

Some Basic Experiments in Extra-sensory Perception: A Background¹

Joseph Banks Rhine

[Original] Editorial Note: ...This extract is taken from an article entitled “Some Selected Experiments in Extra-Sensory Perception” published by J. B. Rhine in the *Journal of Abnormal and Social Psychology* for September, 1936.

EXTRA-SENSORY perception, under the headings of “telepathy” and “clairvoyance”², is not new to science, nor to its psychological branch. Many psychologists have given active attention to it³, and some have contributed to the evidence for its occurrence. Among the latter are Bechterew⁴, Brugmanns⁵, Coover⁶, Estabrooks⁷, and Janet⁸. Of these, Coover apparently did not recognize his own contributions at the time, but several critics have since agreed that his results are positive, while Janet has simply remained silent regarding his findings, allowing others to interpret them. The Danish psychologist, Lehmann, who, with Hansen, offered the only serious criticism of the early telepathic studies, later came himself to be convinced of its actual occurrence.⁹

The most extensive study of telepathy and clairvoyance yet made was begun at Duke University in 1930. The first two years of experimentation were described in a brief report entitled “Extra-Sensory Perception of the Clairvoyant Type”¹⁰, and the first three years were covered by a monograph, “Extra-Sensory Perception”, published in 1934.¹¹ Another report gives the results obtained with a special subject, a professional medium.¹²

The reports mentioned submit the results of well over 100,000 trials made in clairvoyant card-calling and in telepathic thought-transmission. The experiments in which the better conditions obtained seem to allow no other interpretation of the results, and appear to demonstrate an extra-sensory mode

1 [Originally published in *JP*, 1937, 1, 70-80.]

2 Telepathy is the perception of another's mental state by other than the known sensory means; and clairvoyance, perception of objects without sensory action. “Extra-sensory perception” includes both.

3 For a more extensive review see J. B. Rhine, *Extra-Sensory Perception*, (Boston: Bruce Humphries, 1934) , Chap. 2.

4 *Zeitschrift fur Psycho-Therapie*, VII I (1924) , 280-304.

5 “L'état passif d'un télépathe contrôlé par le phénomène psychogalvanique,” *Comptes Rendu du 11me Congrès International de Métapsychique*, (Warsaw :1923) .

6 Experiments in Psychical Research, *Psychical Research Monographs*, (Stanford: 1917), No. 1.

7 *A Contribution to Experimental Telepathy*, (Boston Society for Psychic Research, *Bulletin V*, 1927) .

8 For selected instances and references see Richet, *Thirty Years of Psychical Research*, (London: Macmillan and Company, 1922); and *History of Psychology in Autobiography*, ed. Murchison (1930) , Vol. I, p. 125.

9 After the above was written, the report by Dr. Bender of his experiment in clairvoyant perception appeared. See Dr. D. K. Adarns' article on Bender's work in this issue, p. 63.

10 *Journal of Abnormal and Social Psychology*, XXIX, No. 2, (1934), 151-171.

11 Rhine, *op. cit.*

12 *Character and Personality*, III, No. 2, (1934), 91-111.

of perception. Evidence is offered not only of telepathic and clairvoyant perception, but of the natural character of these capacities. That they have some relationship to certain known functions is shown by the study, and a close interrelationship between the two, telepathy and clairvoyance, is indicated, if not established.

The purpose of this article is to give a brief account of some selected experiments in which both good conditions prevailed and good scores resulted, along with a full account of conditions maintained, and a report of controls and safeguards used. The selections made are chosen to meet the various possible questions or alternative hypotheses that we have to consider in this work. These fall under the following headings: For the clairvoyance tests, the question of sensory cues on the cards used, or of poor shuffling; for the telepathy work, the question of similar habits of order of selection made by the agent and the percipient in choosing and calling the symbols used. And, finally, for both clairvoyant and telepathic perception tests, the question of whether chance alone can explain the results obtained. While these results are selected from our present stock of several hundred thousand trials, they are taken fairly, including all the consecutive trials made under the conditions stated. A given block of data is taken intact, without any omission of results within the series. There was no "selection" in the improper sense of the word.

GENERAL METHODS

The tests for clairvoyance were all made with cards hereinafter called "ESP Cards". These were used in packs of twenty-five, each pack containing five each of five simple designs: \bigcirc + ϖ \square \star (star, plus, waves, rectangle, and circle). The cards were cut from white heavy cardboard which was opaque to light from a 100-watt light bulb. The figures were stamped in fountain pen ink with rubber stamps which left no detectable warping or other tactal stimuli.

In the tests for telepathy, the same symbols were used, but, to exclude the possibility of clairvoyance, the mental images of them alone were used without any objective representation until after the test when the record was made. This made the test one of pure telepathy, with "mind-to-mind transference alone possible.

The results were scored by runs of 25. The scores of a series of runs are totaled and the mean chance expectation for that number of trials found by taking 1/5 of the total trials made in the series. This mean value (np) for 300 trials (12 runs of 25) would be 60. Let us suppose the total score is 90 (an average of 7.5 hits per 25). Then there is a positive deviation from the mean (np) of 30. This is then measured by means of the probable error, which is found by taking $.67449 \sqrt{(n p q)}$, which here would be $.67449 \sqrt{(300 \times 1/5 \times 4/5)} = \pm 4.7$. The ratio D/p.e. (i.e., the ratio of deviation to probable error) gives a value that I have been calling X , which can be converted into a fraction of probability P by means of Normal Probability Integral Tables. In the illustration given we would have $X = 30/4.7 = 6.4$, which would give a P of about 1/50,000; that is, the odds against chance as an explanation of the results are 50,000 to 1, which would of course amount to practical certainty on the point. This method is similar to that used by Coover and Estabrooks and is a standard technique in the field.

THE NEW CARD SERIES

In this series with subject HP, I was myself the recorder and observer. Twenty-five packs of freshly printed cards were used in the series, and the subject had never seen them before. The packs were laid on the table in a stack at the beginning of a sitting, and each was used for three runs, with the exception of two that were used only once and five that were used only twice. A pack was handed to the subject by the observer, and he was asked to shuffle it. The shuffling was always thorough. Then I cut the pack and placed it before the subject, or allowed him to hold it face downward in his hand, removing each card still face downward to another pile on the table after he called it. No one looked at the faces of the cards until the end of the run. There had been no opportunity for anyone to learn the order of the cards nor to study them for sensory cues. The cards were checked after the completion of each run of 25.

In 1650 trials consisting of the first, second and third runs with these new cards, the average per 25 trials was 9.5. This is nearly double the mean value and the X value is very high, 27.0. To give the odds against this being due to chance would require a number of 70 digits. It will be recalled that an X value of 4 is the usual criterion of significance. In these scores the first runs with the new packs were as good as the third.

The results may be grouped as scores in hits per 25 for the 1st, 2nd and 3rd runs as follows: (It will be seen that while all 25 packs were used for a first run only, 23 were used for the second, and 18 for the third. This was due to interruption of the experiments.) 1st runs: 4, 8, 10, 7, 12, 11, 12, 15, 8, 7, 12, 11, 13, 9, 9, 8, 11, 13, 13, 7, 6, 9, 4, 9, 7. 2nd runs : 6, 8, 9, 8, 11, 11, 12, 7, 8, 12, 7, 5, 10, 12, 14, 11, 9, 13, 8, 15, 6, 9, 15. 3rd runs: 8, 14, 10, 8, 12, 7, 5, 11, 8, 10, 6, 12, 10, 8, 8, 10, 10, 8.

The 1st runs average 9.4 hits per 25, and the 2nd, 9.8, and the third, 9.2 per 25. Each group even taken alone is highly significant in its deviations. The ratios D/p.e., or X values, are respectively, 16.3, 17.2, and 13.1.

Any one still skeptical about this mathematical procedure may get a "commonsense" picture of the results by observing that whereas in a chance distribution the scores ought to be distributed so as to give an average closely approximating 5, in these results, we have 9.5; and whereas the mode in the normal case would be 5, in this there is a bimodal distribution, the principal mode being 8, and the second 12. And the median here is 9 instead of 5. Of the total of 66 runs, only 2 are below the expected mean and 62 are above it.

This New Card Series is submitted as evidence against the hypothesis that the results may be due to poor shuffling of the cards, leaving an order that may be already known to the percipient. It matters not how the cards were shuffled, since in the 25 first-run group, the order was entirely unknown to the subject. He had never seen any of the 25 packs before.

Incidentally, this New Card Series answers the question of whether possible sensory cues could have been given by slight markings on the backs or edges of the cards. It would require experience with them to learn these.

THE DT SERIES

This DT (or "down through") series consists of 1,625 trials, or 65 runs, made with subject HP (myself again the observer) under the following conditions: The pack of cards would be shuttled, frequently by the subject himself under the surveillance of the observer (myself again in this series). The shuffling by HP was really profuse,¹² since he felt that he got some advantage from contact with the cards. He habitually did not look at the faces, and could not possibly have "stacked" (*i.e.*, arranged the order of) a pack with the observer watching. The observer always cut the pack and it was left lying on the table while the calls were made. About a dozen different packs were used in the series: sometimes they were new to the subject and sometimes previously used packs were provided. When detectable markings appeared on the cards they were discarded.

The peculiar feature of DT work is the fact that the pack was not touched until after the 25 calls were made. After they were all made and recorded the call-record was checked against the order of the cards in the pack.

The last five calls in the run, representing the bottom of the pack as it lay, were the best. The hits were next best for the first five at the top, and lowest for the central 15 cards of the pack, as they have been for several other subjects—though there is one striking exception.

The 1,625 trials gave a total score¹³ of 482, which has a positive deviation of 157 ± 10.09 . This gives a critical ratio (X) of 14 and a P of 10-20.

The DT data add weight against the hypotheses of sensory cues, memory, and bad shuffling. They bear too upon physical hypotheses involving hyperesthesia and special radiation, on which other data given below will bear. In a word, they offer difficulty to a wave hypothesis, since waves from 25 cards in a pack must give only splotchy confusion.

Groups A, C, and D were made with the observer in the Physics Building and the subject in the Library. The distance was about 100 yards. In group B, the observer was farther away with the cards, approximately 250 yards. The subject was in the same place in the library as in A, C, and D. In it there are four groups and two conditions of distance. Dr. J. G. Pratt, then a graduate assistant in psychology, was the principal observer throughout the series.

The observer and subject synchronized their watches, and arranged to work at a stated time and distance. At the specified time the observer would take the top card from a shuffled pack of ESP cards in the room agreed on and lay it face down on a book in the center of the table without looking at its face. Thirty seconds later the subject in his cubicle in the Duke Library would record a call for the card. At the end of the minute, the observer would remove the card and take the next one. The cards as removed would be kept in order for later recording. Two runs were made per day.

Group E is included for comparison. It was taken with the subject and the cards in the same room, with Dr. Pratt as observer. In a part of the group, the observer held the cards, and in part the subject did. The conditions were not especially good, but the comparison with the results of the distance tests is very interesting.

In Groups A-D, the records were sealed up after each sitting and delivered to me before subject and observer got together. In Group D, I was present with Dr. Pratt as a second observer.

The cards were shuffled between runs, and this shuffling occurred just before using with HP absent from the room. Even if only one card were displaced at an unknown point and a cut made, the shuffling would baffle any guessing even from a perfect memory of any previous order of the cards in the packs used. The only check needed in the shuffling is that obtained by correlating successive card records for the runs. For Group A this averages 4.5 hits per 25, or slightly below the mean chance expectation. Two packs were used at a sitting. Now if we take only the most favorable combinations in each sitting we raise the average only to 5.6. So the results obtained are clearly not explainable by inadequate shuffling,

The scores¹³ may be represented in averages per 25 for the group, and the significance of the derivations represented by the X values.

Group	Conditions	Trials	Number	Average
			per 25	X-value
A	Physics Bldg. and Library	300	9.9	12.6
B	Medical Bldg. and Library	1,100	6.7	8.4
C	Physics Bldg. and Library	300	7.2	5.6
D	Physics Bldg. and Library	150	9.3	7.9
E	Same room	900	8.2	13.7
	Total	2,750	7.7	21.0

With all sensory cues excluded by the conditions, with the shuffling checked satisfactorily, and with the chance hypothesis so clearly ruled out by the statistical evaluation of the data, there is left only the hypothesis of some extra-sensory mode of perception. Anyone suspecting observer and subject of collusion must include me also in the conspiracy in view of Group D in which I was a witness, of Dr. Pratt's handling of the cards.

O-Z TELEPATHY SERIES

In this series subject GZ was the percipient and Miss Sara Ownbey was both observer and agent or sender. Both were graduate assistants in Psychology, and it was felt that an independent observer was unnecessary. In Group A of this series the agent and percipient were in the same room; in Group B, in adjoining rooms, and out of sight of each other. A door was open between and about 10 or 12 feet of distance separated the two subjects. In Group C, there were 28-30 feet between and two walls, in line

¹³ The actual scores are as follows: Group A: 3, 8, 5, 9, 10, 12, 11, 11, 12, 13, 13, 12. Group B: 12, 10, 6, 4, 10, 10, 2, 6, 5, 12, 7, 5, 12, 11, 9, 10, 6, 3, 0, 13, 10, 12, 12, 4, 4, 1, 4, 4, 7, 6, 5, 0, 6, 3, 11, 9, 9, 4, 8, 6, 0, 6. Group C: 5, 4, 11, 8, 4, 9, 9, 8, 9, 10, 2, 7. Group D: 12, 3, 10, 11, 10, 10. Group E: 10, 12, 11, 11, 9, 7, 5, 8, 8, 9, 10, 5, 8, 7, 6, 4, 8, 7, 8, 7, 6, 11, 12, 13, 12, 9, 11, 5, 9, 5, 6, 10, 4, 6, 7, 5.

of vision, though again doors were open between rooms. For all three groups there was an electric fan going, which would effectually obscure any "involuntary whispering", if it occurred. Signalling was effected by a regular and monotonous tap of a telegraph key.¹⁴

The agent, using the symbols of the ESP Cards, but not the cards themselves, and deliberately choosing her order of symbols five in advance so as to avoid falling into habitual preferences, would think intently of her first image and then give the signal to the percipient. The percipient would call out his choice and the agent would record the symbol chosen and check it if correct. She was practised in avoiding following any routine selection, and varied her order systematically. There have been several checks made as to her success in avoiding characteristic patterns which might be followed by the percipient. These were made by correlating the consecutive records of the agent. The check results of three such series, one of which is reported in full below (the Junaluska Series) have yielded for 650 trials an average per 25 of exactly 5. This is the mean itself. Very evidently the agent is successfully avoiding repeating herself or falling into any stereotyped order of selection. A day by day check between agent's series for 8 days gave only 3.9 hits per 25.

The question of similarity in habits of choice may be further checked by cross-checking the agent's record with the percipient's of—let us say—the day preceding or following, that is, correlating runs not intended to match. A cross-check was made of the Junaluska Series given below and it yielded an average of only 4 hits per 25. Habitual similarity of order of choice between agent and percipient can, I think, be said to have been satisfactorily eliminated. Moreover, in the series cross-checked as just described, the runs were made daily and the cross-check, therefore, covers also the question of daily routines or patterns of order of choosing.

Finally, as an extreme test of "patterning" or order-habits, we may check the percipient's records against themselves in consecutive order. The percipient might have a pattern of order without its being coincident with a similar one supposed for the agent. But if he does not have such a pattern, the question of similar order-habits is ruled out. Such a check made on 12 of the percipient's records given below, taken consecutively from a block chosen at random, and on the 8 records of the Junaluska records, yield an average of coincidences of 4.6 between runs, as against an expected 5.

In this series Miss Ownbey was solely responsible for the entire records. In others she was not, as in the Junaluska Series below. In one such short series with GZ¹⁵ as subject the average score is 11.0 and the X value is 7.5. In another, X = 4.2. In both, a recorder was present with the subject. The purpose in presenting this series¹⁶ is, however, not primarily to offer evidence of telepathy as such, but to bear on the relation of distance to scoring rate in considering the wave hypothesis of extra-sensory perception. The rate does not fall off with distance as both the wave theory and the hyperesthesia hypothesis would require. The succeeding series satisfies the point of independent recording.

14 If it be objected that the sounds might be varied unconsciously and serve as cues, it might be replied that no opportunity to learn the cues was provided. The subject did not know when he was right or wrong.

15 It should be said that GZ does not like to keep a record himself, since it distracts him; he feels that he cannot get into the right mental condition and continue to record. He sits with closed eyes and in deep abstraction from surroundings.

16 The question has been raised as to whether the agent might not, through her strong interest in getting good results, be likely to mistake unclear enunciation by the percipient who is off in another room, and to give favorable interpretations of calls not clear. First, the names of the symbols are phonetically quite easily distinguishable; each has a different vowel sound. Second, audition was good with open doors between rooms. Third, the scores for this work with the agent and percipient in the same room are nearly as high as with the two separated.

In presenting the data of this O-Z series, I will give first the 12 consecutive runs from which the check on the percipients' records was made. These were made in one sitting, with three conditions, given below. And below these results will be given all the telepathy tests given GZ by the observer under these conditions (one room and two rooms away) during the summer of 1933, and along with them all of the tests made at the same sittings with both in the same room.

	Same Room		One Room Away		Two Rooms Away	
			10-12 feet		28-30 feet	
	Score		Score		Score	
	Trials	Avg. per 25	Trials	Avg. per 25	Trials	Avg. per 25
12 consec. runs ..	50	15.0	125	19.0	125	16.8
Totals	950	14.0	750	14.6	250	16.0

These scores are so high that their significance is obvious without any computation. The 12 runs given above yielded the following scores, in order, 14, 16, 17, 21, 19, 19, 19, 13, 15, 16, 17, 23. In one of the runs of the 950 trials there was a perfect score of 25.

THE JUNALUSKA SERIES

In this series MFT was percipient, with Miss Ownbey again as both observer and agent. The tests were made with synchronized watches corrected from Western Union time. The distance was over 250 miles, MFT being at Lake Junaluska, N. C., and the observer at Durham. One run was made daily, at first at the rate of one call per five minutes and later at the rate of one every three. Reports were to be sent to me by agent and percipient immediately. As it happened the first three days' report was mailed to the observer instead of to me, but she had already given me her records, and brought the percipient's report direct from the post-office (and it was in the familiar handwriting of the percipient). The records of the next five days were sent to me. The average for these eight runs was 10.1, and gave an X value of 10.8, which is, of course, very highly significant.

"Chance" is excluded as an explanation. Similarity of preferences or order patterns as a possible factor is shown to be lacking. The data meet all counter-hypotheses quite satisfactorily, I think, all that have ever been raised, except that of possible deliberate collusion. Those not knowing the assistant and subject may, by the very pressure of doubt evoked by such unusual phenomena, raise a question as to this. There never is, however, in any research, an absolute answer to this question. The relative assurances that can be had are briefly the following: As already stated, the observer was a Graduate Assistant in Psychology with no slightest reflection cast on her honesty during the four years I have known her. In one who is seeking a psychological training and career we do not expect to find the motivation necessary to practice a planned course of deception such as this. And the marks of deception are lacking. First, when MFT was tested in the same room with the

agent-observer, just before the distance series, she scored an average of only 7.7 hits per 25 trials. At the time she did not expect to do better with distance. Second, several times before and since this series, we have most urgently wanted high scores in special experiments, in some of which the same percipient was used,--and have failed to get them. In the many series of telepathy tests that were statistically significant and were made under good conditions, more than a dozen different agents have been used and nine different subjects. There were fifteen different individuals involved, and in addition a number of observers. The collusion theory would not be seriously applied to all these by many critics. In fact, it has been raised only by those in psychic research who are close to the subject of fraud in mediumistic phenomena.

The hypotheses (and the criticisms) of "inadequate shuffling" of cards have been met by the New Card Series. That of "possible sensory cues" on the cards was likewise excluded by this series, as well as by the DT ("calling down through the pack") Series, and the Campus Distance Series. In telepathy, the question of natural habitual similarity of patterns in agent's and percipient's choices is answered by proper checks on the point in question. Hyperesthesia, involuntary whispering, and the like are excluded by the Distance Series. "Chance" as an explanation is well ruled out by the tremendously significant deviations, and the odds against chance to which they correspond. These cover the criticisms thus far raised against the work, except that of possible collusion. It is clear that this must involve myself as well as many others, since in one of the experiments I was in a position to check this point (Campus Distance Series, Group D). It is freely granted that for the extreme skeptic this may rightly be regarded as the weakest point. All scientific work suffers alike in this respect at some point in its development.

If we are correct in thinking that these results under the conditions stated indicate an extra-sensory mode of perception—that is, perception without the recognized sensory processes being essentially involved—no one will dispute their tremendous importance for the general science of mental life. I need not here indulge in speculation as to the bearing it would have. Some cautious suggestions appear in the monograph referred to above.¹⁷ If these are sound, they open up such large possibilities for the study of mind and its place in nature that we need to go all the more slowly to determine the evidential grounds upon which they rest.

17 *Extra-Sensory Perception.*

Quelques Expérimentations Basiques dans la Perception Extra-sensorielle: Éléments de Contexte

Note originale de l'éditeur : ... Cet extrait provient d'un article intitulé « Une sélection d'expérimentations sur la perception extra-sensorielle » publiée par J. B. Rhine dans le *Journal of Abnormal and Social Psychology* paru en septembre 1936.

Einige grundlegende Experimente zur außersinnlichen Wahrnehmung: Ein Hintergrundbericht

Ursprüngliche redaktionelle Anmerkung: ...Dieser Auszug stammt aus einem Artikel mit dem Titel "Einige ausgewählte Experimente zur außersinnlichen Wahrnehmung", der von J. B. Rhine im *Journal of Abnormal and Social Psychology* im September 1936 veröffentlicht wurde.

Algunos Experimentos Básicos en la Percepción Extra-sensorial: Antecedentes

Nota Editorial Original: ... Este extracto está tomado de un artículo titulado "Some Selected Experiments in Extra-Sensory Perception", publicado por J. B. Rhine en el *Journal of Annormal and Social Psychology* de septiembre de 1936.