

A CONTRIBUTION TO EXPERIMENTAL TELEPATHY

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Introduction by the Editors of the Journal of Parapsychology:

In this issue the *Journal* is reprinting, with the author's permission, the account of an ESP experiment carried out in the Harvard Psychological Laboratories during the mid-twenties. This work of Dr. G. H. Estabrooks, published as Bulletin No. 5 of the Boston Society for Psychical Research (1927), has long been out of print. The report, consequently, is known to most of today's workers in the field only as a bibliographical item of some historical interest.

There are several reasons why the research deserves to be rescued from comparative oblivion. First, it was done in one of the best experimental psychology laboratories of its time. While the separate aspects of the procedure were not particularly new, the research combined the earlier experimental advances in an effective way. It yielded significant results, and, everything considered, represented a distinctive step in the advance of parapsychology toward its present status as a branch of academic research.

Perhaps the most important feature of the Estabrooks experiment for the present-day investigator is the emphasis the experimenter laid on the importance of the subject's attitude toward the ESP test. From the point of view of history, however, there is little doubt that its main significance is the light it throws on the scientific spirit of the times. Although Estabrooks conducted his experiment forthrightly at the graduate-school stage of his career, the psychological journals would not accept his article and he was forced to fall back upon an obscure publication. He was eventually driven by scientific opinion to an unmistakable reserve concerning this work, as may be seen by noting what he later said about parapsychology in his books: *Hypnotism* (1943), *Spiritism* (1947), and (with N. E. Gross) *The Future of the Human Mind* (1961). It should be noted that a similarly adverse effect of the scientific climate was apparent in the case of other investigators who were pioneers in bringing parapsychological research into the university

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laboratory. For example, Coover presumably hid the significant findings of his ESP experiments at Stanford University under a cloud of statistical confusion. Similarly, the late H. J. F. W. Brugmans became inactive and silent concerning his ESP research after publishing, in the early twenties, the work which he had done under Prof. Heymans at the University of Groningen. These and other instances indicate something of the toll that professional and cultural pressures have exacted from parapsychology.

All the more reason, then, why the actual merits of these early researches should be calmly and carefully reappraised, especially in the light of more recent findings. In the case of the Estabrooks work, the value was greater than the author himself realized, so far as the exclusion of alternative hypotheses to that of ESP is concerned.

In *The Reach of the Mind* (1947), J. B. Rhine for the first time presented the results of a statistical evaluation of the decline in scoring which Estabrooks' subjects showed between the first and second halves of the run. Estabrooks himself had noticed the dropping-off in the scores as the subjects went through the series of 20 trials, but he presented this effect as only an incidental observation. This fact is not surprising, since up to that time very little study had been made of the decline effects in ESP data. Rhine showed, however, that the run decline in the Estabrooks data was clearly significant by ordinary statistical standards, and he pointed out that this finding provided compelling evidence against the counterhypothesis of sensory cues. If any of the subjects had been depending on sensory cues, their scores would not be expected to have declined. This finding in the Estabrooks data is all the more convincing because of the consistency with which similar position effects have been found in other ESP and PK records. Two tables have been added at the end of the reprinted paper to show the actual figures and evaluation of this decline effect (see Editors' Appendix).

FOREWORD

The writer begs to express his deep appreciation to the authorities of the Harvard Psychological Laboratory for the freedom which he has been given in his research.

He would also beg to thank Professor William McDougall for

his constant interest and stimulating criticism, likewise to Dr. W. F. Prince and Dr. G. Murphy for many helpful suggestions.

I. INTRODUCTION

Telepathy, if it exists, has not been proven to the satisfaction of all. Indeed we may safely say that if a canvass were made in America, the number of scientific men ready to affirm their belief in telepathy and to back up this affirmation with scientific evidence would be small indeed; certainly a definite minority.

The reason for this attitude is obvious. First, the suspicion of fraud; also a realization of that remarkable sensory and mental acuity which, in some people, may lead to results apparently genuine but in reality wholly due to normal agencies.

Secondly, the extreme difficulty of using true laboratory methods. Up to the present we have worked on the assumption that telepathic power was the gift of a very few. We had first to find a genuine "mind reader" and then obtain his or her coöperation.

Moreover, having found our telepathist and having secured coöperation we frequently found that laboratory conditions were such as to rapidly discourage the medium and that results, even if positive, were not always obtainable on demand.

The efforts of the writer have steadily crystallized along one line, that of obtaining a technique which can be used in any laboratory and which requires as subjects only people who are "normal" in every sense of the word.

Telepathy may or may not exist; but if it does exist we have a right at least to hope that it can be demonstrated under laboratory conditions. Indeed, until this is possible it will be very hard to persuade the orthodox psychologist that it is more than a name for a certain group of frauds and weird coincidences in which he need take no particular interest.¹

The experiments herein reported are simply a portion of the work done in connection with this problem by the writer during the past two years. As the task became more clearly outlined the work was

¹ The writer is familiar with the excellent work done by Coover and by Troland. While both these experimenters worked with mechanical control, and with material which could be—and was—checked against chance, conditions of experimentation were essentially different. It is hoped that Coover can be persuaded to check these results with the technique herein given.

concentrated along certain lines. Nevertheless practically all the first year and a large portion of the second year were thrown away searching for a technique and following up false cues.

The evidence herein presented is given for what it is worth. The technique described is simple and results obtained by it lend themselves to statistical treatment. The writer regards his work as very strong evidence for the existence of telepathy. Whether others, using the same technique can duplicate these results remains to be seen.

The procedure outlined in this article is simple but it was a very considerable time before it was realized that such a technique could be expected to yield positive results.

Most of the work on telepathy up to the present has stressed certain factors which definitely lead one away from any simplified procedure such as is herein used.

First, only certain people seemed to have this ability to any reasonable extent.

Secondly, in a majority of cases they seemed to require some abnormal state of consciousness—or unconsciousness—in order to demonstrate their gift.

Thirdly, in those cases of telepathy which we term spontaneous there seemed to be a very definite emotional factor entering—the case of the drowning man appearing to his wife is typical.

Finally a very serious criticism could be levelled at most experimental work in that the results did not lend themselves to statistical treatment. The critic could always claim that what was not due to fraud or to some type of hyperæsthesia could be explained on the basis of coincidence.

A series of experiments carried on during the college year 1924-25 under various conditions of sending and receiving and with agent and percipient in various states of consciousness left the writer with the firm conviction that chance could not be invoked as an adequate explanation of all the results obtained.

The problem had crystallized to a certain degree. Thus it became quite evident that neither hypnosis, nor automatic writing in the normal subject—if these can be obtained in a *normal* subject—were yielding results which justified their continued use.

Moreover, the material used had been restricted to playing cards for reasons which will be given later.

The work for the year 1925-1926 was begun with subjects in the normal condition and with playing cards as material. It was intended to interest as many people as possible in the problem and it was hoped that by using various methods a technique yielding positive results might be obtained—if it were possible to obtain such a technique.

In this respect the writer was extremely fortunate and almost by accident hit upon the procedure which he has since been using. It was the habit to follow up every lead which seemed to give the slightest promise and to run every cue to earth as quickly as possible.

One of the college men had undertaken to do some statistical work in the nature of working out chance distributions. For this purpose he assembled three friends in his room and ran through a pack of cards several times. His procedure was to have each of his friends write down a card on a given signal after which he turned a card up from the pack and recorded it. His friends were under the impression that the whole thing was merely for checking purposes, as indeed it was.

The results of this experiment showed that when the cards guessed by each of these three men were compared results were above chance. This lead was immediately followed up and the writer as a result adopted his present procedure of "rapid fire guessing"—thanks to a half hour's original work done by a man who has only the most superficial interest in psychic research.

II. TECHNIQUE

The technique which the writer now uses is as follows. One or more subjects are obtained who are willing to give half an hour to an experiment in telepathy. They are told that the experiment will not last more than half an hour and that from the nature of the experiment the same subjects cannot be used more than once. Under these circumstances the average college man can be induced to give this much time.

The subject or subjects are then taken to Room 17 in Emerson Hall. Room 17, as it happens, is really a double room. An inner room is separated from the main room by double doors and is rela-

tively sound proof. Noises such as the scraping of a chair, or loud talking can, however, be distinctly heard between the two rooms which make up Room 17. The writer does not wish in any way to stress the point of these rooms being insulated from one another as regards sound.

The subjects of the experiment are told to ask no questions until after it is over, then the writer talks freely with them and discusses any phase of the work on which they choose to question him—within certain limits.

The men are told that in the experiment which is about to take place they are literally to *guess* cards. The operator and one of the men will remain in the outer room and will cut the cards at random with a knife. Cards will be chosen in this way, the deck being reshuffled and the card returned after each choice.

The signalling device is next explained. This consists of a clock-work apparatus in the outer room connected with a telegraph key in the inner room. It is so arranged that the telegraph key is depressed and released every twenty seconds. That is to say, every twenty seconds the key in the inner room gives a double click—the circuit is closed and immediately opened again.

The percipient is now told that on every double click he is to guess a card. He is to write down the *first* card that flashes into his mind, and is to keep his mind off the experiment as far as possible. In the last series he was actually given a magazine article to read during the experiment and told only to think of what was taking place when the key sounded and then only long enough to jot down the *first* card that occurred to him. Throughout the entire experiment everything possible is done to keep the mind of the percipient off the work in hand and to give him the impression that the experiment has no particular value and need cause no worry whatsoever on his part. Needless to say complete abstraction from the experiment is never obtained.

The percipient is then given a slip of paper on which he is requested to write the numbers 1 to 21. He is told that a series of dots and dashes will be sent as a warning that the experiment is about to begin, that twenty seconds after this series the key will give its first double click and will do so twenty-one times, on each of which he is

to jot down a card, giving the entire title of the card, although care is taken not to mention any card in his hearing.

Both doors are then closed and the operator goes into the outer room accompanied by the other subject or subjects. They seat themselves beside the sending apparatus, the other subject is instructed that he is to act as the battery in that he can concentrate on the cards which are to be turned up as intensely as he pleases. No mention is made about his keeping his mind on his friend.

The operator then shuffles the deck of cards, keeping the cards face down all the time. He next sends a short series of dots and dashes, after which he starts the clockwork sending device.

As previously mentioned this apparatus causes the sounder in the next room to click every twenty seconds.

The deck of cards is now cut with a pen knife and the top card chosen. This is turned face up on a white sheet of paper lying on the table in front of the senders, the operator counting himself as one of these. The pack is then shuffled, and ten seconds—about—after the instrument clicks the pack again cut and the next card substituted for the first one, which is returned to the deck. This procedure is carried on until twenty-one cards have been sent. It is then repeated with any other subjects present.

In addition to this a red light was flashed for every red card and turned off for the black cards during the last two series. This light was about six feet away from the two senders. It was hoped that this would reinforce the idea of redness and blackness for the respective cards but the operator thinks it had no effect whatsoever.

In the great majority of cases a subject was used only once for these experiments. This was owing to the simplicity of the material. In some cases a man was used two or even three times—never more. These cases when they occurred were treated as ordinary experiments.

It was found that using playing cards, a person very rapidly acquired certain habits which were fatal to the experiment. Thus he wanted to give the suites in a certain order, would criticise his choices, could not get away from the idea of a certain card, etc. These habits developed very rapidly. In the first series reported on in the following chapter, two or three subjects were allowed to repeat the experiment once or even twice against the better judgment of the operator

and their results included. He soon learned that this procedure was fatal to his technique and discontinued it or warned the subjects beforehand that, good or bad, their results must be rejected.

Another point is the very interesting fact that with practice a certain type of man can be weeded out beforehand. The very worst type of man for an experiment of this kind is in the instructor in psychology; second only to him is the graduate student in the same subject. They simply cannot attack the problem in the proper spirit but insist on criticising the experiment and, much worse, reacting to it as they think it should be reacted to, and not as they are told to react.

One or two experiences put the writer on his guard. Henceforth all instructors who came in to take the experiment out of curiosity were warned beforehand that their results would be rejected. In future experiments the operator will also do considerable rejecting among his ordinary subjects. The moment a person becomes inquisitive, begins asking about types of imagery, criticises the experiment or appears over anxious or excited, the writer feels justified in telling him that his results will not be included. As long as this is done *before* the experiment and the decision adhered to there can be no objection to this procedure.

The very best type of subject is the one who regards both you and the experiment as a nuisance only to be endured for the sake of gratifying curiosity or for the sake of certain other inducements. Thus the writer was careful after each experiment to demonstrate a few sleight-of-hand tricks with the cards and is quite convinced that fully 50% of his subjects came up in order to see the "three card" trick and merely tolerated the experiments as a means to an end. For the purposes of the work in hand this attitude was excellent.

III. RESULTS

Let us now consider the actual results obtained by using this technique, or techniques so closely allied to it that we may lump our results together. Table No. 1 gives a gross analysis of the results presented in the simplest possible form.

This analysis is based on color. Thus we have two colors to deal with and a fifty-fifty chance. In other words pure chance would allow us to guess the cards correctly as to color in 50% of the cases. Table No. 1 simply summarizes results on a color basis, in other

words, how often did the percipient guess a card of the same color as the card on which the agent or agents were concentrating

Series No. 1 contains twenty-eight experiments of twenty or more cards each. In other words the experiment described in the preceding chapter was performed twenty-eight times, in the great majority of cases on separate individuals.

There are one or two points about this series which deserve special consideration. First, the clock apparatus was not used here. A telegraph key replaced this and was controlled by the operator. However, it was used under conditions which would seem to make any code of signalling quite impossible. The card was exposed as previously described and an elastic band slipped over the key thus closing the circuit and giving a *single* beat. When the next card was exposed this band was removed, the sounder was thus released and this beat, again *single* was the signal for the next card. Thus for each card there was a single beat given as a signal for a guess.

This introduces a time factor. Timing here was not accurate nor was there any reason why it should be. The cards were exposed roughly at the rate of three per minute and since new subjects were being used for the great majority of the experiments it hardly seems possible that they could, in a single experiment, have got onto a sub-conscious time code. It is probably quite impossible to vary the *intensity* of this beat by any manipulation of the key, so this may be ruled out.

The cards were sent when the operators felt he had them clearly visualized and felt that peculiar "inner urge" of which more later.

The operator acted as agent in the majority of the cases but frequently there were one or even two other agents with him who in turn became percipients.

Another point of interest in this series is that a rest was called after the first ten cards and the percipient engaged in conversation on any subject of interest *not* bearing on the experiment. This rest should be borne in mind in view of certain results which come out later.

Series No. 2 contains thirty-three cases of twenty cards each. Here each experiment was conducted with a different percipient and the conditions were essentially those of the preceding chapter with the single exception that the percipient was given nothing to read

TABLE No. 1. COLOR

Number of experiments in various series, number of correct guesses chance would allow, and number of correct guesses actually obtained as to the color of the various cards. Ordinary playing cards used.

	SERIES 1 (28 Cases)		SERIES 2 (33 Cases)		SERIES 3 (22 Cases)		TOTAL (83 Cases)	
	Chance	Actual	Chance	Actual	Chance	Actual	Chance	Actual
First 10 cards..	140	169	165	196	110	131	415	496
Second 10 cards	140	159	165	168	110	115	415	442
Total (20 cards)	280	328	330	364	220	246	830	938

TABLE No. 2. SUITE

Same data as in Table No. 1, but analyzed as to suite.

	SERIES 1 (28 Cases)		SERIES 2 (33 Cases)		SERIES 3 (22 Cases)		TOTAL (83 Cases)	
	Chance	Actual	Chance	Actual	Chance	Actual	Chance	Actual
First 10 cards..	70	88	82.5	101	55	67	207.5	256
Second 10 cards	70	83	82.5	73	55	61	207.5	217
Total (20 cards)	140	171	165	174	110	128	415	473

during the experiment. The clockwork timing device was employed and there was no rest after the first ten cards.

Series No. 3 contains twenty-two cases of twenty cards each. Here conditions were exactly those described in the preceding chapter.

Under total we have all our cases grouped together.

Series No. 1 occurred from Oct. 21, 1925, to Nov. 23, 1925.

Series No. 2 occurred from Mar. 13, 1926, to Mar. 31, 1926.

Series No. 3 occurred from Apr. 12, 1926, to May 18, 1926.

The experiments occurred in the morning, afternoon, or evening, largely in the afternoon. It must be remembered that the writer was merely interested in *demonstrating* telepathy and when he found that his results seemed consistent irrespective of the time of day, he took his subjects when he could get them.

The subjects for Series No. 1 were largely personal friends of the

writer, whereas those for Series Nos. 2 and 3 were largely students—mostly undergraduates—obtained through the courtesy of Professor McDougall who allowed the operator to canvass two of his classes for material.

In Series No. 1, Table No. 1, we had twenty-eight cases. Considering the first ten cards we would have 280 choices altogether. Pure chance would allow us 140 coincidences as to color. We actually obtain 169. Similarly in the second ten cards of this series we obtain 159 coincidences as to color against 140 allowed by chance, and in the first twenty cards we get 328 against 280. The other columns are to be similarly interpreted.

Table No. 2 gives us the same analysis with regard to suite.

We here see several points clearly which are perhaps even better brought out when we express our results in terms of probability.

We are probably justified in using the formula for the Standard Deviation of the Binomial Expansion to obtain the Standard Deviations of normal groups of the size herein used. This formula is $\sqrt{(k p q)}$. (See Brown and Thompson, *Essentials of Mental Measurement*, page 30.) Here "p" is the probability of an event succeeding, "q" of its not succeeding and "k" the number of trials.

Roughly we may say that the distribution represented by the Binomial Expansion approximates that of the Gaussian Curve and may say that the Probable Error is .6745 times the Standard Deviation. Expressing our deviations from the normal in terms of the Probable Error we can arrive at a simple expression in terms of chance.

Thus (Rugg; "Statistical Methods Applied to Education," p. 231).

"The chances that the true value . . . lies within

\pm P. E. are 1 to 1.

\pm 2 P. E. are 4.5 to 1.

\pm 3 P. E. are 21 to 1.

\pm 4 P. E. are 142 to 1.

\pm 5 P. E. are 131 to 1.*

\pm 6 P. E. are 19,200 to 1."

If we are allowed to treat our results on this basis we have the following, using Table No. 1 for our data.

* These figures should be 1329 to 1.—Ed.

Series No. 1:

Excess of first 10 cards over chance is 5.14 times the P. E.

Excess of second 10 cards over chance is 3.38 times the P. E.

Excess of total (20) cards over chance is 6.04 times the P. E.

Series No. 2:

Excess of first 10 cards over chance is 5.05 times the P. E.

Excess of second 10 cards over chance is .49 times the P. E.

Excess of total (20) cards over chance is 3.95 times the P. E.

Series No. 3:

Excess of first 10 cards over chance is 4.02 times the P. E.

Excess of second 10 cards over chance is 1.01 times the P. E.

Excess of total (20) cards over chance is 3.68 times the P. E.

Total:

Excess of first 10 cards over chance is 8.35 times the P. E.

Excess of second 10 cards over chance is 2.78 times the P. E.

Excess of total (20) cards over chance is 7.90 times the P. E.

We thus see that our results are well ahead of chance; in fact, no one would claim that these figures can be explained merely by coincidence. They are far too consistent and the chances are far too great.

One very interesting point well worthy of careful note is the fact that our first ten cards always give more significant results than our second ten cards. This point could hardly be more clear cut and, in the opinion of the writer points to the fact that the faculty underlying this phenomena, whatever it may be, is very easily fatigued—at least with this technique.

Only in one of the series do the second ten cards make a significant showing and this is in Series No. 1. It will be recalled that in this series a rest was allowed after the first ten cards which was not permitted in the other two series.

The number of coincidences in the first ten cards as compared with the second ten cards, taken merely on the basis of superiority, equality or inferiority illustrates our points graphically.

Coincidences in first ten cards as compared with coincidences in second ten cards:

	COLOR		
	<i>Ahead of</i>	<i>Equal to</i>	<i>Less than</i>
Series No. 1	16	3	9
Series No. 2	19	4	10
Series No. 3	11	7	4
Total	46	14	23

	SUITE		
	<i>Ahead of</i>	<i>Equal to</i>	<i>Less than</i>
Series No. 1.....	15	3	10
Series No. 2.....	19	5	9
Series No. 3.....	11	4	7
Total.....	45	12	26

Series No. 1 taken as a whole or the first ten cards of Series Nos. 2 and 3 yield results which cannot be due to chance.

We must bear in mind certain facts with reference to Series No. 1. The subjects were friends of the operator; this gives an emotional element which we cannot afford to overlook. The subjects were also given to understand that they were being merely used as checks in a mathematical sense. This white lie tended to remove any tension. In the later series it was found necessary to be frank about the nature of the experiment in order to maintain interest and obtain subjects.

Again the operator had control of the sending apparatus. Within certain limits he literally gave the signal when the spirit moved him, that is to say when he had a "hunch" that he had the card properly visualized and had a certain subjective feeling of certainty that it was going through.

The writer places considerable stress on this point. Experiments of the preceding year seemed to show that this particular state of consciousness was a great assistance.

All through Series Nos. 2 and 3 the writer could not help feeling a certain personal resentment against his mechanical sending device. It *was* mechanical and would not wait for proper visualization or for that subjective feeling of certainty. The operator did his best to accommodate himself to the machine but was often painfully aware that the signal was being given at times when another ten seconds would have meant much better conditions, at least, from his point of view.

If we are permitted to group our results together as we have done in the last column of Table No. 1 it is clearly evident that we are even more decidedly ahead of chance. This point is not insisted on, but in the opinion of the writer we are entitled to do so.

The writer has not treated Table No. 2 on a chance basis. An inspection of this table will show that suite is behaving very similarly to color but owing to our smaller chance of getting suite correctly the

chances as expressed by our method would not be mathematically significant.

Table No. 3 also gives us a graphic representation of our tendencies. Thus, as an example, "ahead of chance in the first four cards" as to color would mean that three or more cards were guessed correctly as to color, equal to chance that two were guessed correctly and below that one or less were guessed correctly. With suite for the same example ahead would be two or more suites, equal to one, below none.

TABLE No. 3

Relation of actual results to chance in various sections of the different experiments.

	SERIES 1 (28 Cases)		SERIES 2 (33 Cases)		SERIES 3 (22 Cases)		TOTAL (83 Cases)	
	Color	Suite	Color	Suite	Color	Suite	Color	Suite
<i>In First 4 Cards</i>								
Ahead of Chance.....	20	18	16	16	9	7	45	41
Equal to Chance.....	5	7	11	12	6	11	22	30
Below Chance.....	3	3	6	5	7	4	16	12
<i>In First 8 Cards</i>								
Ahead of Chance.....	16	17	19	16	11	11	46	44
Equal to Chance.....	6	7	9	11	4	6	19	24
Below Chance.....	6	4	5	6	7	5	18	15
<i>In First 12 Cards</i>								
Ahead of Chance.....	18	21	20	17	12	11	50	49
Equal to Chance.....	6	2	6	10	4	6	16	18
Below Chance.....	4	5	7	6	6	5	17	16
<i>In First 16 Cards</i>								
Ahead of Chance.....	20	22	21	18	14	14	55	54
Equal to Chance.....	4	3	5	7	2	3	11	13
Below Chance.....	4	3	7	8	6	5	17	16
<i>In First 20 Cards</i>								
Ahead of Chance.....	20	21	21	14	14	13	55	48
Equal to Chance.....	5	5	3	8	3	6	11	19
Below Chance.....	3	2	9	11	5	3	17	16

While we cannot express this as readily in terms of probability Table No. 3 is, in its way, quite as graphic as are the previous tables.

A further very interesting series should be reported at this point. This series was conducted between Rooms 11 and 17 of Emerson Hall. These rooms are about sixty feet distant from each other. The percipient was seated within the inner room of Room No. 17 and

the agent or agents were seated in Room No. 11. The technique was the same as for Series No. 1, that is to say a telegraph key was employed instead of the clock apparatus, and a rest allowed after the tenth card.

We will call this Series No. 4. It was run off between October 21, 1925, and November 23, 1925. The subjects were for the most part those men who had taken part in Series No. 1, this forming, so to speak, a second experiment which was put through at the same sitting. That is to say the subjects were first put through the tests of Series No. 1 and then immediately given the same experiment over again with the agents removed to Room No. 11.

All the doors, four in number, were closed between the rooms and under these circumstances there could be very little auditory communication between them. Results were wholly negative.

Thus thirty-two experiments were put through.

AS TO COLOR

	<i>Chance would give</i>	<i>Actually we get</i>
First 10 cards.....	160	154
Second 10 cards.....	160	153
Total (20) cards.....	320	307

The Probable Errors are respectively for the

First and Second 10 cards ± 6.0

Total (20 cards) ± 8.5

In this series we actually fall below chance as to color, but not significantly so.

AS TO SUITE

	<i>Chance would give</i>	<i>Actually we get</i>
First 10 cards.....	80	76
Second 10 cards.....	80	54
Total (20) cards.....	160	130

So far as suite is concerned we are again below chance, quite definitely so as regards the second ten cards.

Analyzing this series on the same basis as Table No. 3 we get the following:

In First 4 Cards:

	<i>Color</i>	<i>Suite</i>
Ahead of Chance.....	8	8
Equal to Chance.....	11	14
Below Chance.....	13	10

In First 8 Cards:

	<i>Color</i>	<i>Suite</i>
Ahead of Chance.....	9	10
Equal to Chance.....	8	11
Below Chance.....	15	11

In First 12 Cards:

	<i>Color</i>	<i>Suite</i>
Ahead of Chance.....	10	8
Equal to Chance.....	7	7
Below Chance.....	15	17

In First 16 Cards:

	<i>Color</i>	<i>Suite</i>
Ahead of Chance.....	10	8
Equal to Chance.....	7	7
Below Chance.....	15	17

In First 20 Cards:

	<i>Color</i>	<i>Suite</i>
Ahead of Chance.....	10	8
Equal to Chance.....	8	6
Below Chance.....	14	18

We noticed in previous series that the success tended to be emphasized in the first ten cards. In this series we find a very interesting fact. The entire twenty cards are below chance but it is interesting to note the relations which success in the *Suite* the first ten bears to success in the second ten.

As to color we find successes in the

First ten cards exceeding the second ten in 9 cases.

First ten cards equal to the second ten in 9 cases.

First ten cards below the second ten in 14 cases.

which is the *reverse* of the tendencies previously noted but, as to *suite*, we find successes in the

First ten cards exceeding the second ten in 21 cases.

First ten cards equal to the second ten in 5 cases.

First ten cards below the second ten in 6 cases.

which is the *same* tendency we have before noted.

The writer is inclined to ascribe the failure between Rooms Nos. 11 and 17 as due largely to the distance factor with perhaps an element of greater insulation entering in or some factor such as adverse auto-suggestion.

On the suggestion of Professor McDougall the writer attempted to retest as many as possible of his former subjects under conditions which would rule out all possibility of telepathy and use these results as a check. Unfortunately the men thought otherwise and would not cooperate. While they were perfectly willing to give a half hour once, they were by no means willing to do so twice much to the chagrin of the operator.

However, at the suggestion of one of the instructors who has been interested in the course of the experiment the writer worked out a statistical check on his results. He wished to find out just what would be the result if the responses of the subjects were checked up against a criterion which was obviously quite beyond any influence of telepathy. Thus if we could check their responses up against random drawings from a pack of cards with which they clearly had no connection we would expect them in the long run to approximate chance.

Actually the writer checked their responses up against the fifth card ahead in the series. Thus the response to stimulus No. 1 was checked against the actual card drawn for stimulus No. 6. As this card was actually turned up five cards *after* the person guessed for stimulus No. 1 it is inconceivable that any real connection could exist between this card and the response in question. Results show a very close approximation to chance, and are given in Table No. 4.

Similarly a checking up of the entire series shows that the various suites and colors are distributed quite normally in as far as the operator was concerned. That is to say the colors show approximately a fifty-fifty distribution and the suites correspond.

Finally the writer suspected that perhaps the previous cards drawn by the agent might have something to do with the responses of the percipient. Thus perhaps a black card after a red card might go through more easily than a black card following a black card.

The total series was broken up as follows :

Black after 1 Red card.	Red after 1 Black card.
Black after 2 Red cards.	Red after 2 Black cards.
Black after 3 Red cards.	Red after 3 Black cards.
Black after 4 Red cards.	Red after 4 Black cards.
Black after 5 Red cards.	Red after 5 Black cards.
Black after 6 Red cards.	Red after 6 Black cards.
Black after 1 Black card.	Red after 1 Red card.
Black after 2 Black cards.	Red after 2 Red cards.
Black after 3 Black cards.	Red after 3 Red cards.
Black after 4 Black cards.	Red after 4 Red cards.
Black after 5 Black cards.	Red after 5 Red cards.
Black after 6 Black cards.	Red after 6 Red cards.

TABLE No. 4

Results of checking guesses against card drawn by operator five guesses later. Results here given as to color.

No. of coincidences in guessing the various cards of the different series.																				
	I	II	III	IV	V	VI	VII	VIII	IX	X	XI	IX	III	XIV	XV	IX	IIIX	IIIX	XX	
Series 1 (Chance 14)	16	12	13	14	14	16	12	16	11	16	12	14	11	16	10	7	8	15	15	267
Series 2 (Chance $16\frac{1}{2}$)	18	13	14	14	18	23	14	16	16	11	17	11	16	12	15	19	13	17	15	311
Series 3 (Chance 11)	9	12	11	14	12	15	11	10	13	7	8	9	11	10	9	8	14	8	10	215
Series 1, 2, 3 (Chance $41\frac{1}{2}$)	43	37	38	42	44	54	37	42	40	34	37	34	38	38	34	34	35	40	40	793
Series 4 (Chance 16)	14	11	13	16	15	14	19	20	20	22	18	19	12	18	20	17	15	17	20	337

TABLE No. 4 (Continued)

Results as to Suite

No. of coincidences in guessing the various cards of the different series.																				
	I	II	III	IV	V	VI	VII	VIII	IX	X	IX	IX	III	XIV	XV	IX	IIIX	IIIX	XX	
Series 1 (Chance 7)	5	6	4	10	11	11	4	12	3	9	6	7	4	7	5	2	2	8	10	136
Series 2 (Chance $8\frac{3}{4}$)	10	5	8	7	7	13	7	8	10	5	9	6	3	8	6	11	7	4	10	150
Series 3 (Chance $5\frac{1}{2}$)	5	4	8	7	4	9	4	6	5	6	7	5	6	5	5	4	6	5	10	116
Series 1, 2, 3 (Chance $20\frac{3}{4}$)	20	15	20	24	22	33	15	26	18	20	22	18	13	20	16	17	15	17	25	402
Series 4 (Chance 8)	10	5	6	8	6	10	12	11	11	10	8	4	6	7	7	5	5	8	11	155

In as far as the writer can see the positive results obtained are in no way correlated with any such order of occurrence.

Lastly we had the question of the influence of the form of the cards which the agent had been gazing at previously. Thus a club following a club might come through easier than a club preceded by a diamond or a spade.

With this point in view the entire series was again broken up into the following categories:

- Club following Club.
- Club following Diamond.
- Club following Heart.
- Club following Spade.
- Diamond following Diamond.
- Diamond following Club.
- Diamond following Heart.
- Diamond following Spade.
- Heart following Heart.
- Heart following Club.
- Heart following Diamond.
- Heart following Spade.
- Spade following Spade.
- Spade following Club.
- Spade following Diamond.
- Spade following Heart.

Here again, in as far as the writer can see, results are entirely negative.

IV. DISCUSSION

We must bear a number of points clearly in mind when dealing with the subject of telepathy. It seems at least reasonable to suppose that if telepathy does exist it has a normal distribution throughout any section of humanity both as to the receiving and as to the sending end. Thus some of us may be excellent agents, some excellent percipients, but it is highly probable that the majority would be *normal*—whatever that word might signify applied to such a subject. At any rate it is the writer's contention that if such a phenomenon does exist it exists in all humanity and not only in half a dozen highly sensitive mediums.

Consequently we must search for a technique which will take this very weak tendency and magnify it until we can claim proof. Needless to say, this is not easy.

In the laboratory we cannot get the intense emotional state which seems so necessary to a really conclusive demonstration. Neither do we know of any abnormal state of consciousness which we can induce and so guarantee results.

On the other hand I think we must claim positive knowledge on one point, namely that we at least realize our ignorance of the laws which govern its manifestation—and there is some grace even in this state.

Thus it seems to the writer quite unreasonable that we should expect telepathy to act without regard to time or space. From his own results he is convinced that both factors are of great importance. We do not know how long a period of concentration is best on the part of the agent or how long to allow for the percipient to get the impulse. In the opinion of the writer, work in the same room will yield better results and work in widely separated rooms yield practically none—that is, with this technique.

Possibly this is due to the space factor. Possibly due to insulation. It seems a little unreasonable to suppose that “thought waves” can surmount all obstacles with equal readiness.

Again it may be due to auto-suggestion in some form or other. As the writer was merely interested in demonstrating telepathy and did not attempt to vary his results too much until he had succeeded in the former undertaking, he is very vague on these points.

From the results of his own work he feels justified in making the following suggestions for future research:

First, we must work out a technique to *demonstrate* telepathy, if it exists. At present we are mainly interested in this phase of the work and should not attempt too seriously to understand all the laws which govern its appearance until we can show conclusively and under laboratory conditions that it does appear.

Secondly, we must deal with material that is strictly subject to statistical treatment. Playing cards are excellent, mainly because everyone is familiar with them. The writer found that if he attempted this same technique with three colored lights—red, green and blue—his results were negative.

The reason in his opinion is probably as follows: With this procedure we require a minimum of conscious interference. Any person can instantly produce the name of a card. On the contrary, simple as the task is, the subject has to keep these three colors in mind. They are at all times more or less in consciousness. When he receives the signal his first impulse is to give a color. He is just as liable to say brown or yellow. For an instant he is held up and has to criticise his own attitude. This, in the opinion of the writer, is fatal. With cards it is different. There are only fifty-two, he can choose any and is perfectly familiar with all.

The third suggestion is that our technique should be one which uses normal human beings in the normal state *if this is possible*, if for no other reason than that they are comparatively easy to obtain. Moreover by using a technique wherein no subject is used more than once we eliminate to a large degree the hypothesis of fraud.

Fourthly, our technique should be mechanically controlled. In the writer's opinion this may present grave drawbacks in that a machine is a very impersonal affair and the personal factor is probably here of immense importance. However, our technique must eliminate all hypothesis of a subconscious code between agent and percipient.

Fifthly, we must in as far as possible eliminate all charges of hyperæsthesia. The technique here advanced does away with visual hyperæsthesia but the fact remains that the rooms are not sound proof; in fact under the most ideal conditions any rooms could only be *relatively* sound proof. An unconscious code of shufflings, scrapings, etc., or subvocal enunciation might be advanced as an explanation. It is rather far fetched and where we use new subjects each time we would be restricted to the subvocal enunciation. In the writer's opinion it is quite impossible as an explanation.

Sixthly, we must put aside any preconceived notions as to how telepathy should work. Using this particular technique it would appear that the faculty which underlies it is extremely susceptible to fatigue, that it works best with a minimum of conscious interference and that any strain or excitement on the part of the percipient is a pretty certain indication of failure.

The writer obtained his best results in Series No. 1 wherein he had a certain amount of personal touch with the signalling apparatus,

yet under conditions which rendered a subconscious code quite impossible. Also in these experiments the subjects were mostly his personal friends and he actually told them that their work had nothing whatsoever to do with telepathy; they were merely "statistical checks." This naturally did away with any worry. Unfortunately in order to obtain subjects in his later series he found it necessary to be strictly truthful on the later point, so that the subjects realized that they were taking part in a serious experiment. But in this first series we have all conditions eminently favorable.

Finally we should bear in mind that there are literally a dozen different ways of approaching the problem any one of which may lead to its solution. Perhaps some state of "drug" consciousness may facilitate communication, some of the conscious states associated with the various forms of hysteria may supply the key, some form of the "ouija" board may give a conclusive answer and it may be found that only as we take into consideration such factors as sex and the various emotional ties resulting therefrom can we get our solution.

The writer merely offers these suggestions and his own experimental work for what they are worth. Time and a great deal of careful investigation alone will tell whether they are worth anything. However it must be insisted upon, that, if this technique is used it must be used in strict accordance with instructions. The timing of the cards is very important otherwise the cards tend to become confused.

If a second card is exposed, say two or three seconds after the signal for the previous card, a certain type of subject who is rather slow on reaction time will not yet have made his choice and will be influenced by this card. On the other hand, if the card is not exposed until two or three seconds before the stimulus this does not give the other card time to recede from the agent's consciousness.

A fundamental point is the ability to get in quick and friendly rapport with the subjects and put them at their ease as to the experiment, a subject who is nervous or hypercritical or covertly hostile is worse than no subject at all.

Finally, the writer places great stress on his own state of mind when sending. Clear visualization and a confidence that the impression is going through are great aids—at least in as far as he can see. This cannot, of course, be obtained in the other senders but they can at least be told to concentrate.

APPENDIX NO. 1

Herein are given the crude scores of the experiments quoted in the previous pages. The number of coincidences is given which was obtained in the total guesses of each individual card in the various series

TABLE NO. 5. SUITE

No. of coincidences in guessing the various cards of the different series.																					
	I	II	III	IV	V	VI	VII	VIII	IX	X	XI	XII	XIII	XIV	XV	IX	IIIX	IIIX	XIX	XX	
Series 1 (Chance 7)	16	8	11	10	7	10	6	6	5	9	13	7	10	10	10	7	6	6	4	10	171
Series 2 (Chance $8\frac{1}{4}$)	14	13	8	10	6	12	9	11	8	10	7	7	8	7	12	8	7	8	3	6	174
Series 3 (Chance $5\frac{1}{2}$)	12	4	7	3	8	6	4	8	9	6	4	4	6	5	11	5	10	5	7	4	138
Total 1, 2, 3 (Chance $20\frac{3}{4}$)	42	25	26	23	21	28	19	25	22	25	24	18	24	22	33	20	23	19	14	20	473
Series 4 (Chance 8)	10	9	8	6	6	10	7	9	5	6	2	5	6	6	6	6	9	6	1	7	130

TABLE NO. 5 COLOR

No. of coincidences in guessing the various cards of the different series.																					
	I	II	III	IV	V	VI	VII	VIII	IX	X	XI	XII	XIII	XIV	XV	IX	IIIX	IIIX	XIX	XX	
Series 1 (Chance 14)	24	19	17	21	13	19	14	13	14	15	18	15	15	15	18	15	18	17	10	18	328
Series 2 (Chance $16\frac{1}{2}$)	24	21	14	17	18	21	19	19	21	22	17	16	17	20	21	19	20	17	11	10	364
Series 3 (Chance 11)	17	9	12	10	15	11	12	13	16	16	10	12	13	9	14	9	15	8	12	13	246
Total 1, 2, 3 (Chance $41\frac{1}{2}$)	65	49	43	48	46	51	45	45	51	53	45	43	45	44	53	43	53	42	33	41	938
Series 4 (Chance 16)	18	17	15	10	17	17	13	12	14	21	12	15	15	15	16	16	20	17	12	15	307

EDITORS' APPENDIX

TABLE A

Column as Regards the Scoring on Suit
Evaluation of the Difference Between the Top and Bottom Halves of the

Pooled Column of All Four Series

	Top Half	Bottom Half	Total
Hits on Suit	332	271	603
Misses on Suit	818	879	1697
Total	1150	1150	2300

$$\chi^2 = 8.36, 1 \text{ d.f.}; P = .004$$

TABLE B

Column as Regards the Scoring on Color
Evaluation of the Difference Between the Top and Bottom Halves of the

Pooled Column of All Four Series

	Top Half	Bottom Half	Total
Hits on Color	650	595	1245
Misses on Color	500	555	1055
Total	1150	1150	2300

$$\chi^2 = 5.30, 1 \text{ d.f.}; P = .021$$