly related to a significant drop in scoring level, or even to below-chance scoring.[37]

We are willing to hazard the conjecture that the principal motives operative in paranormal impressions appear in such strength that we select and organize paranormal impressions with a very vigorous autistic determination; and we must suspect that over and above the *general* needs of human beings, it will be found that *individual* needs are correlated with individual variations in paranormal perception. The time has come for intensive personality studies to provide clues to the inner dynamics of paranormal experience.

Barriers to the Paranormal

All this may well seem to make the whole problem too easy. If motives fulfill themselves so simply, why does not paranormal perception occur continuously? Perhaps, as Professor H. H. Price once pointed out, the question is not, *Why* do we perceive paranormally, but rather, Why do we not perceive paranormally *all the time?* But the fact is that we live predominantly in the immediate world of time and space and that we inhibit or block our paranormal tendencies. It seems as though we were inclined not only to be afraid of such impressions, but also to feel that they are "queer" and "unnatural." The matter-of-fact civilization of which we are a part intensifies our tendency to rely on our sense organs and to mistrust everything else; consciously or unconsciously, we shut the paranormal out.

The main clue we need, then, is the clue which will point to the way in which the customary blockage or barrier may be penetrated or removed. We are sufficiently motivated, apparently, to make paranormal contact. The problem is to learn how and why we manage so successfully to block

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[36] *Ibid*, pp. 94-96.
[37] *Ibid*, pp. 81-82.

ourselves, and thus how to remove the barriers. The term "blockage" or "barrier" refers to one of the mechanisms which in psychology is conceived to predispose to dissociation. One side of the shield is *motivation*—that which impels us to make paranormal contact—and as far as we can see there is no mystery here; the motives are those with which we are already familiar. The other side of the shield is the process by which we *prevent* ourselves from fulfilling motives through paranormal perception. It is therefore with this aspect of the problem that we shall come to terms in the next article.

Further Tests With Mrs. Leonard

REV. W. S. IRVING
Hon. Associate, S.P.R.

FOREWORD

[The incidents to follow are all taken from Mr. Irving's sittings with the distinguished English medium, Mrs. Osborne Leonard. The communicator, Dora, purports to be Mr. Irving's wife, who died in 1918. Mr. Irving had his first sitting with Mrs. Leonard about three years later, and since then has had the opportunity to sit regularly with her two or three times a year. During this time he has received material of great value to psychical research, much of which has been published in the *Proceedings* and *Journal* of the S.P.R. We presented a paper by Mr. Irving, "Picture-Tests," in the October, 1942, issue of this JOURNAL. Particularly interesting are the "tests" which he has received; these may be divided into three categories—book-tests, picture-tests, and impression-tests. Book-tests, as given by Mrs. Leonard to Mr. Irving and other regular sitters, are undoubtedly familiar to most of our readers and therefore do not need discussion here.* The so-called picture-tests it may be noted, seem to have been the invention of Mr. Irving's communicator, nothing of quite the same sort having been, so far as we know, reported by other Leonard sitters. Briefly, as will be seen in the picture-test to follow, Dora attempts to describe a picture that Mr. Irving will see in the *next day's newspaper,* points thus being given about material as yet unpublished. Sometimes knowledge is shown of material on the back of the test-picture—material the position of which had not been decided upon at the time of the sitting. Frequently personal reminiscences are woven around features of the test-picture. Mr. Irving feels that these tests were not developed to offer evidence for precognition, but rather to convince him that the knowledge shown could not have been derived directly from his own mind. It will also be seen that the impression-tests, in many cases taken from homes never visited by Mr. Irving, serve the same purpose; that is, rule out the hypothesis of direct telepathy from the sitter. It is also important to note that during her lifetime Mrs. Irving was interested in solving puzzles, not infrequently competing for the money prizes offered by various periodicals for the best solutions to a certain type of "picture-puzzle" that interested her. In other words, it is "in character" for the communicator to attempt these tests and, therefore, they may be taken as affording evidence of identity.—ED.]

*See Mrs. Sidgwick's paper, "An Examination of Book-Tests obtained in Sittings with Mrs. Leonard," *Proc.* S.P.R., Vol. XXXI (1920-21), pp. 242-400.

Introduction

In studying the *modus operandi* of picture-tests, or, for that matter, any test material, it well may be that clues will be found more easily in tests that contain ambiguities than in those in which the going is better. Since completing my recent paper on Leonard picture-tests for this Journal (October, 1942), I have been re-examining a number of my other tests and this possibility has rather been impressed upon me, especially in regard to tests taken from cartoons appearing in the daily press. It is some of the thoughts that have occurred to me during this review that I now venture to put forward.

It would seem to be the case that at times it is easier for the communicator to give the *idea* that lies behind the picture than to describe the picture itself. In fact, on occasion it does not appear to be possible for my communicator, Dora, to make Feda (Mrs. Leonard's control) "see" what is an important feature in the picture in question and therefore she would seem to be compelled to fall back on trying to impress her by sound or inspiration. Take, for example, a point in the first test that I am presenting in this paper, a test from one of Poy's cartoons. Dora gets the *idea* that Poy is trying to convey—gets it quite clearly, I think, when she says, ". . . in this picture you could almost sum it up as describing . . . the direction things go, showing the way things go." But Dora cannot make Feda "see" the main feature of the picture itself, the "mechanical mangle," parts of which Feda takes to be an animal's head of which she can only discern the outline. Dora also, apparently, has to inspire Feda to make such seemingly redundant remarks as "He's got a nice *round* face—rather nice *round* face . . . head and face *round*"; and a little later, "it doesn't show the whole of a *round* pool," in order to force on our attention the word "round," which is prominent in the cartoon. That this process is due to design and not to chance, experience has convinced me. On a number of other occasions, in my opinion, a similar process has manifested itself: for instance, the otherwise rather

pointless use of the word "embellished" to call attention to the Belisha beacon in the Dorothea test (*op. cit.*, pp. 259-260).

Feda's remarks are verbatim, except for the deletion of some unessential repetitions. My own remarks made during the course of the sitting are placed in parentheses and explanatory material in square brackets.

I PICTURE-TEST NO. 17

Introductory Note: While still at home, a day or two before my sitting with Mrs. Leonard, I mentally asked Dora to take her test on this occasion from the middle sheet of the *Daily Mail;* and in order to compel her to do this, rather than going elsewhere for her material, which would give greater scope for a chance explanation, I refrained from looking at any newspaper, or any part of any newspaper, save the middle sheet of the *Daily Mail,* from Monday, April 27, 1936 until noon on Friday, May 1, 1936. This greatly limited my communicator and made personal reminiscenses well-nigh impossible, but the experiment seemed to be worth trying. Readers will have to decide for themselves whether the results are explicable in terms of chance, or whether they justify the drastic methods used.

Extract from a sitting with Mrs. Leonard, Tuesday, April 28, 1936, 10:30 A.M.

Sitter: Rev. W. S. Irving

Recorder: Mr. C. V. C. Herbert

FEDA

The pictures, please, Mr. Bill! What did you say? Can't see what it is! She showing me something! You know, it's what you call prongs—prongs. I'm not sure if that is the right description, but I'll tell you it's wrong, afterwards, if she says so.

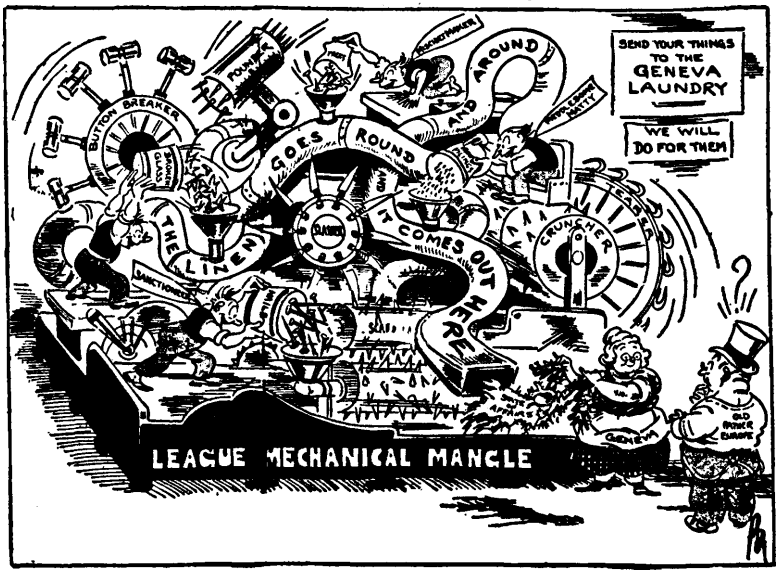
I see something like two or three more or less straight lines —like thin fingers, as if they're pointing in a direction—like lines pointing in a direction, pointers, lines. I'll have to ask her after if that's right. Yes! I think that's right.

ANNOTATIONS BY MR. IRVING

In the *Daily Mail* for the day after the sitting, Wednesday, April 29, 1936, on the middle sheet, is a cartoon by Poy entitled "Europe's Shirt." A strange gadget called "League Mechanical Mangle" is depicted; a number of knife-like and prong-like outlines are to be seen.

There are "lines like thin fingers"—the fingers of the four men engaged in working the mangle. They are busy pouring nails, broken glass, etc., into a long tube which conveys them in the same direction to the same exit.

EUROPE'S SHIRT—*By POY*



International Pulveriser No. 1

Now, in this picture, you could almost sum it up as describing or presenting directions—signs, directions, signals. Giving the signs of the direction things go—showing the way things go.

And stroke—stroke is a better word than lines.

There is an animal in the picture. I can't see what it is yet! An animal that you can't see all of him. You see his face. I see the outline of what I know's his head. Just now I feel as if I'm only seeing part of his body—as if something hides his body. Dora

On the serpent-like coils of the machinery appear the words "The linen goes round and round and it comes out here." Actually, the *meaning* of the cartoon is well "summed up" by the phrase "showing the ways things go"—how things are going in the European situation.

There are five mallets on a wheel acting as button breakers. They are, however, independent of the "lines" or "thin fingers."

There is no animal in the picture. The thick coil of the mangle might well be taken for the body of a snake, but the head is the difficulty. This is the point at which I suggest that Dora, finding that Feda is being led off the trail, begins to impress her with

says it is right! Now I dunno whether he's a nice animal in himself, but he's got rather a nice round face—nice round face. She want you to notice that! His head and face round, not long-shaped. You know the scribe who used to come? Not like him, like this one—round!

the concept "round." (The word appears in good-sized type on the pipe above and to the right of the Slasher.) Feda receives the impression "round," but elaborates it in connection with something of special interest to her—viz., an animal. (Both Mrs. Leonard and Feda are great animal lovers, Feda claims to own several cats, etc.) A somewhat similar "interpretative" process may have occurred in the Dorothea test* where the sign "Bends" seems to have been translated by Feda as "Dorothea bending."

Near him? Near the animal, close to the animal, there's something like half a circle—something shaped like half a circle. I dunno if it really means anything at all, but as if that shows in the picture like this [Feda draws in the air; sitter copies her actions in his notebook]:

As well as the word "Round" close to the Slasher, there is on the left-hand side of the mangle a half circle formed by one of the coils, as below:

Half circles, upside down, like that! I don't think Dora knows what it is, but it's that shape—part of a circle, half circle.

Also, in the upper right-hand corner of the picture another coil forms a half circle, the word "around" being printed on it. It is inverted, however, rather than "upside down," as Feda expressed it.

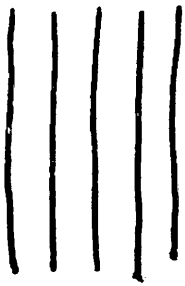
This — I think it's water. There's water in the picture like

There is, of course, no pool or lake in the picture. Certainly,

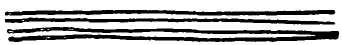
* JOURNAL A.S.P.R., Vol. XXXVI, October, 1942, pp. 256-261.

a pool—a pool of water. Yes, because I see something looking like shadows and lights, do you see, on the ground, like a pool, the edge of a pool. It doesn't show the whole of a round pool or lake—only part, like the edge of a pool. Like the edge of some water . . .

Railing? Railing? A railing or boundary fence of some kind. That's what she calls it! Yes! Railing will do, she thinks. Perhaps it's the better word. You mean best, Dora? No! Better word of the two, she says. Railing, Mr. Bill! What she calls a 'railing.' What? What? Mr. Bill, look! It *doesn't* go like this [Feda draws in the air and the sitter copies in his notebook]:



They go *across*. It goes like that way! [Feda again draws in the air]:



I've got the feeling those strokes she spoke of first have something to do with the railing, or are near it. I'm seeing strokes however, a "laundry" strongly suggests the *idea* of "water." A box containing the words "Send your things to the GENEVA LAUNDRY" is to be seen in the upper right-hand corner of the cartoon. There are also shadows and lights heavily contrasted at the bottom of the picture.

There is a boundary fence or railing around the Mechanical Mangle. In the front it is very black and conspicuous. The lines forming it, as Feda indicated, are mainly horizontal. At the left-hand side the fence is at an angle.

The mallets that are striking the buttons in the coil have nothing to do with the boundary fence, but they are near it; the nearest

again now.

A name beginning with the letter 'P'? Keeps jumping out at Dora all the time from this picture. She keep getting 'P.' Didn't see it objectively? She says, 'I didn't see that objectively, do you understand?' She saw it as meaning something; it was an idea she got that, over the picture, should be a title. You could fit a title over this picture—beginning with the letter 'P.' Not a long word. She says, 'I knew what the word represented before I came here, but that scrap of consciousness won't let me remember it. I haven't got the least idea now.' She says if she could bring in the other nine-tenths she would remember. She knows it's 'P.'

is about one and a half inches away.

The picture is one of Poy's cartoons. The actual words over the picture are EUROPE'S SHIRT—BY POY. The name "Poy," certainly not a "long word," is also to be found in the right-hand lower corner of the picture. [Feda's point that over the picture was a short name beginning with the letter "P" seemed to us so striking that we wrote to Mr. Irving for further information, as follows: "Did a cartoon by Poy appear every day in the *Daily Mail* around about the time of your sitting? If so, did the cartoons always, or usually, appear on the middle sheet? In other words, granted the extra-chance nature of the test as a whole, could this 'P' have been a lucky shot derived from a knowledge that Poy would be on the page in question?" Mr. Irving kindly sent us a letter from the secretary to the Assistant Editor of the *Daily Mail,* which makes the following points clear: (1) A Poy cartoon did *not* appear every day; there was none, for instance, in the April 30th issue. If Mr. Irving's sitting had been one day later there would have been no cartoon the following day, and the point would have been inapplicable. (2) When a Poy cartoon did appear, it was not necessarily on the middle page (that is, page 12). It sometimes appeared on page 10 of a 24-page issue. But Mr. Irving had specifically limited his communicator to the middle page.—*Ed.*]

Nothing to do with what she said about an animal in the picture, but close to—near the picture, or at the back of the picture —see if there's a horse! It came very close, as if she was almost seeing the horse through the picture. Don't forget! It's nothing to do with the animal in the picture—nothing at all.

Antelope? Antelope? She got two things to do with animals. The word 'antelope' kept springing at her here, may have been at the back of the picture. Must be two or three things suggesting animals here, and yet I don't feel the idea of animals would be very obvious at first sight. That's all for the picture, Mr. Bill . . .

The word "horses" is to be found "through the picture"; that is, it appears at the back of the picture. This word is one inch from the title EUROPE'S SHIRT on the test-page, appearing in an article "Fancied Derby Candidate Beaten." Reference by name to various horses are also to be found. The word "horsemen" appears directly behind the picture, near the base.

I can find no mention of "antelope" either on the test-page, or behind the test-picture.

II A DISTANCE EXPERIMENT

Introductory Note: It has for years now been a custom of Dora's to pay visits to various places, including the Rooms of the S.P.R. and some private houses, known to me and unknown, with the object of getting book-tests and recording impressions which she claims to be able to pick up in such places.* Generally Dora herself chooses where she will go for her tests, but in the case to follow I made the initial suggestion. Shortly before the sitting about to be presented, it occurred to me that it would be of interest to ask her to pay a visit to Mr. Whately Carington in Rotterdam. Whether Mr. Carington lived in a house of his own or in rooms I did not know, and I have never myself been in Holland. Let me say here that my knowledge concerning Mr. Carington was slight—apart from what one can learn from his published works. I had seen him at S.P.R. meetings and we had spoken together once or twice. Also we had had some correspondence about the word-association experiments which Mr. Carington undertook in 1934, in which I had been allowed to help at some of my Leonard sittings, but that was the extent of our

*See *Proc.* S.P.R., Vol. XXXVI (1926-27), pp. 190-207, for tests taken from the Rooms of the S.P.R. Another example of a test taken from a house unknown to Mr. Irving follows the Carington test, pp. 206-209, this issue.

acquaintance.* My suggestion as to the test was made through Feda, as follows:

Extract from a sitting with Mrs. Leonard, Monday, September 16, 1935, 10:25 *A.M.*

Sitter: Rev. W. S. Irving *Recorder*: Mr. Oliver Gatty

FEDA

Did you want her to try something? Anything to do? (Feda, I thought of Mr. Carington in Holland.) What to do? (Try to get a book-test from him. It's a suggestion.) A very good one. I can't do it today. Can I do it for the next sitting? She says, 'How am I going to get a link there?' Have you a letter of his? (I'll write a letter?) No! Can you get a letter—any link with him—today? (I think so.) [Sitter then went on to say that if he could not obtain a letter of Mr. Carington's that day, he could probably get one by the next day.] Dora will try anyhow. It may help if you can get a letter from him. Put it by your bed, or just under your pillow—not right under your head—a little to one side. Not sure this will help, but thinks it might. I might link up sufficiently through it. And just before you go to

ANNOTATIONS BY MR. IRVING

In accordance with these instructions, I procured from Miss Newton at the S.P.R. a letter written by Mr. Whately Carington. This letter I placed beside my pillow before retiring on two successive nights—Tuesday, September 17, and Wednesday, September 18, 1935. Before falling asleep I also tried to visualize Mr. Carington.

Note by Mr. Whately Carington, September 29, 1935.

I received Irving's telegram warning me to keep books undisturbed at 2:12 P.M., September 19, 1935, and Gatty's notes of the test on the 27th. Unfortunately, although I had preserved the *status quo* perfectly in my room (I have only one) in this house, certain "overflow" books of mine, located in the room to which the test seems unquestionably to refer, were disturbed by

*We thought it might be of interest to inquire into the extent of Mr. Carington's acquaintance, if any, with Mrs. Leonard. In answer to our questions Mr. Carington wrote Mr. Irving as follows: "I had one personal sitting with Mrs. Leonard in the winter of 1916-1917 in which I got some quite good stuff. And I was present at another sitting in which . . . another person . . . was the actual enquirer, somewhere about 1923 . . . At the time of my word-association test experiments, I exchanged two or three notes with Mrs. Leonard (or perhaps her secretary) about arrangements, etc., but nothing of a personal or intimate nature. I suppose one could say that there would be a somewhat stronger link between us than between two people who had never met or corresponded at all, but not much more than that. I have looked through your MS and find that it accords perfectly with my recollection, and with my memory of some notes, etc., of the circumstances which I happened to come across a few weeks ago."—*Ed.*

sleep, think of him. If you can't get the letter, think of him. Don't reason about him, visualize him. Not in great detail, feature by feature, but just of him himself—more as an individual.

Extract from a sitting with Mrs. Leonard, Thursday, September 19, 1935, 10.25 *A.M.*

Sitter: Rev. W. S. Irving *Recorder*: Mr. Oliver Gatty

FEDA

She would like to go to the 'Long One's' place, you see? (Yes.) Oh, she thinks she's found this place all right, Mr. Bill! (Splendid!) She thinks she's found this place. She didn't feel him there—may have been in and gone out. Doesn't think he was actually in the place when she was. (Oliver Gatty: Just now, Feda?) Wait a bit! She'll have to try to tell you when. The time she's speaking of—yesterday, she says. Not today, not this morning. She went yesterday. Though she didn't see him in it, it seemed as if he must have been in this room and left it with the idea of coming back to it. (Left it?) Yes!

Note by Mr. Carington, continued.

The interesting thing about the test is this: there is one room (and one only) in this house which is indisputably *mine*—in which I have lived and moved and had my being for the last two years. But nothing whatever in the material given could possibly be held without the grossest forcing to apply to this room. On the other hand, there is a room upstairs, occupied as a bed-sitting room by the daughter of the house (aged seventeen) which contains a bookcase of mine in which are about eighty or ninety of my books (in addition to many of hers). I seldom have need to refer to these books. The case is a large one—about six feet long and four feet high. It is divided vertically into three sections as well as longitudinally by shelves in the usual way. To this room the test material seems to apply extremely well, as will be seen from the following commentary on the notes sent me by Oliver Gatty. Omitting the preliminary remarks about my having gone out with the intention of returning—I very seldom enter the room—we have the following:

the occupant and cannot be replaced with certainty. The book-test as such is accordingly invalidated; but the descriptive passages are of great interest. See below.

She found herself in a room, and while she was in the room—it's a fair-sized room, she says, good-sized room.

Yes, a fair-sized room describes it adequately. My own room is definitely *small.*

But my first impression in the room was of ships, boats, and water. Now, I haven't always got full vision in a new room, but the thought conveyed to me by the room was of a lot of ships, boats, and water. I think this may be a thought or impression felt by the usual occupant of the room. I think it was something I picked up, she says.

"Ships, boats, and water . . . this may be a thought or impression felt by the *usual occupant of the room.*" (The italics are mine, and note the phrase.) I consider this a good point. The usual occupant of the room in question is a keen oarswoman and has recently taken up sailing with enthusiasm. Without doubt thoughts of this kind have been considerably in her mind during the last few months.

I saw a window which I—oh! which at first sight I thought was a long one. On further examination, trying to adjust my vision to it, I thought something cut its length, giving it an appearance of unsuspected breadth. Wait a minute, Dora, please—it's important! You know it is! The light area was broad—short and broad—something made it so, something about the window made it so, short and broad.

"Window . . . giving it an appearance of unsuspected breadth," etc. Actually the room is lighted by two low square windows in the same long wall; the effect produced is emphatically that of low, broad lighting. This is in contradistinction to my own room, for example, which has a single very high window.

There seemed to be a kind of ledge near the window—rather wide ledge, not exactly a window sill, but a rather wide ledge or a piece of furniture pushed near the wall acting as a ledge or shelf.

"A kind of ledge . . . not exactly a window sill . . . rather wide ledge." A good point. Outside the windows at window sill level runs a wide ledge-cum-gutter about eighteen inches wide and lined with lead. It is, so to speak, part of the house and is strong enough to walk on. The windows themselves have fairly wide (say 12″) internal sills.

Wait! Where's the barrel? Is

"Something shaped like a bar-

something shaped like a barrel in this room? Doesn't feel like a room where you'd keep a barrel! Could it be a picture of a barrel in the room? I get the picture of a barrel in the room. Nobody photographs barrels!

Does he do sewing in this room? A barrel and sewing. Is there something half sewn? Somebody left something—don't put 'sew,' put 'stitch.' I've a reason for that. There's something left undone in this room with regard to stitches—to stitches, and it's something that if one walked into the room with the intention of occupying it for any length of time, one would notice it rather quickly, quite quickly.

Some books? Coming in from the door, I'm trying my usual way —working round from left, walking round on the left. I'm ignoring any odd book, you see, that there may be. I'm waiting till I come to a set of shelves.

And near the set of shelves, I get flowers—flowers are in something light-colored, put in something light, something that reflects the light, put in light. I don't know if—I see light reflected in something in which the flowers stand.

rel." There is, of course, no barrel in the room, nor is there any picture of one, so this last part is wrong. But there is a fat cylindrical leather pouffe which could certainly suggest a barrel, though it is too low for true barrel proportions.

"Sewing." A certain amount of sewing is done in the room, though "stitching" is perhaps a better word in the sense that it is mostly a matter of minor repairs rather than of serious sewing. "There's something left undone in this room with regard to stitches." On the afternoon of Wednesday the eighteenth, the day before the sitting [and the day when Mr. Irving's communicator claimed to have visited the room], a dress was started and at the time of the sitting was (and still is) unfinished. The dress might be "noticed rather quickly" if one had entered the room with the intention of occupying it for any length of time.

"Waiting till I come to a set of shelves." This is not good. The set of shelves is immediately on the left as one comes in the door.

"Flowers." There are flowers —usually, and probably on the eighteenth—in a vase standing on the bookcase; but the vase is dark and does not reflect the light particularly.

I think there's a round table just there, a round one.

Yes. There is a round table in the center of the room.

Has anything been given as a prize there? Something to do with a prize. Dora kept thinking, 'I'm winning a prize here. There's something being given me—being given me as a prize.' She's very sure about that prize! She hopes that'll be a good point. But, oh! the difficulty is distinguishing between an object that may have been given as a prize and the concrete thought of somebody who might wish for a prize, do you see? She says that it's like a materialized thought that we sense — or thought-form. You must remember that strong wishes leave their photographs on what you call the atmosphere.

"Prizes." This is perhaps the best point of all. The four shelves in the right-hand third of the bookcase are filled with prizes gained by me at Eton. These have, of course, a good deal more sentimental value for me than the books of reference, technical works, stray detective stories and what not, which are to be found in my own room—about none of which I care twopence. It seems possible that it might be this lack of sentiment, so to speak, that directed attention to this room rather than to my own. The winning of the prizes, for instance, was a matter of immense importance to me at the time . . .

[At this point Dora began to give the book-tests. Her description of these tests, which are not given here, was interrupted several times to give the further impressions that are found below. —W. S. Irving.]

Book-tests. As already stated, these have been invalidated by movement of the books, assuming that it is referring to the room dealt with above. The book-test material certainly does not apply to the books in my own room. One might make out a rather forced case for supposing that it applies to the books in the section containing the prizes, and thus particularly mine, but the rest of the test seems to me so good that I feel it would be a pity to spoil it by introducing strained comparisons.

There's either a second entrance or a cupboard door looking like a second entrance. Not three doors, are there, Dora? She thinks there are! Wait a bit. I must get that right! Looks like

"Doors." There are two cupboard doors in the room, besides the entrance door. These cupboard doors are almost large enough to be entrance doors, but they are papered over like walls,

three doors! They couldn't all be entrances, but they might be mistaken for them . . .

Any atlases on these shelves? Get the idea of geography, maps, or guidebooks — that kind of thing. That right, Mr. Bill?

Will you ask if any dried leaves in the room — dried or preserved leaves? Gathered leaves or branches in this room? Not leaves of a book but gathered leaves. I think 'leaves' is a better way of putting it. I didn't see them exactly—did not see their shape. I got the impression of sprays of leaves or gathered leaves. Gathered leaves came to my mind more than anything.

In this house or building . . . she's got an idea of uneven ceilings—uneven ceilings, as if you could go in one part, find it much lower than another. For instance, in this house [Mrs. Leonard's] all rooms are about the same height. But in this place of his, she got the idea someone had thought about this unevenness of ceilings and that's what she picked up. Now, Mr. Bill, she think she rather stoomped [*sic*] now. (Why?) About giving any more about this.

as is common in this country, and no one casually inspecting the room would notice them.

"Atlases," etc. The usual occupant of the room keeps a number of school books in the case, including some on geography. At the time of the test there was an atlas among these books.

Actually the *flowers* I mentioned above turned out, on further inquiry, to be *dried*. But there are no other flowers in the room (and were no others at the time of the test) so that if a success is scored on the "dried leaves" a miss will have to be chalked up for the "flowers."

"Ceilings." The point is not bad, though it would have been better if Dora had been less specific. There is no appreciable difference in *level* between the ceiling of the room in question and of that which it adjoins. On the other hand, the room in question has dormer windows and the ceiling or wall (whichever you prefer to call it) above, between, and on either side of them slopes at an angle of about 45 degrees. Also, there is a similarly sloping area along one side of the ceiling of the adjoining room. The ceilings of the whole of the upper floor (three rooms and bath) would certainly give an impression of irregularity, but the feature of being at different levels is negligible.

III TEST FROM THE HOME OF MR. STANSFIELD

Introductory Note: The test which follows is from the home of Mr. C. E. Stansfield of Reading. Mr. Stansfield, a Member of the S.P.R., has on several occasions taken notes for me at my Leonard sittings, but I have never been in his home. Mr. Stansfield has also had some sittings of his own with Mrs. Leonard, under a pseudonym. To the best of our knowledge, Mrs. Leonard is ignorant of his identity. Dora has often taken book-tests and impression-tests from Mr. Stansfield's house.* Unlike the experiment of visiting Whately Carington in Rotterdam, which I suggested, the experiment to follow was initiated by my communicator. I had not seen the *Illustrated London News,* a picture from which apparently figures in the test. I rarely see this magazine; in any case, the number in question was not available at the time of the sitting.

Extracts from a sitting with Mrs. Leonard, Thursday, January 20, 1938, 10:34 *A.M.*

Sitter: Rev. W. S. Irving, taking his own notes in duplicate. Carbon copy sent to Miss Newton, Secretary of the S.P.R., by registered post from Whitstable (Mrs. Leonard's railroad station), same day, at approximately 2:30 P.M.

FEDA

Oh! Oh! You know the Stannies? Mr. Bill! Will you ask whether one of the Stannies, the other day, had some kind of a crash? I feel something dropping, and dropping very sharply and heavily—with force, do you see? With force, and as though a crash comes with it, and a feeling 'Oh dear, how annoying!' Rather a feeling of 'Oh, I'm so cross about it! How angry I am about that!' Whatever it was that went crash, it wasn't a new thing; it was something they'd had for

Note by MR. C. E. STANSFIELD, *January* 25, 1938.

"Kind of crash." An alabaster bowl under an electric light bulb in the sitting room is badly cracked. We first noticed it some ten days ago [that is, about five days before the sitting]. We are expecting it to crash, and the maid has been warned. The word "crash" was used repeatedly in discussing the situation. There is not only anxiety lest one of us should be struck on the head, but annoyance which was also expressed. The bowl was bought in August, 1932. We had some

*For particulars of earlier tests taken from the home of Mr. Stansfield, see *Proc.* S.P.R., Vol. XL (1930-32), pp. 152-161.

some time and they would link it up with the lady who passed over, but, she says, it doesn't matter.

Oh! Wait then, Mr. Bill! Look! Something copper—something copper or bronze. Have they found something—one of the Stannies? Something—copper, it felt like. Something they came across which hadn't been used for some time? As if they were saying, 'Oh, where has this come from again?' Where has it been? Must have been put away for some time! There was a little conjecturing going on about it, Mr. Bill. That's right, Mr. Bill! Wait! Look! This thing that she's speaking of, she's trying to show me, made of copper. I feel something rounded. (Draw it, Feda.) Don't think I can. Something rounded with a surface that is not quite flat. Well, you see, Mr. Bill, I feel that this has got some little indentations in it—indentations in the surface of the copper—little grooves and things. And near the edge, near what you call the edge, there's—this is awful difficulty in getting what we wanted and the three of us [Mr. and Mrs. Stansfield and their daughter] went about London to find it. We link it up "with the lady who passed over."*

"Something copper or bronze . . . hadn't been used for some time . . . rounded . . . found by one of the Stannies . . . not used lately . . . obvious!" On reading this I . . . remembered a picture I saw last Friday, January 21st [the day after the sitting], in the *Illustrated London News*. It seems to fit admirably. Mary [Mr. Stansfield's daughter] had not seen it. I left the magazine open on the table and when she came in she found it there and exclaimed, "Why, that's the bronze thing of the script!" The letter press fully explains the words "conjecturing going on about it."

ANNOTATIONS BY MR. IRVING.

On reading the letter press in the *Illustrated London News* for January 22, 1938, which Mr. Stansfield sent me, I found that the description of the bronze *Kuei* given therein corresponds so closely with the description

* Through correspondence we have acquired further information on the point Feda makes of "linking up the thing that went 'crash' with the 'lady who passed over.'" To summarize a letter from Mr. Stansfield's daughter to Mr. Irving, it appears that on August 27, 1932, Miss Stansfield and her parents went on a shopping tour in London, buying at this time the alabaster lamp bowl in question. Immediately afterwards they had a sitting with Miss Campbell, during which another daughter, Olive, who had died some time before, purported to communicate. Evidential material seems to have been obtained, and, in particular, the communicator referred to details of the shopping trip. They therefore associate this alabaster bowl with "the lady who passed over," i.e., with Olive Stansfield.—*Ed.*

cult—a milled or vandyked* pattern. She gets that because, going round it with your finger, you go in and out, up and down, but she's not sure which you would call it. They go like little dips up and down. You might call it corrugated — corrugated. There are also some dots on it, d-o-t-s, some little rounded dots going round it, forming a pattern. Now, I've been very careful about this, Mr. Bill! These things form a pattern and this thing—the copper, she says — exists, but it has not been made use of lately. That is obvious!

given at the sitting that it seems reasonable to suppose that this letter press may have formed part of the source from which the test was taken; a kind of book-test plus picture-test, in fact. Below will be found a point by point comparison of the letter press and the script.

Comparison of Script with Letter Press Concerning the Bronze Bowl, Page 43 of the Illustrated London News for January 22, 1938.

Script Extracts	*Letter press extracts*
Something copper or bronze . . . hadn't been used for some time.	A beautiful bronze ritual *Kuei* of about 1100 B.C.
Where had it been? Must have been put away for some time. There was a little conjecturing going on about it.	. . . bronzes recovered from the ground. In some instances the task was the easy one of identifying the bronzes with names . . . A number, however . . . taxed the ingenuity of the Sung scholars.
I feel something rounded . . . with a surface that isn't quite flat . . . indentations on it . . . little grooves.	A round vessel . . . the bodies, too, are in higher relief than the rest of the decorated zone.

*"Vandyke: to form with points or scallops."—*Webster's New International Dictionary.* The "scallops" are easily seen in the original illustration in the ribbing around the middle of the vessel. Feda's use of the word "vandyked" is curious. We asked a dozen people of decidedly better than average vocabulary to define the word; not one had even an approximate idea of its meaning. It would be a matter of interest to know whether Mrs. Leonard could, in her normal state, define, or correctly use, the word.—*Ed.*

You might call it corrugated, corrugated.	Vertical ribbing on the belly.
A milled or vandyked pattern.	Marked with stylized scales.
There are also some dots on it, d-o-t-s, some little rounded dots going round it, forming a pattern.	A groundwork of spirals, the so-called thunder pattern fills the spaces.
. . . but it had not been made use of lately. That is obvious!	The *Kuei* were used as containers of cooked grain and other vegetable food . . . As to date, the aforementioned *Kuei* . . . is assigned by Professor Jung Kêng to the Shang-Yin period . . . about the eleventh century B. C.

Note: This particular test raises interesting questions. If we may assume (as I think we have good grounds for doing) that the picture of the *Kuei* is what my communicator described—was she aware that this test was one that indicates prevision? Did she fail to make this clear to Feda, or was she herself ignorant of the facts? On the present evidence we cannot come to any conclusions. It is worth noting that this was the first time a picture-test of this kind had been given to me which had to be verified, or "solved," by someone other than myself. Until I received Mr. Stansfield's notes, of course, I did not know the real nature of this test. [Mr. Irving has sent us a letter he received from Mr. H. Ebden, publisher of the *Illustrated London News*. Mr. Ebden says, in answer to Mr. Irving's inquiries, that the number dated January 22, 1938, was first obtainable in London and the provinces on Friday, January 21. It will be noted that this was the day *after* the sitting in which the *Kuei* test was given.—*Ed.*]

Postscript: Just as we were about to go to press, Mr. Irving sent us a letter Mrs. Leonard had written him in answer to a question he put to her, "What does the word 'vandyked' mean to you?" Mrs. Leonard wrote, under date of August 4, 1943, as follows: ". . . 'vandyked' suggests to me a pointed edge, a pattern such as  . I hope this is helpful?" Mr. Irving points out that, although Mrs. Leonard has some idea of the meaning of the word "vandyked," her knowledge does not seem to be so complete or so definite as that of the communicator of the script.

Predicting Good and Bad Scores in a Clairvoyance Experiment: A Final Report*

GERTRUDE RAFFEL SCHMEIDLER

Is clairvoyance a capricious, evanescent power that appears unpredictably in the work of a gifted few; are the results of some experimenters due to sheer luck in the quest for such gifted persons? Or is there an underlying naturalness and reasonableness in both the successes and the failures, the clue lying in the *attitudes* of those whose clairvoyance is tested? The experiment about to be described, which contrasted the scores of subjects who expected to get chance results in card-guessing with the scores of subjects who hoped to do better than chance, was undertaken in the hope of finding at least some tentative answers to these questions. The first two series of the experiment were reported in a recent number of this JOURNAL.† A third series has since been undertaken, and the combined data will be reported below.

Conditions of experimentation: The materials guessed were standard ESP cards, 25 in a deck. It has been shown that an average score of five is to be expected by chance (i.e., in a binomial situation). Considerable care was taken to keep sensory cues or familiar response patterns from influencing the results. Not only was the subjects' room separated from that in which the cards were kept by an intervening room and a corridor which made a right-angled turn; but also the experimenter was not aware of the order of the cards in the next list at the time when she conversed with the subject. It thus seems impossible that

* This experiment was conducted for Dr. Gardner Murphy, and made possible by moneys available to him from the Hodgson Fund of Harvard University. His advice determined the topic of the research, and his criticisms and suggestions have guided its course.

† Schmeidler, G. R., "Predicting Good and Bad Scores in a Clairvoyance Experiment: A Preliminary Report," JOURNAL A.S.P.R., Vol. XXXVII, July, 1943, pp. 103-110. The third check of the data which was being made at the time of publication of the article revealed certain scoring errors. These have been corrected in the tables published here.

sensory stimuli should have guided the subjects in making correct guesses.

To ensure complete randomization of the target lists, they were made up in accordance with statistically approved tables of random numbers. An assistant assigned two numbers to each symbol and then made out the lists according to the order indicated in the tables. Decks of cards were then arranged to correspond with these lists. The lists were numbered consecutively to form a permanent record of the targets. After the subjects' guesses had been scored by the experimenter, they were checked by an assistant. Later a separate check was made in the offices of the A.S.P.R.

Procedure: It was predicted that subjects whose attitude toward the research was friendly would on the whole score better than five. Each subject was therefore questioned at the beginning of the session as to his feeling toward psychic phenomena in general, and toward telepathy and clairvoyance in particular. According to their answers the subjects were then divided into "sheep" and "goats," the sheep being those who wondered if such phenomena could occur or who believed in their reality, and the goats those who did not. In order to keep the sheep interested and cooperative they were put in agreeable surroundings and plied with cigarettes and sometimes with candy. As they finished each run they were told their scores. They were given time out for discussion whenever they wished, and were encouraged to vary their methods of guessing.

The goats, on the other hand, tended to be somewhat hostile to the experiment from the first. In an attempt to maintain their attitude of boredom or mild annoyance during the experimental hour, they were put in an ugly room, were given no intermissions or rest periods, and were not told their scores. It was predicted that they, unlike the sheep, would make scores at the level of chance or lower.

Results: These predictions were on the whole borne out. After the first few weeks of experimentation the scores of the sheep and the goats were compared. The sheep average

was considerably above chance expectation, and was thus higher than that of the goats, who scored very slightly below chance; statistical treatment showed that the difference between them was at the time of the first report great enough to occur only three times in a hundred on the basis of chance expectation (see Table I).

Table I

Analysis of scores obtained by open-minded subjects and by subjects who expected to score at chance.

	Series I		Series II		Series III	
	Open-minded	Subjects who expected to score at chance	Open-minded	Subjects who expected to score at chance	Open-minded	Subjects who expected to score at chance
N (in decks)	129	200	127	175	133	199
Deviation	+56	—10	+33	—12	+31	—12
Mean	5.43	4.95	5.26	4.93	5.23	4.94

These data might be considered satisfactory in research dealing with a more conventional psychological problem; but a higher standard of significance is to be desired in clairvoyance experiments: the question whether results are repeatable is always raised with particular emphasis here. It was therefore decided to repeat this experiment, and to continue repeating it, until the difference between the sheep and goats either became non-significant or became unmistakably significant. The arbitrary criterion set for a clearly successful experiment was that the scores would be such as to occur by chance only one time in a thousand.*

* An accident occurred during the first repetition, which should perhaps be mentioned here. The experimenter had decided to use, as nearly as possible, the same number of subjects and the same number of runs for the second series as for the first. The assistant who was checking the records mislaid the data of two of the subjects; the experimenter therefore used two subjects too many. When the error was found, we discarded the data of the two subjects tested *after* the predetermined number had been obtained. The data of one other subject were also discarded: a boy had presented himself after the conclusion of the third series, asking to be allowed to "take the test." One of these subjects had an average lower than chance, but the others did slightly better; the result of including the three would have been an increase of twenty in the number of sheep runs, and a net deviation of –2.

Table II

Analysis of scores obtained by open-minded subjects and by subjects who expected to score at chance: Series I, II and III combined.

	Open-minded	Subjects who expected to score at chance
N (in decks)	389	574
Deviation	+120	—34
Mean	5.308	4.941
C. R.	3.04	—0.71
P	.001	.....

The first repetition of the experiment gave substantially the same results as the previous series. On the second repetition the results were again similar; and this time the criterion was met (Table II). The experiment was therefore closed, on the basis that it had shown that subjects who hope to score above chance in card-guessing tend to do so when tested under agreeable conditions; but that subjects who expect only chance scores and are tested under disagreeable conditions will tend to score at or below chance.

Discussion: These dual findings may cast light on one of the anomalies of research in ESP. Many experimenters have reported scores above chance, like those of the sheep (and often far more striking). Other experimenters have obtained negative results, similar to those of the goats. But the point advanced here is that negative results are to be expected if subjects are skeptical and bored, or uncooperative for some other reason, such as a too formal approach on the part of the experimenter.* Thus the negative find-

* See, for instance, "The Experimenter-Subject Relationship in Tests for ESP," in which the authors, J. G. Pratt and M. M. Price, conclude that failure to find evidence for ESP may be due to the "unfavorable" way in which the experimenter approaches his subjects. (*Journal of Parapsychology*, Vol. II, 1938, pp. 84-94.)

ings of one experiment need not be interpreted as casting doubt upon the positive findings of other research, unless the attitudes of the subjects are known to be similar in both cases.

A closer examination of the data corroborates this point. Since the experimental situation is a social one, the attitude of the experimenter is presumably communicated, at least partially, to the subject. As is not unusual, the experimenter's interest and enthusiasm were greater during the first series than in the later ones; and it was in the first series that the sheep average was highest. Similar decline effects appeared in a comparison of the subjects' first runs with their subsequent runs, and of their first few guesses with later guesses within the same run. These effects will be discussed in more detail elsewhere. They are a familiar phenomenon in ESP research* and consistent with the hypothesis that *attitude toward the task* is one determinant of success in card-guessing.

Further discussion of factors differentiating sheep and goats: As several factors differentiated the sheep and the goats, questions naturally arise as to just which conditions were responsible for the difference in scoring level between the two groups. It would be interesting to find if some of the factors are significant and some negligible; we shall therefore look at the procedure in more detail.

Each subject was asked at the beginning of the experimental session if he had ever had any experiences of the type of telepathy, premonitions, crossed letters, and so on. If his answer was in the negative, he was asked if he had ever heard or read of such experiences; an affirmative response was invariably obtained. Then came the crucial question: "When that happens (or when you hear or read about it) is your first reaction, 'Oh, it was just a coincidence,' or do you first think, 'How interesting!' and then,

* See "The Midas Touch in Psychical Research," appearing in the April, 1943, number of this JOURNAL, and "A Further Report on the Midas Touch," in the issue of July, 1943. Professor Rhine has also described similar declines.

'Maybe it was a coincidence.'" Subjects who described themselves as uniformly skeptical were classed as goats; while those who admitted to even a momentary hesitation ("How interesting!") were put in the same category as those who wholeheartedly embraced the hypothesis of ESP.

Once a subject was classified as sheep or goat, he was exposed to a standard set of agreeable or disagreeable physical conditions. Goats were expected to make fifty successive runs without knowledge of their scores. Sheep were given a rest period after each run, during which they were shown their score and encouraged to discuss the procedure and the problem; their maximum number of runs per session was ten, and they were allowed to stop when they pleased. Sheep were offered cigarettes and sometimes candy; goats were not. The signal which notified goats that it was time to guess a new list was a rather raucous buzzer, while the corresponding signal for the sheep was a small light. The sheep were given a new record sheet for each run; but the goats were expected to fill out sheets on which many runs were crowded together. Sheep were placed in a reasonably pleasant office; goats in a "dark room" with no windows and with battered furniture. How important were these factors in the results? And how important was the initial separation of subjects on the basis of their attitude?

A further separation according to attitude may give us a clue. Most of the goats were not especially interested in ESP. They had heard it mentioned or had seen articles about it; they knew that the question was considered significant by some people, but it certainly did not seem significant to them. They might, in spite of that, help me out by giving me an hour of their time, but the problems that I was studying left them cold. The scores of this group averaged almost precisely at the chance level, with a mean of 4.998.

Two of the goats, however, had a different attitude. Both were psychologists. They had read many criticisms of ESP, and had already reached the conclusion that the

reported phenomena were artifacts due to faulty control of conditions, faulty observations, or careless treatment of the data. On that basis, they felt that the only healthy research on the topic was research designed to show up previous malobservation, and thus explain away the positive findings of other laboratories. They therefore disapproved of this experiment. They had discussed the subject often enough to feel identified with their arguments: they wanted to "disprove" ESP. Their average scores were 4.8 and 4.5 for fifty runs, which is slightly below chance, and is consistent with the hypothesis that they had unconsciously aimed at getting the cards wrong. This analysis implies that, within the group not accepting ESP, neutral subjects tend to get chance results and hostile subjects tend to get below-chance results. Since the physical conditions were the same for all goats, a further implication is that these conditions were of negligible importance.

Another scrap of evidence points in the same direction. Although the method seemed straightforward enough to be able to differentiate between sheep and goats, there were four cases in which it failed to do so. Two of the subjects were misclassified due to inadequate questioning. One accepted the possibility of telepathy, and was considered a sheep; but he told me later that he was firmly and unalterably convinced that *clairvoyance* could not occur. Since our experiment dealt with clairvoyance, he should, of course, have been considered a goat. The other said initially that he disbelieved in all such phenomena. When the experiment was completed he showed a great deal of interest in his scores; and said then that he was sure he could guess better than the average!

Two subjects changed their minds during the experimental session. Of these, one was a highly self-conscious graduate student, unwilling to give a direct answer to any question. His vacillating responses placed him among the sheep; but a further discussion after a few trials made it clear that he should have been classed as a goat. The other

subject was an undergraduate who had not given much thought to ESP. Questioned originally, he described himself as completely skeptical. But during the experiment he wavered, thinking sometimes that ESP was just nonsense, and sometimes that it occurred. This wavering, if it had been reported at the beginning of the hour, would have put him in the other category.

Thus two subjects were tested under the less agreeable conditions, and then found to be sheep; and two were tested under the more agreeable conditions before it was found that they were goats. Their data could not be included in the other tabulations, and are given separately in Table III. Since they are consistent with the results of the other subjects who shared their attitudes, they indicate again that attitude is a stronger determinant of the scores than is the laboratory setting, although little emphasis can be placed upon such a small block of data.

Table III

Scores of subjects who were misclassified originally.

	Subjects who hoped to score better than chance, but were tested under the less agreeable conditions	Subjects who expected to score at chance, but were tested under the more agreeable conditions
N (in decks)	98	12
Deviation	+37	—1
Mean	5.38	4.92

It should be pointed out, however, that the conditions of this experiment were not nearly unpleasant enough to test the point adequately. Although the goats were in an ugly room, they may hardly have noticed it, since they knew that most of the rooms in the building were ugly. Though they had no formal rest periods, they were allowed to set their own pace, and thus did not become unduly fatigued. And so for all the physical conditions, which were calcu-

lated to build up mild annoyance or boredom rather than to create active antagonism. I conclude that *attitude,* rather than the pleasantness or unpleasantness of the situation, was the major factor.

Future Experiments: If on the basis of this experiment I were to conduct another which aimed at finding below-chance ESP scores, I would try to exaggerate the factors which were present here. In two of the subjects there was an active and almost belligerent skepticism. Well and good, but could we find it in a higher proportion? Two possibilities present themselves. One is to search for people who have identified themselves with arguments against ESP, and to use them exclusively. The other is to modify the procedure, making the experiment one of precognition, a problem to which many people respond with so fervent a disbelief that, according to this hypothesis, they would obtain very low scores indeed.

We might also greatly exaggerate the unpleasant conditions of the research by putting the subjects in surroundings which are extremely, rather than only mildly, disagreeable. Very bright or very dim lighting, irritating sounds, nauseous odors, electric shocks, smugness on the part of the experimenter; these are just a few of many possibilities. If the subjects continue to believe that the experimenter wants them to get high scores, their scores will probably decline as their resentment and dislike of the situation increases.

One point may be emphasized here. Probably it is not the objective conditions that are influential, so much as *the subject's reaction* to the conditions. A neat and thorough experiment would measure, perhaps physiologically, or through indirect questioning, the subject's antagonism. Scores should be correlated with the antagonism, not with changes in the conditions themselves.

It may seem curious that this lengthy discussion of the goats is not to be followed by one of the sheep. The reason is that the skeptics seem to present a simpler case, and thus

one which is more amenable to discussion. An experiment is now under way to explore the conditions for high scores; and if it is successful it will yield more detailed hypotheses than the author can advance at present.

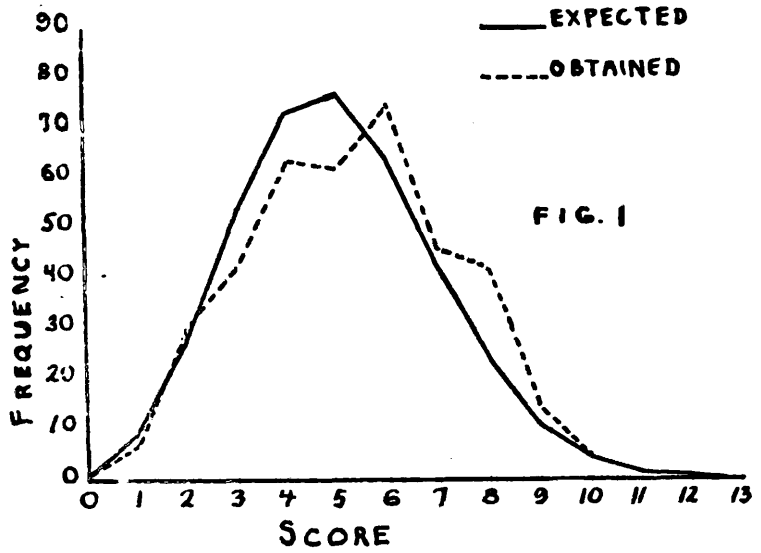
Patterns of success and failure: The pattern of success and failure in guessing may give some indication of the manner in which ESP occurs. To take a hypothetical case: if the sheep scores had averaged five except for a few runs with a score of twenty-five, we could readily say that some strange faculty appears occasionally in a striking form. When it is present, cards in a distant room can be guessed correctly; when it is absent they can not. Similarly if the goats' guesses had corresponded to the expected frequencies except for an extra half dozen scores of zero, one could say that when a subject does not want the theory of clairvoyance to be established, he will occasionally use his clairvoyant ability in such a way as to "disprove" the theory.

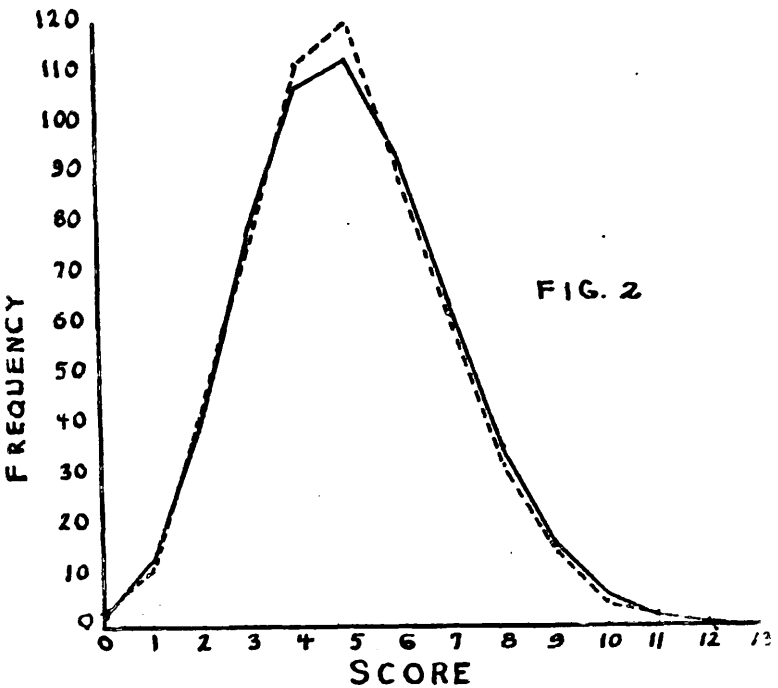
Although at first glance the data do not seem to cor-

Table IV

Comparison of obtained frequencies with frequencies predicted in a binomial situation.

	Runs made by open-minded subjects N = 389		Runs made by subjects who expected to score at chance N = 574	
Score	Obtained	Expected	Obtained	Expected
0	1	1.47	3	2.18
1	7	9.18	12	13.55
2	30	27.54	42	40.64
3	42	52.82	76	77.95
4	63	72.63	112	107.17
5	62	76.24	120	112.51
6	74	63.50	91	93.74
7	46	43.10	62	63.60
8	42	24.23	32	35.76
9	15	11.43	16	16.87
10	5	4.59	5	6.77
11	1	1.56	2	2.30
12	1	.47	1	.69





FIGS. 1 AND 2.
Distribution of scores obtained by Sheep (Fig. 1, N in decks = 389) and Goats (Fig. 2, N in decks = 574).

respond to this pattern, they can probably be subsumed under a modified version of the same theory. Table IV shows the distribution of scores that would be expected in a binomial distribution, and also the distributions actually found for sheep and goats. The sheep had fewer low scores, and more high scores than would have been expected, while the converse is true of the goats. In other words, as can be seen in Figs. 1 and 2, the curve of the goats is shifted slightly to the left; that of the sheep is shifted to the right, so that the mode occurs at six instead of at five. (It is interesting to note that the mode of the sheep scores was at six for each of the three experimental series, showing again that the data are consistent within themselves.)

This relation between the expected and the obtained distributions can best be accounted for if each run is considered not as a single unit, but as a complex process. The run is composed of twenty-five guesses; and as the subject prepares to write each symbol the question arises as to whether that particular guess will be right or wrong. If a subject who hopes for a high score has one brief clairvoyant flash during the run, his score may be higher by just one hit than it would have been otherwise. The data would thus conform to the pattern that was found here; i.e., that the clairvoyant capacity "comes on" for a brief period, then "goes out," then "comes on" again. Although other lines of evidence indicate that this brief account is oversimplified, we may conclude that in the college population studied here, accuracy in card-guessing occurs fitfully and for short periods.

Conclusions: (1) Evidence of clairvoyance was found repeatedly in a normal college population. (2) The subject's attitude was one determinant of his ESP score. Subjects who hoped for high scores in card-guessing tended to score above chance; subjects who were hostile to the hypothesis of ESP tended to score below chance. (3) Apparently clairvoyance occurred in brief flashes rather than for long, continuous periods.

Book Reviews

THE ROLE OF CONJURING IN SAULTEAUX SOCIETY, by A. Irving Hallowell. Vol. II of the Publications of the Philadelphia Anthropological Society, Philadelphia, University of Pennsylvania Press, 1942. 96 pp. $1.25.

The "shaking tent" performances of the Woodland Algonkian Indians have often been cited for their similarity to modern spiritualistic séances. It is interesting to learn that these dramatic demonstrations of "conjuring," first described by alarmed Jesuit fathers in the early seventeenth century, still exist on a feeble scale among scattered Indian groups in Northern Canada.

A. Irving Hallowell, an anthropologist at the University of Pennsylvania, has written a very valuable paper about the "shaking tent," based on first-hand field experience among the Saulteaux Indians of the Berens River in Manitoba. Hallowell knows these Indians well, has published work on other phases of their social life, and approaches his subject with a thorough grounding in modern psychology and anthropology. Most fortunate of all, perhaps, Hallowell is well aware of the "scientific bias" instilled by our own culture, which makes it so difficult for scientists to comprehend primitive life and primitive metaphysical notions. Consequently, Hallowell's objective description of an Indian séance is combined with a patient endeavor to conceive of the world as the Saulteaux see it.

Prior to this paper, only two professional ethnologists had published first-hand observations of a "shaking tent" performance. Hallowell himself has seen only four such séances; but he supplements his own observations with the comments of Jesuit fathers, early travelers' tales, and the unpublished material of other ethnologists. Besides describing a séance, the author also estimates the incidence of conjurers in the population of today and in the past, gives an account of how a man becomes a conjurer, lists the occasions for conjuring, and concludes with a sociological discussion of how conjuring functions in the society of the Saulteaux.

The conjuring lodge is a barrel-like structure ranging from four to twelve feet in height. It is supposed to start shaking as soon as the conjurer steps inside—preferably even before. Once within the "shaking tent," the conjurer—now invisible to his audience sitting outside—begins to invoke a large selection of spiritual beings. With their aid he is able to exercise clairvoyant powers, and claims to see into the future as well as into the past of his followers. He also obtains news of distant friends, recovers lost articles, and finds out the hidden causes of disease. Souls of living human beings can

sometimes be drawn into the lodge by the conjurer—either for a friendly visit—to give the latest news—or for malevolent purposes, in which case sickness, insanity, or death of the victim are said to result. A conjurer may also summon the soul of a rival conjurer to his lodge for a showdown competition of supernatural power. Generally speaking, however, the conjurer exercises his clairvoyant powers for the good of the community. He helps to stall off mythological cannibal monsters who threaten the camp. He foretells important events. In past times, when Indians went on the warpath, he used to report on the movements of the enemy. And finally, in the case of food shortage or famine, he directs hunters to where game can be found.

The spirits contacted by the conjurer are not usually spirits of the dead. They may be anthropomorphic beings who also appear as characters in Saulteaux mythology. They may be "masters" of the various animal species—moose, caribou, etc. Of these the most important is *Mikinak,* the Great Turtle, who serves as messenger and as a sort of intermediary between the conjurer and other spirits. *Mikinak* is a great clown, who talks in a throaty nasal voice not unlike Donald Duck—for the voices of the spirits are distinguishable from that of the conjurer. *Mikinak* is particularly popular with the audience; they always laugh at his jokes and ask him good-natured questions. There is considerable repartee at such a séance.

One can see that there is a lot of difference between a Saulteaux séance and a modern spiritualistic séance in our culture. However, there are some interesting parallelisms. In the first place, although spirits of the dead are rarely summoned, they can be made to appear if the conjurer has sufficient power. An eyewitness of such an occasion gave Hallowell an account of the "spirit return" of a man's father. Observe, in the following dialogue, how similar this sounds to communications in a typical spiritualist séance:

"J. B.: Is that you, father?

F.: Yes, my son.

J. B.: Who are you with?

F.: I'm with my grandchild. Ever since I left, I've always been happy. I've never been hungry. I've never been thirsty. I've never suffered any pain. It is a beautiful country where I am living. When I was alive I always tried to do what was right. Try to do the same thing, my son; don't do anything wrong to anyone. If there is ever anyone who says something bad to you, don't answer. That's the way I tried to act. If you act this way you will be glad. You'll see me some day too. I see some people I know sitting outside. I see my oldest daughter!

Suddenly another voice, that of a child, came from the conjuring

lodge. C.: I see my mother sitting there. Don't do that, Mother. I don't like to see you do that. (*The woman whose father and adopted daughter appeared in the conjuring tent was crying.*) You hear my voice here. I'm happy. It is always bright like day where I live. It is never dark. There are pretty flowers where I live, it's like a great garden. And there are lots of us. There are great singers there, too. Don't forget what I am telling you. Live right and some day you'll find me" (p. 58).

There are several remarkable aspects about this similarity: the return of dead relatives to their loved ones, in whom they are still interested, the assurance of continued existence in a happy "Summerland," and the revelation that moral conduct on the earth plane influences one's happiness in the hereafter.

The latter is not particularly typical of native belief. It leads one to suspect Christian influence, and most of the conjurers, despite their belief in aboriginal mythology, are at least nominally Christian. Hallowell does not discuss the influence of acculturation in this connection, but in another paper dealing with the Saulteaux*, he informs us: ". . . the notion that eternal happiness or misery in the life after death is in any way dependent upon the conduct of one's life 'here,' is derived from Christianity, even when held in a modified form."

"Spiritualistic" séances for the purpose of summoning the dead are extremely rare in native North America. That is what makes this account so interesting. The Maidu of California have such séances; and in the following quotation it appears that Maidu spirits are also concerned with the ethical behavior of those still on earth—although this does not necessarily influence their chances after death. I quote the following for purposes of comparison:†

"The spirit told the assembled people what was happening at the place he came from; he also told them to be kind to one another; and he would foretell whether the acorn and other crops would be good. The spirit would discourse thus for half an hour, then drop the rattle, and depart with the same roaring sound with which he arrived."

Another feature reminiscent of our own spiritualist séances is the production of "apports"—although Hallowell does not use this term. Lost articles are sometimes tossed out of the tent to their owners. One conjurer is said to have produced some keys which a baby dropped overboard on a canoe trip; and the same man is

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* Hallowell, A. Irving, "Spirits of the Dead in Saulteaux Life and Thought," *Journal of the Royal Anthropological Institute of Great Britain and Ireland*, Vol. 70, Pt. 1 (1940), p. 35.

† Gifford, Edward Winslow, "Southern Maidu Religious Ceremonies," *American Anthropologist*, Vol. 29, 1927, pp. 245-246.

credited with restoring a lost rifle to its owner. In fact, if a canoe overturns in the rapids, the conjurer may hand out all the goods from his conjuring lodge.

Similar feats, mentioned by other ethnologists, put to shame the miracles of Madame Blavatsky. A fifty-pound sack of flour, for instance, is magically transported for a hundred miles. One conjurer is said to have had fresh blueberries in his lodge in the dead of winter.

A regular feature of Saulteaux séances is the production of a tobacco, which can be smoked and which is credited with properties of magical increase. Hallowell examined some of this magic brand at a séance. It is not the commercialized variety of tobacco which the Indians use, and its source is a mystery to both the Indians and the ethnologist.

Many of the Ojibwa-Saulteaux conjurers in past times performed the "Davenport trick" of being trussed up and magically untied. But Hallowell did not witness an exhibition of this kind, for it is not part of the repertoire of the Berens River conjurers.

There are thus many points of similarity to the phenomena of a modern spiritualist séance. Perhaps the most interesting of these is the "shaking tent" itself, which may be compared with the medium's cabinet in our culture. This similarity must be more apparent in the winter-time when the conjuring lodge is set up inside of a log cabin. The shaking of the structure is not common in our society. However, a Dr. Hooper, writing of a séance, speaks of the whole house as trembling: "I have seen and felt the floor, walls, and contents of a room vibrate with the power of the mighty rushing wind."*

Note Dr. Hooper's mention of the wind. The Indians themselves speak of a mighty wind—inside the tent—as being the force which shakes it. It is the "Masters" of the winds who are responsible. Similarly, cold breezes are frequently mentioned as coming out of the cabinet or from the neighborhood of the medium in modern spiritualist séances.

In trying to account for the phenomena of the Saulteaux séance, Hallowell finds himself in a difficult position. In the first place, he is convinced that most of the conjurers are not deliberate impostors and charlatans. On the other hand he does not believe that the lodge shakes by supernatural means. He explains the visions and sensations of the conjurer as being projections, heightened by his intense playing of a socially important role. In other words, Hallowell believes that the conjurer shakes the tent himself—but not as if he were consciously practicing fraud. We might ask how Hallowell

* Lawrence, Edward, *Spiritualism among Civilized and Savage Races*, Black, London, 1921, p. 18.

would account for the "apports" in terms of such a psychological explanation.

The Indians readily admit that some individuals shake the tent—despite the supernatural disease sanction against such trickery. But their skepticism is directed toward individuals, not toward the institution itself. They point out that there are cases of very old men who have conjured for hours. How could they have enough strength of their own for such exertion? Hallowell says that a conjurer must have physical stamina, for in first-class performances the tent is seldom still, and at the same time singing and talking go on in different voices. Yet this exertion was not reflected in the "spirit voices" Hallowell heard. A case is reported of a one-armed conjurer who used to shake a tent for six hours at a stretch. And the author gives other instances. Furthermore—although Hallowell did not see it—there are cases reported of tents shaking without an occupant—sometimes with part of the clothing of the conjurer inside. In this way a conjurer was said to set three or four lodges in motion.

For their interest as psychological documents, Hallowell quotes from other sources the impressive declarations of converted conjurers—some on their deathbeds—who were asked to explain how conjuring was done. One old Indian, reported by J. G. Kohl, declared:

"I have become a Christian, I am old, I am sick, I cannot live much longer, and I can do no other than speak the truth. Believe me, I did not deceive you at that time. I did not move the lodge. It was shaken by the power of the spirits. Nor did I speak with a double tongue. I only repeated to you what the spirits said to me. I heard their voices. The top of the lodge was filled with them, and before me the sky and wide lands lay expanded. I could see a great distance around me, and believed I could recognize the most distant objects."

Here is the testimony of another Indian:

"I possessed a power which I cannot explain or describe to you. I never attempted to move the lodge. I held communication with supernatural beings, or thinking minds, or spirits which acted upon my mind, or soul, and revealed to me such knowledge as I have described to you . . . " (p. 74).

It might be mentioned that Saulteaux conjurers did not strike Hallowell as being in any way pathological—in contrast to the Chukchee shamans described by Bogoras. Hallowell gave Rorschach tests to nine individuals who had conjured at one time or another. Apparently the conjurers seemed to be quite normal individuals, not distinguishable, as a group, from other Indians.

As far as conscious deception goes, it is clear that trickery would

be difficult in this particular society. There is no fraternity or association of conjurers, in which there might be a pooling of trade secrets. There is no pattern of handed-down information from father to son. In fact, one individual cannot teach another how to conjure. One can only conjure if one has had a particular kind of dream—a dream which must be repeated four times. Violation of this commandment may bring about supernatural punishment in the form of disease. Finally, it is considered a bad thing to conjure too often. It musn't be done for fun—or to show off.

This paper of Hallowell's is full of fascinating information. A psychical researcher might regret the absence of "tests" and controlled experiments, but this was not the purpose of Hallowell's investigations. The main emphasis of his paper is on the sociological function of the séance—how it keeps alive belief in the characters of Saulteaux mythology and in the general metaphysical beliefs of the Saulteaux—how it supports the *mores* of the society, allays fears, provides entertainment. The veridical aspect of conjuring is a secondary matter to the author. Nevertheless, anyone interested in psychical research can learn much from such comparative material.

The book is illustrated by a cross-section diagram of a conjuring lodge, as well as by some photographs—probably unique—of a lodge in the process of construction. There is also an excellent bibliography.

Victor Barnouw

HYPNOTISM, by George H. Estabrooks. E. P. Dutton & Co., New York, 1943. 249 pp. $2.50.

This volume contains a popular presentation of certain fundamental facts about hypnotism. As the author states in his preface, it is not desirable to allow this field to remain shadowed by popular ignorance and superstition. It is the purpose, then, of this book to remove some of the confusion surrounding the subject, and to make a step in the direction of rescuing hypnotism from the rather anomalous position it now holds in the minds of many people.

The chapter headings give an indication of the scope of the book: it deals with methods of induction of hypnosis, posthypnotic suggestion and autosuggestion, the basic nature of hypnotism, the use of hypnotism in crime and warfare, and with Hitler as a "hypnotist." These topics cannot all be discussed in detail in this review, but certain comments may be in order, particularly in respect of the points at which Dr. Estabrooks touches on psychical research.

Although the discussion of hypnotism in relation to psychical research does not occupy many pages, the author deals with a num-

ber of well-known cases which are of either direct or indirect interest to our field. The "Doris Case" of multiple personality is mentioned, the opinion being stated that Dr. Walter F. Prince "almost certainly created this case of multiple personality through the use of hypnotism," more or less building up the Doris case "on the spot," paralleling the Beauchamp case (p. 106). In view of the fact that there is at least *some* published evidence that several of the secondary personalities had come into being years before Miss Fischer met Dr. Prince, it is difficult to understand Dr. Estabrooks' assertions.

The work of the well-known Boston medium, Mrs. Soule (Chenoweth), is discussed at some length, and the contrasting views of the two Princes (Walter F. and Morton) are presented. Although no conclusions are reached in regard to an "explanation" of the powers of this sensitive, the author does state (pp. 114-115) that, in his opinion, her work, as well as that of Mrs. Piper and Mrs. Leonard, has not yet been satisfactorily "explained away." In further discussing the "spiritistic," or mediumistic, trance, Dr. Estabrooks observes (p. 112) that the spiritistic trance and the hypnotic trance are "identical to all intents and purposes." The statement is also made (p. 14) that in hypnotic trance the subject, left to himself, "will generally do nothing at all." This would seem to be an inconsistency, in view of the unsolicited activity commonly found in the mediumistic trance. The observation seems to this reviewer to be just one more example of the author's tendency to *oversimplify* in his treatment of the problems of psychical research—an oversimplification that can only result in confusing the casual reader on a large number of points.

To return to the purpose of the book, however, which was to remove some of the many misunderstandings currently held regarding hypnotism. The author does succeed, it would seem, in clarifying some essential points. It is brought out, for example, that it is *not* true that only the weak-willed can be hypnotized, that women are easier to hypnotize than men, that alcoholics and criminals are easier to hypnotize than "normal" people, and, finally, that it is somehow "dangerous" to be hypnotized. As the author seriously points out, there is more "danger" to the operator than to the subject.

Finally, in view of the current world situation, it is of interest to consider the chapter dealing with the use of hypnotism in warfare. Although more is left unsaid than said in the discussion, one has the impression that there probably is a place for the use of hypnotism in warfare, though one must be wary in writing of such applications of suggestive techniques. There is, however, a real possibility of using hypnotism to obtain vital information from the enemy, although it would not be a simple matter of hypnotizing a war

prisoner and asking him what he knows; more subtle suggestive devices are indicated, and may even be in use at the moment.

In summary, it may be said that *Hypnotism* is a popular book, written by a psychologist well qualified to hold opinions on this subject. A wide variety of topics is covered and a number of interesting lines of thought concerning the practical applications of suggestive techniques are presented. The subject matter is such that a person of no previous knowledge of hypnotism should at least acquire, through reading the book, a bowing acquaintance with some of the problems of the field. In view of the merits of Dr. Estabrooks' work, it is to be regretted that more time and attention could not have been given to purely practical matters. The proof-reading was apparently hasty, and some proper names are misspelled.

Ernest Taves

Notice to Members

The Publications Committee wishes to thank the large number of members of the Society who responded to the recent "Questionnaire" concerning the JOURNAL. The Committee would particularly like to express its appreciation for the many helpful letters and comments that were received. Members who have not yet returned the "Questionnaire" are urged to do so as soon as convenient.

INDEX TO VOLUME XXXVII

(1 9 4 3)

Index of Subjects

INDEX TO VOLUME XXXVII

(1 9 4 3)

Index of Subjects

Index of Names