# The Journal of Parapsychology

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# *The* Journal *of* Parapsychology

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# *The* Journal *of* Parapsychology

Volume 9 DECEMBER, 1945 Number 4


## EDITORIALS

### PARAPSYCHOLOGY AND DUALISM

A CONFLICT arises in the minds of many people who are thinking today about recent developments in parapsychology. The conflict is most generally expressed thus: "I can follow through the experimental work in ESP and PK and can see that they are not subject to the *same* laws of space-time-mass as is the objective world of physics; but I have to get off when the conclusion is reached that this justifies a nonphysical order of reality. I simply cannot revert to old-fashioned dualism."

Among scientific people there is almost universal disapproval of any departure from the monistic view that "all is one in nature and there are no two ways about it." The universe must not be divided! Man, personality and all, has to be fitted somehow into the physical world. There has been no adequate evidence as yet showing how the fitting is to be done. There is, however, a feeling, a habit of thought—by now a dogmatic conviction—that there is only one order of reality and that it is a physical one. It is natural that this attitude should determine the reaction of scientific people to parapsychology. Probably in many instances the reason for hesitation to accept the work done in that field is that it would seem to compel us to become dualists and to accept the concept of a more or less animistic or spiritual world, the like of which was long ago discarded by the sciences as mere superstition.

We must remember that nevertheless in the practical affairs of men the old soul-body dualism still predominates. Not only is it the religious concept of man, but many other social institutions besides the church more or less take it for granted. The court, the school, the institution of the family, and still others assume that

man is primarily a mind that has ways unlike the physical world (for example, intelligent purpose, free will, group sentiments, rational judgment, and the like) and that this mind is as real as the body, real enough for business dealings, legislation—in short, for civilization. All the romantic, aesthetic, and ethical objectives in life likewise rest on the conception of man as primarily a mental being whose mentality dominates his physical conduct.

It is not hard to understand the development of the bias against this simple dualism. Science has progressively unified the separate objective phenomena of nature. When the basis of sound was discovered to be motion and when, one after another, heat, light, electricity, and all the chemical changes were resolved into manifestations of movement, the pattern became fixed in our minds for all the sciences to follow—the pattern of physics. Biology in general, as far as it has developed, has been largely a physical science. There has been a strong tendency for the infant science of psychology to follow along the same lines, as just another extension of physics, although in so doing, it has veered away from many of its own most fundamental problems. With a minority of notable exceptions members of the psychology profession have found it easier to steer clear of the more basic questions concerning the nature of human personality and attack, instead, the problems of behavior that allow a better alignment with the objective operations of the physical sciences.

Whatever the difficulties encountered in following the pattern at other points in psychology, the conflict is sharp and clear in parapsychology. When we find that ESP transcends distance and time and that PK transcends distance and mass relations, there is no doubt we come upon a clean break with the physical tradition in the science of living beings. Here is something that is not of the physical universe; it is extraphysical. For those who know the research results this cannot be denied except by futile haggling over the definition of the word "physical." We have of course to deal with the "physics" of today, not what it may be tomorrow, just as we are limited to the parapsychology we already know and must leave to the future our judgments regarding later findings. It would be sheer speculation to make suppositions about either in advance.

On this basis we can now recognize the existence of two distinct

types of causation (or determination): one, the more familiar energetic action which characterizes the physical universe as far as it is known; and another which is found, in these paraphysical manifestations, to transcend (that is, to follow other principles than) the physical systems we know. If this confirmation of a second causative principle in nature brings us back to dualism, there appears to be no proper escape from it.

There are, to be sure, certain well-known philosophical "escapes" from the problem which were offered centuries ago by such profound thinkers as Hobbes, Berkeley, Leibniz, Spinoza, and Kant. One or another of these views of the relation between body and mind has long satisfied those who can be pacified with untested speculation. What the scientific worker today acutely needs, however, is a hypothesis of mind-body relations which not only is in line with the facts at hand but which also can itself be the target of further research. He is seeking not something to rest on so much as something to go to work on.

In the light of the research in parapsychology we may say that mind and body show definite fundamental differences; yet we know also that they do manifestly interact. Our trouble is that we cannot see how systems which are utterly unlike can possibly interact without in some respect belonging to a unifying system somewhere in the background. Interaction is inconceivable without basic common properties. It is therefore only a *relative* sort of dualism that is required to describe the facts turned up by parapsychology. No absolute dualism would be logically acceptable.

It is true, this relative dualism could just as well be regarded as a monism too—but only as a *relative* monism. We may therefore have our choice of terms, dualism or monism; but since these terms are no longer very sharply descriptive, the word "relativism" may serve to designate this view.

This relativist hypothesis is in some important respect or other different from each of the classical mind-body hypotheses. But one of its virtues lies in the fact that it fits the present need for a way out for the scientist who seeks, as he is bound to do, a completely unified system, "one world." He has no choice, it seems, because all knowable events can be known only through some kind of influence directly or indirectly exerted upon the psychophysical person-

ality of the knower, and this very interaction implies a unifiable basis in the background for everything discoverable. Thus we may say that while the relativist hypothesis pictures psychical and physical processes as both real and relatively distinct, they are rooted in a common system of energetic determinants which as yet is not known to science.[1]

There is no reason whatever to suppose this common psychophysical substrate as undiscoverable. Rather, it is something for which we may now earnestly begin to search. It is at present merely a logically required reality, but that is enough to make it an objective for research, even though not at all enough for conclusive acceptance. This elemental system may be, and probably is, much more than a mere subconnection for the interaction of the body and mind of man. The imagination can do much with the possibilities associated with this unknown level of underlying energy. But all we know yet, even inferentially, is that there has to be some such fundament for the interaction of body and mind.

A relativist view of man, then, should go far to relieve parapsychology of its horrors for the monistic scientist; for it is as monistic as our knowledge allows, even while it is as dualistic, too, as the newer facts of parapsychology require.

J. B. R.

## ON THE FURTHER RECONSIDERATION OF TELEPATHY AND CLAIRVOYANCE

THE ARTICLE in our last number entitled "Telepathy and Clairvoyance Reconsidered" has drawn a gratifyingly large amount of discussion. This correspondence would normally be published in

[1] The following excerpt from Professor C. D. Broad's presidential address before the Society for Psychical Research, 1935, are of interest in this connection:

"We must therefore consider seriously the possibility that each person's experiences initiate more or less permanent modifications of structure or process in something which is neither his mind nor his brain. There is no reason to suppose that this Substratum would be anything to which possessive adjectives, such as 'mine' and 'yours' and 'his' could properly be applied, as they can be to minds and to animated bodies. . . . It does not seem impossible that it should be some kind of extended pervasive medium, capable of receiving and retaining modifications of local structure or internal motion. But I do not think that we have at present any adequate data for further speculations about its nature."

This hypothetical medium, suggested in connection with the explanation of telepathy, would probably be identifiable with the common background we are here inferring as necessary for *all* psychophysical interaction.

our section on Letters and Notes were it not for its extent and for the large amount of duplication in its content. But we wish to bring these comments before our readers in a form that preserves their essential value, and to this end we are preparing a digest of the letters associated with the article. The necessary delay in securing the approval of the various commentators will postpone publication of this digest until our March issue.

This matter is mentioned for two reasons: The first is to call attention to the fact that the discussion is still open. No topic since the founding of the *Journal of Parapsychology* has provoked such lively discussion as that which has been raised over the case for telepathy and clairvoyance. We shall welcome comment from others who may be inclined to join in this reconsideration of the explanation of ESP.

Our second objective is to invite attention to the sequel to this article. It appears in this issue under the title "Precognition Reconsidered." Those who read the first article will recall that it was the evidence for precognition which upset the traditional views of telepathy and clairvoyance. The reconsideration of precognition along the lines of logical analysis which were followed in the discussion of telepathy and clairvoyance raises further problems and suggests additional experiments in that newest and most challenging of all parapsychological hypotheses.

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# EXPERIMENTAL PARAPSYCHOLOGY IN WARTIME BRITAIN: A REVIEW

By D. J. West[1]

## Introduction

So formidable have been the difficulties under which psychical investigators in Great Britain have labored during the past five years that it would have been no reflection on them if all experimental work had been abandoned. With so many people called up, and with the remainder shouldering the burden of many wartime commitments, spare time has been very scarce. In England, psychical research is of necessity a spare-time occupation, for it is not yet recognized as a university subject, and there is no "Department of Parapsychology."

The Society for Psychical Research has always been the main center for the organization and publication of British research. During the war it has had to manage without its research officer, and the exorbitant costs of printing, coupled with the strict paper rationing, have considerably reduced the volume of its publications. There have been times when aerial attacks on London prevented meetings for research or any other purpose, but the loyal exertions of the Society's honorary officers have kept its machinery well oiled and ticking over. Now that the situation is returning to normal, the S.P.R. is ready to resume its scientific crusade for truth with renewed vigor and determination.

In spite of everything, research has progressed surprisingly during the years of war. A few individual efforts have resulted in some outstanding pieces of work which, by their high quality and originality, make up for the inevitable lack of quantity.

[1] Mr. West, Honorable Assistant Secretary and Member of the Council of the Society for Psychical Research in London, has already been introduced to these pages through an article reprinted in our June number. He is at present studying for a medical degree at Liverpool University.

## Carington's Work on the Paranormal Cognition of Drawings

Great advances have been made in the field of ESP research. Whately Carington's statistically assessed experiments on the paranormal cognition of drawings (1) need only very brief mention because Carington's two main papers have been published in America, as well as a short but excellent review by Stuart (11).

Carington's experiments were aimed at the production of a valid, practical, and repeatable technique that could be guaranteed to give, if not invariable success, at least statistical significance in a reasonably small number of tests.

The first report describes five experiments in each of which the procedure was as follows: Ten words were selected randomly from a dictionary by the experimenter and illustrated by ten drawings which were known as "originals." The originals were displayed in the experimenter's room, one at a time, on ten successive evenings. (In one of the experiments the originals were displayed at five-minute instead of 24-hour intervals, but this does not affect the principle.) On each evening, groups of percipients, scattered about the country, tried to reproduce the original on display.

The originals from all five experiments were randomized and sent, together with the percipient's drawings, to an independent judge who scored every drawing against all 50 originals without knowing which drawings were intended for which originals. A full point was awarded when a drawing plainly and literally represented the specific object of the original, and half a point when there was a slight doubt.

Drawings of a particular evening did not score significantly higher on originals of that evening than on originals of other evenings, but percipients from a given experiment scored significantly higher on the originals of their own experiment than on the originals of other experiments. In other words, although percipients were being influenced by the originals of their experiment, they were being influenced by all ten originals on every night and not by just one per night. It was this phenomenon that Carington named "displacement effect."

The following table[2] summarizes the results of the first five experiments:

HITS BY THE DRAWINGS OF EXPERIMENTS

| On the originals of | I | II | III | IV A | IV B | Total |
|---|---|---|---|---|---|---|
| I | **51.5** | 7.0 | 5.5 | 40.5 | 23.0 | 127.5 |
| II | 77.5 | **18.0** | 7.5 | 51.0 | 27.0 | 181.0 |
| III | 36.5 | 12.5 | **6.5** | 45.0 | 36.5 | 137.0 |
| IVA | 25.0 | 7.0 | 4.0 | **41.0** | 24.5 | 101.5 |
| IVB | 82.0 | 19.0 | 10.0 | 106.0 | **84.5** | 301.5 |
| Total | 272.5 | 63.5 | 33.5 | 283.5 | 195.5 | 848.5 |

P = .001

Two further experiments, numbers VI and VII, are presented in the second report. Experiment VI was designed to determine whether there would be any difference in scoring on originals "displayed" in sealed envelopes and unseen by the experimenter and originals that were both seen and copied by the experimenter. Owing to the insensitivity of the cross-scoring technique, neither condition gave a significant score. Experiment 8 had no special features except that the result was significant.

In his next paper, Carington developed a much more sensitive method of scoring. Regarding his large body of data as a substantially random sample representative of the general population, he compiled a catalogue of frequencies with which various objects tended to be drawn by percipients. On this basis, he was able to allot appropriate scores to each of his observed hits, giving a high score to hits on objects that are usually drawn with relatively small frequency. A hit on a comparatively rare object like a chemical retort would be given a larger score than a hit on a common one such as a house.

The statistical evaluation was based upon Fisher's method of logarithmic scoring. Carington did the scoring himself, but he counted as hits only those correspondences that were very specific and undeniably literal.

[2] Taken from Example I (1, Part I, p. 132).

This method of scoring was much more sensitive than the cross-scoring technique. The following table[3] shows the results of an application of the catalogue method to the whole of the drawing experiments:

| Experiment | P |
|---|---|
| I | .06 |
| II | .001 |
| III | .85 |
| IV | .13 |
| V | .91 |
| Total I–V | .01 |
| " VI | .005 |
| " VII | .02 |
| " VIII | .000001 |

The catalogue assessment is open to certain criticisms. Subjects that are topical at one time among one group of percipients may not be topical at a different place and time, so that the scoring method may not be universally applicable. It is true that the effects of topicality might be expected to cancel each other out over a wide range of subjects, but the numbers of originals in any one ESP experiment are relatively restricted.

Carington's own experiments are proof against the topicality objection. He used 11 sets of originals altogether, and every one gave a positive score. Bias due to topicality would not be in a single direction like this. There is also considerable intrinsic evidence that the effect was negligibly small in the data under consideration.

It is important to notice that Carington's results were significant according to both the cross-scoring and the catalogue methods of evaluation, which considerably increases their value as evidence for paranormal cognition.

## The Soal-Goldney Experiments on Precognitive Telepathy

The story of Dr. Soal's important card-guessing experiments is well known—how Soal had for years been trying to repeat Rhine's experiments; how he had performed thousands upon thousands of trials without discovering a trace of telepathy; and how at last he

[3] Taken from Table 1a (**1**, Part IV, p. 163) and Table 2 (*ibid.*, p. 166).

gave up the search. Then Whately Carington, who had found a displacement effect in his experiments with drawings, began to urge Soal to re-examine his data to see if any of the percipients were scoring significantly on the cards on either side of the target. Soal set to work without the slightest expectation of success; but, to everyone's surprise, two percipients were found who really had shown this displacement effect to a significant degree (**9**).

Interest now centers upon the Soal and Goldney experiments with one of these displacement percipients, a London photographer named Basil Shackleton (**10**). These new experiments combine in a single research all the good features of previous ESP experiments. The conditions were irreproachable. Fraud, hyperaesthesia, coincidence, hallucination, and every other hypothesis that has ever been used to try to explain away ESP results were considered and successfully disproved.

For material, five cards bearing animal pictures were employed. The assessment was directly numerical and the result unambiguously and unquestionably extrachance. P = approximately $10^{-35}$.

The experimental set-up was very carefully thought out. The percipient, with an experimenter controlling him, sat in a separate anteroom with the communicating door ajar to facilitate hearing. In the main room was a card table bearing a wooden screen which had a small aperture or window. On the side of the screen remote from the anteroom sat the agent with a box, its open side toward him, placed below the window in the screen. On the floor of the box, face downward, were the five animal cards. On the opposite side of the screen sat a second experimenter with cards bearing numbers one to five. To one side of the card table, watching the agent, there was sometimes an observer or independent witness.

The procedure was as follows: The agent shuffled the five animal cards out of sight of the experimenter and arranged them face downward in the box. A previously prepared, and until then closely guarded, series of random numbers, one to five, was given to the experimenter at the card table. When the experiment began, this experimenter held the card bearing the number appearing first in his random series up to the window in the screen for the agent to see. Supposing it happened to be a three, the agent would turn over the

card third from the left in the box and concentrate upon it. On hearing the experimenter call, "Ready," the percipient in the anteroom would record a guess by writing down, in the first division of the form provided, the initial letter of the appropriate animal.

With this method, the agent and the observer are the only people who know which animal card is being presented on any given occasion, and they preserve silence throughout the guessing. The experimenter who calls, "Ready," sees only his random series of numbers, so he can give nothing away.

At the end of 25 calls, the order of the agent's animal cards was noted and the guesses were checked by the experimenters and observers. Hallucinatory errors in recording could not explain the result because the agent's and percipient's series had been made completely independently and the correspondences could be checked by any impartial observer. Fraud was out of the question because significant results were produced under the described conditions in the presence of a variety of independent witnesses. Effective collusion between agent and percipient would have required a code of signals, but even that would not help in the precognitive guesses.

Results showed that the percipient scored significantly on the card one ahead, but not on the target card itself. When the rate of guessing was quickened, the percipient scored on the card two places ahead. In some of the experiments, the card looked at by the agent was determined, not by a previously prepared random series, but by picking counters from a bowl. Under these conditions, the guessing must have been precognitive because the counter determining the card one or two ahead had not been picked at the instant a guess was made.

Consistent absolute failures were recorded when the agent only pointed to the card backs instead of turning them over and looking at the faces. The agents varied considerably. With some the results were consistently very successful; with others they were chance. The agent appeared to be the determining factor. Shackleton's faculty apparently consisted of a telepathic precognition of the agent's mental perception of the card.

Apart from the very highly significant scoring rate there was further independent statistical evidence of the presence of ESP:

1. There were highly significant differences in the rate of scoring on the five different animal symbols.

2. When the agent's card sequence presented, on either side of the target, cards that were the same as each other, or both the same as the target card, the circumstance was called a case of "multiple determination." On the ESP hypothesis, it might be expected that in such cases the telepathic effects of the symbols would reinforce each other; and in fact it was found that with one particular agent multiply-determined guesses were significantly higher than the rest,[4] with $P < 3 \times 10^{-5}$.

3. J. B. Rhine and Betty Humphrey (**6**) examined the Soal-Goldney data and discovered a highly significant chronological decline in scoring rate. They tried to confine attention to standard sessions, ignoring those in which any special conditions prevailed. In this they may not have succeeded, because Soal and Goldney maintained that the most significant element of the decline was due to a change in procedure; but whatever the correct interpretation of the decline may be, it remains a highly significant extrachance effect.

### Miscellaneous Small Experiments

No other recent English card-guessing experiments can compare with Dr. Soal's epic work. Small numbers of telepathy trials are performed at odd times by different people, but they are haphazard attempts rather than planned researches and do not attain to published reports.

In 1941, I performed, under good conditions, 2,000 telepathy trials (**14**), my father acting as percipient and myself as agent. A CR of 2.85 was produced. These trials are worth mentioning only because they showed immediate success with the first available percipient, which is in striking contrast to Dr. Soal's experience. It is possible that my habit of changing the card symbols frequently may have helped, but it is more likely that it is the mental relationship of agent and percipient that determines success.

[4] See Appendix B (**10**, p. 136).

Dr. Rhine examined my data and calculated the distribution of successes in the five segments of the run and in the five trials of the segment. The distributions were similar, but the trials were too few to make the observation statistically convincing.

In 1942, I carried out a larger series of experiments comprising some 30,000 clairvoyance and precognition trials (**15**). I was working alone and acting as my own percipient. The experiments were unwitnessed and the conditions very variable, the idea being to give the faculty its head to see if anything interesting would be found which could be tested more rigidly at a later date.

The results were highly significant and the level of scoring good. The extrachance effects were legion: sudden transition to negative scoring when the session was prolonged beyond a certain point; correlated variations in scoring rate when ordinary trials were alternated with precognitive trials; scores brought about by inhibition of runs of failures instead of by increase in runs of successes; marked change in intrasegmental distribution of successes coincident with a change in attitude of the percipient; short phenomenal successes.

I came to the conclusion that these bizarre ESP effects, too numerous to mention in detail, were probably determined by psychological factors. When I arranged for some test sittings under evidential witnessed conditions, my "faculty" dwindled to nothing and has never reappeared.

## Clairvoyance Machines

A new development in ESP research is the introduction of machines for testing pure clairvoyance. One school of thought maintains that clairvoyance in the strict sense is impossible, that some mind-to-mind or telepathic element is essential to ESP. These machines should settle the question.

Denys Parsons has devised and constructed, and I have duplicated, a machine that uses counters of five different colors. The counters are shuffled out of sight and loaded into the machine, in which they lie in a column, one on top of the other, in an unknown order within a vertical tube. Operation of a lever at the base of the tube allows the counters to fall one by one, after the fashion of

a slot machine, into one or other of five concealed compartments, according to the position of a five-way switch operated by the percipient. A counter that falls into a compartment marked with its own color is regarded as a success.

At the end of 100 trials the compartments are opened and the successes counted. Any significant deviation must be due to pure clairvoyance. Precognition of the mental experience of the person checking the score is not the explanation because the checker never learns the actual order of the successes; he knows only the net result.

The machine has a spy-hole through which an agent may watch the counters when it is desired to perform an experiment under telepathic conditions. So far, neither Parsons nor myself has succeeded in obtaining any significant results with his machine under either telepathic or clairvoyant conditions. Mr. Shackleton was among the percipients tested by Parsons.

An electrical clairvoyance testing machine has been constructed by Mr. G. Redmayne (**7**). The percipient is given five press buttons, only one of which is connected with a certain circuit inside the machine. The percipient has to try to press the right button. When one of the buttons is pressed, the machine records a success or failure and selects, automatically and randomly, the target button for the next trial. When the recording device is arranged so as to register only the total numbers of successes and failures, the machine is adapted for testing pure clairvoyance on just the same principle as Parsons' device

Redmayne's machine can be used for testing precognition, and the recording mechanism can be made to register the sequence of successes and failures on a circular paper disc.

The random selection of one of five circuits is accomplished by a motor-driven rotating selector. This has five vanes and can be stopped in any of five positions by a magnetically operated brake. The brake is made to go on and off at irregular intervals by means of a thermal delay switch. The speed of the rotating selector is varied by an automatically varying resistance in series with the driving motor. Fraudulent scores, such as would result from pressing two or more of the buttons simultaneously or from other nefarious

maneuvers, are prevented by special devices installed within the machine.

Redmayne is using his machine in actual ESP trials, and we look forward to some interesting results.

## PK Experiments

It is a relatively short time since news of the existence of PK reached England, but already a number of British researchers have tried to repeat the American experiments. It is rather early to give a fair survey; but the last number of *Proceedings*[5] contained three PK reports. In spite of the experimenters' endeavors to follow exactly the American procedure, two of these reports describe only chance results. The third, by Dr. Thouless, describes some extra-chance effects, but they were obtained with spinning coins and not dice. Dr. Soal has performed a few PK trials with only chance results. It is understood that some large-scale PK experiments are in progress at Cambridge, but so far without producing any evidence for PK. The PK situation is reminiscent of the early British card-guessing experiments which were for a long time persistently unsuccessful.

Denys Parsons is at the moment engaged on PK trials in which three dice are enclosed in a sealed glass jar containing clear, viscous fluid. The throw is accomplished by inverting the jar and allowing the dice to fall slowly through the viscous fluid to the bottom. This slow fall might be expected to assist any paranormal physical forces by allowing them a longer time to act.

## Carington's Quantitative Study of Trance Personalities

In another branch of the field, considerable success has attended attempts to bring the phenomenon of mediumship into the realm of controlled experiment. The first step was made in 1930 when the Saltmarsh-Soal method of estimating the statistical significance of mediumistic statements was applied to some sittings with Mrs. Warren Elliott (8).

A long investigation into the nature of trance personalities, the

[5] August, 1945.

last report of which was published in 1939, was carried out by Whately Carington (2). Although this investigation does not belong strictly to the war period, it is so important and has received so little attention that I must give some account of it here.

Carington applied the psychological association test to the medium, Mrs. Leonard, and her control, Feda, measuring their respective reaction times to a standard list of words. It was thought that such a set of reaction times would be characteristic of a particular personality, much as a set of fingerprint measurements is characteristic of a particular body. If that were so, all that would be needed to prove Feda an independent personality would be to show that her reaction times were significantly different from Mrs. Leonard's.

Unfortunately, this expectation was shattered when Besterman tested the late Oliver Gatty's reaction times: first, while he was thinking of his scientific pursuits, and, second, while he was thinking of his life in the country. Even though they only corresponded to different states of mind in the same person, the two sets of reaction times were as dissimilar as would be reaction times taken from two separate individuals.

It followed that the test could not be used to prove that unrelated sets of reaction times came from different personalities, because one of the personalities might be no more than a pose adopted by the other. On the other hand, it was still possible to use the test for proving the opposite: namely, that two personalities claiming to be different are in reality the same if their reaction times are closely related. It could hardly be supposed that one personality would have so intimate a knowledge of another's mental life as to be able to simulate the appropriate reaction times over a long list of words. Expressed colloquially, without reference to the considerable mathematical and experimental technicalities, if there should be a high degree of correlation between two sets of reaction times obtained on separate occasions, then the presumption would be that it was the same personality at work in each case.

When the data from Mrs. Leonard and Feda, and Hereward Carrington's data from Mrs. Garrett and her control, Uvani (3), were examined, it was found that the reaction times of medium and control were not independent, as would be expected if they were

separate personalities, but were correlated to a significant degree. It would be a fairly safe inference that, in the two cases under examination, the supposed spirit controls were only manifestations of the mediums' own personalities.

An interesting point was that in both cases the correlations between the mediums' and controls' reaction times were negative or reciprocal; that is to say, a word for which the medium would give a long reaction time would call forth a correspondingly short response time from the control, and vice versa. This "countersimilarity," as Carington named it, establishes the intimate connection and supposed common origin of the personalities of medium and control just as surely as a positive correlation would do, but it suggests that the control is formed of material suppressed from the normal personality. It is well known that a repressed or secondary personality tends to be the direct opposite of the primary.

The same tests were applied to two ostensible "spirit communicators" purporting to manifest through Mrs. Leonard. These showed no appreciable correlation with the normal Leonard personality, from which it would appear that if they were secondary personalities they were of a different type from Feda, the control, and are formed in a different way.

In the next test the same spirit communicators were made to manifest through different mediums, but there was no appreciable similarity between the two sets of reaction times so obtained. However, even on the spirit hypothesis, it is not supposed that the measurements are pure reaction times of the communicator, but rather some function of the reaction times of both communicator and medium. The preponderance of the two mediums' influence might have masked any similarity due to the common communicator.

An attempt was made to eliminate the influence of the mediums. The reaction times of a pair of spirit communicators were tested through different mediums; but instead of the individual reaction times, the differences between the reaction times of the two communicators were used as working data. In this way, distortions due to the mediums were made to cancel out, and a significant relationship was demonstrated between the same pair of communicators manifesting through different mediums. This is nothing like a

proof of the spirit explanation, but it does seem to indicate that the communicator impulse, unlike the control, is external to the medium. The result of the experiment using differences was complicated by the fact that the observed correlation was once again negative, as if one or both communicators had had opposite effects on the reaction times of the two mediums.

### C. D. Thomas' Proxy Sittings with Mrs. Leonard

A development in mediumistic investigations that has been exploited recently is the proxy sitting, in which the person who visits the medium is standing proxy for someone else. The spirit communications are supposed to be addressed to the absent sitter. This procedure has a great advantage from an evidential point of view, because the less the person sitting with the medium knows about the significance of the communications, the less information will he be capable of giving away.

The Reverend C. Drayton Thomas reported a series of proxy sittings in July, 1939 (**13**). Unfortunately, their evidential value was considerably lowered by the mentioning of the communicators' names in the medium's presence.

Two very interesting cases were reported by Mr. Thomas a few months later (**12**). In both of these he received spontaneous messages through Mrs. Leonard from two strange communicators who claimed that a letter would arrive from their relatives very shortly. In both cases, Mr. Thomas did receive letters from relatives between one and two weeks later. The messages were given to them and were claimed as highly evidential.

### Hettinger's Investigation of the Ultra-Perceptive Faculty

More strictly experimental was the work done by Dr. Hettinger with psychometric mediums, first published in his book *The Ultra-Perceptive Faculty* and later in the *Journal of Parapsychology* (**4**).

Psychometric mediums ask to be shown some personal belonging, such as a necktie, upon which to concentrate their thoughts. They are able to make—presumably by means of some sort of ESP—statements (items) concerning the absent owner (subject). Dr.

Hettinger sought for some method of demonstrating statistically that the items given by psychometrists were beyond the range of chance coincidence.

In the first series of experiments, Hettinger adopted what he called the "checking method." In the course of some eight sittings, the medium was given 40 objects belonging to 40 different subjects. Separate records were made of the items given by the medium in connection with each of the 40 objects. The subjects were instructed to score their records by putting a *1* against each item that applied to them. Each subject was given five records to score, his own and those of four other subjects.

Of the 40 subjects in this experiment, 29 obtained a higher score on their own record than on any of the four corresponding check records. Taking 0.2 as the probability that a real score will exceed the check scores, the chance expectation from 40 records is 8, compared with an observed figure of 29. This gives a

$$\mathrm{CR} = \frac{21}{(40 \times 11/40 \times 29/40)^{1/2}} = 7.6, \text{ which is significant.}$$

There are many criticisms to be made of this argument: first, with regard to the statistics. In calculating the deviation from chance expectation, Hettinger uses the obvious value 0.2 for the probability of a success, but in calculating the standard error from the binomial formula $(npq)^{1/2}$, he substitutes the observed ratio of successes and failures for the theoretical values of $p$ and $q$. Considering that his object is to prove that these are not chance ratios, this procedure is very strange. However, in this case, as in the evaluations of later experiments, the effect on the level of significance is negligible.

The statistics are open to a much more serious objection. Hettinger discreetly refrains from associating any probability with his observed CR. The fact is, it would be improper to use tables of the normal probability integral when the binomial formula has been applied to such a small number of trials.

It is therefore not without some justification that critics have complained about Dr. Hettinger's statistics; but though there may be faults in his evaluations, his results are none the less significant.

In the present example, the antichance odds could have been calculated directly from the binomial expansion: namely, P = sum of first 12 terms of the expansion of $(1/5 + 4/5)^{40} < 10^{-11}$.

Another aspect of the investigation open to criticism is the unsatisfactory nature of the conditions at the sittings. The medium was in the habit of fingering the object or pressing it to her forehead. It is true that the objects were enclosed in sealed envelopes, but palpation would serve to identify such things as combs and hairclips. It would be some guide to what classes of items would be likely to score hits if only the sex of the owner were revealed in this way. The objection is answered by a later experiment in which significant results were obtained when the medium did not touch the object; but it is a pity this rule was not followed throughout.

Another unsatisfactory point was the presence of Dr. Hettinger, who knew to whom some of the objects used at the sittings belonged and who was acquainted with some of their owners. Unconsciously he may have given the medium indications of when she was on the right track.

A very important objection to the checking experiments is the fact that when the subjects were given the records to score, they were told which record sheet belonged to them. This might have given rise to bias in the scoring; in fact, the suspicion was confirmed when Hettinger investigated the effect of misinforming five of the subjects as to which were the records intended for them. Not one of them scored highest on the record that was really his own.

Hettinger tried making his subjects score without giving them any information. Only one in nine subject scores was higher than the corresponding control scores. The subjects complained that the conditions made their task of scoring too difficult and tiring, so that the method had to be abandoned. This rather suggests that the medium's successes were not of very high quality.

The method of admixed control was the next to be tried. Each subject was given 24 items to score, 12 of which were intended for him and 12 of which were controls. In the absence of paranormal cognition, it is an even chance whether the subject puts a *1* against a true item or against a control. The result was as follows:

| | |
|---|---|
| Score on correct items | 328 |
| Score on control items | 222 |
| Total items | 550 |

This gives a CR $= \dfrac{328 - (1/2 \times 550)}{(550 \times 1/2 \times 1/2)^{1/2}} = 4.5$, which is significant.

In this, as in later series, Hettinger has a variety of methods of evaluating the result, but they do not give any more information and are generally less sensitive than the one given above.

Seeking always for a more sensitive technique, Hettinger modified the method and had the subjects choose between paired records instead of paired items. The subjects also scored the individual items the same as before. The new method of scoring gave no significant result; the old method gave a result on the border line of significance:

| | |
|---|---|
| Score on correct items | 207 |
| Score on control items | 162 |

On the whole, the method of choosing between paired items, one of them a control, seemed to be the most sensitive, and it was used in all the later experiments. The total result for the control experiment was:

| | |
|---|---|
| Number of correct items accepted | 2,570 |
| Number of control items accepted | 1,913 |
| Total | 4,483 |

$$CR = \frac{328.5}{(4483 \times 1/2 \times 1/2)^{1/2}} = 9.8$$

In these experiments, Dr. Hettinger has introduced a simple and apparently effective method of assessing objectively the quality of mediumistic material. It is to be hoped future experimenters will make full use of it.

In 1941, Dr. Hettinger published a second book of experiments (5). In this he described a most ingenious method of directing the psychometrist's faculty. Without informing his mediums that any unusual experiment was in progress, he arranged for his subjects

to be looking at magazine illustrations at the time of the sittings. Out of 3,513 items given by the sensitives under these circumstances, 585 (16.68%) applied to the corresponding magazine material.

Experimenter and subject had their watches synchronized and they both noted the time at intervals of approximately a minute. In this way, items given by the sensitive could be divided into groups corresponding in time to the perusal of particular illustrations.

The principle is original, and many of the correspondences described in the book are very striking. Unfortunately, their scientific value is considerably lessened by Hettinger's disinclination to adopt any statistical scoring technique. Individual hits are described and discussed as if every hit were paranormal, which is absurd in view of the earlier experiments with the same mediums. Other investigators working with Hettinger's sensitives have failed to obtain satisfactory results.

## Official Recognition

My brief survey of the progress of experimental work is now complete; but besides the technical advances, British parapsychology has made considerable gains of late in the way of academic recognition. The following is a list of university honors conferred upon psychical researchers recently:

J. Hettinger: Ph.D., London, for his book *The Ultra-Perceptive Faculty.*
L. J. Bendit: M.D., Cambridge, for a thesis entitled "Paranormal Cognition, Its Place in Human Psychology."
V. G. Kirk-Duncan: Ph.D., Oxford, for a thesis entitled "A Study of Certain Aspects of Prima Facie Extra-Sensory Cognition."
S. G. Soal: D.Sc., London, for his distinguished work on ESP.

In 1940, with a bequest from F. D. Perrott in memory of F. W. H. Myers, a studentship in psychical research was founded at Trinity College, Cambridge. Tenable for one year, the studentship has an emolument up to £300. It was awarded to Whately Carington in 1940.

In 1943, Whately Carington was the recipient of another official blessing when the Trustees of the Leverhulme Research Fellowships awarded a grant for two years in support of his experiments in paranormal cognition.

On July 4, 1945, the British Broadcasting Corporation, an organization that is proverbially reluctant to be associated with anything unorthodox, allowed Dr. Soal to broadcast a fifteen-minute talk on his ESP experiments. The talk was published and favorably commented on in the *Listener,* an organ of the B.B.C.

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# PK EXPERIMENTS WITH TWO-SIDED OBJECTS[1]

By ELIZABETH MCMAHAN

**ABSTRACT: Two experiments are reported in which two-sided objects were used instead of the usual dice to test the PK hypothesis. The apparatus used for throwing the objects at first consisted of chutes and later of a revolving tube. In Experiment I, cardboard squares were used, and the subjects, who were mostly college students, were tested individually. Experiment II involved the use of plastic discs and the testing of children and adolescents in a social atmosphere. Each subject in the second experiment had two turns; the first turn gave significantly high scores, the second, low scores. There was a significant difference in rate of scoring between the first and the second turns.**

**Miss McMahan is a graduate student in psychology and a research assistant in the Parapsychology Laboratory.—Ed.**

THIS PAPER is a report of experiments in which subjects threw two-sided objects and attempted to influence them mentally to turn with a specified face uppermost. The experimental work heretofore reported under the heading of psychokinesis has, with one exception, dealt with the throwing of dice, which have six faces. The exception is Thouless' report of coin-spinning tests (**10**). In our attempts to discover the nature of the PK process, it is important not only to see what effect size of the dice and number thrown at a time have upon scores, but also to find out whether significant results can be obtained with objects of different shapes. The experiments herein reported, then, were designed primarily for testing the hypothesis of PK with the use of flat, rather than cubical, objects.

The data are divided into two parts. The first, Experiment I, totals 2,300 runs and consists of two series in which the subjects worked individually. The total score is insignificant, but the experiment laid the foundation for the one that followed, and contributed to its success. The second part of the data, Experiment II, which contains the significant findings with which most of this paper deals,

[1] For his interest and guidance I am greatly indebted to Dr. J. B. Rhine. His aid and encouragement were indispensable in the carrying-out of these experiments.

is composed of four sections having a total of 675 runs. This experiment is based on the testing of subjects in groups rather than individually.

## Experiment I

In both series comprising Experiment I approximately the same conditions obtained. The first of these series was begun in March, 1944. Most of the subjects were Duke students. In these tests 20 small cardboard squares were thrown through a randomizing chute. Each subject was tested individually, and at each session had one "turn" consisting of 50 throws. A turn filled two rows across the record page, each row having five columns and each column five entries of throw scores. A record sheet contained the data of two

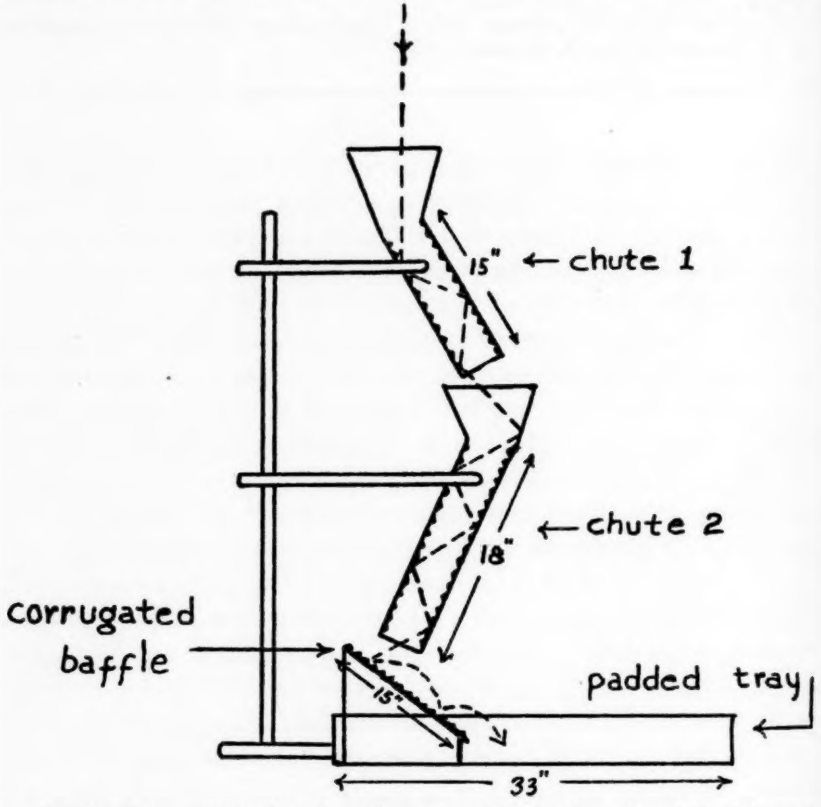



Fig. 1. Apparatus used in Experiment I and in Sections A and B of Experiment II. The broken line suggests the path taken by a card or disc.

complete turns. It was decided to count 20 squares as a standard "run." Every throw, then, equalled a run and each subject did 50 runs or 1,000 single card-throwings in a turn.

The squares used in Series 1 of Experiment I were red on one side and blue on the other, and measured ½ inch on the edge. They were made by glueing together colored sheets of cardboard and cutting them into the size desired. The target color for each turn was at first left to the preference of the subject, but was later determined by the need to equalize the number of throws for reds and blues.

The squares were dropped from a dice cup through two cardboard tubes, one set at an angle above the other and both lined with corrugated cardboard. The squares fell upon a corrugated cardboard baffle from which they bounded into a padded tray. A diagram of the apparatus is given in Figure 1. The experimenter, E.M., counted both the hits and the misses, called them aloud, and recorded them. Then she picked up the squares, put them into the dice cup, and handed them to the subject who again dropped them into the mouth of the uppermost chute.

Chances on a painting were given as prizes for good scores.

*Results*

After the completion of 26 sessions, or 1,300 runs, the records were analyzed. The total number of successes gave a negative deviation from chance of 35, which is insignificant. The results showed also that the squares were biased to favor the blue side, and for the next series a new set of squares was made which appeared to be less biased.

A limit of 1,000 runs was set for the second series (Series 2), which was carried out with the new squares under conditions identical with those obtaining in Series 1. This second series yielded only 17 more successes than chance expectation, which is not significant.

Since a great deal of interest has been shown in the position effects found in hit distributions, the records were analyzed for these effects, the data from the two experiments being pooled for these analyses. Both the horizontal and vertical distributions showed

typical declines from the first to the second halves, but the differences are not significant. The horizontal distribution of hits in the five columns in a set (as shown in total deviation from chance for 460 runs per column) is as follows:

$$\underbrace{+76 \quad -50}_{+26} \quad -7 \quad \underbrace{+27 \quad -64}_{-37}$$

These deviations do not vary significantly from chance; a chi-square test gives a probability of approximately .62. But the CR of the difference between the first and fifth places is 2.06, having a probability of .02, and is suggestive. The vertical distribution for the five entries in a column with 460 runs per entry is:

$$\underbrace{-51 \quad +69}_{+18} \quad -35 \quad \underbrace{+17 \quad -18}_{-1}$$

The chi-square test of these deviations gives a probability of .74; so they do not vary to a significant degree.

When the individual turn or session (two rows of five columns) was taken as a unit and the records for the two series were quartered by vertical and horizontal axes, the quarter distribution (QD) was found to be similar to that which is typical of PK experiments with dice; that is, with the upper left quarter containing the most hits and the lower right the fewest. A less typical but somewhat comparable QD was obtained when the row of five columns (the "set") was quartered. The squares shown below give the distribution in quarters, first of the session and second of the set. Chance expectation is 10.00 hits per run. The number of runs, the total deviation, and the average score for each quarter are given. Underneath is shown the critical ratio of the difference between the first and the fourth quarters. (In the QD analyses for both the session and the set, the middle column in every row had to be omitted; and in that for the set, the middle entry in every column was also omitted in order to equalize the runs in each quarter.)

QD of the Session
1,840 Runs

| | |
|---|---|
| 460<br>+45<br>10.10 | 460<br>-2<br>10.00 |
| 460<br>-19<br>9.96 | 460<br>-35<br>9.92 |

$CR_{d(1-4)} = 1.18$

QD of the Set
1,472 Runs

| | |
|---|---|
| 368<br>+38<br>10.10 | 368<br>-34<br>9.91 |
| 368<br>-5<br>9.99 | 368<br>-6<br>9.98 |

$CR_{d(1-4)} = .72$

## Experiment II

Experiment II is composed of four sections (A, B, C, D) and was conducted between August 22 and September 14, 1944. Several innovations made this experiment quite different from Experiment I.

First of all, the cardboard squares were discarded in favor of plastic discs, ⅞ of an inch in diameter. These were found to be much more satisfactory, since they are rigid and resist the wear and tear of frequent handling. They are also brighter than the squares, and subjects like to work with them better. In Sections A and B the discs were white, with a red center on one side and a blue center on the other. In C and D they were yellow with blue crosses and circles.

The manner of throwing the discs was somewhat different in Experiment II from that in the first experiment. In Sections A and B the stationary chutes of Experiment I were used, but in B a slight addition was made: a flat piece of cardboard was fastened across the mouth of the top chute in such a way that it was easily tripped by pulling a string. The discs were placed on the cardboard, and the subject pulled the string when he was ready. Sections C and D were carried out with the use of a revolving tube made of a length of cardboard tubing about 5½ inches in diameter. It was mounted on a metal rod used as an axis and had numerous wire baffles inside to turn the discs as they fell. One end of the tube was closed. A diagram of this apparatus is shown in Figure 2. After the discs were dropped into the mouth of the tube, they tumbled through the baffles to the closed end where they were allowed to come to a complete stop. Then the subject tipped the

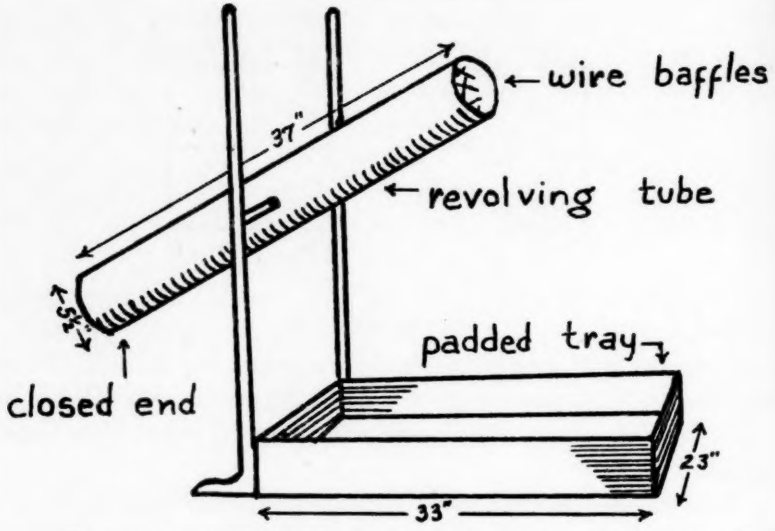



FIG. 2. Apparatus used in Sections C and D of Experiment II.

mouth of the tube downward, and the discs fell out into the padded tray where they were counted.

Ten discs were thrown at a time, and hence two throws made a run. The target face for each turn was determined by a die-throw. Just before the last session (Section D), it was decided that no target face would be used three times in succession. This was done following the observation that after a particular face had been target several times, the subjects tended to prefer it to the other face, as was evidenced by their remarks. It was feared that this fixation might inhibit good scoring on the opposite face.

The record pages were identical with those previously described, but in this experiment the trials of each subject filled out only one set of five columns at a turn rather than two sets as in Experiment I.

Perhaps the most important innovations in Experiment II were the use of individuals of the lower age levels as subjects and the testing of these subjects in groups rather than individually.

In view of the fact that other experimenters, such as L. E. Rhine (**9**), Bond (**1**), Price (**2, 3**), and Reeves and Rhine (**4**), had reported success in ESP work with lower age groups, it was decided to try children and adolescents as subjects in the present

PK experiment. In the first section (A) children from 5 to 11 years old were tested; in Sections B and C, adolescent girls were the subjects; and in Section D, the subjects were young people ranging in age from 16 to 20 years.

The results of previous experiments with the lower age groups have indicated that these subjects score best in a light social atmosphere. In order to provide such a favorable atmosphere, "PK parties" were planned and six or seven individuals were invited to each. Prizes of candy, toys, and movie tickets were given for good scores, and refreshments were served at the end of each session. These attractions, coupled with the fact that the experiments were conducted in the home of some one of the subjects, made the experiments seem more like parties. Each session lasted approximately 2½ hours.

In a typical session the subjects drew slips of paper to determine the order in which they would take their turns. Then each subject, as his turn came, threw the discs, while all the other subjects gathered about the apparatus watching the performance and "helping" mentally. Thus, although each turn was to some extent an individual test, it was also intended to involve action of the group as a whole. The experiment was taken as a game, and the shouts of joy or the groans that arose following a throw indicated clearly whether a score had been good or bad. After each individual had had one turn, the first subject had a second turn, and so on until everyone had had two. In Section A, instead of waiting until the end of the session, refreshments were served at the half-way point, after each subject had had one turn. This was done in an attempt to divert the children who appeared to be getting restless. Their second turns followed the refreshments.

I had throughout the conduct of Experiment II the generous assistance of Miss Betty M. Humphrey of the Parapsychology Laboratory. At every session she was present as observer, and alternated with me in recording and picking up the discs. Her assistance and counsel were a valuable part of the research, and I here gratefully acknowledge my indebtedness to her.

The subjects and experimenters were fairly well acquainted in

most cases, and the latter were able to keep things moving on a party level without sacrificing good experimental conditions.

*Results*

The total results for the four sections, stated separately and in combination, are shown in Table 1. None of the CR's shown in

Table 1

TOTAL RESULTS BY SECTIONS

| Section | Runs | Deviation | Av. Score* | CR |
|---|---|---|---|---|
| A..... | 175 | −34 | 9.81 | 1.15 |
| B..... | 175 | +32 | 10.18 | 1.08 |
| C..... | 150 | +40 | 10.27 | 1.46 |
| D..... | 175 | −13 | 9.93 | .44 |
| Total..... | 675 | +25 | 10.04 | .43 |

*Chance expectation is 10.00.

Table 1 is significant. Likewise the vertical and horizontal distribution analyses yielded no significant results. When the four sections are pooled, the horizontal distribution for the five columns in a set (a single turn) is:

$$\underbrace{0 \quad -11}_{-11} \quad -1 \quad \underbrace{+17 \quad +20}_{+37}$$

each of the five figures representing 135 runs. The vertical distribution within the column is:

$$\underbrace{-27 \quad +57}_{+30} \quad -2 \quad \underbrace{-1 \quad -2}_{-3}$$

with the same number of runs represented by each figure as in the horizontal distribution. As would be expected from these results, the QD of the set is not a typical one, but the deviation of the first quarter is highest, as the square below indicates. There is, then, a diagonal decline, though not a significant one. (The middle column of each set and the middle entry of each column had to be omitted from this analysis.)

QD of the Set
432 Runs

| | |
|---|---|
| 108<br>+18<br>10.17 | 108<br>+13<br>10.12 |
| 108<br>-22<br>9.80 | 108<br>+6<br>10.06 |

$CR_{d(1-4)} = .36$

It was in the analysis for session distribution of hits that the evidence for PK appeared. As previously stated, each subject had two turns. In every session the first turn (for all of the subjects together) had a positive deviation and in every case except one (Section C) the second turn had a negative one. In Section C the deviation for the second turn is barely positive (+3) and is much lower than that for the first turn (+37). The CR of the difference between the first and second turns for all four sections pooled is 3.45, which has a probability of .0003 and is quite significant. These results are shown in Table 2. When the pooled results for all four

Table 2

Comparison of First and Second Turns for the Four Sections

| Section | First Turn | | Second Turn | | $CR_d$ | $\chi^2$ |
|---|---|---|---|---|---|---|
| | Runs | Dev. | Runs | Dev. | | |
| A..... | 87.5 | + 19 | 87.5 | −53 | 2.43 | 5.90 |
| B..... | 87.5 | + 55 | 87.5 | −23 | 2.63 | 6.92 |
| C..... | 75 | + 37 | 75 | + 3 | 1.24 | 1.54 |
| D..... | 87.5 | + 2 | 87.5 | −15 | .57 | .32 |
| Total..... | 337.5 | +113 | 337.5 | −88 | 3.45 | $\Sigma\chi^2 = 14.68$<br>d.f. = 4<br>P = .006 |
| | CR = 2.75 | | CR = (−) 2.14 | | | |

sections are arrived at, the decline effect is very striking. The significant positive deviation on the first turn (with a CR of 2.75) merits particular interest, for it indicates that if each subject had been given only one turn, the total positive results would have compared very well with those reported in dice-throwing experiments. The results of Table 2 are presented graphically in Figure 3.

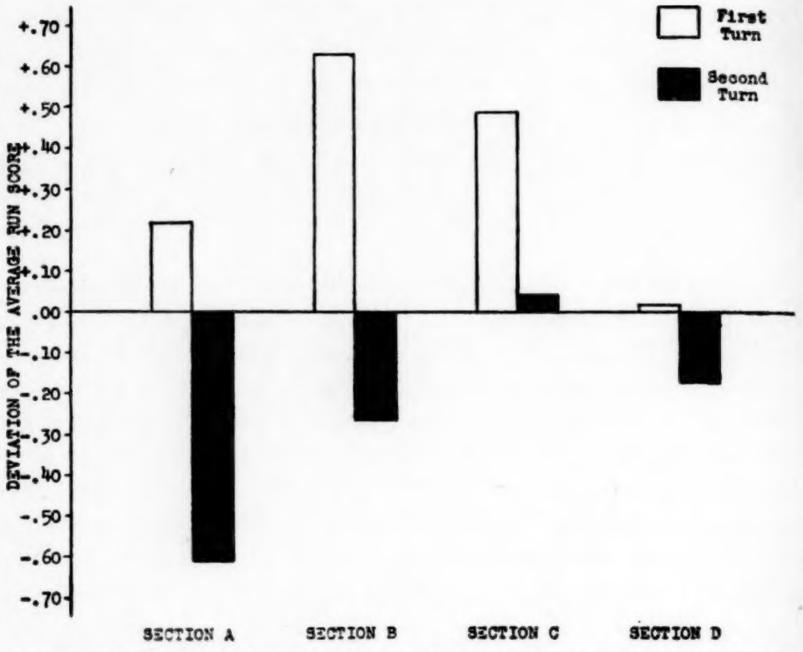



FIG. 3. Comparison of the deviations of the average run scores for the first and second turns for each of the four sections.

*Counterhypotheses*

In order to establish the fact that these results were due to the causal influence of the mind upon the falling discs, several counter-hypotheses must be dealt with.

*Chance.* The hypothesis of chance is immediately disposed of by the CR of the difference of 3.45. Its probability of .0003 is quite significant. (Some correction would be appropriate for the fact that this evaluation is not the only one applied. The corrected value of P = .0009.) The CR for the first turn taken alone is 2.75 with a P of .003. Even when this figure is corrected for selection of one out of two possible sections of data, the resulting P of .006 is still significant.

*Skilled Throwing.* Since in every case the discs were thrown by means of chutes having either corrugated linings or numerous wire baffles, the hypothesis of skilled throwing need not be consid-

ered. The subjects either dropped the discs into the mouth of the chute from a dice cup or pulled a string which released them mechanically so that no manipulation of the discs was possible. Even careful placing of the discs in the chutes could not have influenced the results, for the baffles and corrugations, as well as the further randomization afforded by the discs as they fell against each other in their rapid descent, would have rendered useless any such attempt.

*Faulty Discs.* From an inspection of the data, it was found that according to total deviations the red and cross targets were favored. The results by target faces are:

| Target | Runs | Deviation |
|---|---|---|
| Red | 175 | +14 |
| Blue | 175 | −16 |
| Cross | 162.5 | +58 |
| Circle | 162.5 | −31 |

On the basis of the faulty-disc hypothesis the difference between the first and second turns might be explained by supposing that there were more runs for the favored target in the first turn and more for the unfavored target in the second.

The actual distribution of target faces in the session is given in Table 3. In it are presented the runs and deviations for both target faces in each turn when the four sections are taken separately and when they are pooled according to the discs used. The set of discs having red and blue centers was used in Sections A and B, and that having crosses and circles was used in Sections C and D. Therefore Sections A and B are combined, as are C and D.

The pooled results of this table show that the number of runs for the unfavored targets (blue and circle) either equalled or exceeded that for the favored targets (red and cross) in the first turn. In the second turn the number of runs for the favored targets equalled or surpassed that for the unfavored. Obviously this distribution is directly opposite from that expected on the basis of faulty discs. For all faces the decline from the first to the second turn is remarkably consistent, as is shown in the presentation of the sections individually in Table 3.

Table 3

RESULTS FOR THE DIFFERENT TARGET FACES BY SECTIONS

SECTION A

| Turn | Blue | | Red | |
|---|---|---|---|---|
| | Runs | Dev. | Runs | Dev. |
| 1 | 37.5 | + 4 | 50 | +15 |
| 2 | 50 | −34 | 37.5 | −19 |

SECTION C

| Turn | Cross | | Circle | |
|---|---|---|---|---|
| | Runs | Dev. | Runs | Dev. |
| 1 | 37.5 | +27 | 37.5 | +10 |
| 2 | 37.5 | + 2 | 37.5 | + 1 |

SECTION B

| Turn | Blue | | Red | |
|---|---|---|---|---|
| | Runs | Dev. | Runs | Dev. |
| 1 | 50 | +41 | 37.5 | +14 |
| 2 | 37.5 | −27 | 50 | + 4 |

SECTION D

| Turn | Cross | | Circle | |
|---|---|---|---|---|
| | Runs | Dev. | Runs | Dev. |
| 1 | 37.5 | +16 | 50 | −14 |
| 2 | 50 | +13 | 37.5 | −28 |

SECTIONS A AND B POOLED

| Turn | Blue | | Red | |
|---|---|---|---|---|
| | Runs | Dev. | Runs | Dev. |
| 1 | 87.5 | +45 | 87.5 | +29 |
| 2 | 87.5 | −61 | 87.5 | −15 |

SECTIONS C AND D POOLED

| Turn | Cross | | Circle | |
|---|---|---|---|---|
| | Runs | Dev. | Runs | Dev. |
| 1 | 75 | +43 | 87.5 | − 4 |
| 2 | 87.5 | +15 | 75 | −27 |

*Optional Stopping.* The hypothesis that the experimental work may have been stopped when a point of significance was reached in an otherwise chance series can be rejected on the basis of the results shown in Table 2. The fact is, the results of Experiment II were significant from the end of the first session on. After observing the consistency of the session decline throughout the four sessions, the effect was sufficiently established to warrant varying the procedure so as to throw more light upon the nature of the decline observed.

*Recording and Checking Errors.* The only remaining counter-hypothesis alternative to PK is that of errors in recording, checking, and computing. In the description of procedure it was mentioned

that at every session there were two experimenters present, one of whom called the hits and misses aloud and recorded them while the other observed the proceedings. Besides this observer there were the six or seven subjects who were very interested in the scores and checked on every figure. The counting of misses as well as hits facilitated accuracy in recording and guarded against overlooking hidden or misplaced discs. Mistakes in recording, then, are extremely unlikely. All the data have been rechecked and recomputed without the discovery of any errors.

The counterhypotheses of chance, skilled throwing, faulty discs, optional stopping, and errors in recording have proved insufficient to explain the results. The only remaining explanation is PK.

## Discussion

### *General Remarks*

There is only the coin-spinning work of Thouless to compare with my own results, and in his experiments only one subject, himself, took part. No great amount of generalization, then, is possible. It is of some consequence, however, that the experiments herein reported confirm those of Thouless in the conclusion that the two-sided object, like the cube, is subject to the PK effect. Whether PK operates equally well on two-sided objects and on those having six faces will not be determined until more data are available and a good comparison is made. Such an experimental comparison is one of the scheduled sequels to the present work.

In addition to the relative novelty of using discs, these experiments are of interest because they were carried out with the lower age groups and in a social atmosphere. The group tests in ESP with children reported by L. E. Rhine, Reeves and Rhine, and Price were conducted under conditions similar to those which obtained in the experiments reported in this paper. Bond's tests were made in the classroom situation where less informality was possible, but where the children were nevertheless in a social atmosphere. In *Extra-Sensory Perception After Sixty Years* (**8**, p. 279) it is pointed out that children, being accustomed to playing with each other, are less likely to be inhibited by each other's presence than would be true of adults. This suggestion is borne out by the fact

that Section D of Experiment II, which was carried out with young people aged 16-20, gave the least evidence of PK of the four sections. These young people comprised the oldest age group and had not been as closely associated previous to the party as had the subjects in the other three sections.

*Position Effects*

The evidence for PK in these experiments derives mainly from the consistent decline in scoring from the first to the second turn in each session. This decline effect is quite familiar to readers of the JOURNAL, for it has been a rather consistent characteristic of results of the researches both in PK and in ESP. It has been found in almost every logical subdivision of the data: in the record column, in the set, in the half-set, on the page, and in the experiment as a whole. This effect has been mentioned in individual reports and, with respect to certain subdivisions in the data, has been discussed at length in three surveys of the PK experiments: that of the QD of the page (**5**), the QD of the set (**6**), and the QD of the half-set (**7**).

In the only other PK test using two-sided objects which has been published, the coin-spinning experiment by Thouless, a significant decline was found in the experiment as a whole. It is interesting to note that in both these experiments the evidence for PK is found in the significant decline of scoring. The chronological decline in Thouless' data, while not strictly comparable with the decline found in the session in the disc data, is nevertheless very similar. In his coin-spinning tests Thouless was both the experimenter and the subject, and the entire experiment was probably something of a unit for him, just as the session was for the different groups of subjects in the disc tests of Experiment II.

Further experimentation with two-sided objects is needed in order to discover whether the characteristics found in the data for six-faced objects are generally found here also. Questions concerning the effect of size, density, and number of objects; distance; method of throwing; and the various conditions of the experimental situation are as applicable to the disc tests as to those using dice.

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# PRECOGNITION RECONSIDERED

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In a recent article under the title "Telepathy and Clairvoyance Reconsidered" (9), an attempt was made to reappraise the status of the hypotheses of telepathy and clairvoyance in the light of the evidence of precognition that has accumulated during the last twelve years. As a result of this reconsideration it was concluded that, as things now stand, there is acceptable evidence of clairvoyance, or the ESP of objects, but no conclusive evidence of telepathy. It was pointed out that the evidence hitherto accepted as establishing telepathy could also be adequately explained by the hypothesis of precognitive clairvoyance. It became clear, therefore, that if there is any true telepathic ability, new experiments are needed to isolate and distinguish it from this alternative of precognitive clairvoyance. What is more, it is found to be very difficult to design a crucial experiment that will isolate telepathy. We cannot, of course, tell what may be developed when other experimenters give attention to the problem, but at present no fully satisfactory and decisive test procedure for telepathy is available.

The evidence for clairvoyance was shown to rest upon four types of experiment: the ESP experiments using the matching techniques, the ESP shuffle research, the electrical machine tests, and the PK work. The PK research was presented as affording evidence of clairvoyance since the process of PK depends on the purposive direction of, and hence upon knowledge of, the rolling dice (or other objects) used in the tests. Such knowledge can come to the subject only by way of clairvoyant perception.

## Possible Types of Precognition

In the course of the reconsideration of the evidence of telepathy and clairvoyance it was necessary to give some thought also to the nature of precognition (Pcg) itself, especially since it was a form of the Pcg hypothesis that created the present issues concerning telepathy and clairvoyance. Frequent use was made of the hypotheses of *precognitive telepathy* (Pcg-T) and *precognitive clairvoyance*

(Pcg-C), but without either one having been experimentally isolated and independently established. They were, however, relatively familiar concepts of Pcg. But another and more novel type of Pcg came into the discussion as a hypothetical possibility, one that, so far as I know, had not been discussed before, although it is simple enough and is a possible explanation for some of the experimental results obtained in the tests for telepathy and clairvoyance. This is *precognitive sensory perception* (Pcg-SP). By this is meant a percipient's advance experience of his own later sensory perception—a perception which he is going to have some time in the future—a sort of "memory in reverse." If, for example, one foresees with significant accuracy the order of cards in a deck which he is going to see later on, there is clearly the distinct possibility that he is mentally going ahead, not to the cards themselves in an extrasensorial fashion, but to his own visual experience. This would be the most plausible route to expect him to follow if it could be done. No ESP, therefore, need be involved, though of course in the case just mentioned, it would be a possible alternative.

The often reported experience of *déjà vu* (or "seen before") would suggest Pcg-SP. In a typical experience of this kind, a man, let us say, enters his stateroom for his first ocean voyage. He exclaims at once that he has seen the room before in every detail; yet he has seen no photographs of it and has never before been aboard a ship. Such experiences are hardly ever conclusively interpretable, but some of them do suggest Pcg-SP. Perhaps there had been a previsionary dream, forgotten yet resident enough in latent memory to produce an experience of recognition. J. W. Dunne deals with precognitive dreams in his book, *An Experiment with Time,* but he skirts the issue of whether the Pcg he assumes to occur is sensory or extrasensory. He evokes a hypothesis of superdimensional advantage of the dream mind over ordinary cognitive apprehension.

It must be recognized, moreover, that if Pcg of *any* process of the mind is possible, there is no known reason why it may not be possible likewise with *all*—with future moods, future dreams, future reasoning, future hallucinations, or with any other mental state. It was doubtless the fact that research in Pcg grew directly out of the ESP tests that led those who have been working experimentally with Pcg to think of it only as another form of ESP and thus in-

separably linked up with telepathy and clairvoyance. It seemed logical enough at the time to assume that if it was the same ESP that functioned over short distances and long, it was the same process that made possible ESP of present events and cognition of future ones.

As it was, therefore, no question was raised in our thinking as to whether Pcg was anything else than precognitive ESP (Pcg-ESP) until these recent discussions over Pcg in relation to telepathy and clairvoyance arose. But now that the hypothesis of Pcg-SP has come up as a problem, it has to be considered as a possibility where the conditions do not specifically preclude it, and its bearing on the evidence must be examined. It becomes a counter-hypothesis not only for telepathy and clairvoyance, but for Pcg-ESP as well. In short, we shall have to take into account from now on any form of precognitive perception, sensory or extrasensory.

Once these hypothetical conceptions of Pcg are presented, the next step is to find out what kind of experiment will be required in order to provide a crucial test of each type of hypothesis. What will it take to prove the independent existence of precognitive sensory perception, precognitive telepathy, and precognitive clairvoyance? Once these requirements are made clear, we can turn to the consideration of the evidence already on hand that may bear upon these hypotheses, and to new experiments if this proves to be inadequate.

## Precognitive ESP Isolated

It would appear best to undertake first of all to distinguish experimentally between Pcg-SP and Pcg-ESP (the latter, of course, representing both precognitive telepathy and precognitive clairvoyance, provided they both occur). If this step is successful, it will *then* be in order to look into the problem of the identification of precognitive telepathy and precognitive clairvoyance as distinct phenomena.

The general procedure for testing Pcg-ESP so as to exclude Pcg-SP is relatively easy. One of the ways of doing it is as follows: Instruct the subject to attempt to identify a specified series of items (for example, ESP card symbols) in the order in which they are going to be at a given future time. Between the time of making the predictions and the specified time for the check-up, have

the cards shuffled in a random fashion, one that is designed to prevent the act of shuffling from being in any way mentally influenced so as to make the *cards* match the *predictions*. That is, the cards must be shuffled in such a manner that they cannot be influenced by the operation of PK.

When the list of predictions is checked against the randomized card order, *neither the card order nor any record of it is to be observed (visually or auditorially) by the subject.* Anyone else may see them, but if the possibility of Pcg-SP is to be excluded, the subject himself should not be allowed any sensory perception of the symbols. If the scoring is significantly high and the above conditions are all met, the proper explanation then must be that the subject has not only gone ahead in time but has either perceived the card order directly by precognitive clairvoyance or has foreseen by precognitive telepathy the symbols which were later perceived by the experimenter as he observed the card order in the act of recording and checking.

Readers who are familiar with Pcg methods will readily see that it is therefore not difficult to modify Pcg tests to exclude Pcg-SP. In fact, some of the series of Pcg tests that have already been reported have incidentally had the necessary conditions for ruling it out. And among them are some of the best; that is, those which have gone furthest in eliminating the various other counterhypotheses that have to be considered and met.

For example, in the following series of tests based on the prediction of card order, the subjects at no time saw either the order of the cards or any record of them:

| *Year of Publication and Reference No.* | | *Identifying Condition* | *Experimenters* |
|---|---|---|---|
| 1940 | (5) | Comparison of one- and ten-day precognition | Hutchinson |
| 1941 | (3) | Comparison of precognition with four ESP tests[1] | Humphrey and Pratt |
| 1942 | (8) | First comparison of two- and ten-day precognition | Rhine |
| 1942 | (4) | Second comparison of two- and ten-day precognition | Humphrey and Rhine |

[1] In terms of the total deviation obtained these tests were not significant as originally reported, but a later study revealed a significant quarter distribution of hits on the record page (2).

There is another series that belongs in this group, one not derived from card tests. This is Tyrrell's Pcg experiment reported in 1938 (13) in which he used an electrical machine with a mechanical selector. While the subject knew with each trial whether he was right or wrong in his choice of one of five boxes, he did not know which of the other four boxes was the target if he missed. Actually he had to make his choice *before* he could exercise even this limited Pcg-SP, and this fact would prevent its use in directing his judgment.

It is reasonably certain, to judge from the reports, that in the following series also the subjects did not see their target objects (drawings and cards) or any record or reproduction thereof, unless in minor instances which could not account for the results.

| *Year of Publication and Reference No.* | *Identifying Condition* | *Experimenters* |
|---|---|---|
| 1940 (1) | Precognitive displacement in GESP test with drawings | W. Carington |
| 1943 (11) | Precognitive telepathy as displacement effect | Soal and Goldney |

In certain other Pcg experiments, on the other hand, a point was made of getting the subject to observe the card order during the checking procedure as an additional precaution against error and as a help in maintaining active interest in the test. This was the case with the following researches:

| *Year of Publication and Reference No.* | *Identifying Condition* | *Experimenters* |
|---|---|---|
| 1941 (12) | Precognitive card matching | Stuart |
| 1941 (7) | Effect of rewards on precognitive card matching | Rhine |
| 1942 (14) | Prediction of fall of dice | Woodruff and Rhine |

In summing up the case for the independence of Pcg-ESP from Pcg-SP, it can be said that the evidence is the best there is on the problem of Pcg. While, therefore, we will keep the Pcg-SP hypothesis before us and while it represents in itself a theoretical possibility, there is at least no need to question further the conclusion that Pcg-ESP is firmly established in so far as the exclusion of Pcg-SP as a counterhypothesis is concerned. And what is equally important, if more evidence on the point is desirable, clear-cut methods are available for the further investigation of Pcg-ESP.

## The Problem of Isolating Precognitive Sensory Perception

When we turn to the question of methods for testing the hypothesis of Pcg-SP, it is quite a different matter. This is because it is not so easy experimentally to exclude the possibility of Pcg-ESP as it is that of Pcg-SP. It may be that we shall find no direct experimental way to do so.

The trouble is that when a subject attempts to predict what he will sensorially perceive at some future time, there is no way to prevent the stimuli from being perceived by Pcg-ESP too. The objective stimuli have to be there if SP is to function, and they are thus fully available to ESP.

The only way to try for Pcg-SP that is yet known is to use an *indirect procedure* such as the following: Instruct the subject to proceed as in the test described above for Pcg-ESP, until a "level" or average score is obtained for comparison. Then explain that *in addition* he will now be allowed to look at the cards during the check-up, and assure him that since SP is so much more efficient than ESP, he can now rely upon precognizing his own visual experience of the cards instead of relying upon ESP. It will obviously be best to use naïve subjects for such tests in order that attitudes may be more dependably controlled.

If there is marked improvement in the test results, naturally considerable importance can be attached to it, though with reservations. Then a more elaborate study will be justified to discover whether or not effects peculiar to SP make their appearance (the conditions allowing for both SP and ESP effects). For instance, in visual SP the size of the stimulus object, the degree of illumination, the barriers interposed, and other variations have very marked effects on success, but they have not been found to affect ESP so far as they have been tested. If the experiment showed that when the cards were clearly visible to the subject during the check-up, the results were much better than when he was hardly able to distinguish them, that would argue for Pcg-SP. (There would be need of a control, permitting only Pcg-ESP to operate, in order to show that for ESP, variation in the physical conditions of the cards made no difference.)

This suggested procedure may serve as an illustration of the general way in which the hypothesis of Pcg-SP might be brought to test in the course of a series of experiments.

Nothing of the kind has ever been done, and I know of no evidential basis for commenting on whether or not Pcg-SP occurs. There is, I recognize, little or no scientific value to speculative thinking from a priori grounds as to whether we ought to expect to find Pcg-SP; such thinking too often becomes an easy substitute for experiment and may thus be a barrier to active scientific exploration. But it is human to venture opinions, and they often challenge other students to disprove them by experiment. With this introduction, then, I venture to suggest that there probably is no Pcg-SP. If there were, to cite one of the arguments that have influenced my own judgment, there should have been better results in the three series of tests listed above in which the subjects saw their own cards in the check-up. Indeed, if Pcg-SP could be counted on to show anything like the ratio of efficiency to Pcg-ESP that SP bears to ESP (and it seems not unreasonable to think it should), then Pcg should be so effective as to be a familiar everyday phenomenon to psychology, and no problem for *para*psychology.

The problem is one for research. Whether Pcg occurs apart from ESP (for example, as Pcg-SP) or, as we have for years supposed, is an exclusive function of ESP itself (merely the ESP of future events), is a question of great eventual importance to psychology. For it is one thing if the mind in general is capable of Pcg, if the transcending of time is a fundamental property of all mental life, and quite another if it belongs only to the special capacity of ESP which, alone among mental processes, has been found to transcend space. We need not speculate far along these lines to appreciate the need to clear up the question of Pcg-SP experimentally once and for all for what it may mean in the final understanding of the relation of the mind to the space-time universe.

### The Problem of Distinguishing between Precognitive Clairvoyance and Precognitive Telepathy

It is a fair question to ask whether there is any point in attempting just now to push further toward an experimental distinction

between precognitive telepathy (Pcg-T) and precognitive clairvoyance (Pcg-C). There is assuredly enough work in prospect on the problem of discovering whether there is *any* true telepathy of any kind, or whether ESP is *entirely* clairvoyant in character, and that problem would certainly appear to come first. If it eventually becomes clear that there is a genuine phenomenon of telepathy, then and only then, will it be time to inquire if it is also precognitive.

Moreover, until a better technique for investigating telepathy is developed than those so far thought of, we cannot hope to make a clear experimental test of *precognitive* telepathy. In fact, there is at present no adequate procedure in sight that could provide a crucial verification of Pcg-T. It would almost appear that this phenomenon is forever beyond reach by direct experimental attack; for if one mind predicts what another is to be thinking of later, what is to prevent the later "thinker" from simply getting the predicted thought by telepathy and duplicating it? There seems to be no direct way to control this, and unless there is, no crucial proof of Pcg-T is possible. We shall apparently have to resort to indirect methods in attempting to close in on that hypothesis if the time comes when it is the center of research interest.

On the other hand, it is possible to test Pcg-C to the exclusion of Pcg-T in a quite conclusive manner. As a test of Pcg-C the following outline of procedure is proposed: Let the subject choose one of five symbols, for instance, and press an appropriate button on a mechanical selector to register this choice. The machine will select a symbol by a complex random procedure and will illuminate (or otherwise indicate) the selected symbol in a closed chamber within the apparatus. No record of the particular selection will be made, and no one will ever know *which* symbol was thus selected. If the same symbol is chosen by the subject as is selected by the machine, a hit will be registered. In any case, a trial will be recorded. If significantly high scores are tallied, the result should be attributable to Pcg-C. This assumes that the machine is truly random in its selection and of course works as it is intended to do.

Tyrrell's machine (13) meets the requirements for this test sufficiently that the significant results obtained with it which he reported in 1938 may be regarded as evidence of Pcg-C. The machine

tallies only the number of hits and the total number of trials. The fact that the subject opens the box and keeps it open until the machine makes the selection and lights the box (if the correct box is chosen) might appear at first thought to allow Pcg-SP to guide the subject in his choice; but as has already been said above, this possibility is precluded by the very fact that the subject has to make his choice before Pcg-SP can be exercised. The reasoning would be quite different if the subject were allowed to open up all of the boxes after each trial. Then his Pcg of which was right and which was wrong would enable him to make his choice on the basis of the hypothetical Pcg-SP. But, as stated, his choice precedes the possibility of the exercise of Pcg-SP. Pcg-T, too, is ruled out, for there is no other observer of the selection made by the machine.

To my knowledge, no other unmistakably Pcg-C work has been done as yet. But with clairvoyance an established capacity, and with telepathy far from being so, one is naturally driven to suspect that the evidence of Pcg-ESP now on record was probably contributed by Pcg-C. We cannot and need not say that Pcg-T did not in any degree enter into the production of the Pcg-ESP results. Much will depend on whether "pure telepathy" is established in future research on that problem. It is entirely feasible, however, to deal with Pcg-C experimentally, keeping it independent of the problem of telepathy and Pcg-T. That would be a ready point of approach.

## General Comments

It cannot be emphasized too strongly that this discussion of Pcg and the one in the preceding article on the types of ESP do not in any way alter the general status of either Pcg or ESP. Whether ESP is all telepathic, all clairvoyant, or partly both, it is still ESP! Whether Pcg is Pcg-T, Pcg-C, or even Pcg-SP or any combination of these, it is still Pcg! *The total weight of evidence for ESP and for Pcg are exactly the same as before.*

How strong is the case for Pcg? This question is relevant here, for the answer will to some extent determine how seriously we take the various issues that have been raised in this paper. We cannot rightly be concerned about the explanation of Pcg and the forms

that it may take until we are reasonably certain that the phenomenon occurs.

The early Pcg experiments (**6**) carried out here at Duke University with the use of the preshuffle card-calling technique gave significant results; but the conclusions were set aside when the ESP shuffle work (**10**) showed a possibility that ESP might have aided in the shuffling so as to make the deck of cards match the predictions without benefit of Pcg. In a series of experiments that followed, the cards were shuffled by machine. In these experiments, by Hutchinson (**5**), Stuart (**12**), Humphrey and Pratt (**3**),[2] and myself (**7**), it was found that the subjects gave significant scores when attempting to predict the order of cards to be found in the deck after mechanical randomization.

The results seemed to be conclusive. Indeed, the case for Pcg was regarded as established until we went so far as to entertain the counterhypothesis that somebody (subject or experimenter) might be influencing the fall of the cards inside the mechanical shuffler itself, utilizing the direct action of the mind over a moving physical body. We had, at the time, strong evidence of PK in the dice-throwing tests. It was necessary, then, to go a step or two further in the direction of designing a more adequately controlled Pcg experiment. Accordingly, Miss Humphrey and I planned a series of experiments in which the subject attempted to predict the order of cards as they would be after being shuffled and cut mechanically, the number of shuffles and the point of cutting being determined by printed temperature reports in a specified newspaper for a specified day in the future. It was believed that this would at least reduce the force of the counterhypothesis of PK sufficiently to put it out of consideration. Two experimental series conducted on this plan gave significant results (**4**, **8**). There the matter stands so far as the Pcg investigation at Duke University has developed.[3]

Tyrrell, in London, described tests of Pcg made with his electrical ESP apparatus (**13**) which we have already described. We have either to accept Tyrrell's conclusion that Pcg was demonstrated

[2] See footnote 1.

[3] There had also been a series of die-prediction tests by Woodruff and Rhine (**14**) giving results explainable only by Pcg or PK or both. The Pcg hypothesis seemed definitely favored, but this particular case was not conclusive.

or to suppose that the subject either clairvoyantly interpreted the selector mechanism or psychokinetically influenced it to select the box which was chosen and opened Both of these alternatives seem pretty weak, but so too at first do the ESP shuffle and PK hypotheses when offered as counterexplanations in the card-Pcg tests mentioned above. It is hard to decide whether a counterhypothesis is reasonable until a control test is made for the purpose.

Whately Carington's discovery of displacement in the forward direction in his GESP tests with drawings gave results that look like Pcg. Subjects were found to score successfully on the drawing just ahead of, or on the one coming just after, the actual target drawing; and yet the drawing to come after the target was not selected until the next day.

There may be a flaw here in the fact that the scoring depends upon the judge who decides whether a subject's reproduction of a drawing is a hit. His judgment of hits involves delicate discrimination and might be so influenced by his own ESP ability that it affected the results. If this were so, no assumption of Pcg by the subject would be necessary to explain the forward displacement. The point is that if the judge exercises any ESP ability in making his judgments, he may be the one who thereby introduces the higher scoring in the forward displacement.

Perhaps this is not so reasonable an alternative as another that arises when we consider how Carington selected his original drawings. He opened a book of mathematical tables at random, selected certain digits by rule, turned to the corresponding page of Webster's Dictionary, and took the first reasonably drawable word. Now in the opening of a book at random in the course of a selection of a drawing against which the reproductions of the subjects of the previous day are to be matched (for displacement), it is conceivable that Carington's ESP ability might have played a part. That is, he may have been able thereby to select a drawing that matched the subject's productions. It would be, it is true, a complicated act of ESP, to combine the numbers of the book to be opened with the page in Webster's and to link these with ESP of the prevailing trends or types of drawings already made by the various subjects. It is asking a lot to offer this as a counterhypothesis. No one could

be driven to consider it, perhaps, if the alternative were not that of the incredible Pcg hypothesis itself. Mechanical selection of the numbers would have ruled out this counterhypothesis.

The Soal and Goldney experiments on Pcg-T are too complicated for brief review. In general, the technique was GESP. Five cards were used, one of the five being selected as the target for each trial. It was observed that the subject succeeded in hitting the next card after the target before it was looked at by the sender, and even before it was selected. With these advance hits significantly above chance, the case looked like Pcg. In some of the experiments the agent selected the card to be looked at by choosing a colored chip from a bag. This seemed to randomize the selection of the card so that the subject could get no knowledge of which one was to be looked at next except with the aid of Pcg. It seems most reasonable to think that the Soal and Goldney results offer evidence of Pcg, but there *are* counterhypotheses which are about as plausible as the ones offered against the other Pcg researches. It is possible, for example, that the experimenter selecting the chip which determined the choice of the target card was merely matching the prediction already made by the subject over in the other room. Ordinary ESP ability *without Pcg* would permit this. There are other possibilities, too, which hinge on the fact that the selection of the target by the experimenter was a free one instead of a fixed or mechanical one. In order to satisfy the logical requirements for a Pcg test, the determination of the event predicted will have to be beyond human influence (and in calculating the extent of this human influence, the parapsychological hypotheses themselves have to be taken seriously). But, as was previously pointed out, it may not be possible to demonstrate Pcg-T crucially even if it be a real phenomenon.

It is possible, however, to say in summary that some of the Pcg research meets all of the extreme critical requirements we have been able to level against it. Since it does, the relative value of the other work is much enhanced; for as the increasing weight renders Pcg a more plausible hypothesis, the force of the counterhypotheses discussed above is much reduced.

Obviously such a hypothesis as that of Pcg is going to remain

a challenging one for a long time to come. Much more work will be done, and for most people more evidence will be needed. But those who have followed through these investigations from their beginning twelve years ago find it difficult to see any logical escape from a verdict favorable to Pcg. More than that, Pcg is a logical deduction from the now well-established fact that ESP is not spatially limited. At any rate, the research justifies the direction of our efforts from now on toward discovery of the nature of the process. The same results that accomplish this end will incidentally serve also the purpose of strengthening the general case for Pcg itself.

So completely incomprehensible to the modern scientific mind is this concept of Pcg that a great many "reconsiderations" will doubtless be necessary in the evolution of an understanding of it. To our present way of thinking, Pcg simply does not make sense. How, for example, can the *effect* (that is, the perception) of an event precede the *cause* (that is, the predicted event)? How can a person be stimulated by objective stimulus patterns not yet in existence? How can volitional choice be free, if events are predictable? How can we predict events and then, if there is true volitional freedom, alter our choice of action in the light of the precognition?

These questions remind us sharply that if we had to explain a fact before we accepted it, there would be little advancement possible in science. Rather, it is just such puzzles, such "unexplainable" phenomena, that offer the greatest challenge to the spirited inquirer. We know, for one thing, that the more difficult the explanation of a phenomenon, the greater the amount of discovery which is necessary for its ultimate understanding. By this standard, the very incomprehensibility of Pcg is a measure of the magnitude of its scientific importance as a discovery. It must indeed be, like those strange, inexplicable sparks of electricity in the eighteenth century, the clue to a great realm of realities silently awaiting the arrival of the explorer.

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# AN ESP EXPERIMENT WITH ENCLOSED DRAWINGS

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**ABSTRACT: The free response method is here applied to tests of clairvoyant ESP. Drawings of unspecified objects were sealed in opaque envelopes. Unselected college-student subjects made free drawings on response sheets clipped to the face of the envelopes. The response drawings were ranked by independent judges as to their similarity to or association with the stimulus drawings. No statistically significant relationship was found between the responses and the stimuli they were intended to match. But when the responses were matched to the just-previous stimulus, significant positive scores showed a consistent relationship. The phenomenon indicated is that of "displacement" in which the subject's response identifies the wrong stimulus.—Ed.**

MOST of the exploratory work with the free response method that has been reported at intervals in this JOURNAL has followed the guide of traditional beliefs about the problem of "telepathy." The stimulus situation has consisted of a person looking at a drawing or picture, and the respondent has been instructed to draw whatever came to mind during the time the stimulus picture was being looked at. Such a method is an undifferentiated ESP test inasmuch as hypothetically a veridical response may be telepathy or clairvoyance or a combination of both.

The present report is concerned with an application of the free response method to experiments in ESP of the clairvoyant type. The stimuli were mounted drawings individually sealed in opaque envelopes in such a way that the sealed envelope constituted the drawing board for the response. The responses were observed by student experimenters who were themselves unaware of the nature of the stimulus drawings. The order of the stimuli was unknown to anyone until the end of the experiment.

While the attempt to identify clairvoyantly the contents of a

[1] The research was carried out while the writer held the Thomas Welton Stanford Fellowship in Psychical Research at Stanford University. This report has been approved by the Psychical Research Committee and is Communication No. 14 from the Psychical Research Laboratory.

sealed envelope is a device as old as psychical research, the soundest precedent for use of the method experimentally is the work reported by Rhine (2) of the use of enclosed stimulus cards in ESP card-calling tests. The total work of those experiments gave an average score of 5.19 successes per 25 ESP cards called for 4,971 runs. This yielded a highly significant CR of 6.7. Rhine broke down the data into three divisions: runs in which the scores were immediately checked and available to the subject, runs in which there was a brief delay in checking, and runs in which there was a delay in checking of several days. The average run scores were 5.69, 5.07, and 4.88, the long-delay average being significantly below chance.

The method used here necessarily comes into the long-delay class. Only four of the 96 subjects participating saw the results of the check-up at all. So, while it is not safe to generalize card-guessing test results to cover the free response method, there would be some expectation that significant results might well be in a negative direction.

## Method

The four stimulus pictures for each experiment were drawn (or, in one instance, pasted) on 8″ × 5″ sheets of heavy white paper. These were then stapled between two 9″ × 6″ sheets of white cardboard. Each shielded drawing was placed in a heavy manila mailing envelope 7½″ × 10½″ in such a way that the flap and seams of the envelope represented the facing-upright position of the stimulus inside. These envelopes were securely sealed and shuffled until the identification of any one as containing a particular drawing would be simply a guess. Two edges of each envelope were then folded back about a quarter of an inch so that the envelope would fit into another of the same size. The inner envelope was placed upside down and reversed in the outer envelope, which faced the stimulus picture inside toward the blank, unseamed side of the outer envelope. A spring clip was placed at the top of each so that the four envelopes became, in effect, sturdy clip-boards. The outer envelopes were lettered on the backs A, B, C, and D, respectively, the letters having, of course, no rational relation to the enclosed stimulus pictures.

Blank response sheets were cut to size so that when a sheet was slipped under the spring clip it just covered the face of the envelope.

*Experimenters*

The four student experimenters who participated at different times in this experiment were not responsible for the design of their assigned projects, nor can they be responsible for this summary report; but in every other sense they were collaborators, and deserve more than footnote acknowledgment of assistance. They were Miss Joan Whitmore, Miss Jean Burns, Miss Margery LaMotte, all senior students in the Department of Psychology, and Mr. Lynn Zimmerman, a student in the Graduate School of Business of Stanford University. The four experimental series, which they undertook as part of their work in a reading and research course in Psychical Research, are identified here as Series W, B, L, and Z respectively.

*Procedure*

The stimulus pictures for the various experiments were selected arbitrarily, but with the intention of making them mutually distinct in both form and meaning. They were, for Series W: a Box (a soap box roughly and somewhat obscurely drawn in pencil), a Ball (a ball with the figure 8 on it drawn clearly in ink), a Dog (a clear outline-drawing in ink), and a Tree (a schematic outline of a conifer tree drawn in ink). For Series B the stimuli were: Flags (a colored pattern made up of various flags, cut from a magazine illustration), Kayo (a colored copy of the comic strip character), a Gun (an ink-drawing of a revolver), and a Bell (a schematic outline-drawing in ink). For Series L the stimuli were: a Cup (an outline ink-drawing of a teacup and saucer), a Bayonet (an ink-drawing), a Die (an ink-drawing of a cube with spots indicated), and a Girl (a colored cartoon of a girl in a bathing suit). For Series Z the stimuli were the same as those for Series B except that a pencil-drawing of a flying Bird was substituted for the Bell.

The procedure was sufficiently uniform in the various series that a description of one will represent all, with such exceptions as are noted at the end of this section. The experimenter took the four prepared envelopes and record sheets to his university living quar-

ters and sought out subjects among his friends and neighbors. The subject was given one of the envelopes (at random) with the attached response sheet. He was told that there was a drawing or picture of some kind in the envelope and that he was to draw something on the record sheet. The experimenter explained that the object of the drawing was to record whatever salient idea or ideas came to mind as he held the envelope, and not to test drawing skill. The subject was allowed to take as much or as little time as he wished. After the first drawing was finished, he was handed another of the four envelopes and was instructed to proceed as before with this new drawing. Each subject made four such responses, and 24 subjects served in each series. The subjects were all undergraduates at Stanford University. Eight were men and 88 were women.

The experimenter coded each drawing (usually by naming it for some salient item, such as "Horse," "Boat," etc.) and kept a record of the order of presentation of the A, B, C, and D stimulus envelopes and of the coded responses.

When the responses of all 24 subjects were secured, the experimenter returned the stimulus envelopes to my office and left them there. He retained all other records. After careful inspection to note that the sealing of the envelopes was intact and in good order, I removed the stimulus drawings and for the first time made a record of which stimuli were in envelopes A, B, C, and D respectively. This record I placed in my files.

The stimulus pictures were now returned to the experimenter in random order and without any indication of the true order on them. He matched the responses and stimuli by the preferential matching method (7). With this method every possible stimulus-response pair is ranked in two ways: For the first matching the four stimulus pictures are ranked in 1, 2, 3, 4 order to each response. The stimulus which has the most similarity to, or association with, the response is given first rank, and the stimulus which appears least associated with the response is given fourth rank. For the second matching the four responses of each subject are ranked in 1, 2, 3, 4 order with respect to each stimulus. The stimulus pictures and responses were later returned to me, and I matched the material independently. All the data and matching records were then brought

FIG. 1. Responses of subject Leary in Series Z. The upper four pictures are the stimulus pictures in the order presented. The lower four pictures are the subject's responses in order. Each response was drawn on a response sheet clipped to the face of a sealed envelope containing the stimulus.

together in my office where, for the first time, we learned which responses were made to which stimulus drawings.

The ways in which the individual experiments varied from this procedure were as follows: (1) In Series L the experimenter failed to keep a record of the *order* of presentation of the stimuli. (2) In Series Z the experimenter found it more convenient to have a room assigned in the laboratory and to make regular appointments with the subjects. (3) In Series Z the order of presentation was systematically permuted instead of being a shuffled order. (4) In Series W the experimenter and I made only one matching of the responses. In order to get scores on this material comparable to the other series, the responses were later matched by a third independent judge, Miss Elizabeth McMahan of the Parapsychology Laboratory at Duke University.

## Results

The accompanying cuts present a brief illustration of subject performance. Figure 1 shows the stimuli of Series Z and the responses of Mr. Leary as subject, the only set of responses to get a perfect score. The responses are typical as drawings, but the incongruous details introduced are unusual. The somewhat superficial similarities that would guide the judge to give a high rank to the true correspondences are apparent on brief inspection. The first stimulus and response pair have the common element of a human face. The second pair both have a crescent moon. The third pair are metallic objects. The fourth response has a bird as one of the details. One of the most unusual features of these responses is their dissimilarity to any but the correct stimulus. Aside from the "boy-bicycle" association, it is difficult to find similarities among the noncorresponding pairs. The matching record is illustrated in Figure 2.

Figure 3 shows a somewhat more typical set of responses from Series W that illustrates the type of ambiguity the preferential matching method is designed to deal with; namely, that a response may well be similar to more than one stimulus picture. The first response, a house, is easily associated with all four stimulus pictures. It is box-like like the Box; one of the adjoining trees looks much like the outside lines of the number on the 8-Ball; the peaked roof and

order presented. The lower four pictures are the subject's responses in order. Each response was drawn on a response sheet clipped to the face of a sealed envelope containing the stimulus.

Series Z Subject: Leary

JUDGE: C.E.S.

| First Matching | | | | | | Second Matching | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|
| Stimulus | | C | D | B | A | Stimulus | | C | D | B | A |
| Response | I | **4** | 1 | 2 | 3 | Response | I | **4** | 1 | 2 | 3 |
| " | II | 1 | **4** | 2 | 3 | " | II | 2 | **4** | 3 | 2 |
| " | III | 2 | 3 | **4** | 1 | " | III | 3 | 2 | **4** | 1 |
| " | IV | 2 | 3 | 1 | **4** | " | IV | 1 | 3 | 1 | **4** |

JUDGE: L.Z.

| First Matching | | | | | | Second Matching | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|
| Stimulus | | C | D | B | A | Stimulus | | C | D | B | A |
| Response | I | **4** | 1 | 2 | 3 | Response | I | **4** | 1 | 3 | 3 |
| " | II | 2 | **4** | 3 | 1 | " | II | 2 | **4** | 2 | 1 |
| " | III | 2 | 3 | **4** | 1 | " | III | 3 | 2 | **4** | 2 |
| " | IV | 1 | 3 | 2 | **4** | " | IV | 1 | 3 | 1 | **4** |

FIG. 2. An example of a matching record for one subject after it has been transposed from a random order to the true order of stimuli and responses, and the ranks converted to weights. In this case the best possible score resulted, since in all four matchings the true correspondences were weighted "4" for each response. The material judged is shown in Figure 1.

the trees match it easily with the Tree, and the idea association of house and Dog is a common one. But to a reasonable judge ranking the stimulus pictures the Box and the Tree would be favored over the Ball and Dog. The rabbit presents a similar problem of judgment. In idea it is probably associated most closely with the Dog, but its formal similarity to the number on the 8-Ball is striking enough to emphasize that relationship as well. The mean rank of these responses was 1.313, as compared to a chance mean rank of 2.500.

*Matching Results*

In previous reports of preferential matching results the measures treated statistically were the ranks of 1, 2, 3, or 4 given by the judges to the correspondences. The "mean rank" was the basic unit for comparison. While this procedure has kept the statistics close to the observations they represent, it involves a difficulty inherent in the use of rank scores—namely, that the numerically lowest ranks designate the highest amount of the attribute being ranked.

FIG. 3. Responses of subject Haight in Series W. The upper four pictures are the stimulus pictures in the order presented. The lower four pictures are the subject's responses in order.

With this procedure a rank of 1 designates *more* similarity than a rank of 4. A mean rank of 2.3 represents *more* relationship than a mean rank of 2.7. A significant positive score shows a significant *absence* of relationship.

To avoid this awkwardness in this and successive reports, the measure tested statistically will be the complementary *weight* of the rank instead of the recorded rank. The weight is the opposite of the rank (precisely, *wt.* = 5 − rank); so that this change amounts simply to the substitution of a weight of 4 for each rank of 1, a weight of 3 for each rank of 2, and so on. The distribution of weights is precisely symmetrical with that of the ranks and the same theoretical constants apply. The change of unit results merely in aligning the direction of score deviations with the direction of the judgments they represent. The greater the amount of similarity in the material judged, the larger is the weight score.

Table 1

STATISTICS OF THE WEIGHTS ASSIGNED BY THE JUDGES TO THE TRUE CORRESPONDENCES BETWEEN RESPONSES AND STIMULI

| Experiment | No. of Responses | Mean Weight | No. of Subjects | Mean Score | SD | CR | 2P |
|---|---|---|---|---|---|---|---|
| Series W.... | 96 | 2.474 | 24 | 39.583 | 6.089 | .33 | .74 |
| Series B.... | 96 | 2.315 | 24 | 37.042 | 8.801 | 1.61 | .11 |
| Series L.... | 96 | 2.492 | 24 | 39.875 | 6.313 | .09 | .93 |
| Series Z..... | 96 | 2.555 | 24 | 40.875 | 7.424 | .57 | .57 |
| Total..... | 384 | 2.459 | 96 | 39.344 | 7.357 | .87 | .38 |
| "Chance"... | ... | 2.500 | .. | 40.000 | 7.071 | ... | ... |

The mean weight of the true correspondences of all 384 responses was 2.459, as compared to the chance expectation of 2.500. The negative difference is not significant. With the subject performance as a unit (that is, with the sum of four matchings of the four responses by each subject constituting a unit weight score), the average score per subject was 39.344 as compared to a chance expectation of 40.000. The negative difference yields a critical ratio of .87, having a probability of .38.

Within the individual series there was, likewise, no evidence of any but chance performance. Series B showed some indication of

negative relationship in the true correspondences, but its CR of 1.61 is not significant. The specific results are arrayed in Table 1.

Thus, despite the occurrences of such persuasive correspondences as those of Figure 1, the statistical evidence showed nothing but a chance relationship between stimuli and responses.

*Displacement*

Evaluation of the true correspondences is a test of one hypothesis: that of consistent relation between the stimulus and the response *intended for it* by the respondent. However, there is no theoretical ground for supposing that this intent is necessary for ESP (that is, that the respondent identifies the intended target or nothing), and there are many observations of apparent "displacement" to support the contrary view, that the subject frequently identifies the wrong target. Carington, in his work with free drawings, observed a significant number of displaced hits on the stimuli just before and just after the intended stimulus (**1**). The outstanding case of displacement is that reported by S. G. Soal, in which the tendency of his subject, B. S., to identify the succeeding card rather than the presented one was so consistent that experiments later designed to make use of the tendency were spectacularly successful (**5, 6**).

In the present work two simple hypotheses of displacement were tested: the supposition that these subjects consistently responded to the just-previous stimulus (the envelope that had been turned over to the experimenter), and the supposition that they consistently responded to the next stimulus (the envelope that was next to be turned over to them).

The evaluation of displaced correspondences is carried out exactly as that for the true correspondences. Displacement to the just-previous response is tested by noting the weights given Response II when compared to Stimulus 1, Response III when compared to Stimulus 2, and Response IV when compared to Stimulus 3. Displacement to the just-following response is tested by noting the weights given Response I when compared to Stimulus 2, Response II when compared to Stimulus 3, and Response III when compared to Stimulus 4. For the backward displacement all first responses are left out of consideration, and for the forward displacement all fourth responses are dropped. But since the stimuli

were presented in random order, the remaining responses are a statistically proper sample.

Because of the absence of a record of the order of responses in Series L, this series cannot be studied for displacement.

The results of the other three series are summarized in Tables 2 and 3. The mean weight upon the hypothesis of backward displacement is 2.685. The critical ratio of subject performance scores is 2.95, which is significant. The different series vary greatly in their contribution to this result. Series W gives virtually chance scores, and Series B, with a mean weight of 2.872, is highly significant alone.

The mean weight upon the hypothesis of forward displacement is 2.484, which is not significantly different from chance expectation. The contributions of the individual series do not vary significantly.

Table 2

STATISTICS OF THE WEIGHTS ASSIGNED BY THE JUDGES TO THE CORRESPONDENCES BETWEEN RESPONSES AND THE JUST-PREVIOUS STIMULUS (BACKWARD DISPLACEMENT)

| Experiment | No. of Responses | Mean Weight | No. of Subjects | Mean Score | SD | CR | 2P |
|---|---|---|---|---|---|---|---|
| Series W.... | 72 | 2.535 | 24 | 30.417 | 7.047 | .34 | .73 |
| Series B.... | 72 | 2.872 | 24 | 34.458 | 6.191 | 3.45 | < .001 |
| Series Z..... | 72 | 2.649 | 24 | 31.792 | 4.950 | 1.74 | .08 |
| Total..... | 216 | 2.685 | 72 | 32.222 | 6.349 | 2.95 | .003 |
| "Chance"... | ... | 2.500 | .. | 30.000 | 6.124 | .... | ... |

The backward displacement is significant even when the probability is multiplied by three to correct for the fact that three hypotheses (direct correspondence, backward displacement, and forward displacement) are tested.[2]

[2] The probabilities as given in the tables have been multiplied by two to take care of the fact that, in this exploratory work, the deviation from chance expectation might be in either direction. The task is to find evidence of consistent relationship. But the hypothesis of displacement, at least to date, presupposes that the stimulus shows consistent similarities or positive associations with the displaced response. That is, a significant avoidance of an unintended stimulus could be considered displacement only by a somewhat academic logic. So the probability associated with the summarized results is really the probability of getting a positive mean weight with a CR of 2.95 when three hypotheses are tested. This probability is .005.

Table 3

STATISTICS OF THE WEIGHTS ASSIGNED BY THE JUDGES TO THE CORRESPONDENCES BETWEEN RESPONSES AND THE JUST-FOLLOWING STIMULUS (FORWARD DISPLACEMENT)

| Experiment | No. of Responses | Mean Weight | No. of Subjects | Mean Score | SD | CR | 2P |
|---|---|---|---|---|---|---|---|
| Series W.... | 72 | 2.575 | 24 | 30.917 | 5.859 | .75 | .45 |
| Series B.... | 72 | 2.514 | 24 | 30.167 | 6.256 | .13 | .90 |
| Series Z..... | 72 | 2.361 | 24 | 28.333 | 5.273 | 1.52 | .13 |
| Total..... | 216 | 2.484 | 72 | 29.806 | 5.918 | .28 | .78 |
| "Chance"... | ... | 2.500 | .. | ..... | 6.124 | ... | ... |

*Secondary Questions*

Two secondary questions are pertinent for comparison of these findings with other ESP research. The first is that of stimulus effectiveness. Do some stimulus pictures appear consistently to produce more recognizably related responses than others? If ESP is analogous to sensory perception, an easily recognized ink drawing of a dog should be more readily perceived than an obscure pencil drawing of a soap-box. Furthermore, insofar as cognition is dependent upon motivation, a human figure should be more readily identified than a billiard ball.

The second question is that of position effect. Is the first response of the subject the best? Is there a decline in performance through the four trials, or is there some "learning"?

The stimulus and position effects may be investigated in the scores of the direct correspondences and the scores of the backward displacement; in the former because it is the response made at the time of the stimulus presentation that is conventionally expected to show ESP effects, and the latter because the significant results of these experiments occurred in the backward displacement distribution.

*Stimulus Effect*

The individual stimuli used and their mean weights are listed in Table 4. The mean weight scores of the direct correspondences vary from 2.833, for the Ball in Series W, to 2.021 for the Bell in Series B. Even if we attribute all the variation to the differences

among the stimuli, the range of CR's from 1.72 to −2.47 shows no individually significant deviation of the mean weights from the chance mean of 2.500. Nor is favoring of any obvious category of stimulus evident. Representations of living things, for example, which would include Dog, Kayo, Girl, and Bird, average 2.563. Colored stimuli—Flags, Kayo, and Girl—average 2.475. Other categories are as negligible.

An analysis of variance gives a more precise test of the variability that may properly be attributed to the stimulus differences within each experimental series. The analysis of variance yields estimates of the population variance from: (1) the variation among classes, and (2) the variation within classes (the classes in this instance being the responses to individual stimuli). The statistic $w$ is a ratio found by dividing the first estimate by the second. Tables

Table 4

STIMULUS EFFECT: SCORES OF THE INDIVIDUAL STIMULI ACCORDING TO THE DIRECT CORRESPONDENCES AND THE DISPLACED BACKWARD CORRESPONDENCES

| Stimuli | Direct | | | Displaced Back | | |
|---|---|---|---|---|---|---|
| | No. of Responses | Mean Weight | CR | No. of Responses | Mean Weight | CR |
| Series W | | | | | | |
| Ball | 24 | 2.833 | 1.72 | 17 | 2.059 | −2.01 |
| Box | 24 | 2.437 | − .33 | 17 | 2.529 | .13 |
| Dog | 24 | 2.354 | − .75 | 19 | 2.684 | .89 |
| Tree | 24 | 2.208 | −1.51 | 19 | 3.000 | 2.41 |
| Series B | | | | | | |
| Flags | 24 | 2.583 | .43 | 21 | 2.905 | 2.05 |
| Kayo | 24 | 2.354 | − .75 | 17 | 3.029 | 2.41 |
| Gun | 24 | 2.271 | −1.18 | 18 | 3.056 | 2.61 |
| Bell | 24 | 2.021 | −2.47 | 16 | 2.687 | .83 |
| Series L | | | | | | |
| Girl | 24 | 2.667 | .86 | .. | ..... | .... |
| Bayonet | 24 | 2.562 | .32 | .. | ..... | .... |
| Cup | 24 | 2.479 | − .10 | .. | ..... | .... |
| Die | 24 | 2.354 | − .75 | .. | ..... | .... |
| Series Z | | | | | | |
| Gun | 24 | 2.771 | 1.14 | 18 | 2.778 | 1.07 |
| Bird | 24 | 2.667 | .86 | 18 | 2.722 | 1.04 |
| Flags | 24 | 2.479 | − .11 | 18 | 2.639 | .65 |
| Kayo | 24 | 2.292 | −1.07 | 18 | 2.500 | .00 |

are available to test whether $w$ is significantly different from chance, if the number of degrees of freedom of the two estimates is known.[3]

Analysis of variance tests of the variation attributable to differences of effect among the stimuli in each experimental series give: for Series W, $w = 1.85$; for Series B, $w = 1.34$; for Series L, $w = .49$; and for Series Z, $w = 1.11$. Each of these $w$'s has three degrees of freedom in the numerator and 96 degrees of freedom in the denominator. Since a $w$ of 4.00 is necessary for significance, there is no evidence of variation in scoring among the direct correspondences attributable to differences in effectiveness of the stimuli.

Table 4 lists also the mean weights of the stimuli as matched on the hypothesis of backward displacement. Except for Ball in Series W with a mean weight of 2.059, the mean weights range fairly regularly from 2.500 to 3.056. Again there seems no favoring of the stimuli upon any principle.

The analysis of variance tests give for Series W, $w = 3.46$; for Series B, $w = .53$; and for Series Z, $w = .25$. Each of these $w$'s has three degrees of freedom in the numerator and 68 degrees of freedom in the denominator. Since a $w$ of 4.08 is necessary for significance, there is no evidence of variation in scoring among the displaced correspondences attributable to differences in effectiveness of the stimuli.

*Position Effect*

Since there are but four responses by each subject to stimuli presented in a random or systematically permuted order, any consistent difference of performance attributable to the position of the response in the series of four should be readily shown by comparison of the mean weight scores for the four positions.

For the direct correspondences the mean weights were: for the first response, 2.493; the second response, 2.559; the third response, 2.323; and the fourth response, 2.417. The estimated standard deviation is .107 for each of these scores. Their deviations from a total mean weight of 2.448 are therefore negligibly different from chance.

An analysis of variance test reveals this absence of position

[3] Rider P. R. *Modern Statistical Methods*, p. 133.

effect more precisely. With a $w$ of 3.85 necessary for significance, the observed $w$ was .93 with three and 284 degrees of freedom. There is, therefore, no evidence of consistent variation in score with respect to the order of response.

The displacement-back scores yield even less variability with respect to position. The first response does not have a score. The mean weights of the remaining responses as matched with the just-previous stimulus are: second response, 2.677; third response, 2.681; and fourth response, 2.698. These scores are so nearly identical that it is evident to inspection that the differences are negligible.

An analysis of variance test gives $w = .01$ (an almost significantly small value, which, however, merely reflects the fact that the mean weights of the three positions are astonishingly similar). There is again no evidence of significant variability in scoring attributable to position effect among the displacement scores.

## Discussion

The only significant statistical finding of this research is the phenomenon of consistent similarity between the responses and the just-previous stimulus. Upon the ESP hypothesis this is a displacement phenomenon. The response of the subject is affected primarily by the stimulus he has returned to the experimenter, either because some feature of the experiment makes this stimulus attention-demanding beyond the one being worked on, or because some latency factor operates to slow up the effect of the stimulus.

What are the alternative explanations? The possibility of sensory cues was ruled out by adequate enclosure of the stimuli, and in any case sensory cues could not account for the latency of response. Gross errors of recording or procedure could have no consistent effect, since responsibility was so divided that no knowledge of the order of the data was available to any one person until all records were complete. The possibility that subjects might have a natural order of performance that would accidentally coincide with the stimulus order was made ineffective by presentation of the stimuli in varied orders. The statistical methods are conventional.

One somewhat complex alternative hypothesis may be expressed as follows: Series B is the only individually significant series, and without it the total would not be significant. Series B differed from

Series Z in that in Series B the stimuli were presented to the subjects in shuffled order, while in Series Z the stimuli were systematically permuted in order. Might not the difference be due to some fluke of randomization in the stimulus presentations in Series B?[4]

Suppose, for example, that the subjects had a general tendency to draw human figures on their *second* response. Then suppose that inadequate randomization in shuffling accidentally presented the stimulus Kayo a disproportionate number of times as the *first* stimulus. This would look like displacement but would be merely a coincidence of unknown probability.

The most direct test of this question may be set up by sampling the backward displacement data of Series B in such a way that the four stimuli occur in each position with equal frequency. The sample would constitute virtually the same data as would have resulted if the stimuli had been systematically permuted instead of shuffled. If inadequate randomization accounts for the significance of Series B, then this sample should have a mean weight close to chance.

Fortunately the randomization of Series B was adequate to provide a large sample. The stimuli A, B, C, and D occurred with frequency 6, 8, 4, 6 in the first position; 8, 5, 5, 6 in the second position; and 7, 4, 7, 6 in the third position. Since no stimulus occurred fewer than four times in any position in the series, the sample was made up by taking the first four scores for each stimulus in each of the three positions (the order of selection being determined by the alphabetical order of subjects' names). It finally consisted of 48 of the 72 individual stimulus-response scores of the backward displacement data of Series B.

The mean weight of the equal-frequency sample was 2.963. This is negligibly different from the total mean weight of 2.872. It has a CR of 3.46 and a probability of .002; so it may not be considered close to chance. The evidence is thus clear that faulty randomization cannot account for the difference between Series B and Series Z.

A counterhypothesis that presents much difficulty is an alternative ESP hypothesis intrinsic in the preferential matching method. This is the possibility that the judge uses ESP in his ranking. Since unambiguous responses like those of Figure 1 are rare, his uncon-

[4] This counterhypothesis has been suggested by the Stanford Psychical Research Committee.

scious ESP knowledge of the true order may cause ambiguities such as those of Figure 3 to be ranked favorably. For example, in Figure 3 when it is known that the rabbit corresponds to the 8-Ball, the similarity of form easily justifies a first choice for this pair. But suppose the rabbit had actually corresponded with the Dog. Then the idea-association would be salient and would obviously justify a first choice for this pair. Since such "knowledge" by ESP is theoretically possible, there is no sound refutation of this hypothesis. The major argument against it is that the significant results were those of displaced correspondences, rather than the true correspondences. Supposing that the judge were to employ ESP, there is nothing in either the motivational or cognitive aspects of the situation to guide such a systematic ESP error.

That the subjects' free responses show evidence of a displaced ESP remains the only adequate explanation of these results.

The absence of evidence of stimulus or position differences is, perhaps, significant in another sense, at least, for ESP research procedure. If, as is implied, the nature of the stimulus makes no difference in clairvoyant ESP, the conventional card-guessing techniques should be much superior as method to the free response technique, other things being equal. The old belief that the stimulus itself must be interesting or impelling to be effective is not substantiated.

The lack of position effect in the displaced scoring poses a curious problem. The absence of stimulus effects upon displacement showed the displacement to be positional only. The responses matched the just-previous stimulus, whatever its nature. But the fact that the means for all three positions were virtually identical suggests that the relation is more physical than psychological. Psychologically there would be an expected difference between the response to a first stimulus and the response to the third of a series. But if the displacement was the result of something similar to a physiological latency, no position effect would be expected, and the displacement would be a simple time function. (This is the view suggested by the Soal and Goldney results.) No time measurements were made in the present work, however, so the lead cannot be followed here.

The absence of significant relation between the true correspond-

ences remains to be explained, but such explanation had better wait for more evidence. If the "chance" result is to be questioned, it must be on the grounds that some performances were significantly better than chance and some were significantly poorer than chance. The latter possibility can be entertained with more assurance if there is evidence from the free response method of significant negative scores indicating avoidance of the stimulus by the respondent analogous to significant negative deviations in card-guessing experiments. An example of this was presented in a previous article (**8**), but what appears to be a more conclusive case will be presented in a later report.

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# MINOR ARTICLES AND NOTES

[Under this heading will occasionally appear briefer publications having value and interest for our readers but being in some respects less complete or less conclusive than our major articles.—*Ed.*]

## POSITION EFFECTS IN THE SIX-BY-SIX SERIES OF PK TESTS

By J. B. RHINE and BETTY M. HUMPHREY

**ABSTRACT: A group of PK test series having a common experimental procedure are summarized here in a single report. In all tests six dice were thrown by cup with the six-face as the target. This face and this number per throw are most generally favored among subjects. Significantly high scores were obtained over the total of 186,624 die throws, but there was no control series to deal with the question of biased dice. It was found on analysis, however, that the QD, or quarter distribution of hits on the record page, showed a significant decline from the upper left to the lower right quarter. This QD is typical of the comparable PK work reported.—Ed.**

IN THE FIRST two papers in which we reported the survey studies made on the quarter distributions (or QD's) of the hits on the record page, we included a series known as the Six-by-Six Series, referring to it as "awaiting publication." The present note presents the essential data of the Six-by-Six Series, rounding out the information given in the QD papers. There is little more to be emphasized in this paper than the finding of the quarter distributions themselves. The importance of the position effects represented in the QD's is so great, however, that we believe a significant or even a suggestive QD pattern is ample justification for the presentation of a separate report.

It will be recalled that the more familiar or typical QD pattern observed in the PK records is one which shows the largest number of hits in the upper left-hand, or first, quarter of the page, and the smallest number in the lower right-hand, or fourth, quarter. The difference in score average between the first and fourth quarters has been found to be a significant one for the pooled QD for all the

researches to which this analysis has been applied (1). This drop from the first to the fourth quarter, referred to as the "diagonal decline," was the result of the presence of both a horizontal and a vertical decline—that is, a falling-off in rate of scoring from left to right on the record page, as well as from top to bottom. With these two declines, we are sure to find a diagonal decline as well.

## Procedure

There are actually a number of series combined in this Six-by-Six Series—or, rather, a number of groupings of data. There was no single, definite experimental plan for the various groupings. Some of the work represents unfinished experiments. Thus the Six-by-Six Series is a collection of all the available records of tests in which six dice were thrown with the six-face as the sole target in experiments conducted at the Parapsychology Laboratory at Duke. This excludes completed experiments separately reported.

A number of the research associates of the Parapsychology Laboratory were involved, principally J. L. Woodruff and Margaret Pegram (now Mrs. Reeves). They were assisted by Seymour Van Wiemokly and 23 others who, along with the three named, served as subjects in throwing the dice. A great deal of the work was done merely to achieve personal satisfaction and to discover whether better-than-chance scores could be produced.

From December 1, 1936, to September 22, 1937, the subjects made 31,104 throws, six dice at a time, or the equivalent of 7,776 standard runs of 24 die throws each. The six common commercial white dice which were used measured 11/16 of an inch. They were thrown from a cylindrical cardboard box after being shaken, and were bounced on a special dice table with a padded top and sidewalls.

All of the tests were carried out in the presence of an observer who recorded the throw scores, calling them aloud as he recorded them in order to allow the subject to check the accuracy of the reading. Each column was totaled as it was completed.

The record sheets had ten columns with 24 spaces in each column. It was customary to pause at the middle of the page—that is, after five columns had been recorded—and summarize the scor-

ing done up to that point. There was, however, no other break in the page at the midway point, and the pause at the end of the fifth column was only slightly longer than any other, since it was necessary only to total the scores for that column. Later it was considered a possibility that the half-page might be a somewhat distinct "set" or subdivision of the record page, and when the study of the QD's progressed to the point of application to the set as well as the page, this series was included in the set analysis.

## Results

The total of 7,776 runs gave 33,645 hits, which is 2,541 above expectation. This is not, however, an unusually high scoring rate as the PK tests go, the average being 4.33, where 4.00 is expected; but with this large number of trials, the critical ratio is very high, being 15.79. Unfortunately, the plans for a control series to deal with the question of faulty dice was not carried out, and there is simply no way to tell whether the CR of 15.79 is due to the PK effect or to faulty dice.

However, for the QD analysis, or for any study of position effects, there is no need to concern ourselves with the question of whether the dice are faulty. All we need to be sure of is that the same dice were used throughout the area in which the hits are compared and that the dice were all thrown for the same target face. Since these conditions were observed, the Six-by-Six records make suitable data for distribution study.

When the record page for all the Six-by-Six results is divided into four quarters by a horizontal and a vertical axis and the numbers of hits are obtained, a typical QD pattern for the record page is found, with the highest rate of scoring in the first quarter, and the lowest rate of scoring in the fourth. The figures are given in the square below, showing first the number of runs represented in each quarter, then the total positive deviation, and under that the run score average.

Another and still more important feature of this QD of the Six-by-Six page is that the 1-4 difference is significant, giving a CR of the difference of 2.42. (The actual score difference is .1489, and the standard deviation of the difference is ± .0615. This CR is equivalent to a probability of .008.)

7,440 Runs

| | |
|---|---|
| 2286<br>+833<br>4,364 | 1434<br>+433<br>4,702 |
| 2286<br>+796<br>4,348 | 1434<br>+309<br>4,216 |

$CR_{d(1-4)} = 2.42$

As it happens in this instance, the CR of 2.42 is a much more meaningful figure than the CR of the total deviation of 15.79 because the smaller CR represents a difference that cannot be accounted for by any of the familiar counterhypotheses, whereas the larger one could have been the result of imperfect dice which favored the six-face.

There seems to be little indication that the structure of the half-page affected the subjects as did that for the page as a whole, for when the half-page is broken down into quarters and the QD of the half-page obtained, the QD pattern is neither typical nor significant. It will not be reproduced here since it has already been shown in our paper on the QD of the set (2).

A marked but not significant chronological decline is also found in the data. This is obtained by taking the work of each subject separately and subdividing it into three chronological series, as nearly equal as possible. The usual rules are followed by which the record of a session which falls upon the point of equal division of the data is added to the earlier period. In Table 1, it will be seen that the scoring rate in the three periods dropped from 4.37 to 4.35 and then to 4.27.

Table 1

CHRONOLOGICAL DISTRIBUTION OF HITS

| Chronological Period | Runs | Deviation | Av. Run Score |
|---|---|---|---|
| First | 1878 | +696 | 4.37 |
| Second | 1734 | +613 | 4.35 |
| Third | 1542 | +424 | 4.27 |

$CR_{d(1-3)} = 1.59$

## Discussion

While the more familiar alternatives to the PK hypothesis are plainly to be dismissed as inapplicable to these data, there is a possible combination of counterhypotheses that is less easily met than any single one alone. If the dice were biased in favor of the target face and were thrown more vigorously at the beginning of the page, the bias would have a greater chance to be effective, and thus more hits would be obtained in the first portion of the page. Then as the test progressed and the subject became more fatigued or bored, the vigor of the throwing would gradually subside and there would be inadequate opportunity for the bias to be effective. Therefore fewer hits would occur in the latter part of the page. In some such manner it might be supposed that a typical QD with a diagonal decline could be produced.

As we have explained earlier (4), this combination of counterhypotheses is not applicable to the more important series in the QD analyses; for example, six of the eight series in the most recent QD survey (3) are easily shown to be entirely exempt. No data obtained by mechanical throwing or release of the dice are touched by this combination of possibilities. Likewise all the experiments in which equal numbers of throws are made for all the faces are safe, as well as those in which the records for all faces show diagonal declines in the QD's, since the patterns produced by a bias in favor of one face would be canceled when the other faces were targets.

The Six-by-Six Series, however, has no specific evidence which rules out this combination hypothesis. But since it is the only series thus far encountered in which that is the case, since its QD's are so consistent from subdivision to subdivision (see Table 2), and since the series is in line with the others both in type of QD and in score averages, it seems more reasonable to regard its QD's as attributable to the PK effect—as we have had to do unequivocally in the other researches which more adequately met all the counterhypotheses. The discussion will therefore continue on the assumption that PK has produced the significant position effects reported.

On the character of the QD's of the page given in this analysis there is little to add here that has not been said in more general terms in the papers presenting the QD surveys. There is something,

however, to be said for the consistency of this QD pattern. First of all, it may be noted by looking back at the QD square, that there is both a vertical and a horizontal decline. Another characteristic is the internal consistency of these QD patterns when the results are broken down into the more natural subdivisions of the total series, as presented in Table 2. First are shown the QD's for the page for the three main subjects, J.L.W., M.P., and S.V.W. Next come two incomplete experiments with other subjects, which are identified as "Incomplete No. 1" and "Incomplete No. 2," and finally "Miscellaneous." Thus there is a total of six subdivisions in the Six-by-Six Series with no duplication. Each one represents a grouping for convenience made in advance of the analyses.

Table 2

DEVIATIONS OF THE AVERAGE RUN SCORE FOR EACH QUARTER IN THE QD'S OF THE PAGE FOR THE SIX GROUPINGS OF THE DATA

Subject: J.L.W.

| | |
|---|---|
| .38 | .31 |
| .42 | .24 |

Subject: M.P.

| | |
|---|---|
| .38 | .29 |
| .34 | .32 |

Subject: S.V.W.

| | |
|---|---|
| .44 | .31 |
| .42 | .02 |

Incomplete No. 1

| | |
|---|---|
| .24 | .34 |
| .26 | .15 |

Incomplete No. 2

| | |
|---|---|
| .40 | .19 |
| .44 | .24 |

Miscellaneous

| | |
|---|---|
| .42 | .39 |
| .28 | .18 |

The striking consistency with which the diagonal declines appear in these patterns is more impressive than the CR of the difference of the total pooled QD.

Whatever the determining factor may be which has produced the distribution presented in this paper, and however confusingly it is intermingled with a complex of other factors—some of which make up what is called "chance," some associated with the inevitably imperfect dice, and some with the subjects' eccentricities in handling

the dice—there still stands out in this, as in most other PK researches, this dominant patterning tendency in which the scoring falls off downward and to the right, giving a diagonal decline in the QD pattern. This tendency of the scoring rate to fall is the best clue to the character of the PK effect that has yet been obtained. What does the clue identify in the PK process? What does it tell us psychologically about the nature of the phenomenon? These are questions to which we must now set ourselves.

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# GLOSSARY

In order to avoid constant redefining of commonly recurring terms in papers appearing in this JOURNAL, the following definitions are submitted for convenient reference. Words defined elsewhere in the glossary are italicized in the text of the definitions.

*For a simple description of those terms marked by an asterisk, as they apply to the ESP test data, see Chapter VIII and the Appendix of *A Handbook for Testing Extra-Sensory Perception* by C. E. Stuart and J. G. Pratt. A mimeographed copy of the relevant pages will be sent on request to subscribers who do not have the book mentioned. Further explanation may be found in any elementary statistical text.

AGENT: In tests for *telepathy*, the person whose mental states are to be apprehended by the *percipient*. In *GESP* tests, the person who looks at the *stimulus object*.

AVERAGE SCORE: Average number of *hits* per *run*.

BM (BLIND MATCHING): The technique in which the *subject matches* a *deck* of *ESP cards* to five *key cards* which are laid out face-down before him in an unknown order. Unless otherwise stated, the order is also unknown to the experimenter.

BT (BEFORE TOUCHING): The technique in which the top card of the face-down *deck* is *called* and, after being called, is laid aside for *checking* at the end of the *run*. Each card in the *deck* is treated in the same way.

CALL v.: To attempt to identify a *target* or *stimulus object* (or mental state of an *agent* in *telepathy*).

CALL n.: The *response* described above; also the resulting selection.

CHANCE:* The complex of undefined causal factors irrelevant to the purpose at hand.

CHANCE EXPECTATION = MEAN CHANCE EXPECTATION: The most likely *score* if only *chance* obtains.

CHANCE AVERAGE: *Mean chance expectation* in terms of *average* per *run*.

CHECK: To determine a *score* after the completion of a *run* by comparing the order of the *subject's calls* with the order of cards in the deck.

CHI-SQUARE: A sum of quantities each of which is a *deviation* squared divided by an expected value. Also a sum of the squares of *CR's*.

(Occasionally the square of a simple CR may be used as chi-square.)

CLAIRVOYANCE: *Extrasensory perception* of objective events as distinguished from *telepathic* perception (of the mental or subjective events of another person).

COVARIATION: Correlation evaluated in terms of theoretical means and *standard deviations.*

CR (CRITICAL RATIO):* A measure to determine whether or not the observed *deviation* is *significantly* greater than the expected random fluctuation about the *average.* The CR is obtained by dividing the observed *deviation* by the *standard deviation.* (The *probability* of a given CR may be obtained by consulting tables of the probability integral, such as Pearson's.)

CR OF THE DIFFERENCE: The observed difference between the *score averages* of two samples of data divided by the *standard deviation of the difference.* (Where the samples to be compared are of equal number of runs, the difference between total *hits* may be divided by the *SD* of the total number of *runs* of both samples.)

DECK: Twenty-five *ESP cards,* five of each suit.

DEVIATION:* The amount an observed number of *hits* or an *average score* varies from the *mean chance expectation* or *chance average.* A *deviation* may be total (for a series of *runs*) or average (per *run*).

DIE THROW: The throwing or mechanical release of a single die regardless of the number thrown at the same time.

DT (DOWN THROUGH): The technique in which the cards are *called* down through the *deck* before any are removed or *checked.*

EMPIRICAL CONTROL: An experiment which wholly or partially follows the main experiment with the exception that the conditions are designed to exclude the possibility of *ESP.*

ESP (EXTRASENSORY PERCEPTION): Response to an external event (perception) not presented to any known sense.

ESP CARDS: Cards, each bearing one of the following five symbols: star, circle, three parallel wavy lines (called "waves"), square, plus.

ESP SYMBOLS: See plate opposite page 1, this JOURNAL, Vol. 1, March, 1937.

ESP TESTS: A considerable number of techniques come under this heading which are conveniently represented by initials, the principal ones being: *BT, DT, PT, GESP, BM, OM, STM.*

EXPECTATION; see CHANCE.

EXTRACHANCE: Not due to *chance* alone.

FREE MATERIAL: *Stimulus objects* that are not limited to a known number of categories.

GESP (GENERAL EXTRASENSORY PERCEPTION): A technique designed to test the occurrence of *extrasensory perception,* permitting either *telepathy* or *clairvoyance* or both to operate.

HIGH-DICE TESTS: Tests of *PK* in which the aim of the *subject* is to try to influence a pair of dice to fall with the two upper faces totaling eight or above.

HIT: The correct correspondence of a *subject's call* or response with a *stimulus card* or *object.*

HIT FREQUENCY DISTRIBUTION: The grouping of the total *hits* in a *series* of *runs* with respect to their original position in the *run.*

KEY CARD: One of the five cards (where there are five suits) against which the cards of the test *deck* (i.e., *target cards*) in the *matching* tests (*OM, BM, STM,* etc.) are *matched.*

LOW-DICE TESTS: Tests of *PK* in which the aim of the *subject* is to try to influence a pair of dice to fall with the two upper faces totaling six or below.

MATCHING: A form of *calling* in which a *target card* is placed opposite the *key card* which the *subject* selects to identify it. Also, in the evaluation of *free material,* the act of a judge in identifying a given *response* with a *stimulus object.*

MEAN CHANCE EXPECTATION; see CHANCE.

OM (OPEN MATCHING): The technique in which a *subject matches* a *deck* of *ESP cards* to five *key cards* which are face-up before him.

P (PROBABILITY):* A mathematical estimate of the expected relative frequency of a given event if chance alone were operative.

PARAPSYCHOLOGY: A division of psychology dealing with the paranormal—those psychical effects which appear not to fall within the scope of what is at present normal and recognized law.

PERCIPIENT: The person who makes the *calls* in a test situation.

PK (PSYCHOKINESIS): The direct influence exerted on a physical system by a *subject* without any known intermediate energy or instrumentation.

RESPONSE: The act of the *subject* in attempting to identify the *stimulus object.*

RSR (RUN SALIENCE RATIO): A measure of *salience* within the *run.*

RUN: A succession of *trials,* usually the *calling* of a *deck* of 25 *ESP cards* or symbols. In *PK tests,* 24 single *die throws* regardless of the number of dice thrown at the same time.

SALIENCE: The relation of rate of success in the end *segments* of the *run* to that of the middle *segments;* also the relation of the rate of success in the end *trials* of the *segment* to that of the middle *trials.* TERMINAL SALIENCE: A higher rate of *deviation* in the end *segments* of the *run* (or in the end *trials* of the *segment*) than in the middle *segments* (or *trials*).

MIDDLE SALIENCE: A higher rate of *deviation* in the middle *segments* of the *run* (or in the middle *trials* of the *segment*) than in the end *segments* (or *trials*).

SCORE: The number of hits made in one *run.*
TOTAL SCORE: *Score* of any number of *runs.*
AVERAGE SCORE: *Total score* divided by number of *runs.*

SCREEN: An opaque barrier used between the *subject* and the card or *agent.* The main types of screens are illustrated in this JOURNAL on their first introduction in print.

SD (STANDARD DEVIATION):* The theoretical root mean square of the *deviations.* It is obtained from the formula $\sqrt{npq}$, in which $n$ is the number of single *trials,* $p$ the *probability* of success per *trial,* and $q$ the *probability* of failure. (For *ESP cards,* $SD = 2\sqrt{\text{no. of } runs.}$)
SD OF THE DIFFERENCE: For both *ESP cards* and *PK tests* using dice, the *SD* of the difference is equal to $\sigma_s \sqrt{1/R_1 + 1/R_2}$ where $\sigma_s$ is the *SD* of a single *run* and $R_1$ and $R_2$ are the number of *runs* in the respective samples compared. This gives the *SD* of the difference for *run score averages.*

SEGMENT: One of the five consecutive sets of five *calls* in a *run* of 25 *trials.* The first five *calls* would constitute the first *segment;* the second five, the second, etc.

SERIES: Several *runs* that are grouped in accordance with a stated principle.

SEVENS TESTS: Tests of *PK* in which the aim of the *subject* is to try to influence a pair of dice to fall with the two upper faces totaling seven.

SIGNIFICANCE:* A numerical result is significant when it equals or surpasses some criterion of degree of chance improbability. Common criteria are: a probability value of .01 or less, or a *deviation* in the expected direction such that the *critical ratio* is 2.33 or greater.

SINGLES TESTS: Tests of *PK* in which the aim of the *subject* is to try to influence dice to fall with a specified face up.

SR (SALIENCE RATIO): A measure of the relation of the rate of success in the end *segments* of the *run* (or in the end *trials* of the *segment*) and that of the middle *segments* (or *trials*). (For details of the manner of obtaining SR's, see Vol. 5, pp. 193-195.)

SSR (SEGMENTAL SALIENCE RATIO): A measure of *salience* within the *segments* of the *run.*

STIMULUS OBJECT: The *ESP card* or drawing or other object, some identifying characteristic of which is to be apprehended by the *subject.*

STM (SCREENED TOUCH MATCHING): The technique in which the *subject* makes his *call* by pointing to one of five positions or exposed *symbols* under a special *screen*. The experimenter places the *target card* so designated in the position pointed to. The *screen* blocks all vision by the *subject* of the *cards* and their manipulation by the experimenter.

SUBJECT: The person who is experimented upon. Most commonly the *percipient* in *ESP*, though also the *agent* in *telepathy*.

TARGET: In *ESP tests*, the *stimulus object*. In *PK tests*, the faces of the die (or combination of faces) which the *subject* attempts to bring up in the act of throwing.

TARGET CARD: The *card* which the *percipient* is attempting to perceive (i.e., to identify or otherwise indicate a knowledge of it).

TARGET DECK: The *deck* of cards the order of which the *subject* is attempting to identify.

TARGET FACE: The face on the die which the *subject* tries to turn up as a consequence of direct mental action.

TELEPATHY: *Extrasensory perception* of the mental activities of another person. It does not include the *clairvoyant* perception of objective events.

TRIAL: A single attempt to identify a *stimulus object*.

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# THE PARAPSYCHOLOGY LABORATORY OF DUKE UNIVERSITY

The Parapsychology Laboratory is a branch of the Department of Psychology of Duke University. Its beginnings date from 1930 when its research program was launched by members of that department under the sponsorship of Professor William McDougall and with the favorable interest of the President of the University—at that time, Dr. William Preston Few. The first report of the work done was issued in book form in 1934 under the title *Extra-Sensory Perception*; in 1937, the *Journal of Parapsychology* was established; and in 1940, five members of the Laboratory staff collaborated to publish *Extra-Sensory Perception After Sixty Years*, which summarized not only the work of the Laboratory but similar research, past and present, in other places as well.

*Problems of Research.* The main topic of investigation has been extrasensory perception in its various phases—telepathy, clairvoyance, precognition—and psychokinesis, or the direct influence of the mind over matter. Other work has been planned, and to some extent undertaken; but the main objective has been to arrive first at a fair understanding of extrasensory perception and psychokinesis themselves before turning definitely to other problems of parapsychology. However, an experimental staff is being trained and methods developed for a broad and inclusive program which awaits only the time and the necessary financial assistance for its execution.

*Financial Support.* The Laboratory is supported jointly by Duke University and by a small group of donors who have thus far desired to remain anonymous. The Prince Memorial Fund, established in 1934, provided two fellowships in Parapsychology at Duke over a five-year period. In 1935, the William McDougall Research Fund was created at the University with a special committee, appointed by the major contributor and approved by President Few, to administer it. This is called the Parapsychology Research Committee and is self-perpetuating. In order to insure that the purposes of the contributors will always be respected, the Committee has been given the legal authority to constitute itself a board of trust, independent of the University should the need ever arise.

Thus far, the Laboratory has had no permanent endowment and has been maintained by current contributions. This situation has resulted in a lack of long-term security and has worked definitely against the planning of large undertakings. It has also occasioned the serious loss of valuable, trained personnel.

In the judgment of this Committee, it is highly important that, either by will or gift, grants be made of sufficient size to establish an endowment that would guarantee not only the permanence but the stabilization of the research program of the Parapsychology Laboratory.

(Signed) ALICE M. BALDWIN, *Dean, Woman's College, Duke University (Chairman)*
HORNELL HART, *Professor of Sociology, Duke University*
O. K. MERRITT, *President, Renfro Hosiery Mills*
J. B. RHINE, *Professor of Psychology, Duke University*
The Parapsychology Research Committee, Duke University

## *"Any one will renovate his science . . ."*

"The great field for new discoveries," said a scientific friend to me the other day, "is always the unclassified residuum." Round about the accredited and orderly facts of every science there ever floats a sort of dust-cloud of exceptional observations, of occurrences minute and irregular and seldom met with, which it always proves more easy to ignore than to attend to. The ideal of every science is that of a closed and completed system of truth. The charm of most sciences to their more passive disciples consists in their appearing, in fact, to wear just this ideal form. Each one of our various *ologies* seems to offer a definite head of classification for every possible phenomenon of the sort which it professes to cover; and so far from free is most man's fancy, that, when a consistent and organized scheme of this sort has once been comprehended and assimilated, a different scheme is unimaginable. No alternative, whether to whole or parts, can any longer be conceived as possible. Phenomena unclassifiable within the system are therefore paradoxical absurdities, and must be held untrue. When, moreover, as so often happens, the reports of them are vague and indirect; when they come as mere marvels and oddities rather than as things of serious moment—one neglects or denies them with the best of scientific consciences. Only the born geniuses let themselves be worried and fascinated by these outstanding exceptions, and get no peace till they are brought within the fold. Your Galileos, Galvanis, Fresnels, Purkinjes, and Darwins are always getting confounded and troubled by insignificant things. Any one will renovate his science who will steadily look after the irregular phenomena. And when the science is renewed, its new formulas often have more of the voice of the exceptions in them than of what were supposed to be the rules.

—William James, "What Psychical Research Has Accomplished," from *The Will to Believe*.