REVIEW

DR RHINE'S RECENT EXPERIMENTS ON TELEPATHY AND CLAIRVOYANCE AND A RECONSIDERATION OF J. E. COOVER'S CONCLUSIONS ON TELEPATHY [1]

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Dr Rhine's investigation on telepathy and clairvoyance possesses several distinctive features.[1] One of the most startling of his results is the very large number of successes he had amongst his subjects. Most of those who believed in the possibility of extra-sensory perception had supposed that it was rather a rare capacity. By retesting his most successful cases, Dr Rhine has obtained so many positive results that he has completely got rid of the difficulty which has often been the bugbear of this kind of investigation, the possibility that a small preponderance of successful results might be due to chance. He has evolved new experimental techniques for the separate measurement of clairvoyance and telepathy. Most important of all is the fact that his methods are so simple and his results so clear that his experiments can easily be repeated, and it will be possible without difficulty for other experimentalists to convince themselves whether Rhine's conclusions are valid or whether they are due to some flaw in his experimental methods.

Dr Rhine himself is inclined to protest against the idea that every new investigator in this field must set himself afresh the task of proving the reality of extra-sensory perception, instead of being allowed to consider that the matter has already been proved by past researches. It must be remembered, however, that even if the possibility of extra-sensory perception has already been demonstrated, (which, whether rightly or wrongly, is by no

[1] Read at a private Meeting of the Society, 30 January 1935.

[1] *Extra-Sensory Perception*, J. B. Rhine, Boston Society for Psychic Research, 1934, 169 pp.

means universally admitted) yet past experimentation seems to indicate that the capacity is rather rare, so that any particular case of extra-sensory perception is still improbable and must be examined very critically before it is admitted. This is particularly the case when we bear in mind how often flaws of experimental technique have led to mistakes in this field in the past. In any case, such a novel claim as that of measurable telepathic and clairvoyant capacity in as many as one in three or four persons must be regarded as intrinsically very improbable, by no means to be rejected if it is scientifically proved, but as making necessary a very critical examination of the procedure by which it is claimed to be proved and its careful verification by other workers. If Dr Rhine's results were established, it would make a revolutionary change in our attitude towards this subject, bringing us near to S. G. Hall's ideal that telepathic phenomena should be reproducible at will at any time in any laboratory.

Dr Rhine gives a summary of previous work in support of his contention that extra-sensory perception has been already proved. Amongst other investigations he quotes that of Coover [1] as providing positive evidence of extra-sensory perception. Since it is a common opinion that Coover's results were entirely negative and show nothing but chance distribution, I have thought it worth while to re-examine Coover's figures and will discuss these before proceeding with Rhine's own work.

Rhine is undoubtedly right in saying that Coover's results actually show strong evidence against chance. There seem to have been two reasons why Coover himself drew the opposite conclusion: first, he adopted an absurdly high limit for the deviation from mean expectation which might be attributed to chance, and, secondly, he did not consider the possibility that clairvoyance might be active where telepathy was impossible.

In what follows I am considering only the results that he obtained with 10,000 guesses by 100 students of 40 playing cards (a pack without court cards) in which alone there were sufficient observations for statistically valid conclusions to be drawn. These were divided into two approximately equal groups: one in which the card drawn had not been seen by the experimenter when the subject guessed, and one in which it had. Coover, looking only for effects of telepathy and not for those of clairvoyance, treated the first group as a control group in which the effect looked for was not

[1] *Experiments in Psychic Research*, J. E. Coover, Leland Stanford Junior University Publications, *Psych. Res. Monog.* no. 1, 1917, pp. xiv + 641.

present and in which, therefore, chance factors alone were operating.

The following are Coover's results :

		Card.	Colour.	Number.	Suit.
Card not seen	Observed	141	2,491	488	1,252
Total 4,865	Expected	121½	2,432½	486½	1,216
	Difference	+19½	+58½	+1½	+36
Card seen	Observed	153	2,556	538	1,344
Total 5,135	Expected	128½	2,567½	513½	1,284
	Difference	+24½	−11½	+24½	+60

The general tendency of both of these series is clearly to exceed mean chance expectation, and in approximately equal amounts. Coover concludes that since the factor of telepathy cannot be present in the first series, the approximate equality of the two groups is due to the fact that the deviations of both are due to chance. A safer conclusion would seem to be that if any factors are present causing deviation from expectation, these are operating in approximately equal amounts in the two conditions of experimentation. At any rate, we shall be justified in lumping the two groups together for statistical consideration. For the remainder of the discussion of these results, I shall do this since it will give us the advantage of the higher significance to be obtained by larger numbers.

The result of throwing the two groups together is as follows :

		Card.	Colour.	Number.	Suit.
Total 10,000	Observed	294	5,047	1,026	2,596
	Expected	250	5,000	1,000	2,500
	Difference	+44	+47	+26	+96
Probability of chance occurrence of difference		·005	·4	·4	·025

In statistical enquiry, chance is generally regarded as sufficiently excluded if the odds against the chance occurrence of a result are fifty to one. If the odds against chance are greater than this we can conclude that the result is indicated with sufficient probability for rational acceptance, although of course our degree of conviction will be greater if the odds against chance are heavier. If, however, the conclusion to be established is a negative one, we shall not consider the absence of an effect sufficiently indicated unless the observed result would follow from chance alone at least once in ten times. If the odds against chance lie between 10 to 1 and 50 to 1, the results are to be regarded as inconclusive and must be repeated until there is a definite indication one way or the other.

The difference between observation and mean chance expectation of the number of cards guessed altogether right by Coover's subjects shows a probability of chance occurrence very much below this limit, being 200 to 1 against. The existence of some factor favouring correct guessing of the cards is strongly indicated. It might be objected that any form of extra-sensory cognition is *a priori* so improbable that we shall be right to insist on a much more severe criterion of significance than we should need, let us say, if we were trying to investigate the difference in fertility of manured and unmanured fields. To this objection, there are two replies. First, the question at issue is not, at the moment, whether or not extra-sensory cognition occurred amongst Coover's subjects but whether there was a factor in his experiments favouring correct guessing (such a factor might be some unnoticed error of method). There seem to be no grounds for regarding the presence of such a factor as very improbable. A much more important consideration, however, is that if we are convinced of the *a priori* improbability of extra-sensory cognition, that will be a sound reason for accepting the indications of a 200 to 1 odds against chance with less conviction than we should otherwise feel; it is no reason at all for regarding heavy odds against chance as evidence in favour of the operation of chance.

Coover's conclusion is, however, not a verdict of "not proven". His conclusions are definitely negative: "That various statistical treatments of the data fail to reveal any cause beyond chance operating for R cases (p. 123). . . . That no trace of an objective thought-transference is found either as a capacity shared in a low degree by our normal reagents . . . or as a capacity enjoyed in perceptible measure by any of the individual normal reagents (p. 124)." These uncompromisingly negative conclusions are most certainly not warranted by Coover's data. It is true that he considered only the evidence from the cards seen by the experimenter since these alone provided evidence for telepathy, but the odds against chance for correct guessing of the whole card in these experiments alone was about 30 to 1, which also cannot reasonably be regarded as evidence in favour of the chance explanation.

Coover does, however, also submit his result to statistical analysis but makes the excessive requirement that a result shall only be deemed valid if the probability of it not occurring by chance exceeds 0·9999779, i.e. the odds against chance are about 50,000 to 1. For this, as Coover calculates, it would have been necessary to have had 316 successes in the 100,000 trials instead of the 294 actually observed. If the same ratio of success had been maintained, the required level of significance would have been reached if the number of tests had been rather more than doubled. Coover's failure to go on is remarkable; particularly his failure to make further tests with those subjects scoring most highly above chance expectation. His negative conclusion is indefensible on his own evidence. How can one conclude from a probability of 200 to 1 against a chance explanation of the observed deviations that "no trace of an objective thought-transference is found"?

I have dealt only with those guesses in Coover's results which were completely right—in colour, number and suit—since these alone show a high significance. That there is a lower degree of significance discernible in the results calculated separately for colour, number and suit is of no importance since we find that this is simply due to the fact that if we eliminate those cases in which the card was guessed completely right, the remaining cases for colour, number and suit, show only chance distribution.[1] That is, whatever capacity the subjects had for guessing right, when it operated at all, led to complete knowledge of the card and at other times all characters of the card were merely guessed at random. This is what the modern experimental psychologist would expect. We do not suppose that the recognition of a card involves separate acts of perception involving colour, number and suit whose simultaneous activity gives complete knowledge of the card, but rather that total recognition of the card is a unitary process.

This leads to a principle of experimentation which it is well to bear in mind. The general principle suggested is that whatever character is used in this sort of experimentation, should have a mean chance expectation not so large that the expected chance deviations from it will be big enough to swamp the deviations due

[1] *Proof.* Let us suppose that the 294 cards guessed completely right are made up of some number x known (by E. S. P. or otherwise) to the subject and 1/40 of the remainder guessed right by chance; x will then be 45, the mean chance expectation from the remaining 9,955 being 249 (to the nearest whole number). The 45 known altogether correctly will, of course, be right in colour, number and suit. Of the remaining 9,955, the following are the number of right guesses of these characters observed and expected:

	Colour.	Number.	Suit.
No. expected - - - -	4,977½	995½	2,489
No. observed - - - -	5,002	981	2,551
Deviation - - - - -	+24½	−14½	+62
Prob. of dev. - - - -	·6	·6	·15

In no case is the probability of the deviation occurring by chance less than one-tenth, so the results are consistent with all the observed successes (other than the 45 completely right) being due to chance.

to the cause under investigation. 50 successes above expectation in 10,000 would, for example, be clearly significant in a character whose mean chance expectation was 250, but would be quite insignificant in one whose mean chance expectation was 50,000. This means also that it is inadvisable to try to calculate a single index (as has sometimes been done) taking into account successes in different characters with different mean chance expectation, since this may result in the swamping of real successes by chance deviations.

Rhine mentions that those subjects of Coover's who did well in the telepathy experiments also did well in the others (clairvoyance conditions). If there were a significant relationship, this certainly would be an important finding. It does not appear, however, that the relationship is any greater than might result from chance. It is true that the one individual who did best in one set of experiments also did best in the other set, but if we work out a correlation for the whole group or for the best eleven subjects, the correlation is found to be ·1 in both cases, and is quite insignificant.

Rhine says that most of the correct guesses in Coover's experiments were made by a small number of people and that, if the answers of this small number are considered separately, they become enormously significant. It is, however, clearly illegitimate to select the best answers and then treat them by a method of calculation appropriate to an unselected sample. We can, however, compare the individual sets of guesses in which 0, 1, 2, 3, etc., are right and compare it with the frequencies with which these would be expected on the hypothesis of chance distribution. This will be a more sensitive method of detecting a tendency to guess right which is found in only a few individuals, than will be the method which uses the mean obtained from the whole group.

The expected distribution on the chance hypothesis is that given by the terms of the expansion of $100 \times (39/40 + 1/40)^{100}$. The comparison between observation and chance expectation is made below:

No. right - -	0	1	2	3	4	5
Frequency observed	3	17	28	21	17	5
Chance expectation -	7·95	20·4	25·85	21·65	13·5	6·65
Deviation - -	-5	$-3\frac{1}{2}$	$+2$	$-\frac{1}{2}$	$+2\frac{1}{2}$	$-1\frac{1}{2}$
No. right - -	6	7	8	9	10	11
Frequency observed	5	1	1	1	0	1
Chance expectation -	2·7	·9	·3	·07	·02	·0035
Deviation - -	$+2\frac{1}{2}$	0	$+\frac{1}{2}$	$+1$	0	$+1$

There is a clear tendency for some individuals to guess right more often than is to be expected by chance. Also it is to be noticed that the improbability of a chance explanation is seen to be greater by this method of examining the results. One individual, for example, has 11 right and the odds against this one case alone occurring by chance amongst 200 subjects are more than 200 to 1.

This table of frequencies suggests that about six of Coover's hundred subjects had measurable power of exceeding chance expectation in the guessing of playing cards. If extra-sensory cognition is at work here, the number is greater, I think, than would be commonly supposed although much below the number indicated in Rhine's experiments. There is some indication that whatever power is measured may be widely diffused to a small extent, since it is to be noticed that not only are there individuals guessing far more right than is to be expected on the hypothesis of chance, but also that the number guessing none right and one right is considerably less than to be expected from chance. The observed distribution is below expectation at the low end as well as above expectation at the high end. This is not merely the result of the fact that the total number distributed on the curve of chance is decreased by the few that have the power of guessing right to a marked degree, since this number appears to be about six, and if the chance distribution were calculated for the remainder, it would mean only that each of the "expected" values was reduced by 6 per cent. which would still leave the zero end of the observed curve below expectation. Unfortunately, however, the number of cases is not large enough for it to be certain that this lowering of the zero end is significant. We can only say that the curve as it stands suggests a fairly wide distribution of a tendency to guess correctly in addition to a well marked tendency in a small number of subjects.

The observed distribution is definitely not consistent with an approximate equality of the tendency amongst all subjects, since if we calculate the expected distribution about the observed mean of 2·94, there are still significantly more high values than would be expected (the odds against the occurrence of the one case of 11 would, for example, still be fifty to one on this assumption).

What is definitely proved, therefore, is that some subjects are guessing more often right than is to be expected on the hypothesis of chance. The indication is that the number possessing to a marked degree this ability (whatever it may be) is about six. There is also a possibility that the same ability may be present to a smaller degree amongst a larger number of the subjects.

Coover's results, then, do not show chance distribution. Do they contribute positive evidence for extra-sensory cognition? The results may be due to this or to some uncontrolled error in Coover's experimental conditions. If he had not been misled by the use of a too severe criterion of significance, he would presumably have scrutinised and stiffened up his conditions to see whether the effect would disappear. Presumably also he would have gone on with the experiment until the probability against chance was even greater than it is. If we think that the existence of extra-sensory perception is probable on other grounds, we may regard this as the most likely explanation of Coover's results; as independent evidence they are not of much value. Certainly they leave the field open for a reinvestigation of the possibility of demonstrating telepathy amongst normal people by card-guessing experiments.

One of the most important changes that Rhine makes in method is the abandonment of playing cards as material and the substitution of a set of five kinds of cards suggested by Dr Zener showing respectively a star, a circle, a rectangle, a cross and two parallel wavy lines. A pack is composed of five of each of these, 25 cards altogether. There is a possibility that the greater ease with which he got positive results than other experimenters using playing cards is due to the superiority of these cards for this purpose. They are, for example, much more easily imaged than playing cards. Also, instead of relying on average results for the large group of unselected subjects, he selected those subjects for further investigation who did well in preliminary tests. This method would, I think, be used by any reasonable investigator who wanted to give extra-sensory perception the best opportunity of demonstrating its existence. Also he has devised methods for demonstrating telepathy and clairvoyance either together or separately. If the experimenter looks at a card, the subject may be guessing it correctly either by clairvoyance or telepathy. If neither experimenter nor subject looks at the face of the card, the subject is presumed to be getting it by clairvoyance. If the experimenter thinks of a card and the subject guesses it, it is presumed to be guessed by telepathy. The only doubtful point here seems to be the demonstration of pure clairvoyance. If a card can be guessed correctly without either experimenter or subject having seen its face, obviously somebody must be clairvoyant, but why the subject? Is it not possible that the experimenter knows it by clairvoyance and the subject gets it from him by telepathy? This is a serious consideration when the attempt is made to demonstrate pure clairvoyance at a great distance. It seems more probable that the subject will be able to establish the necessary *rapport* with a mind many miles away than with a pack of cards, and if the experimenter is present with the pack of cards, his clairvoyance seems more likely than the subject's.

We can best illustrate the kind of results obtained by taking a few typical results. The following are the results obtained with the subject Pearce up to 1 August 1933 (p. 85) :

	Trials.	Correct.	Dev. from m.c.e.	Dev./P.E.
Clairvoyance (removing cards) - - -	8,075	3,049	+ 1,434	59
Clairvoyance (calling "down through" the pack) -	1,625	482	+157	14·4
Pure telepathy - -	950	269	+79	9·5

The first and third of these experiments were done by the methods described above, the second by a particularly striking method in which the whole pack was called through by the subject without any cards being removed until the calling was complete (the D.T. method). The last column shows the ratio of the deviation from mean chance expectation to its own probable error, and is thus a measure of significance. The smallest of these ratios (9·5) means odds against chance of over 1,000,000,000 to 1 ; the others even higher. Chance, at any rate, is effectively eliminated.

Dr Rhine used an amusing variant of the usual method of experimenting when he asked his subjects to give the cards wrongly instead of correctly. Extra-sensory perception was then of course indicated by a score below instead of above mean chance expectation. An interesting point not noticed by Dr Rhine is that an examination of the results obtained by this method indicates a falling below mean chance expectation greater than might be expected from the positive scores of the same subject.

Let us suppose that the subject is able by some means (such as E.S.P.) to know 5 of the 25 cards. These he will name correctly, and of the remaining 20 he will get 4 right by chance, so that his total number right will be 9. Now suppose that he is trying to name the cards wrongly. On the assumption that he will only be certain of naming wrongly the same number of cards as he was previously certain of getting right, and that his other answers will be right or wrong by chance, the number he now gets right will be four. Expressing this generally, if he knows m cards and his other answers are random, he will get $m+(25-m)/5$ correct when he is trying to guess right and $(25-m)/5$ correct when he is trying to guess wrong.

Now Pearce is said (p. 40) to have averaged about 10 correct when trying to guess right and about 2 when trying to guess wrong. This gives a much higher value for *m* in the guessing wrong series than in the guessing right, 15 when guessing wrong and only a little over 6 when guessing right. We must conclude that whereas some cards are well enough cognised to be correctly named, a much larger number are less completely cognised and although the subject cannot name them correctly, he can perform the easier task of naming one of the four kinds that they are not. This is an interesting point although, of course, it has no bearing on the main question of the mode of cognition.

A curious result reported by Rhine is that forcing a subject to go on when discouraged by failure seemed to make him score significantly below mean chance expectation. This is odd since it is, of course, necessary that the subject should have knowledge of the cards in order to guess below mean chance expectation as it is to guess above. He is no doubt right to speak of an inhibition here. The first experiments reported on p. 62 are not conclusive since Rhine made the curious mistake of not cutting the cards between trials, apparently supposing that this would favour correct scoring. Actually it would favour repetition of previous scores whether high or low,[1] and makes the estimate of significance entirely unreliable. Apparently, however, the later evidence was obtained under satisfactory conditions.

On page 86, a distance experiment is reported with Pearce in another building over 100 yards away. In a clairvoyance experiment with the cards removed from the pack for each guess the following results were obtained in 12 runs: 3, 8, 5, 9, 10, 12, 11, 12, 11, 13, 13, 12. An average of 9·9 per 25. Dev./P.E. is here 12·1 and the odds are many billions against chance. There is also reported a successful experiment over a distance of 250 miles by two of Rhine's collaborators. It is not clear, however, whether this was properly checked by independent witnesses. Other very long distance experiments were unsuccessful.

One last result may be mentioned. Pearce on one occasion had 25 successive right guesses in pure clairvoyance. The odds against this occurring by chance are about 600 billion to 1. This of course is no better evidence than what has gone before, but to some it may appear more impressive. Several other subjects gave results which do not quite come up to Pearce's standard but are also entirely inexplicable on any chance hypothesis.

[1] That is, on the hypothesis that there is a correlation between successive series of calls by the same subject. In my own experiments I have found that this is not uncommonly the case.

We need say no more about the possibility of these results being due to chance than that it is altogether excluded. Odds of a billion to one against chance are really no better than odds of a million to one. This is generally recognised by Rhine himself, although he occasionally uses phrases which might lead to misinterpretation by those not familiar with the limited purpose of statistical tests for significance. For example, on page 67 he says: "This makes the odds in favour of the E.S.P. factor and against chance away up beyond the trillions again and well into the zone of entire safety." It must be remembered that all that a statistical calculation of significance can do is to measure the importance of one and only one source of error—the possibility of wrongly concluding that a genuine effect is present from a numerical deviation which is merely due to the chances of sampling. The number of experiments must be increased until this source of error is negligibly small compared with all other possible sources of error. Beyond that point there is no further gain in increasing results under identical conditions. It would be a very optimistic view of any set of scientific experiments to suppose that when the chance of being misled by a sampling error was reduced to, let us say, 1 in 1,000, it was not negligibly small compared with other sources of error. Yet Dr Rhine went on accumulating results under identical conditions when this source of error was below one in billions. This unfortunate concentration on fantastic anti-chance probabilities seems to have led him into paying quite insufficient attention to reporting the precautions taken against other possible sources of error.

When we ask whether the experimental conditions were sufficiently carefully controlled, we are met with the difficulty that it is generally quite impossible to discover for any particular experiment what the experimental conditions were. Very commonly the subject's guesses were checked after each five guesses. Apparently this means that the subject was told what the correct figure was on each of the previous five cards.

This procedure is open to the objection that the subject's knowledge of what cards have already been drawn gives him information as to the changed probabilities of future drawings. Rhine makes the curious mistake of supposing that this would only be effective for the last five and only if they were all of the same suit. Actually it could be effective any time after the first five were checked. Except in the very improbable case of all of the first five cards being different (the odds against which are about 15 to 1), it is theoretically possible for the subject to raise his expectation of chance success by being guided by what has already turned up.

Let us suppose, for example, that one or two diagrams have not appeared in the first five. If he consistently guesses these during the next twenty, his expectation of success during that twenty will be five; this added to the one chance success to be expected during the first five exposure brings the total number to be expected by chance up to six. This, however, is on the supposition that the subject is guided by the first five cards only. In fact, with this method of experimenting, he has additional information from each subsequent five, and his expectation of chance success is increased above six if he is guided in this way to an amount that is not known.

Dr Rhine has other replies to this possible criticism besides the mistaken one already given. In graph No. 3 (p. 137), the ratio of successes by Pearce is shown separately for the 1st, 2nd, 3rd, etc., card in the whole series of 25 taken from a total of 1,375 trials by this method. This graph shows clearly that even for the first five cards, no less significant positive results were obtained than for the others. Similar curves are shown for some of the other subjects. This shows that this factor was not important for these subjects. There remains the possibility that it may have been present for others. Unfortunately it is not clearly indicated what series were given by this method, so that for a considerable proportion of the subjects tested, there may have been an uncontrolled factor present which might give a spurious indication of E.S.P. It is no doubt necessary to keep subjects informed of their success, but it would seem to be a less objectionable way of doing this, simply to tell them when they have made right guesses without informing them of what the cards were when they guessed wrong.

This, however, is a less serious objection to the method of checking by fives than the fact that this seems to be an ideal way of teaching subjects to recognise some of the cards from their backs. This last possibility is probably the most serious source of error in Dr Rhine's experiments. It might be overcome by having a sufficiently large number of packs and making successive tests of the same subject with different packs. Dr Rhine apparently did have more than one pack, but it is not clear how often he took the precaution of making successive tests of any one subject with different packs. It is no answer to this criticism that successful results were obtained with some subjects under conditions in which knowledge of their backs would have been of no service to them (as, for example, Pearce in the D.T. experiments). This proves that, at least, a few of his subjects were not successful by this method. Indeed, there seems no reasonable doubt that if the D.T. experiments were carried out exactly as described, with adequately shuffled cards no other explanation than that of clairvoyance is possible. We have, however, plenty of other evidence in favour of the view that extra-sensory perception is to be found as a very exceptional mental power. The novelty of Dr Rhine's results lies in his apparent demonstration that this power is not uncommon and it is here unfortunately that his evidence is quite inadequately stated.

The conclusions as to frequency of the capacity from the data reported in the present book are summed up as follows in a later article.[1] "The best results are contributed by eight major subjects who showed both clairvoyant and telepathic ability, at approximately similar rates of scoring for the two conditions. In addition to these eight major subjects there were at least ten more minor subjects who scored significantly high in clairvoyance or telepathy. Then there were others, (a majority) among the remaining fifty-nine subjects tested who scored at a good rate, but over far too short a series to be evaluated. There were only seven failures among those who were tried out to the extent of 1,000 trials. . . . It is safe to conclude that extra-sensory perception is not so rare as has been supposed, and on the basis of the proportions mentioned above ought clearly to be found in at least one in every four persons, with a higher ratio most probable."

This is an extremely important conclusion. It is a pity that the evidence for it is so inadequately reported that it is quite impossible to get any idea as to whether the experiments on all of these subjects were carried out under critical conditions. If all or any considerable proportion of them were carried out with packs of which the same subjects had had previous experience by the method of checking after each five, the conclusion would rest on a very uncertain basis.

If Dr Rhine is to carry general conviction of the truth of his finding as to the commonness of extra-sensory perception, it is absolutely necessary that he should state clearly how many of his 18 subjects showing extra-sensory capacity were tested under critical experimental conditions. The minimum requirement for a critical experiment would seem to be: (*A*) that an experiment in which the card is visible to the subject should never be carried out by means of a pack of which the subject has previously seen the back of each card and been informed as to what was on its face; (*B*) the subject should not be informed as to what cards have been drawn until the whole pack is completed; and (*C*) the back of the cards should not be visible to the subject at all unless it is

[1] "Telepathy and Clairvoyance in the normal and trance states of a 'Medium'", J. B. Rhine, *Character and Personality*, 1934, vol. iii. p. 94.

absolutely certain that the figure on the face has made no perceptible modification of the surface of the back. It is quite impossible to discover from Dr Rhine's book how much of his evidence is derived from experiments of this kind and it is entirely possible that even though one or two of his subjects had genuine extra-sensory power, the others were getting successes through inadequate control of the experimental conditions.

Another important conclusion is that the subjects who are good at telepathy are also good at clairvoyance to about the same amount. The evidence is shown in Table XLI on p. 148. Unfortunately the evidence is not very good. The table shows results for seven subjects (one of the major subjects being omitted) with a correlation of ·75 (calculated by the rank-difference method). Plainly no conclusion can be drawn from a correlation between 7 subjects. The data presented are no more than an indication of a conclusion which may be established by examining a larger number of subjects.

It will be gathered that Dr Rhine's procedure is by no means free from objection, and that his presentation is open to the much graver objection that the experimental methods are quite inadequately reported. This is a pity, since a little more care in reporting and more careful discrimination between experiments obtained under perfect and under imperfect conditions would have made this work very much more convincing. It may be that all that Dr Rhine reports is true, but much of his report will not carry much conviction to those inclined to be sceptical. At least we may say that Dr Rhine has shifted the burden of proof on to those who deny that extra-sensory perception is a fairly common capacity. If his results are to be tested it can only be by repetition of his experiments. He has developed an easily applied technique, and those who are not convinced may try the matter out for themselves. It is to be hoped that there will be many carefully planned repetitions of these experiments and that the results (positive or negative) will be published.