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EDITORIAL COMMENT

THE RESEARCH reported during the JOURNAL'S first year has justified, we believe, the measure of confidence needed to go on to wider fields of problems. More collegiate and university studies in experimental parapsychology were reported during 1937 than ever before in a similar period . . . perhaps more than had ever been previously reported altogether.

The explorations in this field have not only multiplied in number but on the whole they have advanced both in experimental precaution and in scientific contribution. The mathematical methods used in evaluations have been carefully examined by highly qualified mathematicians.

Developments of the investigations of ESP have led to a wider knowledge of its nature, of the range of subjects possessing such ability, of the scope of stimuli extra-sensorially perceptible, and of the general physical conditions affecting or not affecting this mode of perception. However it must be recognized that on these topics only a beginning has been made.

But while there will continue to appear reports of similar confirmatory and exploratory studies adding further support to the experimental case for an extra-sensory mode of perception, the time has come to release the first of a series of articles dealing with a more radical outgrowth of the ESP research, namely, with the question whether ESP is limited by time—whether the process of extra-sensory perception can be directed forward into the future (i.e. can precognize events), as apparently it can function outward in space.

Although we regard the report of the preliminary studies on the precognition hypothesis published in this number as a very tentative and far from exciting one, the importance of the question makes the venture of publishing it seem somewhat bold. Possibly it may be regarded as

rashly inviting incredulity upon a field of research only just emerging from the shadow of the gruelling criticism incident to its first impact on a psychological world unprepared with theory for its explanation and acceptance. The situation therefore warrants a word of clarification on the conditions of this step.

The new line of investigation is reported in a somewhat altered spirit noticeable in those engaged in these researches. It is born perhaps of reaction to the unduly subjective and affective attitudes prevailing among the most outspoken critics. This new spirit is one not so much of impatience as of a desire for less waste of effort in useless argument. It might thus be expressed: "Here is what *we* find. The reader is not asked to *believe* it; but he is earnestly invited to *consider* it. If he cannot himself help to test these hypotheses, or offer constructive suggestions for better test-procedures, he should await the further repetition of those who can. The truth must come ultimately from the laboratories, not from public disputation." In brief, less speculation, more experiment!

This new series is launched, then, with a plea for greater objectivity and for recourse to more actual investigation instead of the dogmatism and ridicule which have been the rule in the history of this subject.

On the whole, psychologists of today have shown a reasonable amount of open-mindedness and a willingness to consider, though in properly guarded fashion, the results of the ESP experiments. The present barrage of hastily composed critiques running through the journals is not, as far as we can judge, representative of the leadership in psychological thought today. This leadership is (happily for all new developments) a more careful and tolerant one.

The position of the ESP research on the issue of sensory cues, which was summarized in the December JOURNAL, is carried further not only by the report on precognition tests but by the Martin and Stribic report which is based entirely upon work with the screened DT test procedure, and that of Clark and Humphrey which is reinforced on this aspect by an excellent final series with the use of the screens.

It is worthwhile to draw attention here to the fact that with but one exception all the research reported in this JOURNAL has been safeguarded on the question of sensory cues by the presence of a substantial series of tests with screened cards, at a distance, or in sealed opaque envelopes. The one exception, like the personal account contained in

the anonymous letter published herewith, consisted of experiments with the experimenter himself as the sole subject tested.

On the question of the adequacy of the research personnel, one which looms in importance by contrast as the other alternatives to ESP fade from the present picture, the addition of another college (Earlham) to the thirteen which had earlier contributed confirmatory evidence in publication to the ESP hypothesis, is a point of some interest. There is, of course, no statistics of personnel sufficiency, but there is—in research as in other fields of action—strength in numbers.

Since scientific eminence in the experimenter weighs heavily—perhaps too heavily—in science, it is a handicap not to be able to add the name of the distinguished author of the anonymous letter published herewith, which reports a substantial series of ESP tests confirmatory of that mode of perception. The intrinsic value of the author's analysis of the results however is in itself worthy of publication.

While it is true that our main statistical procedures have been given the stamp of approval by those most qualified to know, there are still problems of that nature to be solved in adapting the evaluative methods more closely to the research needs, both present and future. The articles by Greville and Greenwood serve this latter end: They determine for the investigators their margins of safety in certain analyses of ESP research data.

The JOURNAL will continue its mathematical section until its needs for original mathematical developments and special applications are satisfied. The unprecedentedly large empirical check under way now at Duke University under Dr. Greenwood's direction, involving a half-million matchings, will furnish material for several studies; and solutions, it is hoped, for several problems.

The case of the Latvian girl of low intelligence rating, Ilga K., has attracted world-wide interest on the strength of F. von Neureiter's interpretation of the phenomenon as one of telepathic perception. Bender's careful and balanced analysis of the studies made of this interesting child adds another variation to the contents of this number. His study is especially timely in view of the recent rise to attention of the case of an American child in many ways like Ilga, according to the first reports of the psychologist studying the child. Similarly limited in mentality and of nearly the same age, the American boy shows a number of simi-

larities to the girl described by Bender. It is hoped that a full report of the boy's means of acquiring knowledge will be forthcoming in the near future.

Critical articles concerning the research in extra-sensory perception have lately appeared with increased frequency in other journals. This was to be expected as widened public interest has literally forced a more serious academic consideration of the experimental results. The main issues raised in these critiques (statistics and sensory cues) have been dealt with already in the articles in previous numbers of this *JOURNAL*. This fact, along with the further circumstance that we are apparently in the midst of a flurry of such critical papers which will likely continue for some time with much repetition of contents, suggests the advisability of postponing discussion of them here until a fairly complete survey may be made. We list them in the "Notes" (pp. 71-72) by title, however, for the convenience of those interested in the controversial literature.

THE CASE OF ILGA K.: REPORT OF A PHENOMENON OF UNUSUAL PERCEPTION

HANS BENDER

ABSTRACT: In 1935 Prof. v. Neureiter reported his study of a Latvian child, Ilga K., who appeared to have an unusual ability to "read thoughts." The 10-year old girl, although normal physically, was mentally retarded. She had an I.Q. of 48, and learned to read the simplest words only with the greatest difficulty. Her teacher discovered, however, that she could "read" any text, even one in a language foreign to her, if he stood beside her silently reading the text.

Three studies of the child have been made: One by Prof. v. Neureiter, Director of the Institute of Forensic Medicine of the University of Riga, a second by a German commission from the Psychological Institute of Bonn University, and a third by a commission led by Prof. Dahle of the Institute of Psychology of the University of Riga.

Prof. v. Neureiter and his collaborators concluded that the child's performance could not be explained as due to normal sensory stimulation, and that some extra-sensory capacity was at work. The German commission made cinematographic and dictaphone records of their experiments, and concluded that most of the child's responses were explainable in terms of a very acute auditory capacity. They noted, however, a number of observations that were not covered adequately by this explanation. The Latvian commission, using the same general methods more extensively, came to the same conclusion.

Dr. Bender is on the staff of the Psychological Institute at Bonn University and was a member of the German commission.

IN THE AUTUMN of 1935 the Director of the Institute of Forensic Medicine of the University of Riga (Latvia), published a small book called "Wissen um fremdes Wissen, auf unbekanntem Wege erworben"¹ ("Immediate Knowledge of Alien Thought Contents Acquired In An Unknown Manner"). This article caused a considerable sensation and, more rapidly than is suitable for investigations into obscure and questionable forms of mental activity, it reached the public to which it had become known through a number of press reports. In his booklet Prof. v. Neureiter describes experiments which he had made with a ten-year old Latvian peasant girl, Ilga K., who seemed to have the strange ability to "read thoughts." This had been noticed by her teacher

¹Leopold Klotz Verlag, Gotha 1935.

and by Dr. Kl., a physician who was consulted by the mother because of the child's backwardness.

In his experiments, in which chiefly the child's mother functioned as agent, v. Neureiter and his collaborators came to the conclusion that the transmission of contents of consciousness was not effected through the normal sensory channels and that paranormal* functions were at work. The inquiry was made without any closer contact with parapsychological research. The author even points out that the result of his superficial examination of parapsychological literature was rather unsatisfactory and that this made him doubtful as to the scientific value of his discovery (p. 14). Professor Hans Driesch's personal interest encouraged him to undertake a continuation of the experiments.

Under the auspices of the Latvian Ministry of Education a commission led by Prof. Dahle (Director of the Institute of Psychology of the University of Riga) was formed for the purpose of further investigation of the phenomenon. This commission included a number of scientists—physicists, physicians, phoneticists, psychiatrists and psychologists—who had no close connection with the field of parapsychological research. The Psychological Institute of Bonn University, which had received in detail information on the phenomenon through a collaborator of v. Neureiter's, sought for permission to take part in the further experiments. This was made possible through the intervention and active support of Prof. v. Neureiter and Prof. Dahle.

In the four days in Riga in which the child, who had in the meantime been given a governess, was placed at our disposal we were not able to come to definite conclusions because the phenomenon turned out to be very different from what we had expected and prepared for on the basis of our information and v. Neureiter's article. Further experiments were undertaken by the Latvian commission. In a "Preliminary Report on the Results of the Investigation of the Peculiar Faculty of the Girl Ilga K." in the Riga Rundschau of June 5, 1937, the commission made the following statement: "Ilga's perceptive faculty is based upon an acoustic and possibly an optical capacity of a peculiar structure, also upon a form of contact between agent and percipient, *i.e.*, an active assistance which the mother and also other persons acting as agents give her in the form of an acoustic and optical aid."

This statement seems to express more or less the general consensus of opinion of the different members of the committee without taking account of individual observations. Thus Professor Dahle, always most

* Paranormal as used here means: "Outside the range of the recognized or normal." Ed.

friendly in giving information, wrote me in a letter of October 25, 1937, which I cite here with his kind permission: "There is no doubt that according to the present state of investigation the basic character of the phenomenon is of an acoustic nature . . . I cannot deny that paranormal, *i.e.*, telepathic factors, play a part at least episodically. But that is my personal opinion."

In the spring of this year Prof. Dahle's comprehensive report of the case is to be expected.

In my report I will attempt to sketch the individual phases of the inquiry with as many details as necessary to enable the reader to form his own opinion on this very complex phenomenon. At the same time I shall pay special attention to those observations which seem to escape interpretation by acoustic aid within the threshold of normal hearing.

ILGA'S PERSONALITY AND THE RESULTS OF V. NEUREITER'S INVESTIGATION

Ilga K. grew up as a child of Latvian peasants on a lonely farm near the Esthonian and Russian border. Although small of stature the little girl is physically quite well developed but mentally defective. She was eight years old when she learned to speak coherently and she articulates indistinctly even now—a fact which made an investigation especially difficult. Intelligence tests according to the customary methods resulted in an intelligence quotient of 48. Ilga was 9 years old when she entered the primary school where she learned to read the letters but failed almost completely in trying to read syllables and words. To his astonishment the teacher discovered that Ilga could "read" any and every text, even one in a language foreign to her, as soon as he stood beside her with the text before him. She did not need to look into the book. The mother made similar observations. In her presence the child uttered the results of calculations which were far beyond her calculating ability (even to-day she must use pebbles to make the simplest additions and subtractions from 1-10.) This remarkable transmission phenomenon between mother and child was said to be the reason why Ilga often found objects which had been hidden from her. Considering Ilga's "sweet tooth" this phenomenon caused serious difficulties in her bringing up.

In the summer of 1935 Ilga came to Riga where Prof. v. Neureiter made experiments partly in the presence of numerous witnesses. The child actually did repeat, sometimes without any mistakes, texts which were shown to the mother as agent, and this in a peculiar monotonous tone, the word being divided into syllables. Mother and child sat with their backs to one another at a distance of three meters; afterwards the

mother was placed behind a curtain and experiments were made from one room into another. In the latter experiments v. Neureiter unfortunately first handed the text to be transmitted to the agent and then went to the percipient, Ilga. Numbers, words, meaningless syllables, Latvian, German, French, Latin texts were transmitted which the child uttered without comprehending, in a manner corresponding to the mother's purely phonetical way of reading. Thus for example the text "C'est ainsi que dans les 35 cas de mort subite etc." according to v. Neureiter's report was reproduced in the following manner: "C'est ain-si-que-dan-le-trisdesmit-piec-cas de mort-su-bi-te. . ." Exactly following the phonetical understanding of the mother the child uttered the words just as they are written, and consequently spoke the number 35 in Latvian. The mother, who was very temperamental and ambitiously interested in the success of the experiments, did not behave quietly during these transmissions, but stimulated the hesitating child to continue by crying "Go on" or attempted to move her to do her task better through admonishments "Say it right," etc. In his report v. Neureiter indicates these interruptions merely by exclamation marks. Although he maintains that the interruptions were later avoided by the employment of simpler words, they became more and more the center of attention for the later investigators as one of the means of explicating the phenomenon.

The experiments were continued in the dwelling of the family K. in R. Mother and child were in different rooms (we are not told whether the doors were open or closed). When v. Neureiter, before the eyes of the mother, pointed to a picture in a children's book, the child in the next room immediately announced the object represented. There is no mention of encouragements by the mother (p. 47). Pictures out of a children's book were transmitted when they were shown to Ilga's six year old brother. Neureiter emphasizes the fact that the boy had hardly had time even to glance at what was shown him before Ilga spoke out of the next room (p. 50). Finally v. Neureiter was himself successful in some transmissions. At first in spite of his deep concentration the child remained silent. He writes: "While I was about to shut the book and give up the continuation of the experiments, I happened to glance at the word 'Bruhte' with which one of the last lines of the poem filling the page begins. At the same moment the child in the next room cried: 'Bruhte.' The reception had functioned although, or, more correctly, because I was no longer consciously concentrated on transmitting. At the moment of perceiving the word 'Bruhte', I was surprised that in a modern Latvian schoolbook instead of the usual word 'ligava' an antiquated borrowed word had been

adopted." After this experience v. Neureiter tried artificially to "de-concentrate" himself during the transmission, and, according to his report, achieved the correct transmission of six out of nine Latvian words in this way.

Prof. v. Neureiter makes the following summary of the results of his investigations which had to remain fragmentary because of the limits of the time at his disposal.

1. Every possibility of transmission of the conceptual contents of the agent by the way of the normally known sensory channels—whether optic, acoustic or otherwise—has been excluded. We have to deal with a "paranormal" relation of person to person.

2. Actual contents of consciousness were transmitted. The quality of the transmission was the better the more they moved from the center of consciousness to the periphery.

3. The percipient, too, achieves better results in a state of distraction.

4. Words were reproduced by the child at first purely mechanically without comprehension.

5. A special form of rapport of the percipient with a particular agent must be assumed.

EXPERIMENTS OF THE GERMAN COMMISSION

Neureiter's article attracted considerable attention and was widely discussed. In spite of the sensational exaggerations of part of the press which spoke of "A child that knows everything," or of "The peasant girl who speaks all languages," the "wonder-child of Riga" was finally taken note of by the scientific world. From scientists who in general were not particularly friendly to parapsychology I heard the opinion expressed that we seemed finally to have a clear case of what seemed to be telepathy, a case which was free from complicated interpretations of statistical analysis. Even a professional sceptic, a trick expert whose office it is to expose occult phenomena, wrote me: "Here we have proofs as they ought to be given." Whoever has done work himself in the field of parapsychology may well understand how great an interest was taken in the Riga phenomenon after the first reports. If the transmissions had really been effected by an extra-sensory way of perception, the remarkable case seemed to have occurred that volitional telepathy² (the voluntary transmission of actual thought contents from one mind to another without intermediacy of known sensory channels) functioned—in a short distance experiment at least—with almost com-

²I do not discuss the question whether in the Neureiter experiments any pure-telepathy-situation as it is so clearly postulated by J. B. Rhine had been realized.

plete reliability. The source of error lying in acoustic signs, whether in the form of unconscious whispering or in other aids, could be appreciated by no one better than by a parapsychologist. When I was asked to make a report on v. Neureiter's book in the *Zeitschrift für Psychologie*, my first care had to be to make this question clear. After v. Neureiter had told me in a letter that correct transmissions had also been effected from the sound-proof chamber of the Riga broadcasting station, I took up a positive attitude towards a telepathic interpretation of the report, at the same time pointing out the importance of further guarantees and investigations in contact with parapsychological methods of research.

The Bonn Institute of Psychology, which for several years has made parapsychological inquiries part of its research, took part in the further investigation in contact with the Latvian commission.³ Since according to the information we had received we had to assume that transmissions from a sound-proof chamber would succeed as well as within the reach of hearing, we thought a further testing of the telepathic nature of the phenomenon a relatively simple matter. Consequently in making our preparations we concentrated our efforts upon the investigation of specifically parapsychological problems for which the unusually sure and stereotype functioning of the transmission seemed to be a favorable condition, namely: investigation of the problem of rapport between agent and percipient, investigation of the range of individual agents, measurement of the reaction-time etc.

We planned to start by showing to the agent, by means of a specially constructed automatic projector, words, pictures, etc., which had been prepared by persons not involved in the experiments. Simultaneously with the appearance of these words, etc., before the eyes of the agent, a bulb, automatically switched on by the projector, was to be lit. By means of a moving-picture of the child it would have been possible to measure on the length of the film-strip the time elapsing between the lighting of the lamp (exposure of the word before the agent) and the utterance of the word by the child in case of a transmission (first visible movement of the mouth). By gradually increasing the distance the influence of the latter upon the quality of the transmission and upon the

³ Our Institute was supported by the German Board of Education with means for carrying out the investigation, a sign of the increasing interest being officially shown in Germany for scientific inquiries into this problem. Members of the German commission were Prof. Rothacker, Director of the Bonn Institute of Psychology, Prof. Mensching, a former collaborator of Prof. v. Neureiter's and Director of the Department of Comparative Religion in Bonn, Dr. Dubitscher, as representative of the German Board of Health, and myself.

reaction time was to be examined. The question was particularly interesting whether thought contents of the periphery of consciousness would be more easily transmitted than central contents. For this purpose cards with a picture in the middle had been prepared. The picture was to draw the attention forcibly to itself. A word, in small letters, had been placed on the edge of the card.

The experiments were made in July 1936 in a room of the Riga broadcasting station. We constructed a cell out of curtains equipped with windows permitting unnoticed observation. Everything was done to avoid frightening the child with the new apparatus. On a trip to the shore we got to know the child better and Ilga quickly became friendly. The fact that we could not speak Latvian and needed the aid of our amiable Latvian colleagues rendered our experimental work very difficult.

To get a first impression we began with a short-distance experiment (mother and child being separated in the observation-cell by a screen) the conditions and results of which we recorded by moving pictures and dictaphone. The mother had to take cards with Latvian words (prepared by disinterested persons) out of a box, show them in the direction of the camera, and try to transmit them. An assistant watching Ilga took notes on a slate which she in her turn showed to the camera. Most of the transmissions were successful but the mother could not resist encouraging the child with exclamations such as "Ilga, think!", "Say it right!", "Now go on", etc. Ilga uttered the words in syllables in a monotonous tone of voice, often impatiently. The lips of the woman, who was an unusually excitable motor-type and difficult to manage, were often moving simultaneously with the child's utterances. But no sounds could be heard. In one case, the transmission of the word "saulite" (little sun) occurred so quickly that the mother had no chance of interfering. The dictaphone record of this case contains no audible aids.

Ilga was very moody. She tired easily and was accustomed to being immediately rewarded for her performances by many presents. Thus, to get her to take part in further experiments she had to be induced by a trick (introduced by the Latvian commission in earlier experiments but which, unfortunately, became known to us too late). Little boxes with small toys had been prepared, the contents of which the mother was to transmit. The hope of opening the box after the experiment and of keeping the object after the correct utterance stimulated the child. This was of course anything but a pure telepathic situation.

In this way we got a number of correct answers. But the mother's

encouraging remarks continued and she even raised her voice when the distance between her and the child was gradually increased. We finally brought her into the sound-proof chamber (which, by the way, did not completely isolate if one shouted) in order to send from there, at first with the door open. But already in this situation the child no longer reacted, but said several times: "I can't hear anything, how can I say anything when I can't hear anything?" If the mother raised her voice in encouragement the experiments were again successful. But as soon as the door of the chamber was closed, Ilga protested and no transmission took place. With two-way-loud-speaker connection between mother and child correct transmissions were again achieved with the usual aids. When the loud-speaker before Ilga was cut off before the presentation of the word, the child remained silent or maintained her inability to say anything. Now it is possible that the encouragement could possess the function of forming a close psychic rapport between mother and child without acoustically transmitting the word, or *i.e.*, containing phonetical elements of the word which would enable Ilga to guess it. This possibility was even to be considered in face of the fact that the mother occasionally audibly uttered the opening sound of a syllable and called, for example: "Nu saki (now say) nu'k," when the word to be transmitted was "kupite." We tried to make sure of this by means of a trick: encouragements and aids of the mother were recorded with our dictaphone during an actual transmission-situation. We then let the mother talk with the child and "send" from the sound-proof cell by means of the loud speaker and at the moment of the exposure of the word we switched over to the "artificial mother." Ilga was deceived and thought she heard her mother speaking. But no correct transmissions were achieved nor did Ilga reproduce the word to which the dictaphone encouragements referred. An unforeseen complication, namely, the sensitivity of our amplifier to the sudden beginning of broadcasting, allowed us only a few experiments of this kind. As a result we could not come to any conclusions. It seemed to us however, to be the methodologically best way to gratify the desire of the child to hear encouragements and at the same time to exclude acoustic aids for the words actually sent. Of particular interest was the question whether Ilga would possibly discover from the dictaphone-aids the word to which the latter referred. It was one of the most astonishing and illuminating results of the experiments carried on later by the Latvian commission employing this device that Ilga succeeded in finding the word in some cases. More of this later.

The acoustic encouragements, corroborations and corrections which the mother generally gave during the transmission, were subjected to an exact analysis. At the time it could be shown only in a few cases that the mother audibly mingled in with her interjections the opening sound or separate syllables of the word being transmitted. On the other hand it was generally possible to ascertain that the mother moved her lips simultaneously with Ilga's answers, divided as usual into syllables, without real whispering being detected except in one or two cases. This simultaneous movement of the lips occurred especially when the child read from a book together with the mother (ordinarily without even looking at the text and rocking to and fro in the chair, totally uninterested.) Occasionally Ilga spoke quite fluently up to three or four syllables without the mother having by an explosively uttered "ja" to encourage her to continue. The mother showed obvious movements of the lips as she followed the text with her forefinger. When the attempt was made to tie a cloth about her mouth, Ilga protested and sobbed: "Now you look like a monkey." Of particular interest was one experiment in which the word "bumbina zala" (a green marble) was to be transmitted, but the child said "lo-ti-da," the Latvian synonym for marble in slightly distorted form. The mother called out: "Say the same thing in another way." The child's governess called attention at that time to the cases which cannot easily be brought under the heading of an acoustic theory. She noted that Ilga frequently did not receive the word transmitted but a synonym, for example, "spickas" (matches) for "ceručini." Another case is worth mentioning in which a transmission over a distance of 16 meters was effected. This time the attempt was successful in having the encouragements made by the teacher of the child, with the mother evidently trying very hard to remain quiet. As transmission-object I showed the mother a postcard with the number 8 on it, which was to be hidden and the hiding-place to be guessed by Ilga. After the third request of her teacher, Ilga repeated "kar-ti-na." The observer watching the mother believed that he heard the letters k-m. While Ilga's observers were asking where the card was hidden I urged the mother to send the number 8 in addition. Ilga said: "mother does not think that," and no further answer could be elicited.

As a result of these all too brief experiments in Riga our commission, could come only to the conclusion that while the child undoubtedly displayed an unusual perceptive faculty it functioned only when the person transmitting was within the range of Ilga's hearing. The opinions of the numerous observers on the manner of transmission were

very diverse. Since these diverse opinions each represent a different working hypothesis for a further investigation, I will attempt to bring them into a systematic order. But let me first relate some observations and experiments which I was able to make in R., the home of the child, in the presence of the child's governess and occasionally with the physician Dr. Kl. as witness. It was my object to study the question of acoustic aid with a view to discovering the most promising way of carrying on further experiments. Not only the behaviour of the mother but also the general reaction of the child to acoustic aids had to be examined. How would Ilga react to *voluntary* whispering of the words to be transmitted? For this purpose transmission-experiments were made in three stages:

- a) with the mother's lips closed tightly,
- b) with voluntary, just audibly repeated whispering of the word,
- c) with the usual encouragements by the mother (Now say it, think it over, etc.)

The mother was sitting with her back to Ilga at a distance of three meters; the movements of her mouth were observed by me personally.

The transmission of the word "Dumi" under these circumstances was effected as follows: In case (a) Ilga said reproachfully: "I can't hear at all what mother's thinking." In case (b) she protested animatedly: "You hoot like an owl" and in reply to the question of the governess whether she now knew the word (for the guessing of which a prize had been promised) Ilga said: "No, I can hear, but I can't understand the word. Mother makes a noise like an owl." In case (c) she uttered in the usual monotonous tone at first "du-bi" then "du-bit," and suddenly, clearly and distinctly, "du-mi." All such experiments took a similar course. In this case the child was not attentive to just-audible whisperings, and even rejected a word-transmission effected in this way, although a much desired reward was to be expected for a quick correct answer. Other occurrences support this observation (which had been already made in Riga but had disappeared in the pro and con of discussion.) In an experiment made from one room into the other the child made such a noise with a pail (which had unfortunately been given her as a reward) that the governess could hardly understand the encouragements of the mother. Nevertheless the number 333, which the child repeated as usual with "tris-tris-tris," was successfully transmitted. Transmission-experiments with the little brother as agent, which, according to v. Neureiter's report, had been surprisingly successful, brought no result—a sign that the phenomena are not constant.

It is evident that, considering the apparently complex structure of the phenomenon, individual observations in which no audible acoustic and optical aids could be ascertained were of particular significance. It is only through exact analysis of such individual cases, which v. Neureiter seems to have been able to observe in a far greater number than was possible for ourselves, that one could avoid prematurely assuming that acoustic aids within the normal threshold of hearing were the exclusive means of transmission. The investigation has shown that the latter played a large and very considerable rôle in the transmission but whether, apart from them, still other kinds of transmission, subliminal or genuinely paranormal also were at work, was still an open question. I was much impressed by the fact that, for example, two of four picture transmissions were successful without the governess or myself hearing any aids and without a possibility of optical signs. One picture showed a dog with puppies and bore the inscription in Latvian: "Our dog Meders with puppies." The mother maintained that she tried to transmit "Suns" (dog) but that the child said "kuçens" (puppy). It was striking that numbers were often transmitted immediately and without any *audible* aid so that one can easily understand that v. Neureiter was so impressed by such results that he underestimated in other cases the rôle played by the encouragements and aids. Among these outstanding cases an observation of an incident of purely spontaneous nature was made. During an experiment Ilga suddenly began to cry and sobbed "I won't say anything. My governess wants to go away, that's why I won't." As a matter of fact the governess did plan to leave for Riga in the evening, a fact which she had kept secret from Ilga in order not to spoil her humour in the interest of the experiments. In another instance a transmission over a distance of 28 meters which, although in rather distorted form, was successful according to the description of the governess then taking notes on the child, was later questioned by the governess herself in regard to its exact course; so that I am forced to leave it out with the regret of not having any objective dictaphone record of it, although this distance would have been of significance for the problem of audible aids. Finally it should be mentioned that a repetition of the reading-experiment with the teacher of the child, who had first discovered her peculiar ability shows that the teacher whispers audibly. Ilga, visibly constrained, failed to react.

REVIEW OF THE WORKING HYPOTHESES

These diverse observations led to the development of diverse working hypotheses. We may now consider methodical possibilities for a

further investigation of the phenomenon such as were proposed by the different members of the commission. It was certain that the transmissions were effected only within the range of hearing. The appreciation of the by no means unusual cases in which transmissions were effected without any audible encouragements and aids, or in which these aids did not sufficiently explain the answers, varied according to the attitude of the individual observer, naturally depending upon his scientific branch and training. One group restricted itself entirely to the distinctly audible aids and declared the phonetical analysis of the encouragements and of the possible whispering as the only method of explanation. Others considered the fundamental acoustic character of the phenomenon as beyond questioning, but held it as chiefly belonging to the realm of subliminal stimuli, or thought of a possibility of a colouring of the encouragement by the word to be transmitted.

A physicist who played a leading part in the experiments of the Latvian commission was of the opinion that the transmission could have been effected perhaps in a form of an ultra- or infra-acoustic wave or that one must assume an astonishingly sensitive close observation of the breathing (of the mother). Some of the transmission might be contained in the utterances of encouragement on the part of the mother. Also different intonation, especially at the end of the interjections, might contain elements which could facilitate guessing by intimately connected persons. This physicist, to whom thanks are due for valuable suggestions, was of the following opinion regarding an hypothesis of a direct mental connection, i.e. regarding an hypothesis of telepathy: Such an hypothesis, he wrote in a report, may only be understood as a manifestation of a fourth dimension of true space; and one need not necessarily be a physicist in order to be sceptical of the calling upon of such possibilities.

The well-known German physiologist Geheimrat Abderhalden, who visited the child in her home, thought the child might react upon extraordinary subtle nuances of her mother's and other peoples' utterances. He is convinced that the mother does not purposely supply aids, and ventures the opinion that Ilga might perhaps concentrate her attention particularly on the sense of hearing for the reason that she is lacking in other capacities of a normal child.⁴

Whilst Prof. Rothacker suggested following the traces of hyper-aesthesia, I myself, as a result of my observations at the child's home, considered it advantageous to base some further experiments on the working hypothesis of incidental telepathy as they seemed to me to be more

⁴ A clinical examination of Ilga's auditory sense led to normal results.

easily realized. Therefore I proposed a repetition of the experiment with the "artificial mother" at the same time acoustically isolating the sender. In a close contact situation one might have started by filling the mother's mouth with a liquid, by which procedure the slightest opening of the lips could have been immediately detected. If after patient observation telepathy should not have shown itself to be actually a factor of transmission, then the part played by the acoustic helps, of which the occasional rôle was obvious—be they audible or beyond the threshold of normal audition—would have to be the object of further investigation.

EXPERIMENTS OF THE LATVIAN COMMISSION

This line of investigating the audible aids was followed by the Latvian commission in December 1936 and in April 1937. An expert in phonetics and the Director of a Deaf-and-Dumb Institute collaborated in these experiments. I cite an example from the record of an experiment (Dec. 14th 1936) in which a text of some length had been used for the transmission:

Ilgai Rigā iet eabi, vina vel nebranus uz māju.
(Ilga is all right in Riga; she will not yet go home)

At a distance of 5 meters the child was sitting with her back to the mother who was controlled by the phoneticist and by the deaf mute expert.

According to the record of the Latvian commission the experiment ran as follows:

- M. Now begin to read
 I. Ielga (laughs) Ilga
 M. And what next?
 I. Riga
 M. That's right.
 I. Ir laba (is well)
 M. Now begin to read from the beginning
 I. Ilga Riga ir laba (Ilga is well in Riga/this she says quite fluently)
 M. encourages to continue speaking
 I. vina ve-l ne-bus (she won't yet be . . .)
 M. interrupts Ilga and "br" is to be heard in her encouragement.
 Ilga does not notice it.
 M. repeats the whole beginning of the sentence.
 Ilga repeats it in her turn and adds
 I. ma-jas

At the conclusion of this series of experiments from which this example is drawn, the phonetic expert summarizes: "after every word of

encouragement there follows on the part of the mother the opening sound of the next syllable for example t'd, t'm, etc. The fact that the girl repeats not a whole word but separate sounds or syllables of the word proves that the girl listens attentively". This statement seems to be somewhat in conflict with the experimental report which literally states that in the encouragement of the mother "br" was audible (to which Ilga did not react) in which, however, the opening sounds so exactly heard by the phonetic experts are not to be found. In reading a German text during which the child behaved as described above, the mother gave the usual encouragements "go on", "yes", "then right". Whereas the phonetic expert establishes that the mother adds to each following vowel or consonant the first sound, the physicist remarks that he had concentrated his attention upon the encouragements of the mother during her reading and ascertained that the audible aids of the encouragement were much fewer in number than Ilga's replies. At times Ilga utters words which apparently have no accompanying encouragements. It is as if she grasped them immediately.

After the mother had been taken into the sound-proof chamber, the deaf mute expert was able, while looking through a window without hearing the mother, to read from her lips each of the words being transmitted. This shows that the sound complexes in these cases and probably in many others had been exceptionally clearly formed in the motor organs of speech so that it would be unsatisfactory to speak of a "thought-transference." Since an optical contact between mother and child was not essential to the success of the experiment we omit for the sake of simplification the problem of to what degree Ilga could occasionally orient herself by such oral movements in so far as they were visible to her. In a series of experiments over a distance of 10 meters most of them without mother and child being able to see each other (Dec. 1936 and April 1937), the mother gave so many aids that it was not only possible to reconstruct the transmitted words from the individual aids but also for the observers near the mother and even for persons seated further off to hear the correct solution even before Ilga. It is impossible for me to relate here the very impressive experiments as they stand in the report. (One may read them in Prof. Dahle's publication soon to appear.) However, as I announced, my special interest was attached to those cases in which audible acoustic aids did not seem to me to explain the phenomena satisfactorily.

From the concluding remarks of both experts the fact is to be drawn that the mother, during the transmission, made certain movements of sound articulation of which those of the lips and of the lower jaw

were especially observable. Movements of the tongue could not be ascertained because the mouth was generally only partially open but concerning these movements one could judge to some degree according to the state of tension of the facial and cervical muscles and according to the movement of the larynx. If the mother was some distance away the opening sound was more clearly audible. "After each syllable which the child correctly guessed," writes the phoneticist, "the mother said 'yes' or 'now then', immediately adding the consonant of the next syllable. This consonant communicated at the same time some of the vocal quality of the next vowel." On the basis of these observations the phoneticist comes to the conclusion that the child imitates complexes of sounds which she has picked up in some way or other. Several spontaneous remarks of the child corroborated this opinion, i.e. "the beginning and the end, mother has spoken properly; in the middle I could hear nothing."

With the technique of the "artificial mother" a great number of experiments were carried through. The voice issuing from the loudspeaker delighted the child. According to the records of the Latvian commission she almost crept into the loudspeaker and conversed with it as if it were the mother herself, crying out: "Oh, don't you hear what I say" etc. As the record does not mention whether the observers in these experiments knew or did not know the words to which the encouragement referred—so important a fact from a parapsychological point of view—I asked Prof. Dahle about this point. He informed me that none of the recorders knew the sequence of the words. As I always considered this experiment a very illuminating if not decisive one, I want to emphasize the fact that Ilga actually did succeed in uttering the correct word in some cases. Thus she distinctly uttered the word *pātaga* (whip), *vabole* (beetle), *kakitis* (kitten), *cuka* (pig) and some others. Now Professor Dahle wrote me that the mother's phonetical aid could usually be sufficiently heard by the three chief recorders, *but that in some cases they did not succeed in hearing them.* This latter circumstance seems to have struck another observer as being an outstanding feature of the experiments with the "artificial mother." The physician who drew Prof. Neureiter's attention to the child gave the following account, under the fresh impression of the experiments which convinced him of the acoustic nature of the phenomenon: In a letter which, of course, is not meant as scientific record, he writes that after a long series of words had been sent by the mother and recorded on disks, "when these were played it became positively evident that the opening sound expected by all of us could *not* be heard. And now Ilga was placed in front of these records

and told that she would now hear her mother from a loudspeaker. It was then that the most amazing thing happened. Ilga now, actually reproduced quite a number of words or only syllables in a striking way and did this in places on the record at which *absolutely* nothing could be heard beside the encouragements of the mother; so there was no opening sound . . ." After these experiments the physician had several of the records to which Ilga had reacted with the correct word played to him with an amplifier. The amplifier encouragements of the mother were reproduced in so loud a manner as to be almost distorted and yet an aid which might have contained the opening sound of the word transmitted could not be heard in the least. Dr. Kl. then goes on: "Now *this* was actually the situation which impressed me most of all, namely that Ilga—if all this is correct—is capable of 'hearing' something which I with my normal sound auditory sense could *not* hear even with the highest degree of amplification."

Furthermore Dr. Kl. mentions the strong impression made on him by experiments in which the child's governess sent according to a system of aids (recorded by the Latvian commission on May 1st 1937). Based on the theory of the whispered opening sound as help she had trained herself to such an extent "that with her the sending works at least just as well if not even more smoothly than with the mother. She does it with such skill that it is often only with difficulty—especially at some distance—to notice at all that the next opening sound is being breathed softly.

Experiments which the Latvian commission finally made with the child's schoolmaster (who had first noticed the phenomenon) clearly showed that Ilga also reacted to his audible whispering.

In view of these results the Latvian commission had good reasons for stating in their "Preliminary Report" that "Ilga's abilities are founded upon a specially oriented auditory and possibly also an optical function,⁵ and upon the proved aid of the sender. The contact between Ilga and the sender also plays a certain part which with the mother as sender has been shown to be of the closest nature". Ilga, we may emphasize again, certainly does not "read thoughts", but reproduces without generally comprehending their significance, sound groups which, at least when the mother is sending, are formed in the motor organs of speech. So the phenomenon seems to be a kind of mechanical imitation. (That this is not always the case appears for example in an experiment of the Latvian commission of April 1936 in which from one room into the other the mood of the mother, being in good humour, was to be

⁵ I neglect this latter possibility as not being essential to a transmission.

transferred to Ilga. The mother is laughing and begins to manipulate an apparatus which is similar to a pump and asks: "Ilga, tell me what I am doing?" The latter finally calls out, amused: "Pump. You are pumping." She accompanied this statement with a loud laughter unusual with her.)

In the "Preliminary Report", following the statement of the phoneticist and the deaf mute expert, audible whispering and audible furnishing of the syllables by means of the encouragements (opening sound, etc.) are set forth as the mode of transmission. The cases in which such aids were not audible, or only insufficiently so, are alluded to in the Latvian report in the one sentence: "The contact between mother and child is occasionally so close that it is rather difficult to ascertain the peculiar nature of the aids." This underestimation of these important cases seems to me to be the result of a tendency towards a rationalization and generalization, which, seeking to satisfy the requirements of a full explanation *according to one principle*, naturally sees only one side of a complex phenomenon.

Professor v. Neureiter's conclusions seem to reflect the same tendency in the paranormal direction. In the case of Ilga K. we have to deal with a complex phenomenon of close contactual understanding of which the particular theoretical interest for parapsychological investigation appears precisely in those cases which apparently cannot be explained by normally audible aids. Whether the latter case is based on an acoustic hyper-aesthesia and may then conform to the chief feature of the phenomenon, viz. the acoustic one (of which I am now personally convinced) or whether perhaps gradual transitions to paranormal, *i.e.* telepathic modes of transmission, exist (or a kind of simultaneous collaboration) is still an open question worth further investigation. Perhaps Professor Dahle, who in his personal opinion of the case also considers this possibility among others, will speak of it in his comprehensive report. He will probably outline in detail the more psychological side of this case, the discussion of which, in my function as historiographer of the experimental methods and results, I could not enter upon here. For a number of interesting questions arise, the consideration of which is the real reward for the arduous, and at times trying, experimental work. What part, if any, does the feeble-mindedness of the girl play in the development of her capacity? Are there accounts of similar although perhaps less striking cases in psychiatric literature? Is the child's strange gift a substitute for deficient mental abilities, a sort of "short circuit" of rudimentary intellectual functions which simulate results of thought? Has practice any part in the phenomenon and can

it have influenced its character in the course of time? How can it be explained that the ability functioned spontaneously during the first reading with the teacher? In how far is Ilga conscious of the mode of transmission? Finally, regarding the psychology of the mother, how does she (who certainly does not intentionally practice deception) explain her contact with the child? How do other senders occasionally succeed in gaining contact with Ilga without previous knowledge of a system of audible aids, as for example v. Neureiter's "Bruhte" case, also Ilga's little brother as sender and others? Have we to deal here with involuntary whispering which although often greatly exaggerated in its significance may perhaps indeed play for once a decisive rôle? Thus we come again to the heart of the whole matter: "How had the transmission been effected in the instances less obviously acoustically explainable?"

Many as the unsolved problems of this case may be, it must no longer be cited as an example of genuine thought-transference, which has unavoidably occurred in consequence of v. Neureiter's report.⁶

The controversy over the existence of telepathy is not entailed in the discussion whether the Ilga case is normal or paranormal. For one having an intimate knowledge of the field of parapsychology, the existence of telepathy can no longer be doubtful.

The case in hand shows once again the need for extreme cautiousness in judging facts gained by those observations made under the circumstances of a first meeting with obscure and questionable forms of mental activity. Such observations are more or less dependent upon the individual working hypothesis which in its turn is likely to reflect the position taken up consciously or unconsciously by the individual investigator, according to his scientific viewpoint. I risk this truism only to emphasize the more forcefully the necessity and value of objective records—cinematographic and acoustic—the aid of which in the investigation of such complex phenomena must be considered methodically indispensable.

⁶It must be said that our phenomenon has but little similarity with almost all known cases of spontaneous telepathy. A certain analogy exists in the case reported by the French physician Dr. Quintard of a seven year old boy who solved the most difficult arithmetic problems by "guessing" the solution calculated by his mother. This phenomenon was regarded by several French physicians as of a genuine telepathic nature. Cf. Moser: *Der Okkultismus*, Vol. I, p. 362. *Annales des Sciences Psychiques*, 1894, 221-228.

STUDIES IN EXTRA-SENSORY PERCEPTION: I. AN ANALYSIS OF 25,000 TRIALS

DOROTHY R. MARTIN and FRANCES P. STRIBIC

ABSTRACT: A long series of trials (25,000 in number) were performed under uniform conditions, by a single subject. The DT or "down through" technique was used throughout. All tests were screened. An average of 6.89 with a critical ratio of 29.35 was obtained.

Two control series were employed: The first consisted of a "reverse check," in which the guesses of the subject were checked against the reverse order of the pack. The second involved 25,000 trials of the chance matching of two packs of ESP cards. These series yielded averages of 5.03 and 4.98 respectively, with critical ratios of .51 and $-.28$.

Detailed analysis of the data revealed: 1. marked—but never below chance—daily fluctuations in scoring, with suggested correlation with physical fitness; 2. greater frequency for consecutive hits in Direct than in control series; 3. suggested correlation between scoring success and card position; 4. marked symbol preferences without significant effect on scoring.

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INTRODUCTION

THIS IS the first of a series of articles to be presented in consequence of studies made at the University of Colorado concerning problems in the field of perception without known sensory means. In this article will be presented the data obtained in a long series of trials (25,000 in number), performed under uniform conditions, with a subject who has previously demonstrated high scoring ability in card matching tests.¹ These data, together with two control series of equal length, will be subjected to rather complete analysis. In future reports, it is hoped that attention may be directed to experimental findings bearing upon such problems as adequacy of shuffling, the effect on scoring of the subject's knowledge of his score, and the role of will in scoring. These, together with other problems, are now under experimental consideration in our laboratory.

¹D. R. Martin, "Chance and Extra-Chance Results in Card Matching," J. PARAPSYCHOL. I, 1937, 185-190.

GENERAL CONDITIONS

The materials used throughout these tests consisted of standard ESP cards. These cards were arranged in packs of twenty-five, there being in each pack five cards of each design. Thus, chance alone would yield an expectancy of five correct guesses out of each twenty-five.

The subject was Mr. C. J., who, in a previous series of 380 runs,² averaged 6.4 hits per run. Throughout the present series, the subject, seated at a table, was separated from the experimenter by a screen. The DT or "down through" method was used exclusively. The experimenter shuffled and cut the pack, and placed it face down before her, taking care to avoid seeing the bottom card. The subject then recorded his guesses for the order of the pack. After the completion of each run, and in the presence of both subject and experimenter, the pack was checked with the guessed order. The data thus treated will be termed the "Direct Series."

CONTROL SERIES

Two control series were introduced:

1. *The Reverse Series* in which the subject's guesses for each run were compared with the reverse or "up through" order of the pack, and the "hits" noted.

2. *The Chance Series* in which the data were derived from the matching of two well-shuffled packs. For purposes of checking and of analyses, these pack sequences were recorded.³ This series not only serves as a control, but also provides an empirical check on theoretical predictions.

RESULTS

In Table I are presented the data—both Direct and Reverse—for 1,000 runs (25,000 trials), together with the Chance series. It will be seen that the Direct check yields an average of 6.89 hits per 25, with the highly significant critical ratio⁴ of 29.35. The Reverse check for these data yields an average of 5.03 hits per 25, and the clearly non-significant critical ratio of 0.51. The Chance check yields an average of 4.93 and a critical ratio of —.28, again statistically insignificant.

Further assurance of the significance of the Direct scores, as con-

² *Ibid.*

³ These, and all other records were doubly checked.

⁴ C. R. $\equiv \frac{\text{deviation from chance expectancy}}{\text{standard deviation for chance}} = \frac{d}{\sigma} = \frac{h - np}{1.02 \sqrt{npq}}$

trasted with the Reverse and Chance scores, may be obtained by reference to the Critical ratio of the Difference⁵ between these scores. The C. R. Diff. between Direct and Reverse series yields the highly significant figure of 20.39; that for Direct and Chance yields the similarly significant figure 20.94; whereas the C. R. Diff. for the Reverse and Chance series, .55, is obviously insignificant.

Frequency data for the three series are recorded in Table II and are given graphic representation in Fig. 1. It is to be noted that the mode occurs in the Direct series at 7, and in both the Reverse and Chance series at 4.

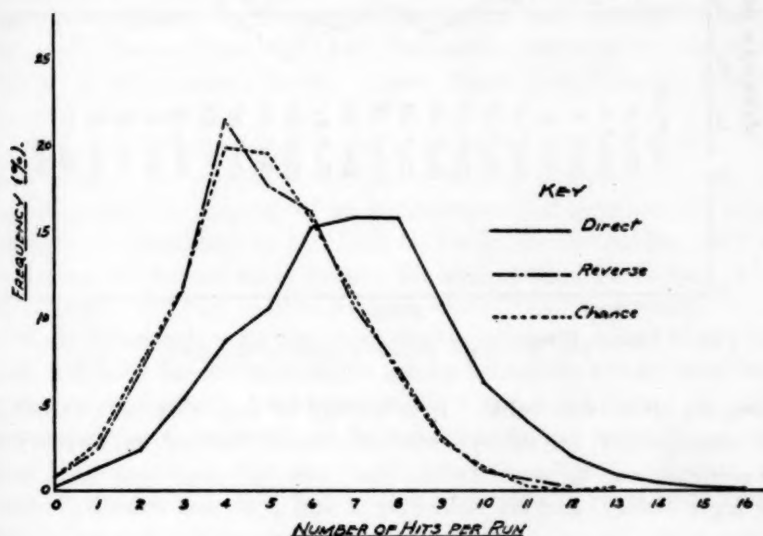


FIG. 1—FREQUENCY DISTRIBUTION FOR DIRECT, REVERSE, AND CHANCE SERIES

ANALYSIS OF RECORDS

1. *Daily Fluctuations in Scoring:* In the endeavor to study daily fluctuations in scoring, summaries of daily records were compiled. These are presented in Table III. A graphic comparison of the daily averages for the Direct and Reverse series is given in Fig. 2. It will be readily noted that the subject's Direct daily average never falls below the chance expectancy of 5 hits per 25, whereas the Reverse average

$${}^5 \text{C. R. Diff.} \equiv \frac{\text{Difference}}{\sigma \text{Difference}} = \frac{\text{Difference}}{\sqrt{\sigma_1^2 + \sigma_2^2}}$$

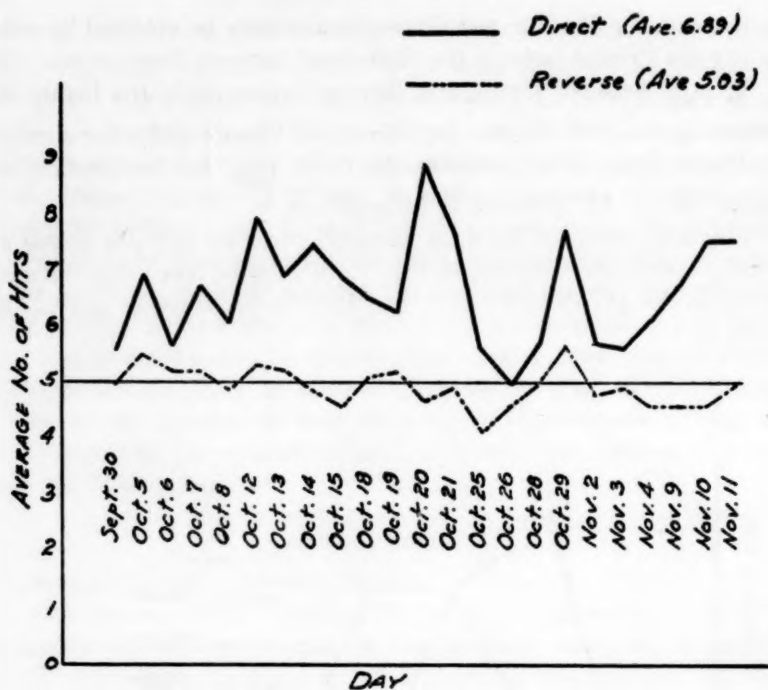


FIG. 2—DAILY FLUCTUATIONS IN SCORING

fluctuates about this value. It is worthy of note that on October 25 and subsequently, the subject reported extreme fatigue; on October 28, he reported that he was "feeling fine" but had slept only four hours the night before; and on November 2 and 3 he was suffering from a severe cold. Some positive correlation between scoring ability and physical efficiency would here seem to be indicated.

2. *Consecutive Hits*: It was noted that on days when the subject was scoring particularly well, he frequently scored several hits in succession. This observation prompted a tabulation of the frequency of occurrence of consecutive hits. This tabulation appears in Table IV. Here again is evident a rather striking contrast between the Direct and the Control series.

3. *The Relation of Scores to Position of Card in the Series*: In order to ascertain whether there might exist a relationship between success in scoring and position of the card in the pack, a tabulation was made of the frequency of scoring for the top card, the second card, the third, etc. This tabulation for Direct, for Reverse, and for Chance

series is contained in Table V. The rank, with respect to hit frequency of each of 25 card positions is also indicated.

It will be noted: 1) that for all card positions, the Direct Series frequency is above chance expectancy (200); 2) that in the Direct Series, the ten highest ranking frequencies appear in the first twelve card positions, whereas the five lowest ranking frequencies appear in the last eight card positions, thus suggesting a possible correlation between frequency and card position. Application of the Spearman Rank Difference method yields $\rho = .84 \pm .04$. In the Reverse Series the frequencies, (with one exception), exhibit a random distribution of hits with respect to card position ($\rho = -.03 \pm .14$). The unique advantage here exhibited by the middle position of card thirteen is not unexpected, since a direct "hit" here necessarily carries with it a reverse "hit". In the Chance Series, random distribution is again exhibited ($\rho = -.04 \pm .14$).

4. *Symbol Preferences*: In the endeavor to discover whether there may exist on the part of the subject a decided symbol preference, and to investigate the bearing of such preference (if any) on his scoring success, the tabulation to be found in Table VI was made. In it will be found the frequencies of guesses for each symbol, the Direct and the Reverse hits for each symbol, and the ratios of hits to guesses.

It is noteworthy: 1) that a marked discrepancy exists between the ratio values in the Reverse and in the Direct Series, the Reverse Series again yielding chance expectancy (20%) and the Direct Series again exceeding this expectancy; 2) that there exists an essential constancy of the ratios within each series. It would thus appear, contrary, perhaps, to expectation, that symbol preference has no significant bearing upon scoring success.

DISCUSSION

The authors wish to emphasize the theoretical implications:

1) Of the Reverse check. It will be recalled that the Direct and Reverse series were derived by subjecting identical experimental data to differing treatments. In the Direct check the intended order of the subject's guesses is preserved. In the Reverse check this intended order is violated. The Direct check yields a highly significant average score (6.89), whereas the Reverse check yields a statistically insignificant average (5.03). Obviously, the only factor which could operate to effect this difference is that of the subject's wilful intention to guess "down through" rather than "up through" the pack.

2) Of the relationship exhibited in the Direct Series between hit

frequency and card position. The far-reaching significance which might attach to this finding is so evident that further and varied experimental investigations have already been initiated.

CONCLUSION

In a long series of trials (25,000) under uniform conditions, an average above chance has been maintained by a single subject. Sensory cues of any known type have been excluded by the experimental controls. Analyses of the Direct data with reference to both Chance and Reverse series establish for these data a unique statistical position which would seem to be independent of all experimental factors except the subject's ability and intention to guess the cards in the order "down through." Should the "up through" technique now under investigation yield parallel results, strong support would seem to be offered for the postulate of extra-sensory perception.

TABLE I.
SUMMARY OF RESULTS FOR DIRECT, REVERSE, AND CHANCE SERIES

Subject	Runs	Trials	Hits	Av./25	Dev.	σ^*	C.R.
Mr. C. J.—Direct...	1000	25,000	6893	6.89	+1893	64.50	29.35
Mr. C. J.—Reverse..	1000	25,000	5033	5.03	+ 33	64.50	.51
Chance.....	1000	25,000	4982	4.98	- 18	64.50	.28

* $\sigma = 1.02 \sqrt{npq}$, the maximum standard deviation upon a matching hypothesis.

TABLE II.
DISTRIBUTION TABLE

Subject	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16
Mr. C. J.—Direct..	2	13	23	53	87	107	152	158	157	108	62	40	20	10	5	2	1
Mr. C. J.—Reverse	8	24	70	116	215	176	162	106	71	32	11	7	2	0	0	0	0
Chance.....	8	28	73	117	198	195	156	112	68	29	13	2	0	1	0	0	0

TABLE III.
DAILY FLUCTUATIONS IN SCORING

Date	DIRECT SERIES					REVERSE SERIES			
	Runs	Score	Av./ 25	Dev. from n.p.	C.R.	Score	Av./ 25	Dev. from n.p.	C.R.
Sept. 30...	30	168	5.6	18	1.6	150	5.0	0	0
Oct. 5...	50	349	6.98	99	6.8	278	5.56	28	1.9
Oct. 6...	50	288	5.76	38	2.6	262	5.24	12	.8
Oct. 7...	50	338	6.7	88	6.1	260	5.2	10	.7
Oct. 8...	40	247	6.1	47	3.6	198	4.95	2	-.15
Oct. 12...	80	633	7.9	233	12.8	425	5.3	25	1.3
Oct. 13...	40	278	6.95	78	6.0	208	5.2	8	.6
Oct. 14...	80	598	7.4	198	10.8	398	4.97	2	-.1
Oct. 15...	30	207	6.9	57	5.1	140	4.6	10	-.8
Oct. 18...	20	131	6.5	31	3.4	102	5.1	2	.2
Oct. 19...	50	318	6.3	68	4.6	263	5.26	13	.8
Oct. 20...	10	89	8.9	39	6.0	47	4.7	3	-.4
Oct. 21...	80	623	7.78	223	12.2	399	4.98	1	-.05
Oct. 25...	20	112	5.6	12	1.3	82	4.1	18	-1.9
Oct. 26...	10	50	5.0	0	0	46	4.6	4	-.6
Oct. 28...	50	287	5.7	37	2.5	254	5.08	4	.27
Oct. 29...	30	233	7.7	83	7.4	172	5.7	22	1.9
Nov. 2...	40	231	5.7	31	2.4	194	4.85	6	-.4
Nov. 3...	20	113	5.6	13	1.4	98	4.9	2	-.2
Nov. 4...	20	122	6.1	22	2.4	93	4.6	7	-.7
Nov. 9...	30	201	6.7	51	4.5	138	4.6	12	-1.0
Nov. 10...	70	528	7.5	178	10.4	323	4.6	27	-1.5
Nov. 11...	100	749	7.49	249	12.2	503	5.03	3	.15

TABLE IV.
CONSECUTIVE HITS FOR DIRECT, REVERSE AND CHANCE SERIES

Subject	2	3	4	5	6	7	8	9	10	11	12
Mr. C. J.—Direct.....	951	259	75	39	18	4	0	0	0	0	1
Mr. C. J.—Reverse.....	582	123	18	6	2	1	0	0	0	0	0
Chance.....	607	133	23	6	1	0	0	0	0	0	0

TABLE V.
RELATION OF SCORES TO CARD POSITION

Card		1	2	3	4	5	6	7	8	9	10	11	12	13
Direct...	Hit Frequency	301	317	324	298	291	304	274	297	278	309	302	296	277
	Frequency Rank	6	2	1	7	10	4	15	8	12	3	5	9	13
Reverse..	Hit Frequency	186	201	200	181	195	214	198	213	205	217	187	188	277
	Frequency Rank	21	11	13	24	17	5	14	6	7	3.5	19.5	18	1
Chance .	Hit Frequency	222	182	188	205	184	192	187	209	190	200	220	208	212
	Frequency Rank	3	23	19	10	21.5	17	20	7	18	11	4	8	5
Card (Con'd)														
			14	15	16	17	18	19	20	21	22	23	24	25
Direct ..	Hit Frequency	258	281	264	255	238	250	266	239	241	228	275	230	
	Frequency Rank	18	11	17	19	23	20	16	22	21	25	14	24	
Reverse..	Hit Frequency	184	178	219	201	204	201	217	187	197	184	196	203	
	Frequency Rank	22.5	2.5	2	11	8	11	3.5	19.5	15	22.5	16	9	
Chance .	Hit Frequency	176	223	211	184	193	196	224	206	198	197	197	178	
	Frequency Rank	25	2	6	21.5	16	15	1	9	12	13.5	13.5	24	

TABLE VI.
SYMBOL PREFERENCE

Symbols	Number Guessed	Direct Hits	Reverse Hits	Direct Hits Guesses (%)	Reverse Hits Guesses (%)
Star	5714	1535	1161	26.85	20.31
Circle	5496	1509	1088	27.46	19.80
Wave	4980	1371	1001	27.53	20.10
Cross	4836	1339	990	27.62	20.47
Rectangle	3974	1139	793	28.66	19.96

13
277
13
277
1
212
5

A COMPARISON OF CLAIRVOYANT AND CHANCE MATCHING

BETTY M. HUMPHREY and JOHN A. CLARK*

ABSTRACT: Forty-two college student subjects were given ESP tests with the BT technique. In a total of 1,800 runs a highly significant positive deviation of 1,365 hits suggested the hypothesis of extra-sensory perception. It was noted that a single subject, who made an average of 8.26 for 380 runs, contributed most of the deviation. The work of this subject is singled out for individual study. In a further experiment to test the possibility of explanation in terms of sensory cues the subject was given 50 runs under screened conditions which yielded an average of 9.28 per run. An empirical chance check, determined by the fall of dice, is offered as a gauge of the significance of results obtained.

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1. INTRODUCTION

THE SIGNIFICANCE of the now standard type of experiment in extra-sensory perception, conducted with the ESP cards introduced at Duke University, has usually been estimated in statistical terms which are dependent for their validity upon the correctness and applicability of certain theorems in the mathematical calculus of probabilities. But not only has the question been raised as to the accuracy of the mathematics involved; also another question is relevant—that as to the general adequacy of even a correct mathematical calculus of probabilities for dealing with all aspects of such problems in induction. It is still largely true today, as Bertrand Russell first remarked, that the theory of induction “is the scandal of philosophy”. Without entering on the controversial issues thus raised, therefore, except to request their continued recognition as controversial in fact and in a rather complex manner, the present writers have sought in the following report to find a test of the significance of some ESP card experiments of a very inelegant but perhaps unquestionable nature. We have brought together a large number of “guesses” as to the suit of a card of the type in question under conditions where extra-sensory perception might serve

* Mr. David D. Webb contributed much in directing the work here reported.

as a guide, and a precisely comparable series of trials guided, there seems every reason to suppose, wholly by chance. In all we have records of 45,000 possibly clairvoyant guesses or trials, 9,500 of these by one subject in whom there was special interest, as against 9,500 comparable trials guided by pure chance. All mathematical calculation of probabilities aside if you will, the contrast between these two sets of records seems very striking, and in general, highly confirmatory of the results which have been reported for the Duke University experiments.

2. CHANCE MATCHING CONTROL

The method of "chance matching" was as follows. A pack of 25 ESP cards was carefully shuffled, and laid aside for the moment. Ordinary dice were then thrown. A cast of one was recorded as indicative of that ESP card suit whose distinctive design is made with one line, the circle; a cast of two was taken to indicate the two line suit of the cross; of three, the suit of the three wave-like lines; of four, the rectangle; and of five, the star. When a six was cast it was disregarded. On this basis a record was taken of twenty-five consecutive and, in the sense stated, significant throws. Then the pack of cards was read through from top to bottom, and the sequence of suits there compared with that indicated by the previous throwing of the dice. Where there were correspondences between the two sequences (e.g. where the third significant throw indicated the suit of the third card in the pack, or the seventh throw, say, the suit of the seventh card) "hits" were recorded, but where there was no correspondence a negative record was taken.

Three individuals did the work, the first running through 80 twenty-five-card decks, the second through 150 decks, and the third through 150. Different dice were used by each experimenter. The results of this work may be summarized in the following table, which makes it easily

TABLE I.
CHANCE MATCHING

	Runs	Hits	Exp.	Dev.	S.D.	X*	Av./25
Experimenter A.	80	398	400	- 2	17.88	- .11	4.97
Experimenter B.	150	738	750	-12	24.49	- .59	4.92
Experimenter C.	150	748	750	- 2	24.49	- .08	4.98
All experiments together.	380	1884	1900	-16	38.98	- .41	4.95

*In this, and in further tables, X is the critical ratio determined by the formula: $X = d/s = \text{deviation over the standard deviation}$.

comparable with many typical experiments in extra-sensory perception and with the usual mathematical expectations.

3. EXPERIMENTAL RESULTS

The work in "clairvoyant" matching was, for the most part, conducted under a modified clairvoyant technique, BT, in which the top card of the face-down deck was called and laid aside by the experimenter, then the card which next remained on top, and so on. These experiments may be divided into three groups representing three periods of experimentation; first, October and November, 1936; second December, 1936, and January and February, 1937; third, March, April and May, 1937. The subjects participating in the first two periods were the students at Earlham College, where the work was done, who offered themselves for experimentation, while those of the third period were chosen from the former subjects to take part in "group experiments." Forty-two different subjects were tested in all, with a total of 1,800 runs of twenty-five calls each.

Of the total of 45,000 calls there were 10,365 correct calls or "hits", which is 1,365 above the normal expectation. This would give the entire group the high Critical Ratio of 16.08. It may well be noted, however, that if the experiments with the most interesting subject, Miss Virginia Bascom, are not included, the Critical Ratio figure of the group will be reduced to 1.67, with an average of 5.08 hits per run of twenty-five trials.

Negative as well as positive results were retained throughout, and except in the last period, no attempt was made to select for observation individuals who were scoring high. Of the forty-two subjects twenty scored at or above the normal expectation under chance conditions; twenty-two scored below. The Critical Ratio figure for four subjects was above + 2.5 and for one subject was below - 2.5. Table Two contains further details.

4. RESULTS WITH AN INDIVIDUAL SUBJECT

The work of Miss Virginia Bascom clearly calls for special consideration. At first this was not at all startling, since in the first period she obtained a positive deviation from the normal chance expectation of only 15 points in twenty runs, so obtaining a Critical Ratio of 1.67. In January and February in the following year (1937) she reached a Critical Ratio of 27.35, with an average of 8.32 hits per run of twenty-five calls. Finally, in the third period of experimentation, her Critical Ratio was 17.08, and her average 8.61 per run.

TABLE II.
GENERAL EXPERIMENTS

Subjects	Runs	Hits	Exp.	Dev.	S.D.	X	Av/25
1. V. Bell.....	40	163	200	- 37	12.65	- 2.92	4.07
2. M. Cooksey.....	30	124	150	- 26	10.95	- 2.37	4.13
3. F. Kruse.....	10	38	50	- 12	6.32	- 1.89	3.80
4. C. O'Hara.....	20	85	100	- 15	8.94	- 1.67	4.25
5. F. Love.....	10	40	50	- 10	6.32	- 1.58	4.00
6. M. Wayman.....	20	87	100	- 13	8.94	- 1.47	4.35
7. J. Dickerman.....	10	44	50	- 6	6.32	- .94	4.40
8. G. Ewan.....	10	41	50	- 9	6.32	- 1.42	4.10
9. C. Hogle.....	40	190	200	- 10	12.65	- .79	4.75
10. P. Williams.....	10	45	50	- 5	6.32	- .79	4.50
11. K. Frank.....	10	45	50	- 5	6.32	- .79	4.50
12. E. Wuenker.....	10	46	50	- 4	6.32	- .63	4.60
13. W. Lankford.....	40	192	200	- 8	12.65	- .63	4.80
14. H. Hecathorn.....	10	46	50	- 4	6.32	- .63	4.60
15. G. Stanley.....	20	95	100	- 5	8.94	- .55	4.75
16. M. Rapp.....	20	96	100	- 4	8.94	- .44	4.80
17. L. Maxwell.....	40	195	200	- 5	12.65	- .39	4.87
18. J. Briggs.....	60	294	300	- 6	15.49	- .38	4.90
19. M. Woollen.....	20	98	100	- 2	8.94	- .22	4.90
20. G. Coney.....	30	148	150	- 2	10.95	- .18	4.93
21. R. Wiebel.....	40	198	200	- 2	12.65	- .15	4.95
22. C. Englebert.....	10	49	50	- 1	6.32	- .15	4.90
23. M. McClure.....	20	100	100	0	8.94	.00	5.00
24. A. Balestrieri.....	10	51	50	+ 1	6.32	+ .15	5.10
25. M. Zimmerman.....	60	306	300	+ 6	15.49	+ .38	5.10
26. H. Walker.....	10	53	50	+ 3	6.32	+ .47	5.30
27. N. Haehl.....	40	207	200	+ 7	12.65	+ .53	5.17
28. R. Liverpool.....	10	54	50	+ 4	6.32	+ .63	5.40
29. W. Sprong.....	10	54	50	+ 4	6.32	+ .63	5.40
30. J. Ross.....	50	260	250	+ 10	14.14	+ .70	5.20
31. R. Gennett.....	10	56	50	+ 6	6.32	+ .91	5.60
32. L. Catron.....	10	56	50	+ 6	6.32	+ .91	5.60
33. M. Scantland.....	10	57	50	+ 7	6.32	+ 1.10	5.70
34. R. Pursley.....	110	576	550	+ 26	20.97	+ 1.23	5.23
35. M. Rush.....	10	58	50	+ 8	6.32	+ 1.26	5.80
36. P. Newsom.....	60	325	300	+ 25	15.49	+ 1.61	5.41
37. M. Woods.....	200	1058	1000	+ 58	28.29	+ 2.05	5.24
38. M. Bollinger.....	10	65	50	+ 15	6.32	+ 2.38	6.50
39. E. Prosser.....	20	123	100	+ 23	8.94	+ 2.57	6.15
40. R. Meijer.....	250	1332	1250	+ 82	31.62	+ 2.59	5.32
41. C. Salyer.....	10	76	50	+ 26	6.32	+ 4.11	7.60
42. V. Bascom.....	380	3139	1900	+1239	38.98	+31.78	8.26
All subjects taken together.....	1800	10,365	9000	+1365	84.85	+16.08	5.70
All subjects without Bascom.....	1420	7226	7100	+ 126	75.36	+ 1.67	5.08

Miss Bascom was tested both in individual and in group tests, although in fewer of the latter. It was found that some of her highest scoring was done in groups. These groups were composed of from

two to six subjects, each of whom had made fairly high scores individually. The method of conducting these experiments was as follows: The observer shuffled the deck of twenty-five cards, and laid them face-down on the table. The subjects then wrote down the symbol of the top card, after which it was removed by the observer, and so on down through the deck.

It was found that Miss Bascom made somewhat higher scores when the observer called the order of cards. For example, the observer would call, "one, two, three" while the subjects wrote down the symbols which they believed were first, second, third, etc. in the deck. This forced the subjects to work more quickly than they otherwise would. Although in the case of the other subjects participating in the group experiments this technique seemed not to alter the scores in any way, in Miss Bascom's case an improvement seemed to follow. In one case, under this technique, she obtained as high as eighteen hits in twenty-five trials.

In experiments done individually Miss Bascom reached an average of 8.13 per twenty-five calls, where this figure covers a period of three hundred runs. Her average for eighty runs done in "group experiments" was 8.72. Table Three summarizes her main work.

TABLE III.
MISS VIRGINIA BASCOM

	Runs	Hits	Exp.	Dev.	S.D.	X	Av./25
1st Period.....	20	115	100	+ 15	8.94	1.67	5.75
2nd Period.....	270	2249	1350	+ 899	32.86	27.35	8.32
3rd Period.....	90	775	450	+ 325	18.97	17.08	8.61
All Experiments Together.....	380	3139	1900	+1239	38.98	31.78	8.26
Individual Experiments.....	300	2441	1500	+ 941	34.64	27.65	8.13
Group Experiments.	80	698	400	+ 298	17.88	16.66	8.72

In none of this work were the backs of the cards screened from Miss Bascom's view. It is not our impression that in this or in like experiments honestly done the chance of sensory cues is appreciable. The suggestion was made, however, that some check for this conviction should be provided, and consequently Miss Bascom ran through an additional 50 runs of twenty-five cards each, as before calling the top card of the deck in each case just before it was removed by the experimenter. In these trials, however, the cards were concealed from Miss

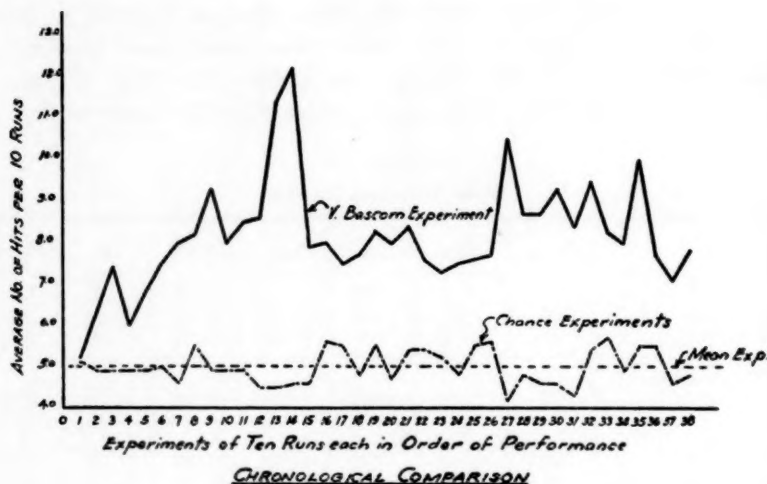
Bascom behind a wooden screen. The subject maintained her ability to exceed the chances in rightly naming the unseen cards, as Table Four shows.

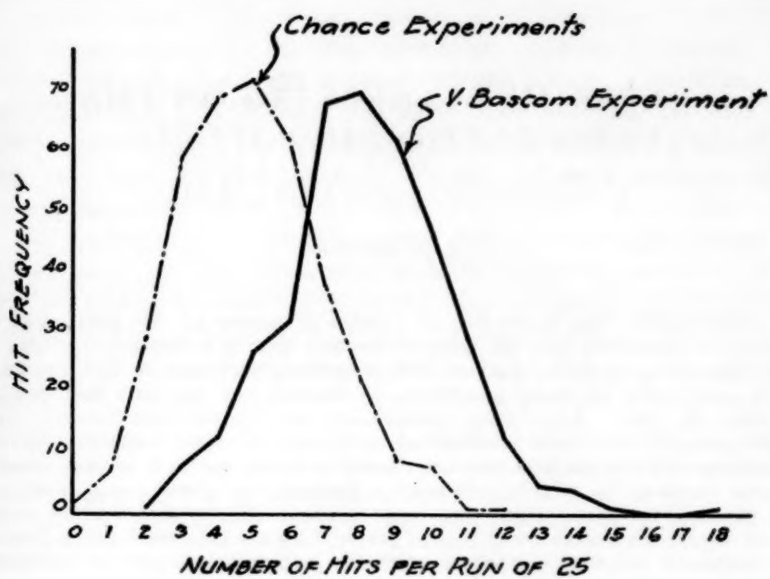
TABLE IV.
SENSORY CUES CONTROL

No. of RUNS	HITS	EXP.	DEV.	S.D.	X	Av./25
50.....	464	250	+214	14.14	15.13	9.28

5. COMPARISON

The contrast between the work of Miss Bascom and the results obtained by chance matching, which, as will be remembered, it is the chief purpose of this paper to point out, may be seen in the following graphs.



CURVES OF HIT DISTRIBUTION

EXPERIMENTS BEARING ON THE PRECOGNITION HYPOTHESIS:

I. PRE-SHUFFLING CARD CALLING

J. B. RHINE

ABSTRACT: This is the first of a series of reports by this author and by others, on researches into the question whether time is a limiting condition in the functioning of ESP. Certain ESP procedures were used to test whether a card order could be called in advance of shuffling and the calls then checked against the pack. After many preliminary tests (summarized herein), these "precognition" tests were administered to 49 subjects under especially guarded conditions in which subjects were not allowed to handle the cards or check results. In the course of the total 113,075 trials a deviation was given such as would not be expected by chance more than once in about 400,000 such lengthy series. Some hypotheses alternative to that of precognition are considered and a position of suspended judgment reached in which the precognition hypothesis is favored but not regarded as proved.

THE HYPOTHESIS that there is an extra-sensory mode of perception has by now passed a number of barriers to its establishment with a fair degree of success. These barriers have consisted mainly of three counter-hypotheses which should be considered as possible explanations of the results of the experiments made to investigate the question of extra-sensory perception. The first was that the experimental results were due to chance; the second, that they were due to sensory cues obtained from the test cards; and third that they were due to peculiar weaknesses in the research personnel. It has now been roundly recognized that the first of these, the hypothesis of chance¹ is quite inadequate to explain the results. Likewise the evidence obtained from the more advanced experimental conditions is showing the second alternative to be equally inapplicable as an explanation, for under these guarded conditions sensory cues from the test cards would be an impossibility. Meanwhile increasing repetition of the work on extra-sensory perception in other college laboratories is fast reducing concern over the third major question—that of the research personnel, by adding to the already considerable

¹ See statement by Professor Burton H. Camp, p. 305, December, 1937, issue of the JOURNAL OF PARAPSYCHOLOGY.

number of studies confirming the earlier work which was done in the Duke laboratory.

As the degree of general assurance as to the occurrence of this type of perception grows, it becomes increasingly possible to advance the inquiry further toward the discovery of the nature of extra-sensory perception or ESP. Toward this goal, those most familiar with the ESP research have long been impatient to advance. As a matter of fact, they have not been idle in this regard, and some important steps have already been taken.

In approaching the question of the nature of the ESP process or phenomenon, one may stress, first, its mental relations, and try to place it in the schema of what is already known of the mind. Or, second, its bodily relations, if there be such, may be sought after in terms of possible organic or physiological associations (or their absence). Finally, one may search after the more external, perhaps more general limiting and determining conditions of ESP such as its place (or lack of place) in the spatial and temporal schemata of events by which orientation in the physical world is achieved.

Beyond likelihood of question, every one of these fields of relations is of great importance. But of first consideration, if not of ultimately prime significance is the third problem, that of the relation of ESP to time and space, for the answer to this must prove a guide to the most fundamental distinctions which can be made in classification.

No sooner was the evidence from the ESP experiments past the criteria of statistical significance with sensory cues securely excluded than was distance between subject and stimulus introduced as a test condition. The consequences of this advance exploration were, as readers of this JOURNAL already know, sharply at variance with the general laws of mechanical nature as far as they are generally understood. As I have shown in a summary of the experiments in which distance was a condition of the tests² there is no effect of space yet discovered in the results of these tests, and it would appear from the data that no appreciable effect is likely to be found.

Now if the ESP process is not detectably affected by spatial conditions we may rightly inquire how it could then be detectably limited by time. For time measurements are made in reference to spatial relations, and time and space are apparently inseparable properties of such processes as we associate them with.

In a space-time world a phenomenon escaping space-limitation would

² See "The Effect of Distance in ESP Tests," JOURNAL OF PARAPSYCHOLOGY, I, 1937, 172-184.

be presumably time-free. Logically at least the hypothesis may be deduced that the process at work in the distance ESP tests should find no more of a barrier in time itself.

The hypothesis that ESP may be unlimited by temporal conditions thus arises inferentially out of the research already reported. Perhaps to those accustomed to rationalistic solution of problems it may be regarded as quite as strong as the evidence for ESP at a distance. But in science, the empirical solution always takes a more final place than the logical, and fortunately the question here raised is subject to a strictly experimental inquiry.

The general problem of the relation of time and ESP has two aspects, that of reference to future time in the question of precognitive ESP, and coordinately, that of the past in retrocognitive ESP. Greater interest, however, has always attached to the former, to the question of the apprehension of future events rather than of past ones, largely because future ones are of greater practical human import generally. Scientific interest and experimental facility too both support this preference and dwarf the importance of the problem of retrocognition by contrast.

If the order of a pack of cards may be called with an extra-chance degree of success regardless of the exclusion of sensory cues and regardless of distance, it should be expected, therefore, according to the hypothesis stated, that the cards could be called as they will be arranged in some *future time*, with *random rearrangement* intervening. From this reasoning it may be seen that the investigation of the hypothetical ability to precognize the future was a natural experimental development of the ESP research, as well as a logical outcome. The hypothesis was inferable and the methods were readily adapted from the routine ESP test-procedure. To the discussion of these methods I shall shortly return and give further detail.

GENERAL BACKGROUND OF THE HYPOTHESIS

The precognition (or prophecy) hypothesis represents, as is well known, what is for many persons a firm belief, and what has been through the ages an important part of religious and occult creeds and philosophies. There have been too, a great many different beliefs and theories held of the means by which it was believed the future might be read by those possessed of the training or the secret or the gift. These vary widely with different peoples and reflect in general the philosophical and religious backgrounds of the time and place of their origin.

Many such systems of prophecy persist today and enjoy a degree of confidence and extent of practice that are alarming.

The public indulgence in seeking to know the future through these prevailing schemes of supposed prevision indicates at least that there is great interest in the subject, and what is more to the point, that there has not been a convincing scientific study and verdict on the subject. It is true, of course, that scientific thought in general would tend to discourage belief in precognition, but likewise would it at different stages of the history of science have discouraged belief in most of the important subsequent scientific discoveries. It is therefore a question for scientific consideration if only in the interest of social enlightenment and mass mental hygiene. It would surely be healthier for society not to rely upon these false beliefs and deceptive practices if such they prove to be, as would appear most in the line of expectation from the orthodox scientific view of today.

On the other hand, the mere remote possibility of the occurrence of precognition is serious enough to fire the dullest imagination. Its implications are the most far-reaching conceivable, both for the theories of the mind itself and for the view of nature as a whole. There is no question that an adequate scientific demonstration of precognition would produce a major intellectual revolution. This fact alone would suffice to warrant an inquiry into the hypothesis of precognitive ESP.

But there are other grounds that might be given for the pursuit of this hypothesis of prescience. These are represented by the frequently-reported personal experiences of apparent spontaneous prevision of events and by the studies which have been made of these cases.

It is not necessary to the point in question to credit these numerous reports of prophetic insight with the finality of their face value. For the critical student this is impossible. But their collective impression may well lead the thoughtful reader at least to a recognition of the importance of further inquiry into their significance and perhaps to some tentative suggestions as to what to look for and how to go about a systematic research.

With one of the smaller collections of apparently previsionary experiences, largely consisting of dreams of the author himself, that of Mr. J. W. Dunne as reported in his book *An Experiment with Time*,³ the public is already relatively familiar. Readers can scarcely avoid the impression that there is something to be investigated in the "dreams which come true" as told by Dunne. But it is too easy to pick possible flaws in the method for one to be convinced of precognition. There

³London: Faber & Faber, 1934.

is too much latitude for possibly over-favorable interpretation as to success and too much chance for rational judgment to play a part in "predicting" the events of the coming day. What estimate to allow for chance coincidence, we do not know. This and other questions arise in the reading of Dunne's work and prevent acceptance of it at face value. But a residue of conviction remains: There is enough probability to warrant further research!

In fact, Dunne's book led to such a study made along similar lines of dream-recording and daily checking-up with diaries, under Mr. Theodore Besterman of the Society for Psychical Research of London.⁴ The results did not bear out Dunne's conclusions, but it should be said that Dunne felt the procedure was somewhat different.

A much less well known but more extensive collection and study of apparently spontaneous precognitive experiences was made by Mr. H. F. Saltmarsh of the Society just named.⁵ The cases treated by Mr. Saltmarsh were most carefully selected for authenticity from the large accumulation in the Society's files made over half a century of time, and number several hundred. A statistical analysis for internal consistency lends conviction to the impression of reality given by the report. Saltmarsh himself, in spite of manifest caution, is led to recognize an element of prescience as revealed by the collection. Again, there is the further collection of the late Professor Charles Richet (*L'Avenir et la Prémonition*), and there are other lesser ones.

Considering the frailties of human testimony however, one hesitates to grant so great a point as to feel convinced that the case for prophecy is made. But on the much smaller point by far, that the topic warrants a good looking-into, one is persuaded at once. And if one were not, there is still a further point to make:

Through all the ages there have been no two supposed phenomena more intimately associated than clairvoyance and prophecy. The two, whether true or false, have always gone hand-in-hand. Both in spontaneous experiences and among the professional clairvoyants or prophets have the two been inseparable. The suggestion is inescapable then, that a research which purports to have established the occurrence of the one phenomenon should push on with some justification to a thorough investigation of the other.

This lengthy effort at an explanation of the grounds for taking up

⁴ "Report of an Inquiry into Precognitive Dreams," *Proc. Soc. Psych. Res., Lond.*, XLI, 1933, 186-204.

⁵ "Report on Cases of Apparent Precognition," *Proc. Soc. Psych. Res., Lond.*, XLII, 1934, 49-103.

the precognition hypothesis is offered because the step will need much justification in the minds of many scientific men of the day. The very fact that an hypothesis of such importance should not have had any systematic experimental investigation prior to the work of the present report is an indication of a deep-seated conviction that to do so is absurd—against all common sense and science.

Indeed, had not the work of Dunne and Saltmarsh freshly raised the question, and the logic of the distance ESP work pointed both to the hypothesis and to an apparently easy method of experimentation, it is very doubtful if the present research could have risen through the resistant layers of indifference and scepticism under which all serious consideration of the question has long lain. In a word, the "days of prophecy" like the "days of miracles" have long been regarded in the halls of science as "past."

PRELIMINARY DEVELOPMENTS

In December, 1933, following his remarkable success in the now well known Pearce-Pratt distance tests, the subject HP was asked to try to predict the order of an ESP pack as it would be after shuffling. He was encouraged to try it by working alone in a preliminary series which would serve to decide only the question of the advisability of further tests.

The procedure was but little different from the established DT procedure for extra-sensory perception. The subject wrote in a record column 25 ESP symbols intending them to coincide with the order which the pack of cards lying before him would have after he had cut it five times in random fashion. The five cuts were intended to represent a very simple preliminary stage of shuffling and thus to introduce a random rearrangement between the predicting and the checking up.

In the 106 runs with this procedure which HP made while working alone he reported an average of 7.1 hits per run of 25 as against the expected chance average of 5 per 25 which was of course a very interesting result. (It is pertinent to recall here that in the 74 runs of distance tests just previously completed he had averaged 7.5 hits per 25. In these distance tests he had no sensory contact whatever with the cards.) It was clearly in order then to attempt this performance under test conditions.

Five other subjects experienced in ESP tests were encouraged to undertake these tests under similar preliminary conditions, except that they were asked to *shuffle* the pack *after* putting the call series on record and before checking up. The shuffling was mainly done by dovetailing

the two halves of the pack together, two such shuffles being given. All but one of these subjects obtained as did HP himself in 116 later runs with this modification, significant scores under the conditions; and the one exception did so later under even better conditions of observation. The totals for each subject are shown in Table I in the left-hand section.

It may be of interest to record that three of the six subjects were graduate assistants in the psychology department at Duke. Their own unwitnessed tests totalled 1,026 runs and averaged 5.8 hits per 25, which gives a critical ratio of 12. The 1,452 total runs of all six subjects averaged 6.0 per run ($CR = 19.3$). Although these results are not offered as a part of the main experimental evidence, there are some points of secondary interest to which later reference will be made.

The next step was to introduce an observer into the test situation. This was first done with subject HP with myself observing the entire procedure and checking all the records. The observer produced the pack of cards and cut it, and then the subject recorded his 25 calls for the predicted order of the pack. He then took the pack of cards under the observer's full vision and made five random cuts with the pack still inverted. The cards were then turned over one by one by the subject immediately under the observer's eye; and with the double observation, the checkup was made and the score (number of coincidences or hits in the run of 25) recorded by the experimenter.

In 110 runs of this procedure HP averaged 6.1 hits per run, and this gave a positive deviation from the mean chance expectation which represented a critical ratio of 5.7. The performance was evidently of extra-chance nature.

Then HP was given 25 runs in which he was asked to follow the call series with from two to five shufflings of the pack, also in the observer's presence. These runs averaged 6.2 and gave a critical ratio of 2.9. Again it was justifiable to go on to further explorations.

Next a definite period of shuffling (15 seconds) instead of a given number of shuffles was specified, still, however, with continuous witnessing by the observer. In 16 runs the average was 7.7 per run and the critical ratio 5.4.

It was apparent from these series of tests that it did not matter so far as extra-chance scoring was in question whether the cards were cut or shuffled, or just how the shuffling was done.

Three other subjects were introduced to these procedures under observation but with the subject handling the cards between call series and check-up. That is, the subject did the shuffling of the packs. Again two of these subjects were graduate assistants. The work of each

EXPERIMENTS BEARING ON THE PRECOGNITION HYPOTHESIS 45

was highly significant, as was that of HP himself. The totals for this set of conditions will be found in the middle section of Table I, giving a grand total of 425 runs, an average per run of 6.1, and a critical ratio of 11.0.

TABLE I.
COMPARISON OF RESULTS IN PRE-SHUFFLING CARD CALLING TESTS UNDER DIFFERENT CONDITIONS OF WITNESSING AND SUBJECT PARTICIPATION

SUBJECT	METHOD	A. Unwitnessed: Subject Working Alone				B. Witnessed by Experimenter Shuffled by Subject				C. Witnessed and Shuffled by Experimenter. No Contact					
		Runs	Dev.	Av.	CR	Experimenter	Runs	Dev.	Av.	CR	Experimenter	Runs	Dev.	Av.	CR
HP...	PDT.	223	469	7.1	15.4	JBR,SOZ,JGP	212	287	6.3	9.7	JBR,SOZ,JGP	482	85	5.2	1.9
GZ...	PDT.	124	21	5.2	0.9	SOZ,MHP....	31	33	6.1	2.9	SOZ.....	654	211	5.3	4.0
SO...	PDT.	260	549	7.1	16.7	GZ,MHP.....	142	93	5.7	3.8	MHP,SOZ....	59	18	5.3	1.1
MHP.	PDT.	642	229	5.4	4.4	SOZ.....	66	6	4.9	0.4
TCC..	PDT.	113	117	6.0	5.4	JBR.....	40	47	6.1	3.7	SOZ,GZ,JBR.	228	12	4.9	0.4
LHG.	PM..	90	110	6.2	5.7	EPG.....	114	43	5.4	2.0
Total.....		1452	1495	6.0	19.1	425	460	6.1	11.0	1603	339	5.2	4.2

*This table is complete for those subjects who performed under at least two of the three general conditions of witnessing. For the complete block of results of the third (right) section, see Table III.

For any problem of less revolutionary consequences these conditions would be adequate to warrant the interest and consideration of critical students of science. But in dealing with the hypothesis of precognition, the slightest loophole would be a fatal defect. Certainly, at any rate, we lose nothing by arbitrarily raising the standards of evidence. For those for whom these preliminary series suffice, they are of course available as evidence. And in fairness to the subjects and the experimenters it must be acknowledged that there is no ground whatever for any active question of the soundness of the tests described above and no reason whatever to believe any possible experimental inadequacy was in any way taken advantage of. (The shuffling of the cards was not screened as in later work because it was regarded as safer to have it under the observer's full attention.)

In the light, however, of the better conditions to be described there is no need to include these preliminary results in the evidence or even to attempt at this point a final estimate of their conclusiveness. We can afford to set aside completely, so far as the main question is concerned, all the tests in which the subject had any tactual contact with the cards during the test.

THE MAIN EXPERIMENTAL SERIES

The principal block of test results which bear on the precognition hypothesis represents something over two-thirds of the total tests made

under the range of conditions to which this report is necessarily restricted. Begun with subject HP early in 1934, this large collection of data has extended over the following four years down to December, 1937, through a total of 113,075 trials aside from the 46,925 trials of preliminary observation referred to above.

The research began in my laboratory under my own observation, but as with the earlier ESP work it quickly attracted the interest and help of others who carried it far beyond my own limited range of achievement. In fact, as in the case of the preceding research referred to, I can only claim to have made a beginning while the main burden of such confirmation and establishment as there has already been was borne by many capable hands that in time took up the exploration.

The names of the experimenters associated with this block of the research are entered opposite the series in which they participated in the table referred to. There are eleven in all. A brief word of introduction may be expected: Three of this group held engineering positions at the time, (Messrs. G. E. Buck, J. L. Michaelson of Schenectady, N. Y., and E. P. Gibson of Grand Rapids, Michigan). Six are already known to readers of ESP literature: (Miss Margaret H. Pegram, Dr. J. G. Pratt, Professor and Mrs. George Zirkle (Sara Ownbey), Mr. J. D. McFarland and myself). All six held at the time positions in a department of psychology, four having been graduate assistants. Dr. Pratt is at present Instructor in Psychology at Duke University and Mr. McFarland holds a similar position at Tarkio College. Mr. Zirkle is Professor of Psychology at Hanover College, and Mrs. Zirkle, sometime Instructor of Psychology at Asheville Normal and Teachers' College. Mr. Earl Stephenson is superintendent of the city school system, Stuart, Iowa; Mr. L. McCartney, an undergraduate student in psychology at Tarkio College.⁶

GENERAL METHODS AND CONDITIONS

The primary condition of this section of the research was that the subject had no tactual experience with the pack of cards on the sequence of which he was making pre-shuffle prediction. The experimenter alone handled them. In much of the work the cards were at a distance from the subject throughout the run but, even when not, obviously there could be no visual cues of a card-order not yet existent. This safeguard is designed to exclude all errors which might be introduced by the subject. If extra-chance results are obtained under these conditions, then

*The capable assistance of these contributors to the research is gratefully acknowledged.

it becomes a question only of the care in the handling of the data and the good faith of the experimenters in so far as existing alternative hypotheses are concerned.

Most of the 15 series of tests reported in this paper are based on the calling-before-shuffling modification of the DT procedure, called precognitive DT or PDT. (Exceptions will be noted.) The calls are made and recorded and the pack of cards then shuffled (face down) by the experimenter and checked against the record of the calls. The amount and type of shuffling varied somewhat with different investigators but consisted mainly of the dovetailing method, and of at least two such shuffles.

In making the calls in these PDT tests the subject either wrote down the symbols or called them orally to a recorder. This record in earlier series was checked against the pack by the experimenter (with the subject as a witness) after the "random rearrangement" shuffling was done. In later series a record of the card order was made and the checking done by comparison of call and card records, thus permitting later re-checking.

The exceptions to the test procedure referred to above consisted in adaptation of simple matching techniques to the precognition problem. One of these involved the open matching (OM) procedure. In this modification (POM) the cards were laid face down before a set of five blank spaces instead of the key cards, the key cards to be supplied by chance after the target deck was dealt out. The selection of the key cards afforded the "random rearrangement" since these were chosen by a specified routine procedure from a second pack which was shuffled by the experimenter after the target deck was dealt. The subject of course tried to match the cards dealt against the set of key cards that were yet to come.

In a similar way the screened touch matching (STM) method was adapted to the precognition research (PSTM). The key cards were chosen as just described, and the subject indicated his calls or choices by pointing to one of five empty shallow boxes which *would be expected to have placed in it later* the card to match that which the experimenter had on top of the pack held behind the screen.

In all but one series the experimenter did the checking with the subject also witnessing when this was not prevented by distance. But in the last series, two witnesses were introduced during the shuffling, card recording, and the checking of correspondences in the call and card records.

The subjects who have participated in these tests are all normal

persons. Of the total 49 there is little generalization to be made, except to say that 32 were grade school children in a public school selected from the school at large on the basis of superiority in earlier ESP tests. Nine of the other 14 were students at Duke University who had shown marked ESP ability. One was a professional medium, three were housewives, and one a city engineer.

A portion of the series conducted with the medium was done with the subject in trance, the so-called control personality cooperating (with no difference in results).

One series represented a brief and incomplete comparison of the performance of subjects in a normal waking state with their performance in post-hypnotic period for which supposedly favorable suggestion had been given them while in trance. A fuller report will be given of the series when further research justifies some conclusion as to the possible role of hypnotic suggestion on the abilities under investigation. Only the general results are given here (Table II). The general conditions described above apply also to this series.

Aside from these two exceptions there was no marked peculiarity about the conduct of the 14 test series reported.

All scores obtained under the conditions described are included here.

RESULTS OF THE MAIN EXPERIMENT

As may be seen in Table II, the 113,075 trials given in 15 series to the total of 49 subjects resulted in an average of only 5.14 hits per 25. But over so long a series this gives a deviation of 614 above the mean chance expectation of 22,615. With a standard deviation of ± 137.2 (corrected for the most conservative interpretation) this gives a critical ratio of 4.5 which represents approximate odds of over 400,000 to one that such a deviation would not occur by chance alone. The chance hypothesis is therefore excluded from further consideration.

Detailed data for the individual series may be had from Table II.

DISCUSSION

Low Score Averages: The most disturbing feature of this report, at first inspection, is the fact made apparent in Table I that the more the precautions (in the three general conditions) the lower the scores on the whole. Only two exceptions occur in the six subjects, namely GZ and TCC and these improve with witnessing. From 5.2 to 6.1 for GZ and from 6.0 to 6.1 for TCC. With experimenter shuffling, however, TCC dropped to 4.9 and GZ to 5.3. But it must be said that

TABLE II.

RESULTS OF PRE-SHUFFLING CARD CALLING TESTS IN WHICH THE EXPERIMENTER HANDLED ALL THE SHUFFLING AND CHECKING

Series	Period	Subjects	Experimenters*	Method and Conditions	Runs	Dev.	SD	Av. Score	CR	
1....	1934-1935	HP.....	JBR,SOZ,JGP....	PDT mostly at distance.....	482	85	±44.8	5.2	1.9	
2....	1934-1935	GZ.....	SOZ,MHP.....	PDT.....	654	211	52.2	5.3	4.0	
3....	1934-1935	SO.....	GZ,MHP.....	PDT.....	59	18	15.7	5.3	1.1	
4....	1935	HJ.....	SOZ.....	PDT.....	98	17	20.2	5.2	0.8	
5....	1935	JGP+EB.....	JGP.....	POM.....	45	12	13.7	5.2	0.9	
6....	1935	AJL.....	JBR.....	PDT.....	45	20	13.7	5.5	1.5	
7....	1935	GZ,SO,HJ....	GZ,SOZ.....	{Pre-hypnotic.....	89	7	19.2	5.1	0.4	
				{Post " period .	93	56	19.7	5.6	2.8	
8....	1935	MHP.....	SOZ.....	PDT.....	66	—	6	16.6	4.9	0.4
9....	1935	TCC.....	SOZ,JBR,GZ....	PDT.....	228	—	12	30.8	4.9	0.4
10....	1935	EJG.....	JBR.....	PDT.....	948	—	59	62.9	4.9	0.9
11....	1935	MB.....	GEB,JLM.....	PDT.....	35	30	12.1	5.9	2.5	
12....	1935-1936	LHG+EPG....	EPG.....	POM.....	154	49	25.3	5.3	1.9	
13....	1936	FM.....	JGP.....	PDT,POM,PSTM	1146	41	69.0	5.0	0.6	
14....	1937	32 school children.....	JDMcF, L.McC,ES.....	PDT.....	335	165	37.3	5.5	4.4	
15....	1934-1937	Misc. 3 Subj.	PDT,POM.....	46	—	20	13.8	4.6	1.4
Total	1934-1937	49 subjects	11 experimenters	PDT,POM,PSTM	4523	614	137.2	5.14	4.5	

*The initials of the experimenters represent names as follows: SOZ, Sara Ownbey Zirkle; JGP, J. G. Pratt; MHP, Margaret H. Pegram; GZ, George Zirkle; JBR, J. B. Rhine; GEB, G. E. Buck; JLM, J. L. Michaelson; EPG, E. P. Gibson; JDMcF, J. D. McFarland; LMcC, L. McCartney; ES, Earl Stephenson.

GZ's work for this phase falls into two periods separated by a gap of about a month. The first block of 97 runs average 6.4, the highest he had done for PDT and the second block of 557 runs averaged only 5.1. This later period was a poor one for all test procedures tried.

Likewise this later condition (i.e. experimenter shuffling) coincided with general decline in scoring in the case of subject HP also.

However, the fact remains that the other four subjects declined when the experimenter took over the shuffling. The first thought occurring to the cautious investigator is that this may be due to the additional safeguarding and that conversely the work under the other two sections A and B were experimentally defective. But while this makes little difference since the work of Table II *alone* is offered for consideration in connection with the precognition hypothesis, it should be said in fairness that there are other possible explanations for this decline. Some subjects such as HP believed they were helped by some contact with the cards and liked to be allowed to shuffle the pack in the usual test procedures. (Although HP's best ESP scoring was done without this—with distance and with no sensory card-contact at all.) The most marked exception to this was GZ who also did his best (6.4)

in the 97 first runs at PDT with experimenter shuffling. He wanted no sensory contact with the cards.

Also it is of importance to note that the most advanced condition was tried last and only after considerable preliminary work. The subjects here in the main declined with long series in all lines of procedure.

Even with the decline of scores with the experimenter shuffling, however, the averages are still comparable to the nearest type of test procedure, DT, as Table III shows. And as both Tables I and II indicate, the results obtained under the safest conditions are highly significant anyhow; this, after all, is the really remarkable and puzzling fact, and not why the average is not higher.

The Question of Selection: In this work, as in all reports from this laboratory, there has been a description of the conditions of the research and then a full reporting of all trials and scores made under those conditions. In Table II series of 20 or more runs are given individual treatment. Three shorter series are grouped under "miscellaneous" but included in the totals.

Hypotheses: Certain hypotheses are ruled out by the evidence, and others have to be considered further. The discussion of these is of first importance.

It was stated above that the results are sufficiently peculiar not to be due to *chance*. It might be said with equally good ground that *sensory cues* were not guiding the subject in making his calls, since sensory cues of future events are not conceivable. Any use either of *rational inference* or of *memory* is excluded by the "random rearrangement" intervening between the calling and the checking.

If the experiments were conducted according to report and the results handled with due care, there is no alternative to the conclusion that extra-chance pre-shuffling calling of card-order has been shown by some subjects. Whether the number of investigations suffices to assure ample confirmation and elimination of possible error and to allay sufficiently any concern as to the experimenter's good faith is a matter for individual judgment. It depends upon the difficulty experienced with the possibility of pre-shuffling calling. The alternative is to await the assurance of further confirmatory reports to follow.

In any case this report adds further strength to the case for extra-sensory mode of perception. That is, against the background of the great amount of work already reported, in which the ESP hypothesis has been confirmed, the weight of these further 113,075 trials which are safeguarded even better against sensory cues by the subsequent shuffling is very considerable indeed.

In other words, it may be regarded as easier to apply the evidence of this report to the further confirmation of the long series of reports favoring the ESP hypothesis, while leaving it still difficult to accept it as evidence of precognition. This larger question requires much more evidence even than the former. The only difficulty in this distinction is that in accepting the ESP character of the test results we have no known grounds for not going the rest of the way to a conclusion of precognition. That is, if the results are not due to chance, or sensory cues, or persistent error, or deception, there is no ready-to-hand alternative to the view that ESP is not time-limited.

But in the course of the research there arose new alternative hypotheses (not entirely unheard of but never before seriously considered) which must be disposed of before the present work can be regarded as conclusive evidence of extra-sensory perception of the future. These hypotheses have been themselves subjected to experimental inquiry in this and in other laboratories, and the researches will be reported in following papers.

Of first consideration is the possibility that in the act of shuffling the pack after the predictive calls are made there might be some guidance through ESP itself exercised by the shuffler. That is, through extra-sensory means he may be aware of the card order as well as of the call order (of which he is generally not sensorially aware) to a degree sufficient to stack it somewhat favorably. Only a little favoring is needed, of course, to produce the 614 obtained above the chance mean in 4,523 runs.

We need to know next, then, what the possibilities are for this hypothesis of an "ESP shuffle." Can it be effected by deliberate endeavor by certain subjects? It was necessary to investigate carefully the answer to this question before going on. The preliminary research on this will be reported in the next succeeding number of this JOURNAL. Doubtless it will occur to some readers to suggest that we might avoid the whole difficulty by adopting the use of *mechanical* shufflers. But those who follow the sequence of these reports will discover that this alternative too offers its experimental as well as theoretical difficulties. Suffice it to say here that the question is left unsettled by these and other possible alternatives.

On the other hand it would hardly be fair to the precognition hypothesis to neglect to mention certain evidential features which favor it even against this further alternative of the "ESP shuffle" hypothesis:

First, if the experimenter's shuffling introduced the extra-chance factor, he ought presumably to have success with all the subjects who

call the cards for him. But whereas I had obtained positive scoring as experimenter with subjects HP and AJL, the averages were approximately chance for subjects TCC and EJG. Likewise Mrs. Zirkle, as experimenter, obtained marked positive deviations from subjects GZ and HP but not from MHP and TCC. (However, Mrs. Zirkle obtained high averages from these subjects on other tests as did I also in the instances given above.) Other experimenters had similar differences. It is, of course, remotely possible that a difference could be attributed to particular subject-experimenter combinations even under the "ESP shuffle" hypothesis, and the above argument is far from conclusive.

A second and more urgent consideration lies in the fact that in most instances where comparative data are available, the scores in precognition tests paralleled the course of scoring of the subject on other test procedures. For example, in the instance of subject FM, when her first 20 runs in each of four types of precognition tests⁷ were grouped, the average for the 80 runs was 5.71 hits per 25. And when the first 20 runs made by FM on the comparable methods of DT and DTSTM (actually 19 and 24 runs because of irregular natural grouping) were pooled, the total 43 runs average 5.75 hits. The comparison continues through the decline which follows in all six methods: The remaining precognition tests given this subject averaged only 5.0; and for the plain DT and DTSTM, 4.7. This strongly suggests that the subject is following a trend peculiar to herself, and that the shuffling by the experimenter, Dr. Pratt, in the precognition tests was not the determiner of the results.

Again, subjects GZ and HP also underwent a great decline in scoring ability during the period of their precognition tests, although at a much slower rate of decline than FM. This decline appears in both the PDT and in the usual ESP tests and suggests strongly that their scoring ability in PDT was contributed by the subject himself rather than the shuffler of the cards. For example, GZ declined in the BT tests during the period from a score average of 8 per 25 for 46 runs to an average of 6 per 25 for 101 runs. The PDT tests began with 97 runs at 6.4 hits per 25 and after a break fell in the 557 later runs to 5.1 per 25. The case of HP, though it would require more detail to describe, is no less marked in its parallel decline in DT and PDT, all of these cases leave the impression that the three different experimenters involved were not causing the drop in deviation, but that the subject himself actually produced the change.

⁷ PDT, POM, PSTM and a special form of POM + STM. See above for account of these procedures.

It would seem wise therefore to regard the above evidence as well as the DT and PDT comparisons below as sufficient to warrant keeping the precognition question well open regardless of the "ESP shuffle" hypothesis, and to suspend judgment in the matter until that hypothesis is further examined in the light of experimental studies. A reasonable position would therefore combine a recognition of the ESP character of the performance tested, with a realization of the possibility that it might not represent actual precognition itself.

Comparison of DT and PDT. If time were to be no restriction upon the capacity for ESP we should not expect to find any appreciable difference between the DT and PDT results yielded by the same subjects under similar conditions. That is, calling before shuffling should be as *successful* as calling afterward (with the pack unbroken, DT). It is worthy of some attention at least that a fairly close approximation is found between the average DT and PDT scores of those subjects having taken tests in both procedures⁸ (see Table III below).

TABLE III.
COMPARISON OF SCORE AVERAGES IN TESTS FOR PRECOGNITIVE
ABILITY AND COMPARABLE ESP TESTS

Subject*	ESP TESTS			PRECOGNITION TESTS			REMARKS
	Pro- cedure	Runs	Av.	Pro- cedure	Runs	Av.	
HP.....	DT	360	5.3	PDT	482	5.2	Total work of both conditions.
SO.....	DT	30	5.8	PDT	142	5.7	Not same period. No DT taken in same period as PDT.
HJ.....	DT	39	5.3	PDT	98	5.2	Approximately same period.
AJL.....	DT	40	6.0	PDT	45	5.5	Approximately same period.
MHP**.....	DT	5931	5.3	PDT	642	5.4	Approximately same period.
	DT	1468	4.3	PDT	61	4.3	Approximately same period. (Low-aim series, both DT and PDT).
EJG.....	DT	152	5.3	PDT	948	4.9	Two methods used—a year apart.
MB.....	DT	24	6.6	PDT	35	5.9	
LHG.....	{ OM	96	7.0	POM	114	5.4	Different periods Precognitive match- ing used.
	{ DT	46	5.6				
FM.....	DT	43	5.7	PDT	80	5.7	Approximately 1st 20 runs of each of 2 DT methods and of 4 precognition test methods used with this subject.
	{ DT			PDT			
	{ DTSTM	200	4.7	POM	1066	5.0	
School children..	DT	218	5.3	PDT	335	5.5	Remainder of tests, both conditions.

*Subjects of Table II omitted from this table are those who did not perform on the ESP test most comparable; hence there is no basis for comparison. The school children in No. 10 series were not the same since both sets of tests, DT and PDT, were not given either group. But both groups are from the same school, are approximately of the same age and status, and were tested by the same experimenters.

**Unwitnessed work, but this subject was a psychology assistant.

⁸In the case of subject LHG the methods were OM and POM and this also shows the greatest difference of any series in the table. This subject's DT average is entered for comparison.

A general inspection of this table will undoubtedly leave the impression upon those familiar with the score ranges obtained in other ESP tests (varying from below 3 hits per 25 to above 18) that there is some unusual similarity in these DT and PDT scores—that the subject's ability in PDT is essentially that of DT scoring, and that the temporal factor is not inhibiting. But again, it could not be regarded as more than strongly suggestive of the occurrence of precognition.

The research may be credited, however, with having produced some considerable presumptive probability of the occurrence of precognitive ESP and then to have raised a new alternative hypothesis which points to a new line of experimentation. Whichever hypothesis proves to be correct, precognition or the "ESP shuffle," a new advance may properly be claimed for the research.

SUMMARY

There has been reported, above, the work of eleven investigators in testing 49 subjects for ability to call cards in advance of the supposedly random rearrangement of simple shuffling by the experimenters. In the 4,523 runs a total score of 614 above mean chance expectation was obtained, which is 4.5 times the standard deviation. The odds are over 400,000 to one that so great a deviation would not be expected to occur by chance alone.

With the chance hypothesis ruled out, with sensory cues impossible from an arrangement of the cards not yet in existence, and the shuffling safeguarded and handled entirely by the experimenter who also did all the checking, there is left no serious alternative to the precognitive hypothesis except that of conceivable unreliability of research personnel or the possibility (not an established one) of the experimenter unwittingly shuffling the pack, guided by ESP, so as to match to a significant degree the predictive calls made by the subject. The first alternative will have to be weighed non-statistically by the individual readers, whose criteria for tentative acceptance will vary greatly. The second hypothesis, the "ESP shuffle," is the subject of another research and a succeeding report.

EXACT PROBABILITIES FOR THE MATCHING HYPOTHESIS†

T. N. E. GREVILLE*

ABSTRACT: The theoretical hit-frequency distribution which arises from matching two shuffled decks against each other represents the most conservative basis for the mathematical evaluation of experimental data obtained with the standard ESP cards. The exact frequencies on this basis are here computed and tabulated.

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THE NATURE OF THE PROBLEM

AS POINTED out in a recent article in this JOURNAL (3), the variance of the correspondences between a subject's call series and the card series in a shuffled deck of ESP cards depends on what assumptions are made as to the nature of the former series. In the same article, two possible hypotheses are suggested: the "Binomial" hypothesis, and the "Matching" hypothesis. From a practical standpoint, the difference in the results obtained through these alternative hypotheses is insufficient to alter any experimental conclusion (3). However, a comparison of the assumptions underlying them is of some theoretical interest.

The Binomial hypothesis assumes that the subject's calls are entirely random: that is, each call is independent of every other. On this assumption (which has been made by most experimenters in this field until quite recently), the probability of exactly r "hits" in a single run through a deck of 25 cards would be given by the $(r + 1)$ -th term in the binomial expansion $(4/5 + 1/5)^{25}$. Complete independence would imply that any call-pattern is as likely to occur as any other; for example, a pattern consisting of 25 calls of the same symbol would be as likely to occur as any other specified pattern. The difficulty in making a truly random series of calls is well brought out by Mr. G. N. M.

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* The author wishes to acknowledge his indebtedness to Professors A. H. Copeland and C. C. Craig of the University of Michigan for certain helpful suggestions in connection with the mathematical development.

Tyrrell in a letter recently published in this JOURNAL (8, 3d paragraph). Although most subjects avoid making an effort to remember their previous calls (such an effort being considered a hindrance to ESP), it is unlikely that complete independence can be realized in a "free-calling" experiment.

The "Matching" hypothesis assumes that the call series, as well as the card series, will contain exactly five calls of each symbol. This, of course, is not generally the case, although at least one instance has been reported (6, p. 187) of a subject who made her calls by writing them down on a record sheet, always calling each symbol just five times. If a subject should deliberately try to shuffle a deck of cards so as to match another deck, the conditions of the Matching hypothesis would again be fulfilled. There is no simple mathematical expression for the probabilities under this hypothesis.

In practice, the value of the variance (and therefore of the standard error) appears to vary, depending on the habits of the individual subject and on the experimental technique used. However, its value can be fixed within certain definite limits. Attention has been called to the fact that a subject could reduce his standard error to zero by choosing to call only one symbol all the time (2). On the other hand, Greenwood has shown (1) that the maximum value occurs under the conditions of the Matching hypothesis. In most cases, the correct value is probably intermediate between the values of 2.000 and 2.041 obtained under the Binomial and Matching hypotheses, respectively.

The Matching hypothesis is, then, of theoretical interest: (i) because it gives rise to the largest possible value of the standard error and is therefore the most conservative basis for computing anti-chance probabilities, and (ii) because it corresponds with the actual conditions of certain possible experimental techniques. In an earlier article (2), Greenwood and Stuart expressed the hope that the exact theoretical hit-frequency distribution on this basis would soon be available. Huntington (5) has obtained the complete distribution for 3×3 and 4×4 decks, but considered the labor of computing the probabilities directly for the 5×5 case to be prohibitive. Sterne (7) actually calculated the probabilities, or relative frequencies, for exact numbers of "hits" from 21 to 25, inclusive, but saw no way to extend the exact computations practically to much smaller values. However, he obtained an approximate distribution (actually correct throughout to three places of decimals) by computing the first four moments exactly and fitting to them a Pearson Type I curve.

METHOD OF SOLUTION

The author has found a practical method of computing the exact values, and it is the purpose of this paper to explain the method and to present the results. In view of the theoretical importance of this distribution and the controversy which has centered around the mathematical aspects of ESP research, it seems advisable to have a full explanation of the matter available to investigators in the field. For this reason, an outline of the method is given, even though it may not be fully intelligible to the non-mathematical reader. Those who wish to follow the solution in complete detail will be able to do so with the help of a standard text book dealing with the mathematics of probabilities and of permutations and combinations, such as Hall and Knight's *Higher Algebra* (4) or Whitworth's *Choice and Chance* (9).

Let S denote the total number of possible distinct permutations or arrangements of an ESP deck, which is $25!/(5!)^5$ or 623,360,743,125,120. Then the total number of possible situations when two decks are matched against each other is S^2 . P_r , the probability, on a single trial, of obtaining exactly r hits is given (4, p. 373; 9, p. 178) by the formula:

$$P_r = \frac{W_r}{S^2} \tag{A}$$

where W_r denotes the number of situations out of the total S^2 in which there are exactly r hits. Let us now for a moment confine our attention to the hits which occur in r specified positions in the deck. For example, r might be 5, and we might consider only those hits which occur in the 3rd, 7th, 12th, 14th, and 21st positions in the deck. The number of situations (out of the total S^2) in which hits occur in all r of the positions thus singled out for attention can be shown (4, p. 125; 9, p. 61) to be:

$$\sum_{ra} K_r = \sum_{ra} \frac{[(25-r)!]^2}{[(5-a_1)!(5-a_2)!(5-a_3)!(5-a_4)!(5-a_5)!]^2} \times \frac{r!}{a_1!a_2!a_3!a_4!a_5!} \tag{B}$$

where the notation \sum_{ra} indicates that the expression following is to be summed for all possible choices of the numbers $a_1, a_2, a_3, a_4,$ and a_5 , subject to the condition that $a_1 + a_2 + a_3 + a_4 + a_5 = r$, the a 's standing for the number of stars, pluses, waves, rectangles, and circles, respectively, found among the cards in the r specified positions.

Considering now the product:

$$V_r = {}^{25}C_r \sum_{ra} K_r,$$

in which ${}^{25}C_r = 25!/r!(25-r)!$ represents the number of different ways of selecting r positions from a total of 25, it will be seen that V_r is the total number of situations in which r or more hits occur, with the added stipulation that any situation in which t hits occur (t being any number greater than r) is counted in V_r not once but tC_r times. In other words:

$$V_r = W_r + {}^{r+1}C_r W_{r+1} + {}^{r+2}C_r W_{r+2} + \dots + {}^{25}C_r W_{25}.$$

It follows from this (see 4 Chs. XIII and XIV) that

$$W_r = V_r - {}^{r+1}C_r V_{r+1} + {}^{r+2}C_r V_{r+2} - \dots + (-1)^{25-r} {}^{25}C_r V_{25}. \quad (C)$$

NUMERICAL COMPUTATION

Theoretically, the values of P_r could be computed from formulae (A), (B), and (C). However, the labor of calculating the values of $\sum_r K_r$ from formula (B) renders this formula unsuitable for practical use. However, it can be rewritten in the form:

$$\sum_r K_r = \{r![(25-r)!]^2/(5!)^{10}\} \sum_r H_r,$$

where

$$H_r = \frac{(5!)^{10}}{[(5-a_1)!(5-a_2)!(5-a_3)!(5-a_4)!(5-a_5)!]^2 a_1! a_2! a_3! a_4! a_5!}$$

in which it will be seen that $\sum_r H_r$ is the coefficient of x^r in the expansion of

$$(5!)^{10} \left[\frac{1}{(5!)^2} + \frac{x}{1!(4!)^2} + \frac{x^2}{2!(3!)^2} + \frac{x^3}{3!(2!)^2} + \frac{x^4}{4!(1!)^2} + \frac{x^5}{5!} \right].$$

It is evident that it would be possible to regard the arrangement of one deck as fixed and to consider the number of hits arising from the various permutations of the other deck. Let M_r denote the number of such permutations which result in exactly r hits. Then $M_r = W_r/S$. The following table gives the values of M_r , and those of P_r correct to seven places of decimals.

EXACT FREQUENCY DISTRIBUTION IN MATCHING
TWO ESP DECKS*

r	M _r	P _r	r	M _r	P _r
0	2,671,644,472,544	.0042859	13	245,747,376,750	.0003942
1	15,865,811,944,500	.0254521	14	58,035,729,000	.0000931
2	45,731,782,136,000	.0733633	15	11,932,686,260	.0000191
3	85,133,419,148,000	.1365717	16	2,133,510,000	.0000034
4	114,899,955,351,500	.1843234	17	331,120,250	.0000005
5	119,649,702,080,045	.1919430	18	44,350,000	.0000001
6	99,898,095,752,500	.1602573	19	5,141,000	.0000000
7	68,610,815,305,250	.1100660	20	497,500	.0000000
8	39,465,495,670,000	.0633108	21	44,125	.0000000
9	19,259,638,091,625	.0308965	22	2,500	.0000000
10	8,049,523,797,520	.0129131	23	250	.0000000
11	2,900,903,187,000	.0046537	24	0	.0000000
12	905,725,731,000	.0014530	25	1	.0000000

[* Ed. note: This table was available to the editors in correspondence of Nov. 23, 1937.]

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VARIANCE OF THE *ESP* CALL SERIES

J. A. GREENWOOD

ABSTRACT: The subject usually knows that there are just five of each symbol in an *ESP* deck, but when he calls them he seldom calls each symbol with exactly equal frequency. Nor is it probable that a human subject will call the symbols in an entirely random frequency. Although it has been shown that a variance assuming equal frequency (the matching hypothesis) and a variance assuming random frequency (the binomial hypothesis) are valid approximations, the exact variance of each run without either assumption is of interest.

A method for finding the exact theoretical variance of any call series is developed. When this is applied to an example the resulting exact variance lies between the customary approximations.

Dr. Greenwood, of the Department of Mathematics, Duke University, has collaborated in the presentation of two articles appearing previously in this Journal.

WHILE THE statistical processes with which *ESP* results are now being evaluated have been given the official stamp of approval and the conclusions of previous methods verified,¹ it seems appropriate to present a separate discussion of probably the most important statistic involved, the variance.²

The approval was based specifically upon the shown validity of assuming either the 'binomial' or the 'matching' hypothesis as approximations to the actual card-calling experiments. (9, p. 299 ff.). The variance of the binomial under the restrictions

$$n = 25, p = .2, q = .8$$

is known to be 4 by elementary statistics. Within the past year the variance of the matching case has been found independently by a number of people³ to be $25/6 = 4.1667$ approximately.

It is generally agreed that in the call series experiments the subjects do not follow, strictly speaking, the multinomial distribution of random calling to produce a theoretical binomial frequency of hits per run of 25 cards. Nor is the author aware of any experiment of the type im-

¹ See references 2, 6, 8 and 9.

² The variance is the square of the standard deviation.

³ The writer is aware of solutions, other than his own, by M. S. Bartlett, T. E. Sterne, E. G. Olds, H. Hotelling, and believes the result of M. S. Bartlett has priority.

plied by the title of this paper in which the subject equalizes the calls in more than a small percent of the runs.

Should any peculiar set of call frequencies give rise to a theoretical variance much greater than that of the matching case it would be necessary to examine every call series for this possibility to avoid possible over-estimation of the significance of the results.

The following pages outline in some detail, for the special case under consideration,⁴ the derivation of the variance for any frequency of calls, the maximal conditions, and an illustration.

An Equivalent Problem. Consider a deck of ESP cards which we shall call the 'target deck' and another deck of 25 cards composed of $S_1, S_2, S_3, S_4,$ and S_5 of the five ESP symbols. The only restriction on the S 's is the obvious one of non-negativeness. Let us designate this as the 'call deck'. We shall obtain the variance for the matching of these two decks against each other, card for card. This corresponds exactly to a subject calling S_1, S_2, \dots, S_5 of the ESP symbols.

Solution. Let the cards of the call deck be arranged one beside the other in a row face up, and indicate two of these cards with i and j . Now if the target deck is arrayed against the other, then at i there will be either a hit or a miss. A convenient notation for these will be $\overset{i}{1}$ indicating a hit and $\overset{i}{0}$ indicating a miss, respectively.

If i and j are the same ESP symbol then if $\overset{i}{0}$ it is true that $\overset{j}{0}$ 19 out of 24 times on the average, for there are 20 ways there can be a miss at j , but one of these is already used at i . A similar argument gives the values for the other combinations appearing in the following table. Within this table $\overset{i}{0}$ occurs $4/5$ of the time, so rows 1.1 and 1.2 must have a weight of 4 over the others.

		i	j	freq.	wgtd. freq.
(1)	1.1	0	0	19	76
	1.2	0	1	5	20
	1.3	1	0	20	20
	1.4	1	1	4	4
				Total 120	

When the i th and the j th call symbols are *different* ESP symbols the situation is slightly more complicated. Let $\overset{i}{0} = j$ indicate a miss at the i th call card, the target card there being *like* the j th call card.

⁴For the discussion of the general case of $t \times s$ cards see reference 3. The writer understands that another independent solution of the general case has recently been worked out by S. S. Wilks.

$i_0 \neq j$ will merely replace the word 'like' by 'unlike' here. Then in the same manner as before we form the table

	i	j	freq.	wgtd. freq.	
(2)	2.1	$0 = j$	0	20	20
	2.2	$0 = j$	1	4	4
	2.3	$0 \neq j$	0	19	57
	2.4	$0 \neq j$	1	5	15
	2.5	1	0	19	19
	2.6	1	1	5	5

Total 120

Since $i_0 = j$ occurs in $1/4$ of all the i_0 cases, rows 2.3 and 2.4 must be multiplied by 3, and as i_1 occurs in $1/5$ of all cases $i_1 + i_0$, rows 2.1, 2.2, 2.3 and 2.4 must total 4 times the last two, which they obviously do.

The number of ways in which the i th and j th call cards can have the same or different symbols are respectively

$$a_1 = \sum_{r=1}^5 \binom{Sr}{2} \text{ and } a_2 = \sum_{u < v}^{1, \dots, 5} \binom{S \ S}{u \ v} = \binom{25}{2} - a_1.$$

We therefore multiply the last column entries of (1) by a_1 and those of (2) by a_2 and combine into the following table of relative frequencies.

	i	j	freq.
(3)	0	0	$76a_1 + 77a_2 = a$
	0	1	$20a_1 + 19a_2 = b$
	1	0	$20a_1 + 19a_2 = b$
	1	1	$4a_1 + 5a_2 = c$

Yule (10, pp. 252-253) gives a formula for obtaining the correlation between the variables i and j (which is seen in our case to be the average correlation between all possible pairs i and j) in terms of the last column of (3). The formula

$$r = \frac{ac - b^2}{(a + b)(b + c)}$$

becomes upon substitution

$$r = \frac{a_2 - 4a_1}{96(a_1 + a_2)} = \frac{a_2 - 240}{5,760} = \frac{60 - a_1}{5,760}.$$

Evidently r is a maximum for maximum a_2 and this is so when all the S 's are 5, as can be quite readily shown. (3). For a situation which includes our matching problem Yule (10, p. 366) gives for the variance

$$\sigma^2 = npq [1 + (n-1)r]$$

where r is the average correlation between all the variables. Substituting our values we obtain

$$(4) \quad \sigma^2 = 25 \times 1/5 \times 4/5 [1 + 24r] = \frac{\alpha_2}{60} = \frac{300 - \alpha_1}{60},$$

which for the maximum value of r becomes

$$\begin{aligned} \sigma^2 &= 25/6 \\ \sigma &= 2.041. \end{aligned}$$

Discussion and Application. By setting $r = 0$ or $\sigma = 2$ we obtain $\alpha_2 = 240$, a necessary and sufficient condition that a given call run have the variance of the corresponding binomial case. Values of S_t giving $\alpha_2 > 240$ cause a larger variance. It is quite easy to find the variance corresponding to any set of calls. For example let the frequencies of the different symbols called be 66553. (Obviously what number corresponds to which symbol is immaterial.) Then $\alpha_2 = 6 \times 19 + 6 \times 13 + 5 \times 8 + 5 \times 3 = 247$ showing a larger variance than the binomial. α_2 corresponding to 77650 is 233 with a smaller variance, and 66661 with $\alpha_2 = 240$ has the variance of the binomial. From (4) and the expression for r it is clear that the maximum α_2 is 250, the minimum is 0, the maximum correlation, r , is $1/(24)^2$, the minimum, $-1/24$. Also the corresponding minimum α_1 is 50, and the maximum, 300.

We are now in position to find the *exact* theoretical variance of any call series data grouped in runs of 25 cards per run, the usual ESP procedure. α_1 or α_2 is computed for each call run of 25 and the result substituted in (4). These values of σ^2 are then summed for all the runs in the series, giving the variance of the series by the well-known distributive law of the variance.

This method will now be applied to the 74 runs of the Pearce-Pratt series discussed in a former issue of the JOURNAL. (4, p. 209 ff.). The following table lists the type-frequency distribution of calls, their frequencies, corresponding α_1 , and frequency times α_1 .

Type	f	α_1	$fx\alpha_1$
55555	7	50	350
65554	25	51	1275
66544	13	52	676
66553	2	53	106
75544	6	53	318
66643	2	54	108
75553	2	54	108
76444	1	54	54
76543	7	55	385
85444	1	56	56

85543	1	57	57
77542	1	59	59
86533	1	59	59
86542	1	60	60
77632	1	61	61
87532	1	63	63
96433	1	63	63
87541	1	65	65
Total 74		3923	

$$\sigma^2 = \sum_1^{74} \frac{300 - a_1}{60} = \frac{74 \times 300 - 3,923}{60} = 304.6167.$$

The corresponding variances for binomial and pure matching case are 296 and 308.333, respectively. The average variance per run of the binomial, Pearce-Pratt series, and matching case is 4, 4.116 and 4.167.

It has been remarked to the author that of the good subjects tested at Duke University, Pearce exhibited a greater tendency to equalize his calls than the others. A sampling of the call series of five of the best subjects shows the variance to be almost identical with that of the binomial, strengthening the author's belief that the binomial is perhaps after all the best assumption to make for the basic hit frequency distribution of scores.

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LETTERS AND NOTES

A SCIENTIST TESTS HIS OWN *ESP* ABILITY

[Ed. Note: The following letter records the observations of *ESP* test results, with the writer as his own subject, made by a prominent American experimental physicist. He prefers to remain anonymous in publication for the reason stated in his concluding paragraph. The present instance warrants exception to the general rule of the *JOURNAL* that the reporter is wholly responsible for the material he presents. This writer is outstanding among American scientists, and has received a high public honor for experimental work in his own field. His connection with an institution of unquestionable scientific repute renders in a measure justifiable the constraint here observed. His good faith cannot reasonably be questioned. While identification of the writer would assure a wide and respectful attention to his report, the editors feel that the account itself merits the interest of students in this field.]

To the Editors:

Upon receipt of the materials from the publishers, and with no expectation of making a positive score over chance, I attempted *alone* some experiments in clairvoyance. I preferred to do this alone, for then I was absolutely sure that, other than chance and clairvoyance, nothing could influence the fall of the cards.

I used throughout the colored pack of 25 cards comprising 5 groups of 5 cards each of different designations. I shuffled the cards before each run of 25 cards, sometimes by one or both of the usual card-player's methods, sometimes by dealing the cards in 5 or 7 heaps and combining the heaps in one or another order.

I then placed all 25 cards in a pile, face downward, and removed them one by one to another pile face downward, calling and recording my calls of each successive card. After the series of 25 was complete I compared the record with the actual order and noted coincidences. I am not conscious that I had any sensory means of knowing the designation of any card in all my observations, amounting now to 6,500 calls, until after each series of 25 calls had been fairly recorded. Perhaps I should add that I early got the impression that higher scores occurred in dim light, and hence I have in almost all these experiments used light only barely bright enough to record by.

I regret now that when beginning the experiments with no expectation that any positive or interesting results would occur, I kept no permanent records, merely recording runs whenever and wherever I found it convenient, carrying the pack of cards in my pocket for the purpose, and jotting down the records on the backs of envelopes or other scraps of paper which I threw away. Thus I called over a thousand cards, and merely preserved a record of the total coincidences in each run of 25 cards.

As the results began to surprise me, I began to use the record pad and made the observations at my house. But at first, I regret to say, I recorded only the calls I made in the columns provided for that purpose, and placed in the second

columns only a check or a cross according as the call proved a coincidence or not. Later I recorded both calls and the actual order of cards.

With these explanations I now proceed to results.

TABLE I.
FIRST 3,000 CALLS

Date 1937	Number of Thousand	Coin- cidences	Expec- tancy	Excess	Av/25	C.R.	Approx. Odds to Chance
Nov. 26 to Dec. 2.	First.	244	200	44	6.1	3.44	3,500 to 1
Dec. 3 to Dec. 17.	Second. . . .	234	200	34	5.9	2.66	250 to 1
Dec. 18 to Dec. 21.	Third*. . . .	211	200	11	5.3	0.86	5 to 1
Nov. 26 to Dec. 17.	First two. . .	478	400	78	6.0	4.28	100,000 to 1
Nov. 26 to Dec. 21.	All three. . .	689	600	89	5.7	3.97	32,000 to 1

*On December 19 I was fighting a cold which soon yielded. Whereas prior to that day 375 calls of the third thousand had yielded an excess of 7, on December 19 the result of 125 calls was a defect of 9. If this date were omitted the remaining 875 calls of the third thousand would give an excess of 20, corresponding to odds against chance of about 20 to 1, and of about 280,000 to 1 for 2875 calls.

These experiments, though seemingly favorable on the whole to the hypothesis of clairvoyance on my part, indicated a decline of the capacity with practice. I continued the experiments a month more calling 3500 additional cards with varying scores, at one time steadily below expectation, and at no time as high as the average of the first thousand. These later observations however yielded some very illuminating results.

Before giving them I revert in Table II to an analysis of the distribution of coincidences in the first 3000 calls. By this I mean the numbers of times that coincidences of zero, one, two, *et cetera* occurred in runs of 25.

TABLE II.
DISTRIBUTION OF COINCIDENCES

Coincidence number	0	1	2	3	4	5	6	7	8	9	10	11
Occurrence number	0	0	4	13	20	21	19	21	8	6	7	1
Expected occurrence	0	3	9	16	22	23	19	13	8	4	1	1*

*Fractional probabilities for higher coincidence numbers than 11 will make up the missing unit in the third line.

It will appear favorable to the hypothesis of clairvoyance that below the coincidence number 5 there is a defect from expectation of 13, while above 5 there is an excess over expectation of 16. The excess at coincidence number 10 is marked.

I now go on to the discussion of the additional 3500 calls made subsequent to Dec. 22, 1937. Total calls 3500. Coincidence 711. Expectancy 700. Excess 11. Standard deviation 18.70. Ratio 0.46. Odds against chance about 2.4 to 1. These results at first sight seem inconclusive.

I must bring up, however, some other considerations. As mentioned in the footnote to Table I, I made negative scores on Dec. 19 while fighting a cold. Many other times while observing the first 3000 calls, I got the impression that low scores accompanied a tired or aching head. To test this impression more rigidly I separated the results of the last 3500 calls into three groups: (1) Morn-

ing calls when in good shape. (2) Evening calls, after work. (3) Morning calls when sleep had been lacking, or I awoke with a sick headache. The results are given in Table III.

TABLE III.
EFFECTS OF OBSERVER'S CONDITION

Condition	Total Days	Total Calls	Coincidences	Expectancy	Excess	Av/25	C.R.	Approx. Odds to Chance
Group 1 Morning—well..	23	2100	476	420	56	5.7	2.99	700 to 1
Group 2 Evening—tired..	16	1100	191	220	-29	4.4
Group 3 Morning—ill....	4	300	44	60	-16	3.7
All Groups ..	35*	3500	711	700	11	5.1	0.46	2.4 to 1

*Some dates had both morning and evening observations.

If the research had been confined to mornings when the observer was well and well rested, 2100 calls would have resulted in odds of about 700 to 1 favorable to clairvoyance. But if the calls had been made only after the day's work or on mornings when the observer was sick or ill-rested, they would have indicated, after 1400 calls, negative clairvoyance, or in other words decidedly less coincidences than chance would lead us to expect. This latter result is quite in harmony with the experience of Dec. 19 when fighting a cold, as noted above.

Although the experiments thus far described seem to result favorably to the hypothesis of clairvoyance, there are considerations which indicate that if clairvoyance is real, higher scores would have been reached if memory and reason were absent. For after making a certain call it is natural to avoid that call till several cards are turned. And yet experience shows that frequently the same designation repeats itself immediately in a pack of well-shuffled cards. Again one may have a preference to associate two designations, for example stars with waves, or squares with circles. Thus the mind may be tempted either to follow or else to combat these tendencies, and in either case to alter the operation of pure chance, or, if clairvoyance is a reality, to overawe its feeble influences by the stronger more customary dictates of the mind. In this way many coincidences due to clairvoyance which otherwise would have occurred probably are lost.

There are two other similar considerations which occur, and which afford a method to test this type of mental interference. The Duke recording pad has each fifth line double. When one has named four different designations, and comes to the fifth space, he will either call the fifth designation, or else he will combat that tendency, and substitute some other. Similarly at the twenty-fifth space, he will either call the designation which memory suggests has been neglected, or else he will refuse to call it. Hence both the fifth and the twenty-fifth calls are likely to score lower than some others. This tendency would mainly be overcome if the observer had had a recorder instead of being alone as I was throughout my experiments in clairvoyance. In Table IV I give a test of this expectation.

Many of the runs of 25 cards each including in all 5250 calls, which were used in preparing this table showed little or no indication of clairvoyance for the whole runs of 25. But when their indications are confined to their first calls only, the score is fairly high, as the table shows. It is unfortunate that I did not preserve

TABLE IV.
SCORES OF FIRST, FIFTH AND TWENTY-FIFTH CALLS

Position	Number Calls	Coincidences	Expectation	Excess	Av/25	C.R.	Approx. Odds to Chance
Line one.....	210	58	42	16	6.9	2.76	300 to 1
Line five.....	210	43	42	1	5.1	0.17	1.4 to 1
Line twenty-five....	210	44	42	2	5.1	0.34	2.2 to 1

and use in this test the records of the first thousand calls, when scoring appeared to be at its height. As anticipated, lines five and twenty-five show very low scores, hardly above chance.

Another impression that grew upon me was that my clairvoyant power (if I may so term it) lacked something of exact focus. I mean that though in the long run coincidences would predominate for the cards intended, there would be also unexpectedly many coincidences with the card turned next before or next after the one intended. It is unfortunate that several thousand calls made when clairvoyance seemed at its best were not recorded completely enough for this test. But for all experiments completely recorded the results follow.

TABLE V.
FOCUS OF CLAIRVOYANT CALLS

Simultaneity	MORNING—WELL OBSERVATIONS:					
	Totals	Coincidences* Obs.	Expect. Corr.	Ratio Percent	Odds	
1. Calls 2 cards before event..	625	102	112	135	76
2. Calls 1 card before event ..	650	124	129	135	96
3. Calls simultaneous.....	675	164	164	135	122	300 to 1
4. Calls 1 card after event ...	650	117	122	135	90
5. Calls 2 cards after event...	625	107	119	135	88

Simultaneity	EVENING AND MORNING—ILL OBSERVATIONS					
	Totals	Coincidences* Obs.	Expect. Corr.	Ratio Percent	Odds	
1. Calls 2 cards before event..	325	64	71	75	95
2. Calls 1 card before event ..	350	86	89	75	119	28 to 1
3. Calls simultaneous.....	375	63	63	75	84
4. Calls 1 card after event ...	350	84	87	75	116	20 to 1
5. Calls 2 cards after event...	325	72	80	75	107

*Space forbids details, but some remarkable scores occurred. Zero occurs once each in lines 2 and 4. Unity occurs twice in line 1, once in line 2, twice in line 4, and once in line 5. Twelve actually occurs once each in lines 2 and 5. I have never met with any of these numbers of coincidences in the intended calls, though I recorded 260 runs with a total of 6500 calls.

**In explanation of the expression "Corr." meaning "corrected" in Table 5, I may remind you that for lines 1 and 5 only 23 comparisons were available, and for lines 2 and 4 only 24. Hence I have multiplied the observed coincidences by 25/23 and 25/24 respectively. The next column marked "Expect. corr." is the expectation for 675 cards to suit the column "Coincidences corr.")

The results of the table are, I believe, peculiarly interesting. They seem to show that while clairvoyance is pretty sharply focussed when the observer is at his best, there is still some slight indication of scattering to the cards next before or next after the one being turned. But when the observer is tired or sick, clairvoyance still may be active but poorly focussed, dividing its positive scores nearly equally between the cards next before and next after.

Regarding Tables III and V, I was inclined at first to suspect that the true value for chance is less than 5, because of the partial control by reason and memory on the calls made. But I do not now think these factors have any such influence. I am convinced that the "negative clairvoyance" of Table III and the percentages below 100 in Table V can be shown to be mathematical consequences of "poor focus" of clairvoyance when the subject is tired or ill, combined with the fact that in my experiments reason and memory inhibit the repetition of the same designation in calls closely juxtaposed.

I now pass to a couple of observations which do not impress me as of much evidential value, but which may be of interest. I do not recall much discussion of them elsewhere. An observer calling a run of 25 cards face-downward, and attempting to be as little under the influence of memory as possible, will be apt to overcall one or more of the designations, and to undercall others. He may even play favorites over long periods of time. I have counted the numbers of calls of each designation in 41 runs of 25 cards each, with the following results:

TABLE VI.
DESIGNATIONS OVER AND UNDER CALLED

Number of Runs	Total Excess Over 5—All Designations	Average Excess per Run	Average Number per run Called of Several Designations:				
			Star	Square	Circle	Cross	Waves
41.....	63	1.5	4.66	4.39	5.51	4.88	5.56

I thought at one time that these irregularities might lead to certain alterations from the figure 5 as expressing the expectation of coincidences per run due to chance, but after reflection I now think not.

Another matter, perhaps of interest, is an analysis of numerous runs to locate that part of the run most prolific of coincidences tending to indicate clairvoyance. I regret that in making up the following Table VII the results of the earlier observations, when scoring seemed to be at its height, are not available, as noted above. In fact, during a small part of the time covered by the following results negative values of excess were the rule. During the most of it, positive excesses not exceeding an average of 1.5 cards per run were the rule.

At the time when scoring appeared to be at its height I made a few tests of undifferentiated telepathy and clairvoyance with the aid of a colleague. I sat on one side of a desk with closed eyes, while my colleague on the opposite side attempted to mentally impress me to give correct calls, and recorded my calls and their coincidences.

Single runs of 25 cards each were made on three successive mornings. They yielded scores respectively of 10, 6 and 9 coincidences per run. In the first run my colleague became rather excited because I called nine cards correctly in the last thirteen of the run, and I was rather elated, too, when I learned of it. The tests were discontinued because my colleague was obliged to go away for a time.

Coming now to the end of this communication, I wish to repeat emphatically

TABLE VII.
DISTRIBUTION WITHIN RUNS OF COINCIDENCES

Part of Run:	CARDS					Total
	1 to 5	6 to 10	11 to 15	16 to 20	21 to 25	
In earlier thousand.....	46	50	43	56	40	235
In later thousand.....	41	37	36	44	29	187
In whole 2000.....	87	87	79	100	69	422

Success is fair from card 1 to card 10; less, card 11 to card 15; best, card 16 to card 20; and poorest, card 21 to card 25. In the earlier thousand, and in the entire 2000, there is some excess over expectation in the total.

that so far as I am aware neither transparency nor markings on the cards, reflections from support, nor any other means of sensory perception affected as much as one single call of all the 6500 made. All the calls are here reported as honestly and without reservations or bias as if my scientific reputation depended wholly upon it. No evidence has been suppressed.

In conclusion, if after having followed the reports of the Duke experiments, and confirmatory results elsewhere, I had remained in doubt of the validity of extra-sensory perception, I must have been convinced of it by my own experiments and the analysis which I have reported in Tables 1 to 5 above. So many harmonious observations unite to verify the hypothesis of a clairvoyant capacity, and so absolutely inexplicable to me are the results on any other grounds, that I regard clairvoyance as demonstrable as gravitation. That its operation and causation are still mysterious does not prevent my acceptance of it, for I have no other resource.

Should you wish to publish this communication you are at liberty to do so, but anonymously. My colleagues agree with me that in the present state of violent opposition to ESP it might be injurious to the organization with which I am connected to publish this paper over my name.

* * *

[The following correspondence with Dr. J. McKen Cattell by one of the editors is published without comment except to inform those who may not already know it that Dr. Cattell, formerly professor of Psychology at Columbia University, is editor of *Science*, *The Scientific Monthly*, and several other important publications. He has been president of the American Association for the Advancement of Science and of a number of other scientific groups; few men in American science have occupied positions of greater influence:]

Dr. James McKen Cattell
Grand Central Terminal
New York City

March 2, 1938

Dear Dr. Cattell:

We have frequently pointed out in our editorial pages that the *Journal of Parapsychology* would welcome critical or unfavorable articles or comments. Up to the present time, however, authors of unfavorable comments have preferred to offer them to other journals. Your letter of February 18 is an exception; and since it states in a particularly compact and definite form a point of view which I have no doubt is shared by many of our critics, we propose to make space for it in the forthcoming number of the *Journal*. If there are any revisions you may care to make, we shall be glad to receive them. If we do not receive any such revisions within a week, we shall assume that you prefer to have your letter appear as it stands.

Very truly yours,
J. B. RHINE.

THE SCIENTIFIC MONTHLY
Editorial Department

Grand Central Terminal
New York, N. Y.
February 18, 1938

Dear Dr. Rhine:

After a somewhat undue delay I have decided that we shall not be able to print the article on "Extra-Sensory Perception" in *The Scientific Monthly*.

I wrote that we might be able to accept an article describing your methods and results just as would be done in reporting any scientific research. This you have not done and what you write about the mathematical validity of results, good faith of the experimenters, etc., is not of scientific interest.

It seems to me that there is no reason why psychologists should undertake to investigate this work just because it has obtained publicity. In the course of fifty years I have known a large number of the claims of psychical research. There are no end of instances where there were plausible results, but not one which was finally proved to be correct.

The simplest argument against the validity of your claims is that I offer to wager \$100 against \$120 that in either a short or a long series, none of your subjects can in my office guess correctly more cards than the chance expectation of one in five.

Very truly yours,

J. McKEEN CATTELL.

Dr. J. B. Rhine
Duke University
Durham, North Carolina
Enclosure

Dictated. Signed in Mr. Cattell's absence.

* * *

Correction:

In his article in the December number of this JOURNAL, Dr. J. B. Rhine omitted from his summary Series 8 of the work reported by Carpenter and Phalen. This series of 2500 trials averaging 4.72 hits per 25, made by one subject with color cards, screened during the test and not handled by the subject, should properly come under the criteria laid down for the survey, and should have been listed under tests with screened cards. Its inclusion would change the final totals in the table on p. 288 to read: "Number of subjects, 119; trials, 145,325; runs, 5813; deviation, 4,597; S.D., 155.5; C.R., 29.5." The total average of 5.8 and the points of discussion and conclusion of the article remain, of course, unchanged.

* * *

Critical Articles in Other Journals:

The following recent critical articles concerning research in extra-sensory perception have been brought to the attention of the editors.

Gulliksen, H. O., "Extra-Sensory Perception: What is it?" *American Journal of Sociology*, 1938, XLIII, 623-631.

Heinlein, C. P. and Heinlein, J. H., "Critique of the Premises and Statistical Methodology of Parapsychology" *Journal of Psychology*, 1938, V. 135-148.

Kellogg, C. E., "The Problems of Matching and Sampling in the Study of Extra-Sensory Perception" *Journal of Abnormal and Social Psychology*, 1937, XXXII.

Rogovin, H., "Probability Theory and Extra-Sensory Perception" *The Journal of Psychology*, 1938, V, 265-270.

Wolffe, Dael, "A Review of the Work on Extra-Sensory Perception" *American J. of Psychiatry*, 1938, XCIV, 943-955.

A discussion by Dr. J. B. Rhine appeared in the January 1938 issue of the *American Journal of Psychiatry* concurrently with Dr. Wolffe's article. A survey of Dr. Kellogg's papers is offered by C. E. Stuart in "A Review of Proposed Hypotheses Alternative to Extra-Sensory Perception", in press for early publication in the *Journal of Abnormal and Social Psychology*.

* * *

ESP Card Imperfections.

Much is being said about imperfections in the commercial ESP cards and even about those with printed backs and hand-stamped faces previously in use. Most of the faults found justify notice, but certain extravagant claims deduced from them should be viewed with the utmost caution. The following questions are important: Have these faults invalidated reported research? Can the cards be used with safety as they are? Is it feasible to improve them?

In the beginning of ESP research at Duke University, when it was assumed as a working principle that the subject should, if he wished, be able to locate directly the card he was calling, the cards used were cut (mostly die-cut) from heavy cardboard, were opaque to a 100 watt light, were hand-stamped with ink that left no tactual impression or warping, and were carefully inspected for secondary cues on the backs. When mechanical shufflers were introduced, the demand for thinner cards came in, and with the introduction of emphasis on distance, screens, and other advances, the back of the card became less important. Finally the precognition tests and related work required merely a pack to shuffle, and preferably, one capable of easy handling ("slip").

The publishers claim that all defects in the present stock can be removed, but not at the present limited demand without prohibitive cost. Since it is much easier to set up simple experimental precautions than to attempt to produce a "perfect" commercial ESP card, it is doubtful whether improvements are at present feasible. Since the crucial work both at the present time and in the future is being and will be conducted under wholly screened conditions, the card criticisms are practically pointless.

* * *

RESEARCH NOTES

I Recording and Checking: The evaluation of the probability of errors in recording and checking in the ESP tests has for some time been the subject of study at Duke University under the direction of Dr. J. A. Greenwood. Certain further safeguarding rules, although in some instances long in use, have come into greater emphasis here and since they have been found to be workable in recording and checking in this laboratory, they are passed on as suggestions to others engaged in similar research, though it is of course possible that in many instances equally effective precautions have already been adopted. Further suggestions found practical by others will be welcomed to these research notes:

1. Wherever possible, make independent records of calls and cards.
2. The checking process should be witnessed by at least two persons when feasible.
3. When the subject is present at the checking process he may act as a witness but should never handle the cards or actually do any of the recording or checking.
4. The call-series column should be covered when the card-series is being recorded in a column next to it. The correspondences between the two should be checked afterwards. In checking down the column it is found advantageous to point with the pen to each pair of symbols to be checked.
5. In checking correspondences between call- and card-series on sheets where there are more than one run, lay a ruler or paper over the adjoining column on one side at least, to help in focusing.
6. Records should, if it is convenient, all be made in ink.

II *Avoiding Sensory Cues in Certain Matching Methods:*

Since some experimenters and many subjects prefer the screened open matching and screened blind matching test procedures but have been deterred from using the commercial ESP cards (or indeed any cards now available in sufficiently large quantities) because of the possibility of guidance by tactual cues, from the cards, (however unlikely this may be) it is suggested that the subject may be asked to use kid gloves (or gloves of any material which easily grips the card and facilitates handling). The experimenter is of course seated in a position (at the open end of the screen; see *Handbook for Testing Extra-Sensory Perception*, page 35) to observe that the gloves are not removed during the test run.

The modification of the Screened Blind Matching technique by carefully covering the five inverted key cards with blank cards (as in Dr. J. G. Pratt's Screened Touch Matching procedure, this JOURNAL, March '37) or enclosing them in opaque envelopes will of course safeguard adequately against visual cues, especially with the addition of the screening of the target deck.

The adoption of these further precautions does not, it need hardly be said, imply a repudiation of any conclusion based upon earlier work in which they were lacking. The aim is simply to find conditions the adequacy of which is more clearly and objectively manifest.

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*.

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

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.

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.

CALL v.: To designate a card by naming one suit either orally or by indicating it manually (as pointing or writing).

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*.

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

CRITICAL RATIO = X VALUE: 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 told (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"), rectangle, plus.

ESP SYMBOLS: See plate opposite page 1, this journal Vol. I, No. 1, March 1937.

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

EXTRA-CHANCE: *Significantly* different from *chance expectation*.

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

- 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*, *TM*, 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.
- OM (OPEN MATCHING):** The technique in which the *subject matches* a deck of *ESP cards* to five *key cards* which are face up before him.
- PERCIPIENT:** The person who perceives. In this journal the common use will pertain to *ESP* rather than to sensory perception.
- PT (PURE TELEPATHY) = TELEPATHY:** The word "pure" emphasizes the exclusion of *clairvoyance*.
- RELIABILITY:** *Significance*.
- RUN:** The presentation and *calling* of 25 *ESP cards* or *symbols* in succession.
- 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 will be illustrated in this journal on their first introduction in print.
- STM (SCREENED TOUCH MATCHING):** The *touch matching* technique with an upright *screen* preventing vision by the *subject* of the handling of the cards by the experimenter.
- SERIES:** Several *runs* that are grouped consecutively or according to some other principle.
- SIGNIFICANCE:** The unlikelihood that a given event (or series) will not on the average occur by chance more often than once in 150 such events (or series). [Arbitrarily taken to mean a *deviation* in the expected direction such that the *critical ratio* is 2.5 times the *standard deviation* (or four times the probable error) or greater.]
- STANDARD DEVIATION:** A unit of measurement in statistical method; that *deviation* above and below *mean chance expectation* which is expected to include about $\frac{2}{3}$ the *chance scores*. For *ESP cards*, S. D. = $2.04 \sqrt{\text{no. of runs}}$.
- SUBJECT:** The person who is experimented upon. Most commonly the *percipient* in *ESP*, though also the *agent* in *telepathy*.
- TARGET CARD:** The card which the *percipient* is attempting to perceive (i.e. to *call*, or otherwise indicate a knowledge of).
- TARGET DECK:** The *deck* of cards the order of which the *subject* is *calling*.
- TELEPATHY:** *Extra-sensory perception* of the mental activities of another person. It does not include the *clairvoyant* perception of objective events.
- TOUCH MATCHING:** The technique in which the *subject* indicates his *call* by tapping or touching one of the five *key cards* while the *experimenter* places the *target card* thus called in front of the *key card* indicated.
- TRIAL:** A single *call* of a presented *symbol*.
- X VALUE = CRITICAL RATIO:** The observed *deviation* divided by the *standard deviation* or by the *probable error*.