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The Journal *of* Parapsychology

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The Journal *of*
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EDITORIAL:

WHOSE FIELD IS PARAPSYCHOLOGY?

It is not enough, for the present at least, to say that parapsychology belongs to the parapsychologists. It is revealing no state secret to say that there are not enough active parapsychologists in the world today to constitute in themselves an adequately self-sufficient group, even for the maintenance of a scientific journal.

To whom, then, does the field belong? And by the "field" we mean the problems and the research and, above all, the interest which promotes and accompanies the research. Who are the people who hold such an interest and by means of it are led either to become explorers or students of the problems called parapsychological and of the evidence for their solution?

If we may take extra-sensory perception as a fair example of a parapsychological topic—and it is, at least, the one that is best known in scientific circles—it would appear sufficient to say that parapsychology belongs to psychology proper, for it has become a matter of habit to regard extra-sensory perception, however else one looks at it, as something which psychology must include within its range as a science. Ample evidence of a reasonably general agreement on this point is available not only in the literature of extra-sensory perception, but in the results of questionnaires designed to obtain a representative poll on that particular question.[1] The penetration of discussions of extra-sensory perception, not only into the laboratory of experimental psychology in many institutions, but also into the

[1] See this Journal, 1938, **2**, 302.

pages of psychology textbooks as well, suggests a growing, even though hesitant, recognition of this relationship.

But while it appears beyond question that, technically, problems of parapsychology are also problems of psychology proper, it seems equally evident that they did not arise in the psychology laboratory; as a matter of fact, they did not arise among psychologists at all. It is clearly not psychological interest that created the field of parapsychology, or gave it its initiation. Only when the investigation of the parapsychical problem became a distinctly technical matter, was the aid of the psychological profession called in; or, rather, only then was the problem forced upon the experimental psychologist. During the first twenty of the sixty years of the experimental history of extra-sensory perception, there were six non-psychologist academic men actively interested in the experimental investigation of extra-sensory perception to each psychological experimenter. On the other hand, during the last twenty years (or, rather, the twenty ending with 1939), there were almost six reports of ESP experiments by psychologists to each report by a non-psychologist academic man. This reversal of proportion, this shift of active research interest from non-psychologists to psychologists, closely parallels the growing complexity of research methods in ESP experiment.

This is not to say that the increase in technicality is due to the taking-over of the field by psychologists, nor, on the other hand, that the taking-over is a result of the increasing complication, though both have perhaps had something to do with it. Undoubtedly, improved methodology makes the results more impressive to psychologists, and beyond question, too, increased interest on their part has led to improvement of method. It has been, on the whole, a natural development.

But, although psychology may ultimately become a claimant to the parapsychology problem when methodological advance has rendered it scientifically genteel, to whom does the field belong while it is still unrecognized—in fact, today, while it is still parapsychology? Whatever it may have to look forward to as its ultimate destiny in the academic classification of fields of knowledge—whether, for example, it thoroughly transforms psychology as a field, or whether it is quietly absorbed by that more general science without any major adjustment—parapsychology has undoubtedly a long way to go on

its own, and it is important to know whence come the interest and effort that initiate its various researches.

If there were any one thing to be said in general about the groups of people interested in the field of parapsychology as we have come to know them through correspondence and other means of contact, it would be that they are seeking either new evidence or confirmation of a religious philosophy of man and his universe or knowledge of some guiding scheme of things that would, in effect, take the place of what we have commonly thought of as religion. The general urge is religious in kind.

And yet, we cannot say that religious leadership has played any dominant part in promoting the investigations of parapsychological problems. Like the psychologists, the professional religionists, the clergy, have neither launched the parapsychical researches nor fostered them in their infancy. If anything, the clergy have stood more aloof than the psychologists, though of course for somewhat different reasons.

Perhaps the only group of professional men—except, of course, parapsychologists, now become relatively professional in late years—who can be said to have played a positive role in bringing the attention of the scientific world to the need of investigating parapsychical occurrences are the social anthropologists. There appears to have been fairly widespread agreement in their reports that they have witnessed, in practices of native magic and religion among various primitive racial groups, behavioral phenomena that they were not able to explain by present-day scientific concepts. Here, by definition, is where parapsychology comes in.

But while anthropology has turned up enough challenging material to warrant the doing of all the parapsychical research that has ever been done, and a great deal more, it does not appear to have been primarily this evidence which actually touched off the research movement in parapsychology or fired the zeal of those who have kept it going. For one thing, when the anthropologist turns up some interesting material of a parapsychical nature, he considers it in the general analysis of the culture under study; and relatively speaking, he is through with it. It is someone else's job to explain the underlying principles of the phenomenon—presumably the psy-

chologist's, whose job it is to account for behavior, however unusual it may be.

Rather, it is the spontaneous parapsychical experiences that occur *within our own civilization* itself that are mainly responsible for forcing the issue and instituting the work called "psychical research"; and it was, in general, the intelligent layman—not the psychologist, the anthropologist, or the religionist—who recognized sufficiently the importance of making a scientific study of these happenings to carry through the organization of the psychical research societies. Many of these men and women were academic people, many were in other respects intellectual leaders and professional practitioners in several fields, but no one profession predominated.

However, even though no single label would identify the group which has established the foundations of parapsychology, at least a general impulse or motive may be said to have co-ordinated their efforts. The very fact of the timing of the rise of psychical research gives us a clue to this. Although there had been centuries of the same types of spontaneous parapsychical experiences, it was not until the last quarter of the nineteenth century that scientific psychical research began. The influence of mechanistic science was undermining the traditional views of man and his world. Contemporaneously, the rise of the Spiritualist movement offered a system of thought that rationalized these "psychic experiences," and rendered them more acceptable, intellectually, at the time. How natural, then, for the mind which was floundering in the waters of disillusionment to grasp at the thought that in these unusual occurrences there might be the objective confirmation of a faith in a spiritualized universe so hard to harmonize with a Darwinistic picture of man's destiny. The literature of psychical research itself in general bears out this view of the forces leading men to do such work: that the inquiry represents a reflection of the best features of both movements, the cult of Spiritualism and the mechanistic philosophy of science.

There was, then, a blending of the desire, on the one hand, to discover the full extent of the role of human personality in the universe, and the determination on the other, to reach whatever conclusions are finally accepted by the soundest of all modes of inquiry, the methods of science. It is to all of those who jointly represent these two aspirations that parapsychology really belongs; their im-

pulse, we suggest, is the impulse that has made the religions and the philosophies, but the way is the way of science.

* * * *

For the first time in the history of the JOURNAL an article on a professional medium appears in our pages. Even in this instance it is not a thoroughgoing attempt to study mediumship as such—that is, with a view to an exhaustive analysis of its nature. The article entitled "Unusual Types of Subjects Tested for ESP: I. A Professional Medium," deals primarily with the question of the acquisition of knowledge independently of the recognized channels. However, the utilization of the medium's own customary performance and the coöperation of her "control" represents an interesting innovation in these pages, one which it is anticipated will be followed by further advances along similar lines. At any rate, we believe that the methods, as well as the results, of the studies reviewed in this article not only warrant, but invite, the continuation of such research on broader and, indeed, more exhaustive lines.

To the frequently asked question whether extra-sensory perception is influenced by atmospheric pressure, at least a tentative answer is offered in this number under the title "Atmospheric Pressure and ESP Score Averages." We suspect it will be disappointing to many of those who have raised the question, but it should at least be enlightening. If it is provocative of controversy, or better still of further research, the JOURNAL remains open to both.

No change in editorial policy of the JOURNAL has met with more expressed approval than the inclusion, in its scope, of philosophical discussions of ESP results relevant to the research. (This innovation occurred under the editorship of Drs. Murphy and Riess.) The speculative article, "The Nature of ESP," in this issue continues this policy. Mr. Saltmarsh is a member of the Council of the Society for Psychical Research (England), and has contributed numerous books and articles to the literature of psychical research, including both reports of research and interpretative discussions.

In two earlier articles appearing in this JOURNAL there was introduced a new statistical measure of ESP called the "salience ratio," or "SR," which is based upon the distribution of hits according to position in the run. In the second of these articles this measure

was used to evaluate the results of a long-distance test of ESP. In the present issue it is applied to measure the results of a test involving distance in time—in other words, prediction of future events, or precognition. Reports bearing on the precognition hypothesis have been accumulating during the last two years, and though none is striking, the combined effect is acquiring considerable force.

UNUSUAL TYPES OF PERSONS TESTED FOR ESP

I. A PROFESSIONAL MEDIUM

By WILLIAM R. BIRGE and J. B. RHINE
Parapsychology Laboratory, Duke University

THE INVESTIGATOR of extra-sensory perception is frequently asked, "What kind of people show ESP capacity?" It would help considerably in answering this question to publish reports of the various types of persons who have been given a sufficient amount of ESP testing to permit a judgment to be made. For this reason, a series of articles is contemplated in which certain distinctive subjects or groups of subjects will be presented together with their test results—regardless of whether the latter were high or low.

This first number of the series is concerned with the professional medium, Mrs. Eileen J. Garrett, and those who know her will undoubtedly agree that she represents a sufficiently colorful personality to figure in the launching of such a series.

Mrs. Garrett has probably become best known to the world as a professional medium, but she has been many other things as well. She was at one time a movie actress; she was in charge of a recreation center in England for service men during the last war; she is the author of several books; and at present is editor of the magazine *Tomorrow*. In her mediumship she has stood out as the most "scientifically investigated" of the profession. She has, in fact, literally walked into the laboratory of the psychologist, the physician, and the psychiatrist, and offered her services as an experimental subject for study. Groups of physicians in this country and in England have made studies of her trance physiology. Among psychiatrists, William Brown of Oxford and one of the leading men in this country have had her in their laboratories.[1] She has been investi-

[1] Although the fact of the American psychiatrist's investigation is well known, no report has been published.

gated by the Society for Psychical Research in London and by the Psychology Department at Duke University. While this by no means exhausts the list of her contacts with scientific inquiry, it suffices to show that Mrs. Garrett, in scientific matters, has "been around."

Mrs. Garrett is preëminently a trance-medium. In the course of her professional practice, she often passes into a trance state in which there are manifest personality changes: the voice, manner, and language customarily take on a more or less Oriental cast, and a personality calling himself "Uvani" is usually represented. Uvani claims to be the spirit survival of a deceased Arab, whose earthly life took place some centuries ago. He acts as a sort of master of ceremonies, claiming to be in contact with other spirit personalities who are not able, as he is, to utilize the organic system of the medium or "instrument." Whatever the Uvani personality may be—and we are taking him here simply in a descriptive way—he is very pleasant to deal with, and entirely coöperative. When the trance session is terminated, usually on the decision of Uvani, the medium returns to her normal state and gives every appearance of remembering nothing of the events that took place in the trance state.

In her role as an ESP subject, Mrs. Garrett undertook two kinds of tests. First, her customary trance performance was utilized as the basic procedure in what is called a "trance-association" test in which the medium in trance produces a series of free associations or spontaneous expressions intended to relate to the sitter (i.e., the visitor seeking the services of the medium). There were two series of these "trance-association" tests at Duke; one in 1934, and one in 1935. A short series of somewhat similar character was conducted later in England. Along with these tests, she was given a second type which comprised two series of more formalized experiments with ESP cards at Duke and a roughly similar series later on in London.

The Trance-Association Tests

The trance-association study at Duke was reported by J. G. Pratt under the title, "Toward a Method of Evaluating Mediumistic Material." While method was paramount, the very interest in method depended on getting meaningful results; and to be meaningful in parapsychology, the results must be obtained under conditions

that exclude all reasonable possibility of normal or recognized causes, such as sensory perception, rational inference, accidental hits, deception, or error in evaluation. The aim was to obtain from the medium evidence of "extra-normal" knowledge (i.e., knowledge unexplainable by recognized principles) of matters associated with the life of the sitter. Beyond that, this particular study did not aim to go; the question of the spirit origin of the utterances, or that of the nature of the Uvani personality, was not under investigation. These problems could not (and cannot, we think) be successfully attacked until methodology advances further, and until certain prior questions are answered.

In the first trance-association series, conducted in the spring of 1934, the following procedure was used: Mrs. Garrett was seated in a comfortable chair, and went into trance as soon as the stenographer or the experimenter indicated readiness. This induction of trance was a matter of only a fraction of a minute, and was done quietly, with none of the usual contortions. Evidence that the medium was ready was given by the change of posture and the expression, "It is I, Uvani. I give you greetings, friends!"

The sitter was brought into the room with the medium as soon as the latter had definitely entered the trance state and retired before the termination of the trance. The sitter was always seated well behind the medium, and was not allowed to speak. His questions, therefore, had previously been written down and were handed to the stenographer to be read aloud. The stenographer recorded as nearly as possible every word that was said by the medium.

There was no evidence whatsoever that Mrs. Garrett attempted to acquire information about the sitters through normal means. She appeared to understand fully the need for the precautions taken and coöperated in every way to render the conditions secure against possible leakage of knowledge by normal channels.

The sitters were chosen in strictest secrecy; only two persons knew who was to be selected on a given day. The sitter did not know more than a few hours in advance, and was asked to come to the laboratory without disclosing his objective to anyone. He was brought into the laboratory in a roundabout way, and was never allowed the slighest contact with the medium until the beginning of the experiment.

In the first series there were twelve sitters, each one of whom was given a sitting of approximately one hour. Only two of them had had any previous experience with mediumistic sittings, but all were very much interested in having the opportunity. The majority were very favorably disposed to the hypothesis of spirit survival, but some were not. All of them, however, were asked to participate as though they were willing to accept that hypothesis—that is, not to display an open skepticism during the experimental session.

When the stenographic records were transcribed, Dr. Pratt abstracted from them a list of items of information. This process of selection involved the elimination of all irrelevant and unidentifiable material, as well as anything conceivably inferable from the few written questions of the sitters. Only those items were chosen concerning which a judgment of the correctness of applicability could be made. Moreover, the points were selected without any knowledge on Pratt's part of the identity of the particular sitter to whom the information applied. The records were numbered, and were presented *in toto* to each of the twelve sitters. No sitter was informed as to which one of the twelve records was his own. Each was asked to pass judgment on the correctness of the points, not only of his own sitting record, but of all the others as well. It was assumed that if a particular sitter were disposed to say, "Yes," to anything the medium told him, he would presumably follow the same tendency in going over all the records. Thus, while his own records would obtain a high percentage of hits, the other records would likewise do so; and the method of evaluation was such that the more checks or affirmative marks an item received by all the other sitters, the less a hit counted for the sitter himself. Thus the "yes-men" and the "no-men" would not only cancel each other out in the end, but neither type could produce extra-chance results simply by indiscriminate denials or affirmations.

In addition to this, the twelve records were checked by each of fifteen control sitters as to the applicability of the points to them. Thus there were, in all, twenty-seven sets of answers obtained for each of the sitting records.

We cannot discuss in detail the statistics of the method of evaluation. The method had previously been used by the English investi-

gators, Saltmarsh and Soal, in a somewhat similar study. The objective of the method was to determine whether the number of correct hits was sufficiently greater than expectation to be "significant"; that is, to admit the conclusion that a theory of chance could not reasonably be applied to the results. The measure of the results, the critical ratio, obtained by the work as a whole in the 1934 series was 3.22; the odds against the occurrence of such a value by chance alone are about 700 to 1, and accordingly Pratt concluded that the results were not explainable by chance.

The series of the second year at Duke marked a considerable advance over the first, and it speaks well of Mrs. Garrett's adaptability to new situations that she accepted the suggested change. It was realized that in Series I it had been possible for the sitter, in going over the twelve records in the checking process, to recognize which of the twelve was his own, through his memory of the session; and, of course, he might take a more favorable (or less favorable) attitude toward the checking of his own record than toward the others. It was understood that this weakness could be avoided only by having the sitter sufficiently removed from the medium so that her utterances could not be heard and subsequently identified. Accordingly, it was arranged that the sitter should be seated in an adjoining room. The intervening wall was of heavy tile, and the door was of unusually heavy double oak. While it was possible by careful attention to hear the murmur of the medium's voice, one could not distinguish the words. Thus none of the subjects knew what was being said during the sitting.

In other respects, the series was conducted as in Series I. There were fifteen sitters in this series, and the checkup was made in the same fashion, except that no control sitters were used. The results were still higher—that is, still further from chance expectation—in this series than in the former. The critical ratio was 5.10, representing odds well over 1,000,000 to 1 against such an occurrence being due to chance alone.

From these studies Pratt concluded that the medium must have had some way of obtaining information which the normal sensory channels could hardly have provided. Such a conclusion seemed to be dictated by the experimental conditions, involving the exclusion

of all sensory knowledge, and by the definitely extra-chance character of the results.

When Mrs. Garrett was in England in 1937, Mrs. Goldney, a member of the Society for Psychical Research, carried out a small series of experiments with her, which were regarded as unsuccessful, but of which thus far only a brief account has been given.[2] These experiments were "proxy sittings"; that is, the sitter was not present with the medium—not even, we must suppose, in an adjoining room. Hence, we cannot make any comparison with the Pratt results—and, in any case, there were only eight sessions, as compared to the total of twenty-seven by Pratt. Still more important, there is no indication that the results were statistically evaluated. The sole basis of judgment at Duke was mathematical.

Formal ESP Tests

Whatever the nature and source of the knowledge shown in the trance-association experiments, it was extra-sensory in character. For this reason, the Pratt study represents a very substantial contribution to the case for extra-sensory perception in the broadest use of the expression. Nevertheless, the Pratt study did not complete Mrs. Garrett's work at Duke, for she was tested also by the more formal ESP procedures. Since the essential details have been given elsewhere,[3] we shall present only a brief discussion of the telepathy and clairvoyance work in 1934, and of the precognition tests in 1935.

When Mrs. Garrett came to Duke in the spring of 1934, we were then in the midst of an ESP test program that involved a number of high-scoring subjects, both in telepathy and in clairvoyance tests. Mrs. Garrett's total results were highly significant in both. But while she showed herself the equal of these more gifted subjects in the telepathy tests, surprisingly—and contrary to the general rule for good telepathy subjects—she did much more poorly in the clairvoyance tests. Even before she began, she showed a preference for the telepathy tests, explaining that she preferred the more personal relation of those experiments to the more objective clairvoyance pro-

[2] Goldney, K. M., and Soal, S. G. Report on a series of experiments with Mrs. Eileen Garrett. *Proc. S. P. R.*, 1938, **45**, 43-87.

[3] Rhine, J. B. Telepathy and clairvoyance in the normal and trance states of a "medium." *Character and Personality*, 1934, **3**, 91-111.

cedure; in fact, it was very evident that card tests tried her patience considerably.

Over a three-week period (in 1934) she averaged 8.7 hits per 25 trials on telepathy, and 5.6 hits per 25 on clairvoyance. These averages are 3.7 and 0.6 above chance expectation. During the last week she had a very severe cold, and was under the care of a physician. Therefore, most of the tests were conducted during the first twelve days, and these constitute the most normal basis of comparison. On telepathy during that period, she averaged 10.1 hits per 25, and on clairvoyance 5.7; that is, 5.1 and 0.7 above expectation. Most other subjects of any outstanding capacity to score had averaged approximately the same in telepathy as in clairvoyance tests.

Consider, then, the interesting fact that when Mrs. Garrett was asked to go into trance and allowed the Uvani personality to participate in the tests, the average for telepathy for the first twelve-day period was 9.1, compared to the 10.1 of the normal personality, and for clairvoyance 5.6, compared to 5.7 for the normal. In view of the fact that the clairvoyance and telepathy averages are so far apart, this similarity of Uvani and Mrs. Garrett is the more interesting.

From the point of view of the adequacy of precautions in these experiments, it is interesting to note that both in the telepathy and in the clairvoyance tests, the results were higher, on the average, when the percipient was in a different room from that in which the cards (or the telepathic agent) were located. This work took place before the full-fledged procedure for the safeguarding against errors of clerical character had been introduced, but there were two experimenters present during the checking-up of almost all of the best results. Besides, it has been found that the more common errors in checking are errors of omission which would tend to lower rather than to raise the score.

Finally, both the normal and the trance personalities declined steadily during the latter half of the three-week period. Not only did this occur in telepathy, but equally so in clairvoyance. The final three-day period was actually below the average expected from chance alone.

When Mrs. Garrett returned in 1935, however, her work in the ESP tests was entirely of a chance nature. Whereas in the first series the total of 640 runs through the deck, including both telep-

athy and clairvoyance, both for the normal and the trance states, averaged 6.3, which gives the very high critical ratio of approximately 16, during the second year's work, in 948 runs, she averaged only 4.9, which is slightly below the expectation of 5 and is quite insignificant. And two years later, during her stay in England, Mr. S. G. Soal, a member of the S. P. R., gave Mrs. Garrett tests in telepathy and clairvoyance to the extent of a total of 723 runs through the deck, with results quite as insignificant as those of the 1935 series at Duke.

Discussion

A number of interesting questions arise out of the investigations of Mrs. Garrett described above. What relation exists between the trance-association test results and the performance of the formal ESP tests? How can the success in one situation and the failure in another be explained? What can be learned from the comparison of the performances of Uvani and Mrs. Garrett in the normal state?

One would like very much to know whether the same abilities which produced results on the telepathy and clairvoyance tests were responsible for the performance in the trance-association experiments. If we may assume that telepathy and clairvoyance are not limited—and, if they are, we do not know today what the limitations are—then it does not appear difficult to suppose that such a comprehensive ESP capacity might have provided all of the information shown in the TA series. Most of such information concerned reference to past events in the life of the sitter or of descriptions of personalities, mostly of deceased relatives of the sitters. But the same clairvoyance that penetrates decks of cards, even at remote distances in space, would logically be expected to transcend time limitations as well, as indeed there is considerable evidence to show is not impossible.

But even when we accept this extension of ESP capacity over limits of space and time, it will still not account for the peculiar motivation which leads to the assembling and organizing of the information. Whether or not there is anything more at work than the mediumistic urge to impersonate and bring comforting communications, cannot be determined from the work reviewed here.

In short, as was recognized from the beginning of the research with Mrs. Garrett at Duke, the source of the knowledge shown in the trance state could not be determined from the experimental condi-

tions and results of the present study. A much more specialized experimental program is called for to achieve that objective. A tentative plan for such a research program is under discussion, and will be presented in a later issue.

If we were to conclude that the trance-association results are entirely ascribable to ESP, it would be obvious at once that the much greater amount of work involved in the TA series would certainly make the card tests preferable.

The decline of score average in the ESP tests is a point of considerable interest. After a little over a week, in the 1934 series at Duke, the high point in value of results was reached, both in clairvoyance and telepathy, and both by Uvani and Mrs. Garrett. From then on, there was a fairly steady decline, and the end was below chance. All four conditions declined together.

This finding strongly suggests that Mrs. Garrett was satiated with such tests, and there were other evidences also which pointed to the same thing. The card tests were foreign to her years of professional practice, and after guessing over 16,000 of them, which involved daily tests over a three-week period, she can be expected to have been "fed up."

When, in 1935, on her return to the laboratory, she was again plunged into card tests, even though there was a variation—this time they involved pre-shuffle card-calling, or precognition tests—it is not entirely surprising that she failed entirely to score significantly above chance. She continued consistently to produce only chance scores during the entire period, even though her work in the trance-association tests was more significant than that of the year before. Two years later, however, when she went to England for a somewhat similar series of card tests with Soal, entirely in line with the latter part of the American phase of her work, she continued to give chance results.

While all attempts to explain success and failure in ESP tests are still somewhat speculative, there is one suggestion coming from Mrs. Garrett herself that might be worthy of mention. It was contained in a letter to Mrs. Goldney, which was published by the latter. In this letter Mrs. Garrett contrasted Soal's conditions with those she had worked under at Duke. Mrs. Garrett stated that while she preferred the easier-going conditions of Soal's laboratory, *she*

felt in Rhine's laboratory that she must get results. We take this to mean that there was less pressure in Soal's laboratory, and that this was a relief; but the results appear to show that the lapse of effort which resulted was certainly of no help in the demonstration of ESP. But the subject's scores first fell to chance expectation in the Duke laboratory itself—which suggests that "pressure" was not a determining factor.

There has been a great deal of discussion, largely of speculative character, as to the nature of mediumistic controls or guides. On that account, any item of information, however small, that would throw light on the question would be important. Accordingly, the similarity between the Uvani and the Mrs. Garrett personalities, in the wide difference they both showed between the average score for clairvoyance and the average score for telepathy, appears quite suggestive. The averages for the two personality states are approximately the same for each of the two types of tests.

We would not wish to conclude that the trance personality is not to be identified with the question of spirit survival in general, although members of the Spiritualist faith commonly accept the control personality as being what it usually claims to be—the spirit of a deceased individual. In any case, it would appear that a beginning has been made on the task of determining the nature of trance personalities, and that the study of this one unusual subject made by a number of persons with a number of methods of investigation would justify beyond question a continuation and extension of the study of professional mediums.

ATMOSPHERIC PRESSURE AND ESP SCORE AVERAGES

By E. P. GIBSON, *Detroit Public Schools*, and
C. E. STUART, *Parapsychology Laboratory, Duke University*

THE QUESTION has often been raised as to whether there is any effect of weather conditions upon performance in ESP tests. Such a question has come particularly from members of certain groups who hold to a semi-occult philosophy. There is a fairly widespread conviction among mediums and other sensitives that their success is influenced by weather extremes. For that matter, the general public holds a belief that weather changes influence behavior, especially moods, and of course it must be remembered that moods may control performance. Certainly there are few who would question the likelihood of obtaining a difference in performance, especially in mental tasks, in cold, crisp, sunshiny weather, as contrasted with sultry, foggy conditions.

By way of scientific studies, there has been considerable inquiry made under the heading of meteorological conditions, but not much that comes under the special topic here concerned; namely, atmospheric pressure. Much of what has been done has been published in obscure European journals that are at present unavailable here. Ellsworth Huntington (1), who has been the primary American student of weather conditions in relation to human behavior, finds little evidence of variation due to the barometric pressure of the atmosphere. A study was made in 1914, which showed that factory workers made more errors the higher the pressure. The only psychologist in America known to be interested in this relation of barometric pressure to behavior is R. H. Wheeler, but his work has not as yet been published.

There was from previous work, then, no clear lead as to what to expect so far as ESP performance and barometric pressure are concerned. And, as a matter of fact, this paper is not based upon a

direct experimental attack on this question; to date no such experimental effort has been made, perhaps from lack of sufficient lead or *a priori* justification. We felt, however, that something worthwhile might be learned from a statistical correlation of such ESP results as were already available, for which we could find the corresponding barometric readings. And, somewhat to our surprise, the first few series of results on which this correlation was made gave encouraging results, and we were led to continue from one series to another as we were able to find suitable material.

The initial premise of significant relation between pressure and performance was not, however, borne out; and as the results will show, the total effect is a negligible one, indicating as far as we have gone that there is no reliable connection between this particular weather effect and the performance of the particular subjects who took part in the ESP tests that were utilized. Nevertheless, it seems worthwhile to report on what has been found to date, even though we realize fully that there is no finality to a research that reports failure to find a relation. This interim report may, however, lead others to correlate their data in a similar way if they have a meteorological station near by from which the essential weather records may be obtained; and presumably, too, even those who have held contrary views will be interested in these results, incomplete as they may be, since they are to date the best we have in this field of study on which to base a judgment.

The method of analysis is that of simple linear correlation. The critical ratio[1] of the average score for the day made by all subjects participating in the series who were tested on that particular day is correlated with the barometric reading for the day. This reading, too, is an average; namely, the average from the morning and evening readings of the Raleigh station of the Department of Agriculture (with the exception of one series which was based on aneroid barometer readings taken in this Laboratory).

There were, in all, seven studies or experimental series that were used for this correlation study. These will be described only very briefly, since it is not of primary importance just what the details associated with each investigation were. They may be seen in

[1] See Glossary. The critical ratio was used because it has a constant theoretical variance regardless of the length of the series.

Table 1, each identified by the names of the experimenters. The table also will show the number of days which permitted a comparison of critical ratios and barometric readings. This is column "N." The column headed "r" gives the correlation of the two figures. The column headed "t" gives "student's *t*," a statistic closely comparable to a critical ratio, but corrected for the number of observations. The last column gives the approximate probability that such a correlation may be obtained by chance. None of these is significantly low. The criterion of significance would be .01. Items 3 and 4 and perhaps item 1 might be considered in the range of "suggestive."

Table 1

EVALUATION OF THE OBSERVED CORRELATIONS

EXPERIMENT	N	r	t	P(t) (approx.)
Pratt-Woodruff	86	+.159	1.46	.18
Woodruff	23	−.225	1.02	.49
Smith-Hull	12	−.656	2.35	.04
Stuart-Pratt	42	+.276	1.77	.09
Rhine: Child	11	+.270	.79	.47
Rhine: Adult	25	+.009	.04	.95
Pratt: Roll-box	35	−.242	1.41	.25
Siegel M	34	−.070	.39	.70
Siegel H	15	−.010	.03	.98
Siegel C	31	+.038	.20	.84

Total correlation of all items: r = +.020

In the light of these results, the following information regarding these tests may be of some interest. Four of the seven series gave significant evidence of ESP effects; the other three did not. These four were the Pratt-Woodruff, the Woodruff, the Stuart-Pratt, and the Rhine series. The two divisions of the Rhine series were each independently significant. The Stuart-Pratt series did not give a significant critical ratio as did the others, but proved to be extra-chance when evaluated in a special analysis that was made by Stuart. This work has already been published in this JOURNAL, and may be consulted for details (5).

It may appear curious that the three insignificant series, the Smith-Hull, the Pratt, and the Siegel, were used in this correlation, since they afford no evidence of ESP. The reason is this: It is theoretically possible that ESP is affected by atmospheric pressure, and might be so affected as to cancel out by its adverse effect on one day

the deviation that it may have contributed by a favorable effect on another day. Thus a correlation might be found without a significantly high average score for the whole series being obtained. This is recognizedly somewhat less likely than would be the expectation of finding the correlation where it was known that ESP occurred; but it should be pointed out that the lowest probability obtained was given by the Smith-Hull series; that is, the lowest scoring series came nearest to giving a significant correlation between daily performance and barometric reading.

The Pratt-Woodruff series was conducted with the clairvoyant card-matching procedure, using college students as subjects. The original work was reported in 1939 (2). The Woodruff series was very similar. The Smith-Hull series has not yet appeared in print. It was conducted with the use of various ESP techniques, and the subjects were patients in a psychopathic hospital. The experimenters were Burke M. Smith and Edyth Hull of this Laboratory. The Stuart-Pratt series was based upon precognitive matching tests with college students as subjects. The subjects matched, with rhythm established by a metronome, each deck of cards to key-cards later to be selected by a shuffling machine. The total results were reported by Stuart in 1941 (5). The Rhine series consisted of two divisions, also using precognitive matching. One division was conducted with child subjects and the other with adults. A report of this was published in 1941 (3).

The Pratt roll-box series used a new device that has not yet been described in print. The roll-box was a locked metal container which held a roll of paper on which were printed rows of 25 ESP symbols. The turning of a knob brought a row into a known place within the box. An opening farther down the roll permitted the subject to write his calls for those symbols in 25 marked spaces. Another turn of the knob turned the subjects' calls behind a window, and brought the printed symbols into view for comparison.

The Siegel data are likewise taken from an unreported study. The investigator, Mr. Paul Siegel, instructed subjects to make designations of screened cards by pressing one of five buttons. The buttons were so devised that a small electric shock could be given when one was pressed. In one series the subject was shocked every time he pressed the wrong key. In another series, the subject was

shocked every time he pressed the right key. In a third series, no shock was given. These are designated M, H, and C respectively in Table 1.

. . . . .

There is little to be said by way of description and summary of these results, except that as stated at the outset they show no significant evidence of relation between atmospheric pressure and ESP performance. Even the few suggestive effects that may be noted are not to be taken seriously. First, the Smith-Hull and Stuart-Pratt series, which have very low average scores taken as a whole, give the smallest probabilities—that is, those most suggestive of relation to barometric pressure; but the third smallest probability is given by the Pratt-Woodruff series which is among the best from the point of view of evidence of ESP. Furthermore, it should be pointed out that the correlation in the Smith-Hull series is negative, while that in the Stuart-Pratt series is positive. So far as sign of correlation is concerned, the ten items in Table 1 may be seen to be evenly divided between positive and negative, and the total correlation of all items gives a coefficient (r) which is very near to zero. Altogether, then, there is no ground whatever for suspecting any relation between atmospheric pressure and ESP performance as far as these measures and the conditions go.

It may be argued that this measure is too crude; that pooling the results of all subjects together may cancel out individual differences. To determine whether this is true would require separate correlation of daily barometer readings with the results of different subjects. Also, it may be that the average of the morning and evening barometric readings is too rough a procedure, since it might obscure many important differences. Perhaps the hour of the day may itself be very important to the subject. Unpublished work connected with the Laboratory suggests, though it does not establish, that this may be so. Accordingly, the pooling of all the scores of all hours of the day to obtain one mass deviation might prevent the discovery of important relations in which atmospheric pressure may play a part. These and other hypothetical points might profitably be raised, but they can only be answered by further research, and could best be answered by a deliberately planned experimental program.

The question, of course, is whether there is any sufficient justification to plan such a program; whether there is any reason at all to expect a weather effect of this or any other kind, especially in view of the foregoing findings. Among the findings that seem most to suggest the propriety of further work is the Smith-Hull series, which gave the smallest probability. The correlation was negative. The fact that the subjects were psychopathic and showed characteristic negativism, and likewise showed strong negative tendencies in their score deviations, leads us to suspect that perhaps this may be less in conflict with the positive correlations of the Pratt-Woodruff and Stuart-Pratt series than would otherwise be the case. It would at least be worthwhile to find out whether psychopathic subjects would in general show a negative correlation. Another reason for taking this particular series more seriously is that it was conducted in Raleigh where the barometric readings were taken. Durham is about 25 miles distant, which, it may be argued, is not enough to warrant any serious question of applicability of the Raleigh readings; nevertheless, any systematic attack upon this question would do well to have the barometric readings taken in the same locality in which the tests were made and, if possible, at approximately the same time of day.

ESP performance has proved notoriously difficult to tie up definitely with any easily measureable effect within the individual or the environment, though certainly some progress has been made in this direction and some relations established (4); but it appears that barometric pressure is definitely not one of the conditions that can be included in this group of established relations.

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THE NATURE OF EXTRA-SENSORY PERCEPTION

By H. F. SALTMARSH
North Devonshire, England

IT MAY BE ASSUMED that extra-sensory perception, just as much as normal sensory perception, is a natural phenomenon.

Perhaps some will be disposed to question the legitimacy of this assumption, but until conclusive proof or, at any rate, cogent evidence to the contrary is forthcoming the principle of economy of hypothesis is sufficient warrant for it.

If this be granted it seems that the proper starting place for any enquiry into the nature of ESP should be the simplest and most primitive form in which it is possible to find this natural phenomenon. If analogy be any guide, it is probable that it will be hardly recognizable as such and we should expect to find a common root from which both sensory and extra-sensory perception have sprung. Now perception is a stage or factor in the response of the living organism to its environment; it is not an indispensable stage, for response may be unaccompanied by any consciousness or modification of consciousness; but in many cases, particularly where the response is of the fruitful kind (i.e., where it tends to the well-being of the organism), it is plausible to hold that an element of consciousness was originally present and that its absence is due to lapsing. In fact it may be that there is some dim subliminal consciousness attendant on all reflexes, however instinctive and automatic they may have become. The fact that many such reflexes may be reached and modified by hypnotic suggestion seems to support this view.

The most primitive form of organic response to the environment is to be found in the tropisms. These appear on the surface to be purely mechanical reactions to stimuli unaccompanied by any consciousness, however dim; they may be, as a general rule, service-

able to the organism because those reactions which were not so would tend to be stamped out in the course of evolution.

The tropisms are general rather than individual and it seems clear that it would be to the advantage of the individual organism if they were supplemented by some discrimination between stimuli: discrimination, that is to say, between what is beneficial and what is harmful. In the well-known story of the amoeba which, being pursued and in process of being engulfed by another, made repeated attempts to escape, it seems not unreasonable to suppose that the victim somehow discriminated between the various stimuli falling upon it and recognized some of them as harmful. When I say "recognized" I do not suggest that anything which could be regarded as conscious awareness was present.

But the fact remains that even the lowliest organisms behave as though they discriminated between the beneficial and the harmful. It seems highly doubtful whether the general response of the tropisms would, by itself, be sufficient for the securing of food and the avoidance of enemies.

Among the more highly evolved organisms, the power of discrimination becomes more apparant; as for example, in cases where one particular kind of food is used. Sometimes it reaches what seems an almost fantastic degree; for instance in the egg-laying procedure of the setaris beetle or the ichneumon fly.

Now discrimination, however rudimentary, constitutes a germ from which consciousness may develop. It entails primitive perception, understanding of meaning, and appreciation of value. We may start, then, from a simple mechanical response supplemented by discrimination between stimuli.

These two factors in the situation interact with each other and develop concurrently. From the tropisms are developed the sensory responses with their appropriate organs; for example, photo-tropism would give rise to the visual sense, and the pigment-spot to the eye; chemo-tropism to the senses of smell and taste, and so on. As discrimination becomes keener under the driving force of evolution, so understanding of meaning grows, and finer nuances of value difference are appreciated, giving rise to fresh values.

If one may speak in terms of human consciousness of an organism which is almost certainly not conscious in that sense, it may be said

that the protist would understand two meanings, viz. beneficence and harmfulness; would experience two emotions, satisfaction and dissatisfaction, primitive pleasure and pain; and appreciate these as values. It would seem also that some rudimentary memory must be involved.

This proto-consciousness utilizes the primitive sense organs to further its ends—the ends, that is to say, of the partnership between the pyche and the physical organism, viz. to obtain food and mates, and to escape from enemies.

To refuse to ascribe to the protistae the minimum of consciousness implied by discrimination between stimuli is but to shift the problem, for it is indisputable that among the higher animals the power to discriminate does exist. The question then becomes, "At what point does consciousness first put in an appearance?" The hypothesis which I have put forward (viz., that the rudimentary germ from which consciousness, as we know it, has sprung is present in the earliest stages and develops concurrently with the organic mechanisms which it employs) seems to me to be more plausible than to suggest that it suddenly springs into being and seizes upon sense organs which have evolved independently. I propose, therefore, to consider the lowliest forms of life which exhibit any appearance of discrimination between stimuli as being in a rudimentary fashion psycho-physical individuals. As a result of the cooperation and interaction between the two factors, the activity of the psyche tends to be canalized by the sense organs. These operate as focussing instruments; that is to say, they concentrate the perceptive activity on certain areas and on certain aspects. Not only does this entail that the world as presented by the senses is a selection from total reality, but it is also to some extent a distortion.

The world of the psycho-physical entity is therefore partially a construct and is dependent on the following three factors: (A) the characteristics and what may be metaphorically called the purposes of the psyche; (B) the kind of sense organs developed by the physical organism; (C) the natural characteristics of the material upon which perception operates, viz. external reality. It might be put this way: (A) is the architect, (B) the builder, and (C) the bricks and other building materials.

Had any of these been different, the building (i.e., the external

world as presented) would have been different. It will be noted that this view avoids the extreme idealism of Berkeley in that it allows the objective existence of an external world.

Some idea of the degree to which "construction" enters into the external world as presented may be obtained by reflecting on the probable difference between the world as it appears to an animal which depends mainly on the sense of smell (e.g., some of the wild canines) and one for whom sight is predominant (e.g., a kite).

As proto-consciousness develops into full consciousness in the course of evolution the external world as presented by the senses is organized in a system of three-dimensional space, one-dimensional time, together with number, causation, and other categories.

It might be that a complete specification of this system would give a full account of reality as it really is, that is to say, that the external world is actually subject to three-dimensional space, and only to three-dimensional space; that there is only one-dimensional time and all events fall within it; that the law of causation is absolute and valid without exception; and so on. But the fact that it is so presented by the senses is not sufficient guarantee that it is so. In fact, there is considerable evidence to the contrary. The antinomies of reason, the absurdities and contradictions which result when metaphysical analysis is pushed to its limit and, above all, ESP and other para-normal phenomena all go to show that our logical system is, at best, only a privileged case, valid in the main within its limits and subject to its fundamental assumptions, but having no application or jurisdiction beyond its frontiers. The important point to be noted is that there is a frontier and a region beyond that frontier.

I want at this point to render quite clear the distinction which I make between the mind (i.e., that which in the human being is the subject of normal conscious processes, such as sensory perception) and the pure psyche. The mind I regard as a psycho-physical entity in that it is dependent for its existence on the integrity of a physical organism. The evidence in support of this view is too well known to require exposition here; I will merely enumerate a few of the main headings: the effect of fatigue, illness, drugs, etc., the part played by the endocrine glands in determining the manifested character, the phenomena of subliminal mentation, of dual personality, and so on. The mind makes contact with the environ-

ment by means of the senses. As has been said above, this canalizes the response; the reaction, therefore, is a partial one directed to a selection of the total environment—a selection which is determined by certain well-defined ends or aims-to-ends.

But the activity of the mind does not exhaust the activity of the psyche, the fact that there is a frontier and a region beyond the frontier wherein the mind cannot operate by means of the usual machinery is sufficient evidence for this statement.

The discrimination of the primitive psyche between the beneficial and the harmful is a total response, moreover it involves a feature beyond the frontiers of the senses, viz. appreciation of value. This indisputably does not lapse with the development of sensory perception but evolves alongside with it.

Evolution may be defined as the widening of the area of increase in the number of points of contact with, and healthy response to, the environment. As applied to the physical organism, in particular to the senses, it is clearly to the advantage of the organism that the response should widen and grow in acuity so that the maximum of benefit may be derived from intercourse with the environment and enemies be successfully avoided or overcome. This must give rise to greater canalization and a larger element of "construction" in the external world as presented.

Unless we hold that evolution is one-sided and applies only to the physical, we must admit that the psyche also widens its area of contact with the environment. Its response, like that of the physical, tends to be healthy but, unlike the physical, it is total; that is to say, it is not canalized.*

There is much evidence in support of this view; in fact the whole range of the phenomena of parapsychology, together with much of normal psychology, might be adduced.

The mind of man does not confine itself to the world as presented by sensory perception. It is interested in ethics, aesthetics,

* This conception of dual response is a very different one for us as psychophysical beings, because our minds are under the compelling sway of the categories; the incompatibility between unity and plurality, between "this" and "that," self and not-self, appears to us to be absolute and ineluctable. Yet there are facts which should teach us the contrary or, at any rate, lessen the hostility which we feel to the idea. For example, the phenomenon of co-consciousness in cases of multiple personality might be cited; or telepathy, if carried to its conclusion; also the possibility of subliminal mental process.

philosophy, abstract science, and a number of other human activities and relations which rest little, if at all, on sensory perception. As remarked above, one of the most important facts is that we are aware of the existence of a frontier and a region beyond the realm of sensory knowledge.

But the mind labors under a great disadvantage in this matter. The tools of its trade are language and logic; they are tools which are fashioned out of sensory experience and adapted primarily for working on that material. The philosopher strives to invent new terms and devise a new technique, but he is "cabin'd, cribb'd, confin'd, bound in" by the categories; he is bound hand and foot by logic, blinded by his eyes, and deafened by his ears. Language has him in an iron grip; if he desires to communicate his thoughts to others or even to formulate them clearly to himself, he must conform to its dictates. Hence the unsatisfactory position of ethics and esthetics from the scientific standpoint. To treat these subjects by logical methods is like trying to repair a watch with the tools of a blacksmith. In spite of this, however, the activity of the psyche constantly makes itself felt, it intrudes into the working of the supra-liminal mind, disturbing the orderly mechanistic pattern. To get to grips with these intruders, they must be translated into terms which the mind can handle; but there are no terms which fit them perfectly. Some people, the deterministically minded, have tried to explain them away; others have made heroic but unsuccessful attempts to incorporate them into the logical system. Others, again, have rested content with acceptance of duality, acknowledging the jurisdiction of logic within its proper boundaries while doing the best they can with the extra-logical world of values—the world of ethics and esthetics—with what faculties of pure psychical activity are able to filter through.

But it is not only concerning values that traces of the activity of the psyche percolate into normal, everyday consciousness. There are also the phenomena of parapsychology, notably ESP.

The theory which I would like to put forward is that ESP is a total, unspecialized response of the psyche to the external environment, but that, in the process of transmission to the supra-liminal consciousness—a process which must be completed before the mind can become aware of this activity—the unspecialized impression must be translated into the specialized sensory terms. During this trans-

mission and translation it may be, and probably nearly always is, that much is lost and much distorted.

If I may use an illustration, the case is parallel with that of the literature of some vanished civilization. We recover nothing but a few fragments written in an unknown tongue. The translations which we are able to make are imperfect; modern language cannot render the full sense of the ancient tongue, and all the beauty and music of the original is lost; thus our knowledge is both incomplete and partially incorrect.

Let one try to describe, as I see it, what actually happens at an experiment in ESP; for example, a simple DT test. There are, first of all, two psyches, viz. that of the subject and that of the experimenter. Though these are, from our point of view, two distinct and separate individuals, yet they may be in some inconceivable way, one. Not only may they share experiences, as in what we call telepathy, but they may be linked together in other ways. It may be, of course, that the incompatibility which we take for granted between separate individuality and identity is fundamental throughout the whole of reality, but we have no right to assume that it is necessarily so. Next there is the situation in reality which we should describe in our spatio-temporal terms as a pack of the ESP cards lying face downwards on the table. Our description would include such spatial details as shape, size, relative position, and so on, together with sensory qualifications of color, texture, etc. It is a pure assumption for which we have no warrant that these details exist in reality as they appear to our minds; in fact, there are grounds for rejecting that assumption. However, it seems to me that we cannot avoid holding that there is actually some *qualification* in reality which gives rise to the sensory impressions on our minds. I cannot conceive as tenable the view that reality is completely undifferentiated—that would be an idealism even more extreme than Berkeley's; all I say is that we do not normally know anything about the actual nature of the differentiations, only how they affect our minds through the senses.

The psyche, being in cognitive contact with the environment, has knowledge of this situation. Whether that knowledge is of the details of the situation as they actually are, or whether it is symbolic knowledge—that is to say, mediated through some form

of selective perception—we do not know; but I think that we can say with some confidence that if the psyche exercises its perceptive powers by means of something analogous to our normal senses, the conditions under which it operates are other than those which govern ordinary sensory perception. For example, the spatial and temporal conditions would seem to be more elastic; three-dimensional space prohibits normal perception of anything more than the back of the top card and a partial view of the edges of the inverted pack, while to the perception of the psyche the faces of all the cards are, potentially at least, open to view.

It may be that for reality time and space do not exist at all, but even if this be so, in my view there must be some other conditions which appear as three-dimensional space and one-dimensional time when presented to the mind through the senses.

The total unspecialized knowledge of the situation acquired by the psyche would be unassimilable by the supra-liminal mind; it has, therefore, to be translated into terms which the mind can understand. I suggest that in this translation errors may occur and that in the process of transmission there are further losses and distortions. Without having any very secure basis for the opinion, I am inclined to think that the losses in transmission are more serious than those in translation.

The rather slender grounds on which this opinion rests is the fact that in experiments where low scoring is aimed at, theory suggests that the deviation should be only one fourth of that which ESP ability of the same order would secure if directed to high scoring; or, in other words, for low-aim scoring to attain the same degree of significance as high-aim, the ESP ability must be four times as great. The authors of *Extra Sensory Perception After Sixty Years* (p. 320) have put forward two suggestions of possible explanations of the rather extraordinary fact that low-aim scores run pretty much at the same level as high-aim. I would add another. If, as I hold, the perceptive responses of the psyche are total and uncanalized by the normal senses, it is plausible to suggest that losses in transmission may occur. For the sake of simplicity I will speak of this transmission as though it were the passing of a message. In order that the message may get through correctly for high-aim scoring, there

is only one out of five possibilities which must be chosen—there are four possibilities of failure to one of success; but for low-aim scoring the position is reversed—any one of four out of five possibilities will give success, and only one, failure. According to theory, the task for ESP is four times more difficult for low-aim than for high, yet the scores in actual practice are substantially the same. My suggestion is that the extra difficulty is exactly compensated by the greater ease of correct transmission. I cannot see, however, that the fact that low scores are being aimed at would affect any possibility of error in what I have called translation.

My view, then, is that the cognitive contact of the psyche with its environment—what we know as ESP—is a total response, noncanalized by our normal senses and, if governed by time and space at all, is subject to conditions less stringent than those with which we are familiar; it would give complete knowledge of the faces of all the cards, but this knowledge is distorted and partly lost in transmission to the supra-liminal mind.

The difference between a successful subject and a failure lies, not in ESP ability, but in the internal mechanisms of symbolism, translation, and communication.

To sum up this discussion, I suggest that man is a composite being, composed of at least two factors, viz., the psyche or spirit and the physical organism: the interaction of the activity of these two factors produces the supra-liminal mind—the manifested personality.

Both factors respond to the environment; the psyche, in cognitive contact with it, understands meanings and appreciates values, reacting in such manner as to secure its evolutionary ends. It utilizes the nonconscious responses of the body, thus assisting in the evolution of the normal senses. This results in selection and canalization and, as a consequence, the world as presented to the mind is partially a construct.

The activity of the mind does not exhaust the activity of the psyche. In certain situations, probably in those wherein there is some degree of dissociation present, some amount of the pure psychical activity overflows, as it were, and emerges into supra-liminal consciousness. In one of its forms this gives rise to what is known as ESP. It occurs in other forms, such as the inspirations of poets,

musicians, and mystics, the flashes of genius, and also, more normally, in the ethical and esthetic intuitions of the common man.

The response of the psyche is total and unspecialized, but the manifestations of this response which emerge into normal consciousness are fragmentary and distorted, because of errors and losses in the processes of translation into sensory terms and internal transmission.

EVIDENCE OF PRECOGNITION IN THE COVARIATION OF SALIENCE RATIOS

By J. B. RHINE[1]

Parapsychology Laboratory, Department of Psychology, Duke University

ABSTRACT: In this article I am reporting another series of precognition experiments; that is, experiments conducted to determine whether extra-sensory perception can penetrate the future. This is the fourth of a series of precognition studies which have appeared under my name and the seventh which has come from this Laboratory. Although in one respect (relative to chance) this report is not one of the strongest cases for precognition that has been presented, it does meet the statistical requirements. On the other hand, it represents a distinct advance over earlier experiments; namely, it introduces the feature of having the subjects predict the order of cards as they will be after the decks have been shuffled, using figures obtained from the *temperature readings* of a specified day in the future to determine the shuffling and cutting of the decks. In the completeness of its randomization, this method surpasses all previous methods of the mechanical selection of the cards.

In all, 2,302 runs were conducted in two major series; one for relatively immediate checking (a two-day delay), and the other involving predictions covering a ten-day period; that is, subjects wrote down their predictions of the order of decks of cards as they would be after the shuffling took place two days or ten days later. When the

[1] The assistance of the following persons in the conduct and reporting of this research is gratefully acknowledged: Marion B. Bragg, my secretary at the time the research was done, who prepared the materials and assisted in the checking; Betty M. Humphrey, graduate research assistant in the Laboratory, who conducted or supervised the analyses; Dorothy H. Pope, general secretary to the Laboratory, who assisted both in the experiment and in the preparation of the manuscript; Dr. J. A. Greenwood, mathematical consultant to the Laboratory, who contributed the mathematical aid essential to the analyses; my wife; and my colleagues of the Laboratory staff, who have read the manuscript and have made many valuable suggestions for its improvement.

check-up was made, it was found that the predictions were not significantly above expectation (chance); but when they were tested for salience of deviations—a test which I introduced in a paper in the September, 1941, issue of the JOURNAL—significant evidence of extra-chance relations was found. By salience is meant either the standing-out of the ends of the run (or a segment of the run) in rate of success as compared to the middle—this is called terminal salience; or, conversely, the standing-out of the deviations of the middle of the run (or segment) as against the ends—this would be middle salience.

This salience effect was found in the earlier study to have appeared not only in the run as a whole, because of the peculiar hit-distribution according to position in the run, but a similar effect was found in the five-trial segments of the run; in fact, the most significant feature of this series was the covariation between the salience ratios (which measure salience) of the run, and those of the segment for the same series. It was likewise true of the transoceanic ESP series reported in the preceding JOURNAL by Rhine and Humphrey. This covariation between the ratios of the segment and those of the run is again the most significant feature of the present report, though there are other statistical supports for the extra-chance character of the work.

For instance, there is a significant covariation found between the two main divisions of the work based upon the adult and child subjects who took part. There is also a U-shaped curve obtained by pooling the deviation distribution of the run for the entire work, which confirms the numerous earlier reports of U-shaped curves found in results of tests conducted with similar test procedures. This fact is a practical or common-sense assurance of a continuity of effect running through investigations of this type from the first report of this Laboratory in 1934 to the present paper, but it is not a statistically reliable one.

The results seem to constitute evidence that the occurrence of ESP is not limited by time, but that it may reach into the future, at least up to the extent of the ten-day period which these tests covered. In fact, the covariation upon which the conclusion was based was no less marked when the ten-day interval was introduced. The next issue of the JOURNAL will report an independent but similar series which bears likewise upon the question of time in relation to

ESP. Other later papers will go into the question of the nature and distribution of the salience effect itself.

Introduction

The work I am about to report has two main features: The first is its concern with the hypothesis of precognition or the knowledge of uninferable future events obtained by means of extra-sensory perception; the other is salience, or the standing-out of certain parts of a run as showing a more striking rate of success than the rest—i.e., greater deviation from mean chance expectation. (Thus, if the ends stand out, the run shows "terminal salience"; if the middle stands out, "middle salience" is shown.) It will be convenient to introduce these two features separately.

Status of Precognition

In recent years a considerable amount of investigation has been done to test the precognition hypothesis, and at the present time the problem of whether future events can be apprehended is much nearer solution than before. As a result of the researches in England of Saltmarsh (12), Tyrrell (14), and Carington (1), and in this country those of Hutchinson (3), Stuart (13), and myself (6, 7), the issue as to whether precognitive ESP occurs has been greatly narrowed. Still other work from this laboratory bearing upon the subject has been completed and is on the way to publication.

Of course the work done is not all of equal weight as judged by present standards, nor is all of it statistically significant (6: pp. 50-53). Moreover, it is generally recognized by the investigators themselves that so difficult a question as precognition requires an extraordinary amount of inquiry before a final decision can be given. Saltmarsh's study was valuable, but it was not an experimental one. It consisted of a comparative treatment of a collection of case studies. Tyrrell's experimental study of predictions based on the operation of an electrical machine would appear somewhat more conclusive if a cross-check of subjects' choices and mechanical selections were possible for the purpose of checking on the question of non-ESP similarity of patterning. Carington's case for precognition rests upon the finding that subjects, in attempting telepathically to repro-

duce drawings hung in his (Carington's) study, anticipated to a better-than-chance degree the drawings of future tests. It is still possible, however, that the judge who matched the drawings, and upon whose judgments the significance of the results depends, may have exercised ESP ability himself, without involving precognition.

But it is not only the English work which still lacks something of perfection in establishing a final case for the operation of precognition. My own experiments, which grew out of the ESP work of the early 30's, began with a procedure in which cards were called before being shuffled. The shuffling was at first done by hand by the experimenter. Later on it was recognized that the experimenter might actually use ESP in placing the cards to advantage in the shuffling; that is, the shuffling might be guided by knowledge of the cards obtained through ESP. The work of Smith, Woodruff, and myself (10) which followed showed in fact that this "ESP shuffle" was a possibility. Accordingly, my next paper met this counter-hypothesis with the report of work in precognition tests which yielded significant results obtained with mechanical determination of the card order. Also, both Hutchinson and Stuart reported results that were obtained in experiments utilizing the shuffling machine for the selection of the cards.

The fact that three such researches agree would appear to be sufficient to establish the conclusion that significant pre-shuffle card-calling can occur even with mechanical selection of the cards. The question, then, is whether this establishes the occurrence of precognition. Obviously it cannot do so as long as there are any reasonable alternatives that can explain the data equally well; and there are at least two of these that seem worthy of consideration. First, it might be supposed that some patterning occurred in the selection of the cards as a result of the mechanical rhythm, the warping of cards, or perhaps unknown causes other than ESP, and that the subject, consciously or otherwise, coincided in his choices with these patterns to furnish significant results.

The second counter-hypothesis would be that in some way, not necessarily known to science, a mechanical effect is exercised on the distribution of the cards in the shuffling machine, as a direct result of the effort of the subject or experimenter, or both. This would be a psychokinetic effect (cf. "telekinesis").

In the case of Stuart's work (13), the finding of the evidence that favors the precognition hypothesis was anticipated neither by the experimenter nor the subject. It was not discovered until long after the experiment had been completed; therefore, neither the psychic shuffle nor the psychokinetic hypothesis would appear to have any application.

Nevertheless, it seemed best to modify the procedure still further and attempt to determine the card order (to be precognized) by methods which ruled out the counter-hypotheses that are left—rule them out at least as far as possible. Therefore, after considering a wide range of possible means of selecting cards, I decided to use some obviously uncontrollable event in nature as the basis for securing random order, something which the wildest imagination could scarcely conceive as modifiable by the purposive attitudes of the subject or the experimenter; namely, the daily temperature. The local maximum and minimum temperature figures for a day specified in advance were therefore used to determine the selection of the cards in the experiment now being reported. The details for the procedure will follow later.

Salience

In the first specific study of salience (8), I mentioned work which showed that when the DT test (calling down through the deck without removing the cards) was used, a higher rate of deviation from expectation was obtained at the beginning and end of the run than in the middle. The hit distribution, when plotted in five points for the run, accordingly gave a U-shaped curve. These curves were reported in 1934, and similar curves were later found by others. There were exceptions, but even with the exceptions there was the general feature that the ends of the runs tended to show greater deviation from expectation (in one direction or the other) than the middle section. This standing-out of the ends, or "terminal salience" as it is called, appeared from the data available to be a general, though not an invariable, effect of the DT test.

The DT work which I reported in the September, 1941, issue of this JOURNAL likewise gave a U-shaped curve, though in this case an inverted one (as should be expected in view of the fact that the total deviation was negative).

The earlier experiment was designed to vary the amount of terminal salience in the run, and also to induce a similar effect in the segments of the run that were produced by interruptions introduced at intervals of every five trials. The effect is measured by the salience ratio (SR).

The principal result, however, was the significant relationship found between the SR's of the run and those of the segments, suggesting that the salience shown in the one is related to that of the other. A similarly significant covariation was obtained between the corresponding SR's of the children's subdivision of the experiment and those of the adult. The results suggested that a patterning of the distribution of hits is to be expected under the conditions of these tests, that this patterning is a matter of position of trials in the run and is in reality the reaction of the subject to the structure of the run as a whole.

An even more conclusive instance of segment-run covariation between SR's occurred in the transoceanic experiment reported by Rhine and Humphrey (9) in the preceding issue. This distance series was not a part of the sequence to which the first salience paper and this one belong, but it contributed heavily to the same total effect. However, since the report of it was not written until after the present paper was completed, reference to "earlier salience work" does not include the transoceanic work unless specified. In any case, except for the general effect (of evidence of ESP through segment-run covariation of SR's), the distance series referred to does not articulate especially with the series of articles of which this is the second.

The work I am reporting herein is a continuation, under altered conditions, of the experimental work described in my first salience article. And to many students of the ESP field of investigation, the most interesting feature of the report will be the fact that in spite of the switch that was made to the precognition procedure, the salience effect of the earlier work continued to manifest itself throughout the present study to a statistically significant degree.

The Plan of the Experiment

In conducting the experiments concerning salience I had in mind devoting about 1,000 runs to each general project. The first work

reported (8) stopped at 1,114 runs. The experiment was done in "batches," the exact size of which could not conveniently be predetermined; and the nearest stopping point after 1,000 was considered the end of a main series. No checking or evaluation was carried out until *after* the close of the batch or Subseries.

Following the completion of the DT project already reported (8), I decided to undertake the transition to PDT, or precognitive DT tests. This was done primarily in the interest of attempting further advances in the investigation of the precognition hypothesis with which my own investigations have been concerned since 1933; but it seemed of primary importance to find out if salience effects that had been obtained throughout the DT research would continue with the PDT. It was recognized as a possibility that the salience might be important even if the CR's were not. The essential difference between the precognition tests and the DT tests lay in the fact that in the former the order of the target deck of cards was not, at the time of the subject's calls, in existence. While this difference in procedures seems radical indeed from one point of view, there is little difference from the viewpoint of what the subject has to do.

A series of 1,169 runs was conducted, for the most part during the month of April, 1941; and upon the completion of this series of PDT, checked within two days, a ten-day PDT series was undertaken (i.e., ten days elapsed between the time of the subject's predictions and the date of the checking-up), which ran through the month of May and contributed a total of 1,133 runs. This brought the research to the close of the school year; and with the breaking up of the circle of subjects who had participated throughout the three general series (DT, PDT, and ten-day PDT), there occurred a necessary (as opposed to optional) stopping-point for the research.

The separation of subjects into two groups on the basis of age was continued here as in the DT series of my previous report. Accordingly, the headings Child Division and Adult Division will be continued in this presentation. (See Figure 1 for a diagram of this experiment.) For convenience of reference back and forth to my first salience series (DT) I shall designate the latter as Series I and the two PDT series of this report as Series II and III.

Both of these Series (II and III) will have, as before, two Sections, making four Sections for each Division (Child and Adult),

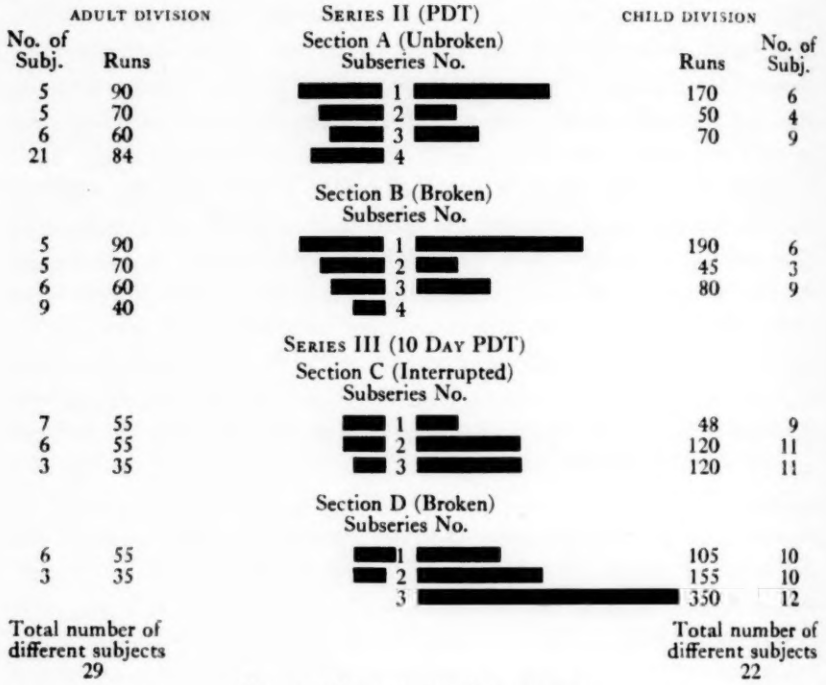

FIG. 1.—DIAGRAM OF EXPERIMENT

or eight in all. These Sections will be designated as A, B, C, and D. They will represent the special conditions of the experiments. They need not be described here except for purposes of identification. In Series II, there is Section A with unbroken runs, and Section B with segmented runs. In Series III there is an interrupted Section, C, in which the run is not only broken into segments but is interrupted by the introduction of other tests at the points of interruption. Finally, there is the D Section which is the same as Section B with the run simply segmented or broken into five parts.

The Sections, as has been stated, represent the different test conditions. The final step in subdivision is the Subseries, of which each Section has from two to four. These Subseries represent the actual steps in which the research is done. Each Subseries includes all the records that were available at the time of the check-up. The number of runs varied from 35 to 350 runs according to the number of subjects available or the interest taken in the test. There was no effort made at limiting this range beyond seeing that not less than 25

runs were made by each group, Adult and Child. As stated above, all the predictions were made before any checking was done on the Subseries. A period of two or (if a Sunday intervened) three days was allowed for conducting the tests making up a given Subseries; the first day following was specified in advance for the checking-up of Series II, and ten days later for Series III. Each batch of records at the time of check-up was divided, on the basis of the age of the subjects, into the Child and Adult Divisions. For most of the Subseries, both Divisions were represented.

Conditions and Procedures

When the transition from the DT work of the earlier report to the PDT tests of this one occurred, I did not know precisely what the results of the DT series were with respect to terminal salience. Only general impressions could be had from the hit-distributions, and it was not until several months later that the statistical work was sufficiently completed to enable comparison of the various test conditions for the amount of terminal salience shown. There was not even available during the period of the research here reported any such measure of salience as the salience ratio. This situation will explain why the conditions and procedures selected for the PDT experiments do not in all points agree with what would have been suggested by the results of the DT work when they were finally known.

Subjects

With but few exceptions, the PDT work was carried out with the same subjects who had participated in the DT experiment. There were 22 children from the local community and 29 adults from the Parapsychology Laboratory staff and its circle of friends. The ages of the child subjects ranged from 6 to 13 and those of the adults above 20. From 3 to 21 subjects participated in each of the different Subseries, with the average per Subseries between 7 and 8. No analysis has as yet been made on the basis of individual performance, and no further description of subjects is needed for the understanding of the results. The study of individual production of salience is in prospect for the entire work in the course of time.

Materials

The commercial ESP cards were used, each deck containing five of each of the five ESP symbols. In Section C of Series III where the run was interrupted either by numbers or by drawings, decks of cards containing these other symbols were used. The present report, however, does not deal with the results obtained on the interrupting trials and is concerned solely with the results on the ESP symbols; accordingly, no further description of these number and drawing cards will be given.

It has already been mentioned that special record sheets[2] were provided for the different types of tests. The record sheet used in Section A was designed simply to have an unbroken run; the 25 spaces are not in any way interrupted or divided into segments. The record sheet used in Sections B and D provided for the breaking up of the run into 5 columns of 5 spaces each, parallel to each other. Finally, the record sheet used in Section C had spaces for the interrupting numbers or drawings between the 5-trial segments of ESP symbols; hence they consisted of, first, a large space, then 5 smaller ones, then a large one and 5 small ones, and so on to the end of a column having 30 spaces in all.

Procedure

The subjects were given the appropriate record sheets and were asked to identify the symbols which would later be printed (hectographed) in the right-hand spaces of each column. As the subject decided on each symbol, he was to record it in the left-hand column opposite. Instructions were given to fill out the spaces in consecutive order.

The subjects were allowed to do as many runs as they wished. The record sheets were collected and held for the day appointed for checking-up. The experimental period during which the tests of a given Subseries were made lasted over two (or, if one was a Sunday, three) days.

At the time of checking, the record sheets were divided into two groups according to the ages of the subjects, and all the runs involved in a single check-up constituted a Subseries, one for the Adult Division and one for the Child. No record sheets were accepted for a given

[2] Specimens are obtainable on request from the Parapsychology Laboratory.

Subseries unless they were made out prior to the day on which the checking was to be done.

After all the record sheets were in my possession, I asked Mrs. Marion Bragg, who was my secretary at that time, to prepare and record the target card order against the subjects' record sheets, and thus the preparation of the card order was duly independent.

Mrs. Bragg's instructions were as follows: "She will obtain from the morning paper of that day (Durham *Herald*) temperature extremes of the day preceding. The smallest digit of these two numbers except zero will be taken as the number of revolutions each deck (of total of 5) will be given in the mechanical shuffler. The total obtained by all the digits of these temperature numbers will give a number which will be used to cut a deck of 25 numbered cards. The number turned up on this cut will be the number to be used in cutting all the decks after they have been taken from the shuffling machine. If the addition of the temperature digits exceeds 25, then 25 will be subtracted from the total and the balance taken to cut the numbered deck. The card record will be hectographed on the subjects' record sheets and the check-up made as usual."

It might be added that where the run was to be interrupted by numbers and drawings, provision was made for the selection of these by the same procedure.

Since there were only five decks of cards used it was necessary when a subject did more than five runs to use the record more than once. That is, if a subject called 10 runs, there would be two runs checked against each of the five decks. The subjects, however, did not know of this; they had only a very general conception of how the checking was done.

When the hectographing was completed, Mrs. Bragg and I checked the records in each other's presence. In order to make the records independent, there was no record left on the sheet of the first checking-over of the hits or the scores; but a separate record of the score was made. The person who made the second check-up marked the hits and recorded his scores on the record sheets. At the completion of the check-up the two checkers compared the scores, and if a difference was found the run concerned was re-checked jointly by both.

Mrs. Bragg then made a hit-distribution for the Subseries ascer-

taining the total number of hits for each position in the run. This was checked against the score total itself and, failing agreement, was re-checked. It may be recalled that an independent check was made on 111 runs of the DT in Series I. This re-check was made by Miss Betty Humphrey without the discovery of a single error in the distribution record made by Mrs. Bragg.

General Conditions

The adults worked individually in filling out their record sheets, but the children took the tests in groups. They were assembled for what was known as an "ESP party," at which light refreshments were served.

Rewards were given for high scores to both children and adults at the rate of ten cents for a score of eight, fifteen for a nine, twenty-five for a ten or above, or the equivalent value in some other form. Rewards were offered also for the successful identification of the drawings and numbers, when they were used (Section C). In the case of the adults, the rewards were handled only half-seriously; as a rule movie tickets were offered the subjects instead of cash rewards.

So far as I could observe, there was no noticeable difference in attitude toward the PDT test from that shown toward the DT. When the ten-day PDT was introduced, some children expressed impatience at having to wait so long before discovering what their rewards were to be. The precognition hypothesis itself did not, at any rate, occasion the children any concern. The adult group had for the most part a fair amount of interest in the hypothesis, mainly from a professional point of view.

Methods of Analysis

In view of my interest in the terminal salience feature, my attention was focused primarily upon the hit-frequency distribution, which was, as stated above, determined after the checking of each Subseries. This was as far as the study of the hit-distributions went at the time. Later on, when the statistical analyses were undertaken, the salience ratio for each Subseries was computed. The description of the method of computing these need not be repeated here in detail since it was given in my earlier paper (8). Moreover, the mathematics of the evaluation of the ratios was described in the article (2) by Dr. J. A. Greenwood accompanying the first report.

The following brief outline of the steps in the procedure may, however, help the reader who does not wish to go back at this point to the original detailed account. After the distribution of hit-frequency through the run was plotted for the entire Subseries, the following steps were taken: First, the number of hits for the run as a whole were concentrated in five subtotals, one for each of the five segments of five places each. This gave the distribution of the run in five hit-totals. Second, the same hit-distribution was recombined in a different way, within the segment itself, by taking the first trial in each of the five segments and totaling these, and then taking the second trial in the segment for each of the five segments and arriving at a second total, and so on through the five trials of the segments. Thus, five *different* sub-totals were obtained, giving, when graphed, the curve of distribution of hits in the *segments*.

Since most of this paper is concerned with the relation between these two curves, it is worthwhile to labor at making the point clear. This can be done best by imagining the run of 25 trials broken into five sections which are set upright alongside each other. The 25 spaces are, then, 5 by 5 in distribution. Now if the total hits for each segment column are obtained the five totals give the distribution *in the run*. If, however, the hits are counted horizontally across to the right there are obtained for the five places in the segments five new totals which give the distribution *in the segments*.

The third step, then, is to find out how these two sets of hit-totals deviate from expectation. Expectation is simply 1/5 of the number of trials represented in each of the totals. The difference between the total hits for each position and the expectation is the deviation, and this may be either negative or positive. For a *measure* of this, the salience ratio is found. Each deviation is divided by the standard deviation, the CR obtained, and this is squared. This CR squared is the chi-square with one degree of freedom, and when this is obtained for each of the five positions in the run and the five in the segment there is only one step remaining; namely, to obtain the salience ratio or SR. This ratio is the sum of the two end chi-squares (which are in 1st and 5th positions) divided by the sum of the three middle chi-squares (2, 3, 4). If there is no salience, this should come out exactly 2/3. The ratios may theoretically vary all the way from zero to infinity. (Salience must not be taken to mean significant

salience; it is a descriptive term, not a statistical value. The value depends upon the probability of its occurrence by chance or upon its relation to other examples of salience.)

The salience ratio for the run is the RSR and that of the segment, the SSR. These may then be evaluated in terms of the probability of the occurrence of these (or greater) SR's, and relations between them may be determined by covariation statistics for which a CR and a corresponding probability may be found. (See Greenwood's article for the procedures.)

Results

Critical Ratio Method

A total of 1,169 runs of 25 trials each were completed in Series II, and 1,133 runs in Series III; together these make a total of 2,302 runs. These totals, as well as their subdivision into Adult and Child Divisions, are shown in Table 1 along with the deviations and CR's. It will be noted there is no significant deviation found for any of the subdivisions or totals presented. As evaluated *by this method*, then, the entire work of the two Series may be ascribed to chance.

Table 1

Results Evaluated by the CR Method

Series	Adult Division			Child Division			Both Divisions		
	Run	Dev.	CR	Run	Dev.	CR	Run	Dev.	CR
II	564	− 9	.19	605	+49	1.00	1169	+40	.59
III	235	−34	1.11	898	+ 5	.09	1133	−29	.43
Total	799	−43	.76	1503	+54	.70	2302	+11	.11

The Salience Ratio Method

In my earlier paper on terminal salience, I suggested that the SR might be a useful measure of extra-chance relations in ESP data. The earlier report was itself an instance of this application of the SR, and the present report is another. With but few exceptions, the SR's are not themselves statistically significant; but their relations to each other as measured by the covariation statistic are, in several different ways of grouping, reliably different from expectation on the chance hypothesis.

The SR's will first be presented in Table 2, which gives both the SSR's (segmental SR's) and the RSR's (run SR's). In part A of the table are the SR's for the two Divisions computed for all the Subseries, in part B for the Section, in part C for the Series, followed by the grand total SR's.[3]

Table 2

SALIENCE RATIOS AND THEIR PROBABILITIES*

A. BY SUBSERIES

ADULT DIVISION							CHILD DIVISION						
Subser. No.	Sect.	d.f.	SSR	2P	RSR	2P	Subser. No.	Sect.	d.f.	SSR	2P	RSR	2P
1	A	2,3	1.53	.50	.53	.94	1	A	2,3	1.88	.41	.22	.52
2		"	.49	.90	.10	.27	2		"	6.22	.10	1.49	.51
3		"	.19	.46	.06	.17	3		"	.28	.62	94.75	.0024
4		"	.16	.40	.60	.99							
1	B	2,3	.67	.90	2.87	.26	1	B	2,3	10.31	.05	1.21	.61
2		"	1.13	.64	.22	.52	2		"	1.41	.53	1.38	.54
3		"	.01	.03	7.42	.08	3		"	.10	.27	1.55	.48
4		"	2.65	.29	1.09	.66							
1	C	2,3	3.83	.19	.36	.74	1	C	2,3	.41	.81	181.00	.001
2		"	.02	.06	.30	.65	2		"	.39	.78	.14	.38
3		"	.23	.53	.05	.14	3		"	2.42	.30	.68	.92
1	D	2,3	.10	.27	.03	.09	1	D	2,3	.53	.94	6.83	.09
2		"	2.19	.35	.14	.38	2		"	8.92	.06	.56	.99
							3		"	8.93	.06	1.46	.52

B. BY SECTIONS

A	8,12	.67	.96	.22	.12	A	6,9	1.51	.20	1.36	.32
B	"	1.01	.50	1.52	.19	B	"	.95	.61	1.33	.34
C	6,9	.23	.21	.16	.10	C	"	.57	.88	1.53	.25
D	4,6	.35	.55	.08	.06	D	"	2.35	.09	1.25	.38

C. BY SERIES

Series						Series					
II	16,24	.75	.77	.56	.73	II	12,18	1.21	.25	1.34	.18
III	10,15	.28	.17	.13	.01	III	12,18	1.07	.35	1.32	.19

D. BY TOTAL

Total	26,39	.49	.33	.34	.08		24,36	1.13	.15	1.33	.06

*2P means that all P-values have been multiplied by 2, to allow for deviation of SR's in either direction from .667.

[3] Somewhat fuller information may be found in the supplementary tables obtainable in mimeographed form from the Parapsychology Laboratory.

The probability for each SR is also to be found in this table. In this research both low and high SR's are of interest. It will be noted that the SR's for the Sections (see Table 2, B) of the Adult Division showed a very definite decline from above the no-salience point of .667 in Series II to very low negative SR's in Series III. Since it is quite conceivable that there is some extra-chance factor responsible for this, it is essential that an evaluation of the probability of these low ratios be effected. This is correctly obtained by computing the P of the SR, and by the multiplication of this P by two to allow for both directions from the median.

Before going on to the study of relations between SR's, there is another study of SR's—a variation—to be introduced as a part of the results. In all of the SR's presented thus far, wherever the SR represents a Section or Series or Division made up of a number of Subseries, the SR in question was obtained by the addition of the chi-squares of the deviations (in the five point distributions described above) of the Subseries making up the larger group. In no case was it made up from the pooled deviations themselves. This accounts for the larger number of degrees of freedom for Section, Series, and

Table 3

SALIENCE RATIOS COMPUTED FROM POOLED HIT DISTRIBUTIONS*

Section	ADULT DIVISION				CHILD DIVISION			
	SSR	2P	RSR	2P	SSR	2P	RSR	2P
A. BY SECTIONS								
A.....	3.09	.24	1.47	.51	17.00	.03	4.70	.15
B.....	.68	.92	.43	.83	1.93	.40	3.40	.22
C.....	.58	.99	.09	.20	.27	.60	.45	.85
D.....	.03	.09	.06	.17	10.05	.05	.87	.78
B. BY SERIES								
Series								
II.....	.05	.14	.33	.70	9.04	.06	3.63	.20
III.....	.33	.70	.07	.19	.75	.86	.87	.78
C. BY TOTAL								
	.14	.38	1.39	.54	8.11	.07	1.27	.59

*The degrees of freedom for all SR's of this table are 2 and 3. All P-values are multiplied by 2, as indicated by the column headings "2P," in order to allow for the usage of SR's both above and below .67; i. e., of either sign.

Division. Now, however, I shall present the results from the pooling of deviations of the Subseries into Sections, the Sections into Series, and Series into Divisions. In this pooling the results are thrown together just *as if there had been no subdivision* into the smaller groupings. In Table 3 may be found the SR's for Section, Series, and Division. These, of course, are not independent of the SR's obtained above (as in Table 2) by combining the chi-squares, but they support them, and since they are different, they afford a basis of comparison with SR's obtained from the combined chi-squares in the study of relations between SR's. The comparison will appear in the covariation treatment to follow.

The U-shaped Curve

When the five-point hit-distribution for the run is made up for the entire 2,302 runs, the solid line curve to be found in Figure 2 is distinctly U-shaped—more specifically V-shaped—with the middle point far over on the negative side of the mean. The fact that this curve is in general conformity with preceding work of similar character warrants attention.

In the broken curve of the same figure is shown the distribution of the entire work as it is found in the segment. This does not present a characteristic U-shaped curve. However, there is some similarity since only the first point deviates from the U-shape. (The actual shape of these curves is, of course, not part of the main thesis of the paper, but serves to illustrate the form which the salience is taking.) These curves are not significantly similar as measured by the coviariation of CR's of corresponding points.

Interrelations Among SR's

In the first report on salience it was shown that when the SSR's and RSR's were paired on the level of the Sections and the Series and evaluated by the covariation statistic, the result was clearly extra-chance in character. This set of relations is borne out or confirmed in the present report. It will be seen from the succession of Tables (4 to 6) to be presented in this section that the SR's of the segments are significantly covariant with those of the run. Even when the SR's are computed from the pooled hit-distributions of the Subseries instead of combined from the chi-squares, the above findings are borne out on this basis as well.

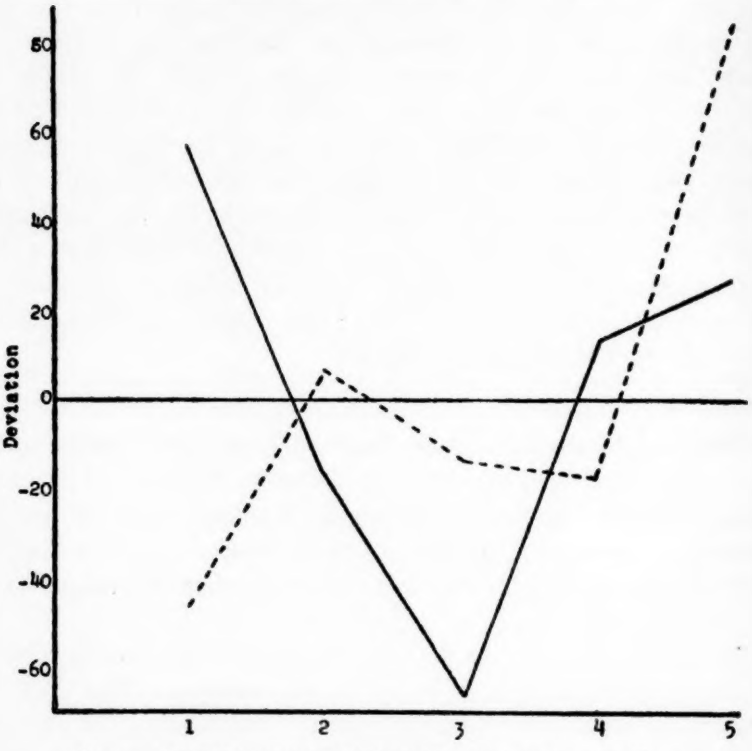

FIG. 2.—DISTRIBUTION OF POOLED DEVIATION FOR THE DATA OF THIS REPORT

(Solid line shows the deviations pooled for the 5 segments of the run; broken line shows deviations for the 5 trials in the segments.)

The question of the independence of SSR's from RSR's on a theory of chance was considered in the preceding salience reports, and the discussion need not be repeated here. The conclusion was stated there that the two measures are independent of each other. At the same time the matter was subjected to empirical check in this Laboratory, and a report was published by Pratt, Humphrey, and Rhine (5) in the preceding number, which bore out the logical position taken. The check was based on a random reshuffle of the hit-distribution of the DT results of Series I. It gave only chance covariation CR's for the SSR-RSR relation.

SSR-RSR Covariation. In the covariation treatment of SSR's in relation to RSR's as shown in Table 4, significant probabilities are

obtained, not only on the Division level where P = .0047, but on that of the Series as well. (P = .0044.) On the Section level it is nearly so (P = .014). Only on the level of Subseries is the result clearly insignificant. It will be recalled that in the earlier study the Subseries level was also lacking in statistical significance. This exception is itself important, and will be discussed below.

Table 4

COVARIATION BETWEEN SSR'S AND RSR'S*

Subdivision	No. of Pairs	CR	P
A. Divisions	2	+3.05	.0047
B. Series	4	+2.91	.0044
C. Sections	8	+2.57	.014
D. Subseries	25	+ .11	....

*The SR's on which the covariation is based may be found in Table 2.

It should be noted that SSR's and RSR's are *positively* covariant, as they have been throughout the study, beginning with Series I (the DT work). Even when, as will be shown below, the adult subjects went into reverse as regards terminal salience and began producing middle salience SR's (below .667), the decline involves both SSR's and RSR's, and the two vary together.

Pooled Deviations. When the deviations of the hit-distribution for the Subseries are *pooled* for the Section and the Series, and the salience ratios are computed from these *pooled* groupings of a larger order, the result of the covariation analysis of SSR's and RSR's closely resembles what was presented above from the usual method of adding chi-squares. While this is not an independent measure, the result could very well have been otherwise; that is, significant covariations could have been obtained by either method of combining without the other being significant. Accordingly, there is strong support in this evidence from pooled deviations for the case of the extra-chance nature of SSR-RSR relations in the present study. The fact that both of them yielded significant results has a further bearing which will be discussed in the section to follow.

The CR's of the covariation between SSR's and RSR's derived from the pooled deviations are, as Table 5 shows, positive. At the level of pooling by Sections the probability of .0016 is significant, while that of the Series level (.017) is not quite so.

Table 5

COVARIATION BETWEEN SSR'S AND RSR'S FROM POOLED HIT-DISTRIBUTIONS*

Subdivision	No. of Pairs	CR	P
A. Series	4	+2.17	.017
B. Sections	8	+3.02	.0016

*See Table 3 for the SR's on which the covariation computation is based.

Finally, there is an intermediate variation to this study of relations between SR's which again is not independent but which adds information as to the extent and continuity of the interrelations between SR's. I refer to the combination of pooling and chi-square combination presented in Table 6 in which the deviations have been pooled for each Subseries up as far as the Series; that is, the hit distribution for the run and that of the segment are obtained for the *pooled deviation* distribution of each Series for the Child and again for the Adult Division. This gives the same four pairs of covariants on which part B of Table 7 is based. At the Series level, the distributions are combined by the chi-square method for the two Series, giving one pair of covariants for each Division. The covariation of these two pairs between SSR and RSR for the two Divisions is as usual positive, and is significantly so with a CR = + 2.63 and P = .0078. This is in line with and supports Tables 4 and 6, though there is interdependence between the data of all three.

Table 6

COVARIATION BETWEEN SR'S FROM DISTRIBUTIONS POOLED UP TO SERIES, THEN COMBINED BY CHI-SQUARES

Grouping	Covariation	No. Pairs	CR	P
Total (II and III)	SSR vs. RSR	2	+2.63	.0078

Child-Adult Covariation. In the report on Series I the SR-covariation between the Child and Adult Divisions was a significantly positive one. (This covariation is found between the Child SSR for a given Subseries and the Adult SSR for the same Subseries. These make one pair. The RSR's [Child and Adult] make up another pair.) In the present report there is a *negative* covariation CR, one that is significant only in the case of the total research

(Series II and III combined). In this instance a CR of −3.27 is found, which has a P-value of .003. Because of its negative sign, this is, however, to be multiplied by 2, giving .006. This is done because there was no *a priori* expectation as to the sign of Child-Adult relations. Table 7 gives the results of the covariation treatment, and gives a column headed 2P for the P-values corrected to allow for the use of deviations of both signs. The other three treatments are not significant, but the relation shown is of the same sign; those of the Series and Section are more or less suggestive, and support the covariation CR of the total, while not being wholly independent.

Table 7

COVARIATION OF SR's BETWEEN CHILD AND ADULT DIVISIONS*

Subdivision	No. of Pairs	CR	P	2P
A. Total	2	−3.27	.003	.006
B. Series	4	−2.19	.02	.03
C. Sections	8	−1.50	.06	.13
D. Subseries	22	− .11	...	...

*The SR's are found in Table 2.

While the Child-Adult SR-covariation does not in itself constitute a strong case against chance, it has favorable bearing on the conclusion reached from the SSR-RSR relations; namely, that there is in the present SR interrelations an extra-chance factor in evidence.

DISCUSSION

Alternative Hypotheses

Chance. In Series I, on which my earlier report on terminal salience was based, it was the SSR-RSR covariation which gave the principal evidence against the chance hypothesis. The same is true in the present work based on Series II and III. While the P-values were slightly lower in the earlier report, they are acceptably significant in this one. In the earlier work there was insignificant covariation on Subseries. The same is true again in the present work. In the earlier Series the probabilities for Division, Series, and Section levels were .02, .002, and .002 respectively. Comparable figures for the present case are .0047, .0044, and .014. These results constitute a clear confirmation of the extra-chance relation obtained in the earlier terminal salience investigation.

In the earlier study (Series I) the total deviation also gave a significant CR. In the present case that is not so. Likewise in the earlier study there were more significant P-values to be attached to the SR's themselves than is true in the present instance. The total SR's for the precognition study, while more or less suggestive, especially in the RSR's ($2P = .08$ and .06), are not significant. However, the pooling of the hit-distributions gives for the Section SSR-RSR covariation a P-value of .0016 and for the Series, .017. While these are not wholly independent, they are of considerable support to the case against chance. Even the Child-Adult SR-covariation, which in the present study becomes a negative relation, is on the whole highly suggestive, especially with a significant CR on the Division level.

While, then, there is confirmatory assurance in certain other measures, the main case for the rejection of the chance hypothesis rests upon the SSR-RSR covariation—based either upon the combination of chi-squares or upon pooled hit-distributions or both. But this report by no means stands alone; for, inasmuch as the present work confirms the earlier evidence of significant salience relations, the earlier reports—both that of Series I and that of the distance work—are likewise of comparable support to the evidence of terminal salience in the present study. (The analyses now under way on a later unpublished study that will be called Series IV give still further confirmation.) While, therefore, the present study could stand alone, the plausibility of its case against chance is considerably reinforced by the bordering studies that confirm its main thesis. It seems reasonable, therefore, to dismiss the chance hypothesis as inadequate to explain the salience relations reported.

Call-Patterning and Preferences. When a number of runs of calls or predictions are checked against the same run of symbols (or deck of cards), the question properly arises as to whether similarity of call-patterns may not sufficiently coincide with the accidental distribution of the symbols so as to produce either unusually high or unusually low scores or score totals. This would be most plausible as an hypothesis where the several runs of calls were made by the same subject. Now in the work here reported, the 2,302 runs of calls or predictions were matched against a total of 70 decks of cards (5 for each of the 14 Subseries); and in all, 51 subjects participated. The

number of runs for the same subject to be checked against the same target series of symbols varies from one to an exceptional maximum of 15. Since the deviation is insignificant, it could only be in its bearing upon hit-patterning in the segment or in the run (and consequent covariation CR's) that the hypothesis would be of interest.

The series of assumptions involved in this hypothesis would be: first, that under such conditions call-patterning by the same subject or group of subjects would likely occur; further, since we are concerned here only with hit-patterning, or the distribution of success according to position in the run, there would have to be some connection between call-patterning and hit-patterning. This would assume that there was inadequate sampling for approximation to chance in the 70 target decks used. Next, it would have to be supposed that the extra-chance coincidence that occurred between the 70 target series and the repeated runs of calls was so distributed by position that there was agreement (enough to give significant covariation) between the distribution in the run and in the segments.

Obviously, that all this would occur together is very improbable but not impossible. Strong support for its improbability may be had from the large chance-matching series by Greenwood,[4] in which twenty call-runs by each of five subjects, making one hundred in all, were matched against each of two hundred decks of cards. The results approximated the binomial distribution in all four moments. Had the hit-patterning to be expected on the hypothesis under discussion been considerable, most probably the scores would have been affected and the variance increased to the point of significance.

The best assurance, however, that this general hypothesis of hit-patterning does not apply to the findings reported may be had by turning to the data themselves. By far the greatest multiplication of the call-runs, as against the target-runs, occurred in Series III, Child Division. Here there were 898 runs checked against 30 decks. In the same Series, but in the Adult Division, there were 235 runs checked against 25 decks. There were thus roughly about three times as many call runs to each target run for the Child as against the Adult; and yet we find that it is the Adult that gives the higher covariation between SSR and RSR (covariation CR of 2.22 as against 1.16). This is a sufficient basis for concluding that the call-patterning

[4] See this JOURNAL, 1938, **2**, 138.

hypothesis cannot be accepted as an explanation. (Series II is not included in this example because the number of runs was approximately the same for both Divisions.)

Errors. In view of the measures taken to avoid errors—measures such as independent checking, independent computation of all essential mathematics, and the keeping of two records of results—it does not seem reasonable to suppose that any appreciable error could exist. Beyond these measures I think one is hardly justified in attempting to go in an individual research; and of course this report is itself but one in a fairly lengthy series of mutually confirmatory studies now published and in preparation.

Shuffling. The extra-chance effects that appear in these results cannot be accounted for by any hypothesis involving peculiar pattern of shuffling of the decks by the shuffling machine. The random superimposed cuts, based upon the daily temperature figures of the weather, would have come in as a correction upon any such mechanical effect, if it had occurred.

Psychokinetic Effect. Likewise, the effect of the use of the "temperature cut" was intended as a corrective to exclude the hypothesis that any direct psychokinetic effect could be exercised, either by the experimenter or by the subject, upon the mechanical shuffling of the cards. The temperature itself would have had to be modified by the hypothetical effect in question, and this seems considerably more incredible than that the machine might be influenced directly.

Salience. The extra-chance character of the results depends solely upon the SR method of measurement; that is, upon salience. Moreover, most of the evidence of non-random determination is contributed by the interrelations between SR's—mainly the segment-run relation (SSR to RSR) with support from the relation between the Child and Adult SR's.

SSR-RSR Relations. With respect to the question of salience, the outstanding result of the present study is the significant covariation between SSR's and RSR's. The continuation through Series II and III of this interrelation already found in Series I provides an instance of confirmation that is unusual in this uncertain field of research. Again, too, it was the covariation of SR's on the level of the Divisions, Series, and Sections that gave the more significant CR's and P-values. It was the SR's of the Subseries in both reports

that gave insignificant covariation. The fact that when the SSR's and RSR's are computed from the *deviations pooled* for the Sections they likewise show significant covariation ($P = .0016$), supports the above findings even though this evaluation is not wholly an independent one. (Incidentally this covariation gives the smallest P obtained in the present study.)

It is not surprising that, whatever the factors determining salience in the run, there should be a similar effect in the segment itself (which is simply a small run of five trials). The correlation found offers in itself no new problem.

The question, however, does arise as to how close a relation exists between the determinants of SSR and those of RSR. This will be much more easily determined when the analyses of the SR's are available in the terms of individual subjects, but something can be inferred from the present state of information, particularly from the fact that covariation is not significant between SSR and RSR at the level of the Subseries. While it is possible that salience is obscured at the Subseries level by effects which cancel themselves out in the combination of the Subseries into the Section, it seems more probable to me that the relation between SSR's and RSR's is simply not predominantly an individual one; that is, perhaps it *does not occur* consistently in the individual record, but is evident only in the massed data.

It will help to see this point if the possibilities of relation between SSR and RSR are first considered; first, the individual subject might show a very high, or a very low, SR in the segment but no salience in the run; second, the converse of this might be true; third, he might show very high, or very low, SR's in both or in neither. Now if the third possibility is the one that obtains, covariation should be found, presumably, on any level of combination of results, unless, of course, in the pooling of results the salience in one direction is cancelled by an equal amount in the opposite direction. If, on the other hand, the first or second possibility applies, the subject will not produce results giving significant covariation between SSR's and RSR's. If, however, there is a *group* of subjects containing a mixture of the two possibilities, the pooling or combining of their results will, with sufficient numbers, give significant covariation between RSR's and SSR's. We would have, then, significant covaria-

tion by a group of subjects but not by any of the individual performers taken alone. The individual might, however, conceivably vary from one possibility to the other, in going from one Subseries to another. These might combine to show significant covariation of SSR's and RSR's in the same way, as just stated.

The same might be obtained even with a *group* of subjects, if *one* Subseries favored terminal salience in the *run*, and another favored it in the *segment;* while the individual Subseries would show significant TS in only one of the two places, the combination of them might give significant covariation. In other words, then, the significant covariation CR's reported above do not necessarily mean that there is a similar tendency toward salience within the segment and the run in the same subject or in any group of subjects. It could well be that there *is* no definite relation between the two types of salience in any given individual; but rather that the tendencies both to salience in the run and to the same effect in the segments are sufficiently alike to be statistically significant.

This is as far as it seems plausible to go at present; but not only will the studies of individual SR's mentioned help in the clarification of the SSR-RSR relation, but other studies which Miss Betty Humphrey and I are making jointly, such as the relation between SR's and total deviation, may likewise be relevant.

Covariation of Child and Adult SR's. The next point worthy of discussion in the SR relations is the decline of the SR's in Series II and III in the Adult Division. In the earlier report (Series I) the Adult Division gave *higher* SR's than the Child. In Series II, however, the Child Division actually increased in size in both SR's; while the Adult Division dropped in both and the RSR even fell below the no-salience point (of .667). In Series III while the Child SR's held up, the Adult SR's went very low. Table 8 gives a summary of these SR's for both Divisions. The decline of the Adult Division is striking. The superiority of the SSR over the RSR for the Adult Division is maintained throughout the three Series, just as the superiority of the RSR persists for the Child. This could of course be accidental. But that the decline of both types of SR's in the Adult Division can hardly be regarded as accidental may be seen from the fact that in Series I the SSR of 1.24 is, itself, significant

($P = .005$; $2P = .01$), while in Series III the RSR of .13 is nearly so ($P = .0065$; $2P = .013$).

Table 8

SR's of the Three Series

Series	Child Division		Adult Division	
	SSR	RSR	SSR	RSR
I.....	.80	.84	1.24	.85
II.....	1.21	1.34	.75	.56
III.....	1.07	1.32	.28	.13

An explanation of the decline of the Adult Division cannot as yet be offered in terms of certainty. One fact of importance in this connection may be obtained in a glance at the diagram of the experiment in Figure 1. The number of runs per Subseries declined as the experiment continued for the Adult Division. The opposite is true for the Child Division, at least for Series III. In view of the fact that the number of subjects was approximately the same for the two groups, it is clear that the children were producing more runs per subject than the adults. Both were free to do as many as they liked in each Subseries, and accordingly this is some indication of interest and desire. The speculative suggestions might be added that perhaps the adults were, to some extent, or in a sufficient number of instances, pressed into participation (and there was some mild social pressure exercised) somewhat against their will, that they had by this time inferred something of the nature of the experiment (and this could be supposed to be apprehended by ESP itself), and that the subject developed sufficient resistance to the performance to activate an unconscious desire to thwart the experimenter. All this seems a bit preposterous to me, but I mention it as a possibility.

I am more inclined to suppose that some of the adult subjects were less in sympathy with the experimental objective than they were in Series I. After three months of repetition, the experiment had become something of a trial, if not indeed a bore. There was an increase (though not a reliable one) in the negative deviation. The changed attitude, which I am supposing, led to an altered set of habits of working during the test, and reversed the direction of salience. This led to a cancellation of deviation at the ends produced

by other subjects so that even the normal chance variance of hits per position was reduced. Such would be the hypothesis that appears most plausible. And when we turn to the chi-squares of the distribution for the segment and the run in Series III of the Adult Division, we find some basis for this view. The end chi-squares were, indeed, very low, while none of the chi-squares of the middle positions in either the segments or the run are significantly high, nor are all three positions taken together significantly high either as totals of the Series or by Sections. In the earlier paper on Series I, when exceptionally *high* SR's were found, they were accompanied by significantly low chi-squares for the middle positions. In the present case the extremely *low* SR's are not then due so much to the *increase* of the chi-squares of the middle positions as they are to the *decrease* of the terminal positions.

Precognition Hypothesis

If the results are to be explained by causes other than accident, and if they were obtained under conditions which barred all other known processes except that of knowledge by the subject of the target object (or some effective equivalent of knowledge), then it must be concluded that knowledge has been shown of the order of decks of cards before such order was yet established. Furthermore, when this order *was* established it was determined by processes and conditions designed to be thoroughly random. The operation of the shuffling machine and the final cut of the deck based upon the temperature maxima and minima appear to involve a randomization effect beyond the range of known powers of man to infer or to regulate. It therefore seems in order to conclude that it is highly probable that the subjects have possessed *significant knowledge of random future events*. Whether or not this knowledge is little (and it is!) or much, is not the question. Whether or not it is conscious or voluntarily controlled or invariably repeatable or psychologically understandable or dependent on statistics for its discerning is equally beside the point. It is sufficient as a first step to determine whether precognition occurs in any measurable degree under any circumstance.

Length of Time in Precognition

If the results have to be regarded as furnishing evidence of precognition, it will also have to be recognized, then, that the ten-day

PDT of Series III carries as much of the responsibility for this as the PDT of Series II, checked on shorter notice. In fact the ten-day PDT gave a CR of covariation of SSR's with RSR's of 2.26, while Series II gave 1.37 (these are comparable, with degrees of freedom about the same). The longer time is the better by this comparison, though not of course significantly better. Assuming, then, that there is some awareness of future events displayed in the two experimental Series as a whole, it is certainly not adversely affected (as measured indirectly by SR covariation) by the introduction of the longer interval between the subject's performance and the final determination of the card order.

Miss Hutchinson (3) did not obtain a significant result on her ten-day precognition series, but did so on a parallel one-day series. I think this probably was the effect of contrast: the subjects were doing both at the same session, and preferred (I assume) to do the more quickly checked one-day tests, since they would the sooner learn what scores (and rewards) they obtained. Anyhow, Miss Hutchinson did not show that ten-day precognition is *impossible.* My own results argue that it is *possible,* since under one set of conditions it appears to have occurred. I see no real contradiction between her results and mine. For one thing, I did not have any parallel one-day series with my two-day.

I do not propose to attempt to account for this phenomenon of precognition over the ten-day period. I have, for that matter, no hypothesis of the means of precognition over a period of *one* day, or over any length of time, large or small.

Following backward the trail of findings in the field of ESP, I find that precognition is not actually any more out of keeping with our knowledge of things than is ESP at a distance in space. It is only the deception of easy but pointless analogies that brings one to conceptualize distance-ESP as more plausible than precognitive-ESP. So long as there is no conception of energetics which would permit ESP under the spatial conditions in which it is known to occur, it remains as completely baffling to the scientific mind of today as is the work herein reported. (See *Extra-Sensory Perception After Sixty Years,* Chapter XIII, "Physical Relations of ESP.") But, to go one step further in this comparison of mysteries, were it not for its

unfamiliarity, extra-sensory perception might appear little or no less difficult of understanding than *sensory* perception. So long as all the enlightenment of present-day science cannot yet inform us through what medium or what means it is that we *experience* (for example) red when a certain wave length strikes the eye, I cannot feel greatly shocked over these newer mysteries[5] (newer, at least, to academic psychology). The ten-day PDT problem is just another one added to the long queue of psychological unknowns, one that may appear much easier as the line moves up.

Summary

The study made earlier on terminal salience in DT tests has been continued on two Series of PDT or precognition tests, one for relatively prompt checking (after about two days) and the other with a ten-day delay between the test and the check-up. The tests were given to two groups of subjects, children and adults, and the results are reported separately.

As measured by the total deviation evaluated through the critical ratio, the results of the two Series are ascribable to chance. The total of 2,302 runs gives a positive deviation of 11.

The salience ratio is again, as in my earlier report on salience, used to measure the relative standing-out of deviation of hits from expectation, as this deviation is distributed over the run in segments of five and over the segment in terms of trials. The probabilities

[5] Relevant here are the following paragraphs from an eminent neurologist:

"We do not know exactly how a sense organ is excited, how a nerve fiber conducts, how a muscle contracts, how a gland secretes, or how the brain thinks, though we have satisfactory evidence that all of these organs do perform the functions mentioned. No biologist with all the evidence before him can fail to make this deduction. . . .

"Whether consciousness is present in any particular neurologic process is a fact that can be determined directly in the experience of the subject, and its presence or absence is independent of our knowledge of the working of the mechanism. When it is present it is the part of science as well as of common sense to accept it—as a datum of experience—in just the same way that we accept sugar tolerance as dependent upon the normal working of the pancreas, though in neither case have we an adequate understanding of the mechanism employed. . . ." (Herrick, C. Judson. *The Brains of Rats and Men.* Chicago, Illinois: The University of Chicago Press, 1926, pp. 328-29.)

G. N. M. Tyrrell said: "It is only familiarity which creates in us the illusion that we understand things when we give them names." (*Science and Psychical Phenomena.* New York: Harper's, 1938, p. 155.)

of these ratios, SR's, are in some instances significant; though for the Divisions in their entirety they are only suggestive.

The extra-chance character of the SR's, and hence of the work as a whole which they represent, rests upon the covariation shown between the SR of the run, the RSR, and that of the segment, the SSR. This SSR-RSR covariation, by giving significant probabilities, confirms the findings of the earlier report (Series I). In both investigations this relation is the outstanding one statistically, but there are other confirmatory features as well. For example, the Subseries covariation is insignificant in both, while the other three subdivisions show significant (or near-significant) probabilities in both the earlier (DT) work and the present (PDT) report.

In the present study the main evaluation is supported, as it was in the first study on salience, by other SR-covariation treatments, particularly that of Child-Adult SR-relations. This is a negative relation in the PDT work, whereas it was positive in the DT, but it is significant at one of the four levels of analysis; in the DT work two levels were significant.

The strongest evidence against the chance hypothesis is given by the SSR-RSR covariation from SR's based on the pooled deviations of the Subseries at the Section level. This gives a P-value of .0016.

The various covariation probabilities, along with the support derived from the previous study, appear to justify the rejection of the chance hypothesis. Mention is also made in this connection of a report which is to follow, offering further confirmation along the main lines of this one.

Consideration of several problems that arise is deferred until further analyses can be completed, especially the study of the individual subject's salience, a survey of the extent of salience, and a more thorough investigation of SR relations, not only to other SR's, but also to the CR's of the total deviation.

One problem especially stands out in this study—that of the precipitate decline of the Adult Division in SR's through Series II and III from its eminence in Series I. A suggestion is made tying this up with the decline of interest and participation in the

experiment on the part of the adults, as shown by the number of runs they took part in. This is in contrast to the children.

Since the results of the Series are regarded as extra-chance in character, and since the conditions were such as to eliminate all extra-chance factors except the hypothetical principle of precognition, the results are taken to indicate that precognition occurs, albeit in a very obscure way that is extraordinarily difficult to detect. (Presumably, were it not difficult it would long ago have been discovered.)

And since a greater contribution to the total statistical result was given by the ten-day precognition test in Series III than by Series II, it may be reasoned tentatively at least that the time factor involved has no appreciable consequence under these conditions.

No hypothesis of precognition is offered, but I mention the fact that the most familiar examples of cognition of any kind still lack an explanatory hypothesis.

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BOOK REVIEW

THOUGHTS THROUGH SPACE. By Sir Hubert Wilkins and Harold Sherman. New York: Creative Age Press, 1942. Pp. 421.

Thoughts Through Space contains a record of the attempt of Sir Hubert Wilkins and his colleagues to find a group of Russian fliers lost while attempting a flight from Moscow to the United States. More especially, however, it is a documented exposition of an experiment in telepathy carried on by Wilkins and by Harold Sherman during the course of the search.

By prearrangement the experiments were to be carried on at a specific hour in the evening during three days of each week of the search. Wilkins, while seeking the fliers in the Arctic regions, attempted to "send" his thoughts, mainly those concerning incidents of the day, to Sherman in New York City who attempted to "receive" them.

Certain precautions were taken in an effort to mitigate the possibility that Sherman might obtain his information by other than telepathic means. Information is given in the form of letters and affidavits concerning the conditions of the experiment. The conditions demonstrate an attempt on the part of the individuals concerned to conduct the experiment in good faith. Any inadequacy that arises is due to the naturally heavy demands made on the time of Sir Hubert by the exigencies of the search. For this reason, validation of some of the points brought out by Sherman are dependent upon the memory of Sir Hubert rather than upon a written record. With the best intentions in the world, a person cannot always remember with absolute accuracy that "this" happened on Tuesday rather than on Wednesday or that he talked to "those people" on the third of the month rather than on the fourth.

There are some incidents recorded in the book which are particularly striking and which make impressive reading. The main difficulty concerns the manner in which they are to be evaluated. No

statistical evaluation has been attempted by the writers and, of course, none is possible. It remains for the reader himself to decide whether the material presented is conclusive.

There are two points about the presentation of the material which make such a decision difficult. In the third part of the book, Sherman's "guesses" concerning the activities and thoughts of Wilkins are given; and in a parallel column is recorded either the comment of Wilkins, a diary entry by him, or a newspaper statement, which bears on the validity of Sherman's remarks. It is not always clear from the form of the statements whether they are entries from Wilkins' diary or whether they are comments made from memory after he had received Sherman's letters. If one is concerned with the adequacy of the experimental conditions, this is a crucial point.

Furthermore, although there usually appears over Sherman's remarks the statement "An exact transcription of tonight's impressions as follows," it is not clear that a complete record of his remarks is given. As a matter of fact, it is pretty clear that this is not the case since the statements are usually followed by dots symbolizing omission. The following excerpt (page 254) is an example: "Several uneasy moments today in air and on ground. . . ."

Another important point is that it is impossible to tell with certainty whether the material published is a complete record of *all* of Sherman's impressions, or whether there has been a selection of only that which was most impressive. If the second alternative was followed, the evidential value of the book is lessened considerably, since, if Sherman made a sufficient number of remarks, a certain number of striking coincidences would be expected. If the first alternative is correct, it is a shame that the system of punctuation used by Sherman has introduced the ambiguity pointed out.

An attempt is made by Sherman to suggest an explanation of telepathy in terms of the electrical energy of the brain and ether waves set up by such energy. He remarks: "Human emotions are the batteries which generate the power, according to their intensity, that is put behind the electrical currents of the brain."

In the reviewer's opinion, this is by all odds the weakest part of the book. First, there is no reason to suspect that sufficient energy is generated by the brain to have any kind of an influence at *any* distance. Second, there is no reason for believing that the differences

in thoughts bring about qualitative differences in the electrical current generated by the brain. Third, there is no possible explanation why the activity of the current generated by Sir Hubert's brain should cause "thought waves" which could be differentiated from the "thought waves" of the thousands of people closer to Sherman than was Sir Hubert. Fourth, considering how infinitesimal the electrical energy generated by the brain is, any (hypothetical) quantitative difference due to variation in the intensity of the emotion would hardly make a difference between the reception of a thought and its non-reception. In view of the many obvious physical difficulties besetting his "radio broadcasting" theory of telepathy, Mr. Sherman would have done well to have omitted it from the book.

In summary it may be said that the book shows an unmistakeable attempt to investigate the phenomena of extra-sensory perception in good faith. Some of the cases given are really impressive and will convince many that telepathy has been demonstrated in this series of experiments. The book, itself, is interesting and gives the impression of careful preparation. Studies of this sort obviously have a place in the field of parapsychology. On the negative side, it can be said that some of the experimental conditions are hazy. There is also some proneness to make too much of ordinary coincidences and to "explain away" deviations from the exact comparability of the actual happenings and of the Sherman impressions. *Thoughts Through Space* is a fair-to-middling contribution to the field of Parapsychology.

J. L. Woodruff.

LETTERS AND NOTES

Cornwall, England

To the Editors of the JOURNAL OF PARAPSYCHOLOGY:

Gentlemen,

Your issue for June, 1940, containing my paper on the Paranormal Cognition of Drawings, reached me only on January 12th, 1942; but, if circumstances permit, I should be most grateful for an opportunity of replying to the criticisms made.

1. [Report too long.][1] Agreed: It seems clear that my intrinsically praiseworthy attempt to leave no relevant issue undiscussed has resulted in certain readers skipping the essential passages altogether.

2a. [Drawings not good experimental material.] I do not think that the question of whether Cards or Drawings are the better experimental material can be settled *a priori*. Drawings involve certain difficulties of their own, as I well know; but I imagine that Dr. Rhine would say much the same of Cards. I think we may reasonably expect that Drawings may prove better for some purposes and Cards for others. Experience alone can show us.

2b. [Drawing data require subjective ratings.] It is, however, erroneous to suppose that the use of Drawings necessarily involves subjective judgments, except in the formal and quite trivial sense that the judgment: "The symbols *cat* and *CAT* refer to the same thing" is a subjective judgment. The judgment that an unmistakeable drawing of a cat is a hit on an original representing a cat is as objective a datum as most that are used in science, and *a fortiori* if the drawing be labelled "cat," as I now make it a practice to require.

2c. [Subjective methods used in scoring data do violence to the analyses based upon the scores.] Moreover, with all deference, it is categorically incorrect to affirm or imply that a subjective element

[1] In order to save the necessity of reprinting the comments of the Board of Review a second time, the editors have put a brief abstract of the Board's criticisms in the brackets which precede each of Mr. Carington's replies. For the original report of the Board see this *Journal*, 1940, 4, 153.

in data submitted to statistical analysis can, *per se*, "do violence" to the methods used. In this case, the only demand made by the methods used is that entry into the columns of the 5 x 5 (or 50 x 50) table should be independent of entry into the rows; and this, so far as subjectivity is concerned, can be absolutely assured by appropriate ignorance on the part of the scorer. Subject only to a somewhat academic reservation, of negligible practical importance, discussed in *Proc.* S. P. R. XLVI, June 1941, it was in fact assured in this work.

The trouble here arises from the fact that the critics have not realized that the judgments called for were purely of the form "This resembles That"; from the conditions of the technique they *could not be* of the form "This is a Success" or "This is a Failure." The criticism would have been valid if the latter alternative had obtained; but, since the judge did not know which originals were used in which experiment, he could not tell which "hits" were successes and which were failures: the objection accordingly fails.

2d. [Lowering of standards of judgment resulted in improved scores for one judge.] The term "improved scores" is ambiguous, especially as no "scores" were given. Lowering the standard, by including the doubtful hits given only half a mark, raised the *significance* of the results, presumably because a fair proportion of the "doubtfuls" were in fact genuine; but, as would be expected, it lowered in all cases the *quality* as defined by 100D/N, which is much more in the nature of a "score" than is the significance. See p. 73 and Table V, p. 70. This shows nothing except that the scorer was reasonably cautious about giving full points.

3a. [A control series consisting of drawings made in the absence of stimuli should have been used.] As indicated also by 3c below, Dr. Willoughby appears to have misapprehended the nature of a contingency table. By scoring (not "matching"), in the manner described, the drawings of all five experiments against the randomized originals of all five experiments, each experiment was, so to put it, controlled by all the other four, which is substantially what happens *mutatis mutandis* whenever a contingency table is used. The non-identity of the percipients in the various experiments would be a more plausible basis for attack, for it could be argued that if the percipients in Expt. I had consisted mainly of seamen; in II, of

farmers; in III, of naturalists; etc., they would respectively tend to draw relatively more nets and anchors, horses and guns, birds and fish, than those in the other experiments; or something on similar lines. But this, of course, was not the case; and *ad hoc* investigation of the part played by the varying ratios of women to men, and of British to Americans to Dutch (unpublished paper) easily shows that these factors were incapable of accounting for the outcome; for "Women Only," "Men Only" and "British Only" all give significant results.

3b. ["Notes" may have accounted for entire excess over chance.] No. If each note had caused one hit to be scored which would otherwise have been missed the effect (obviously) would have been to raise the *total number of hits* by 38. This is NOT the same as raising the number of hits in the leading diagonal of 5 x 5 table ("winning" hits); and, if there were no real effect, there is no reason for supposing that the "excess (of these) over chance" would be appreciably affected one way or the other, much less "accounted for."

I venture to protest that to make a categorical affirmation of this kind, which might well appear extremely damaging at first sight, but is flatly and manifestly not true, is at strong variance with the recognized canons of critical propriety.

3c. [Same judge should have done both experimental and control series.] Concerning repeatability, see below. As regards the possible influence of the scorer, see above, but particularly the text pp. 107 ff.

The Control Scoring done by Mr. Saltmarsh was undertaken, somewhat against my own judgment, as a work of supererogation, and its sole methodological relevance is to the highly implausible possibility indicated on p. 93, lines 15-19. It is entirely extraneous to the main body of the work, the conclusions of which are just as strong without it. (Cf. p. 97). Since it was, as indicated in the text, an afterthought, it clearly could not be done by Mr. Hindson, who by this time knew which originals belonged to the five experiments collectively. If Mr. Saltmarsh, say, had scored all the drawings against the whole 100 originals, as suggested by Dr. Kennedy, it would have added nothing to our knowledge except, presumably, that the actual originals were more, or less, popular than those of the "control" series. (I think that the use of the word Control was

unfortunate here, since the scoring in question could scarcely be held to control anything; it would have been better to have spoken of a set of "dummy" experiments, or the like.)

3d. [Doubt concerning the basis for statistical evaluation.] Apart from the fact that I should have said that my principal result was that worked in detail on p. 69, which is not particularly important, I am at a loss to understand Dr. Willoughby's alternative null hypothesis. It would presumably be true, in the ordinary statistical way, if the various batches of hypothetical originals mentioned were not only randomly selected but indefinitely numerous; but this is expressly excluded by the words "trying to reproduce 10 originals," and could clearly never be realized in practice. The supposition, which Dr. Willoughby seems to favor, that normal people under free conditions are as likely to draw rare objects (e.g., Autoclaves, Orreries . . .) as common (e.g., Houses, Trees . . .) is, with all deference, contrary to common experience and to common sense, and is certainly disproved by experiment. What interests us is whether the *proportion* of drawings of a particular object (or set of objects) is the same when it is used as an original (or the set is) as when it is not.

The phenomenon to which Dr. Willoughby calls attention arises solely from the fact that of five unequal numbers only one can be the greatest; and has nothing to do with precognition.

4a. [Author neglects possibility of precognition and retrocognition beyond bounds of single experiment.] If Dr. Willoughby will refer to the text, pp. 80-81, he will see that I have in no way neglected "the possibility of pre- and retrocognitions beyond the bounds of a single experiment." It is true that I *illustrated* the procedure (p. 84) by the use of intra-experiment data, as a matter of practical convenience; but the 50 x 50 table, on which my discussion was solely based, takes full account of displacement between experiments as well as within them, as I believe is clear from my description of it.

(N.B. Some slight confusion may have arisen here from editorial renumbering of the tables without corresponding alterations in the text.)

4b. [Precognition seems unbelievable.] I have every sympathy with those who find precognition difficult of acceptance, though it happens not to affront me personally in any serious degree, and I

should be sorry to try to thrust it down anyone's throat. Pending accumulation of evidence (which actually seems to be proceeding apace) it would be wise to suspend judgment, but the following reflections seem to be in order: *a.* I do not think it logically permissible to reject any phenomenon as impossible unless it can be shown to involve a contradiction in terms, and I feel that this would be far from easy here unless by the use of illegitimate definitions. *b.* Whatever else may be involved in precognition (supposing it to occur) the percipient's drawing is not the end product of a reflex arc sequence in the sense it would be if he were copying the original. It is therefore likely to be dangerous to discuss the problem in the "stimulus-response" terms appropriate to that type of situation, or to think of it in the corresponding conceptual vocabulary. *c.* On the more general point, I think that most people would agree that "chance" and "cause," as the terms are usually understood and employed, are antithetical and mutually exclusive. If this be so, it follows from definition that, if the probability of an event being due to chance is small, the probability of it being due to *some* cause is correspondingly great, as Dr. Willoughby suggests; but of course it does not follow that the cause we first think of is the true one. *d.* Psychologists above all men should be on their guard against the danger of mistaking emotional resistance for rational conviction. Our notions of Time and Causation are notoriously naive and unsatisfactory—we are only just beginning to think intelligently about Space—and correspondingly liable to promote non-rational attitudes. Personally, I think that precognition, of some sort, is almost certainly a fact in nature, and that it is far more likely to prove capable of illuminating our notions of Time and Causation than these of legislating as to its occurrence. But we must clearly walk very warily.

4c. [Experiment ought to be repeated.] While the importance of repeating experiments of this general type cannot be overemphasized, I should be the first to agree that the procedure reported is far too cumbrous and protracted to be suitable for routine work such as one hopes to see done. In the course of the last eighteen months, however, I have developed a technique which seems very reasonably satisfactory from the practical point of view.

This depends on the use of a Catalogue of all objects drawn or mentioned by the 741 percipients of my first seven experiments, i.e.,

about three times the amount of material reported in my paper. Subject to the obvious proviso that it will need to be extended and brought up to date from time to time, such a catalogue affords the necessary basis for determining empirically the probability that any given object will be drawn by a percipient when it is not used as an original. Naturally, also, the population from which the percipients of a fresh experiment are drawn must be substantially similar (i.e., of "contemporary western culture") to that dealt with in the catalogue.

The extremely tiresome method of "cross-scoring" is replaced by the use of Fisherian negative-logarithm scores, and the ignorance of the scorer by a conscientious literalism—that is to say, by paying no attention to mere "resemblances of shape" at all, and keeping strictly to the principle that, for example, a Horse is a hit on Horse, but a Donkey (or even a Mule) is not.

It is hoped to publish details of this technique, with the Catalogue and results of actual experiments, in the near future.

4d. [Experimentation should not be limited to "believers."] If I had said or implied that "only a 'believer' is qualified to attempt repetition" I hope that (to indulge a minor Irishism) I should have been the first to repudiate the suggestion. But I did not. What I actually *said* will be found in the last five lines of p. 102 and the first seven of p. 103, and I do not think the passage can be held to *imply* anything not directly stated. In particular, the words "a reasonable amount of good will" are not equivalent to "belief." However, I should be sorry to limit the field of potential experimenters, so I am very pleased to withdraw the non-existent implication unreservedly and to wish all good fortune to any "hostile" critic who may wish to experiment.

In conclusion, I should like to thank all concerned for the opportunity their criticisms have afforded me for clearing up (or so I hope) what appear to be essentially no more than misapprehensions as to the principles and procedure of the work.

WHATELY CARINGTON.

Cornwall, England

To the Editors of the JOURNAL OF PARAPSYCHOLOGY:

Gentlemen,

It is not clear to me whether the paper by R. & S. Bugelski in your issue of June, 1940, which has only just reached me, is an attempt to discredit ESP by leading a forlorn hope against established probability theory—or just another attempt to discredit ESP. But in neither event should it be allowed to pass without comment.

While one cannot but admire the authors' industry, it is even more difficult to regard it as other than misplaced. Objectively considered, the paper consists of a demonstration of the fact that, if you flagrantly indulge in that form of improper selection of data known as "optional stopping," you may easily obtain results which chance alone would be unlikely to yield if you do not. This adds no more to knowledge than would inserting a lump of lead on one side of a die and then showing that it no longer falls true.

Such activities, if somewhat unprofitable, are harmless enough in themselves; but the implication (and if none, why publish?) that they are relevant to the credentials of ESP is most pernicious.

It would be perfectly correct *logically* to argue: "Such and such a malpractice" (e.g., deliberately scoring misses as hits, selection of data from best subjects only, optional stopping, etc.) will, or may, lead to spurious results. Exponents of ESP always (or sometimes) indulge in this malpractice. Therefore, all (or some) of their results are (or may be) spurious." If the first two terms were to be substantiated, the conclusion would follow inevitably, and no one would have any right to complain. But to point out the first, however correctly, and allege or imply the third, without a shadow of an attempt to substantiate the second, is to carry the illogical to the verge of the disingenuous.

It might be possible to find charitable excuses for this sort of thing in certain circumstances; but we are precluded from doing so here by the passage which occurs in the central paragraph of p. 145. The authors say, "The fact that if a subject is given enough runs he needs to score only an average of six hits per run to earn a reliable critical ratio suggests that ESP researchers would do well to consider raising the criterion of good results somewhat above that figure,

regardless of the fact that the critical ratio indicates unlikelihood due to chance [*sic*] to the tune of astronomical figures."

This is so perverted a statement that it is difficult to disentangle it; but, if words mean anything, it plainly implies (a) that ESP researchers have habitually, or at least to an important extent, used the average number of hits per run as a "criterion"; and (b) that this average may be a better criterion to use than the critical ratio, at least if it be "somewhat greater" than six. To the best of my knowledge and belief, both these implied propositions are false: the first in fact, the second in theory.

As regards the first: The monograph, "Extra-Sensory Perception After Sixty Years," and the issues of this JOURNAL from its inception to date, may be taken as containing an authoritative and comprehensive statement of the pro-ESP position: I challenge the Bugelskis to quote a single instance from these sources of an exponent of ESP using the average score as a criterion of statistical significance; if they cannot do so, the imputation should be withdrawn. And if it be sought to evade the issue by contending that the term "good results" in the above passage does not mean "statistically significant results," then I must ask, "What does it mean, and in what sense can a result be considered 'good' if it can plausibly be attributed to chance?"

As for the second, it seems unlikely that the authors have excogitated a whole new theory of Probability—as it might be non-Euclidean geometry—and have chosen this somewhat oblique method of announcing it to the world. The indications rather are that they have not taken the steps necessary for ascertaining what a test of significance is, what it is for, the nature of a critical ratio, or how this differs from an average score.

The passage quoted is just about as sensible as to contend that, because a man can secure an income of any size he likes from investments paying only 6%, *if only he invests enough capital*, it is possible for anyone to ensure an equally large income, no matter how *small* his capital may be, provided he invests at 7%!

WHATELY CARINGTON.

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. In case of any discrepancy between glossary and usage in the text of an article, the latter should be followed. Words defined elsewhere in the glossary are italicized in the text of the definitions.

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: *Extra-sensory 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*.

CRITICAL RATIO: The observed *deviation* divided by the *standard deviation.*

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*).

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 (EXTRA-SENSORY 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.*

EXTRA-CHANCE: Not due to *chance* alone.

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

GESP (GENERAL EXTRA-SENSORY PERCEPTION): A technique designed to test the occurrence of *extra-sensory perception*, permitting either *telepathy* or *clairvoyance* or both to operate.

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.*

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.*

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.

PARAPSYCHOLOGY: A division of psychology dealing with the "extra-normal"—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.

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

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

RUN: A succession of *trials,* usually the *calling* of a *deck* of 25 *ESP cards* or symbols.

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

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*).

SALIENCE RATIO (SR): 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.)

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.

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.

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

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

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 2.5 or greater.

STANDARD DEVIATION: The theoretical root mean square of the *deviations.* For *ESP cards,* $SD = 2\sqrt{\text{no. of } runs}$.

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: *Stimulus object.*

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

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

TELEPATHY: *Extra-sensory 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.*