

22 March 1944

My dear Salaman,

The question you ask me is one which appeals to me a great deal, and, though I have constantly seen these claims to immensely high significance made on behalf of work with which I personally have had nothing to do, yet in all of the good many cases in which I have been asked to look at data actually secured with a view to demonstrating extra-sensory perception there has never been any such decisive evidence.

I suppose what gives rise to these very confident statements is something like this case within my own experience, and of which I remember the details clearly:

(one could not investigate evidence of supposed telepathy unless) interested in supposed telepathy had formed the conclusion that one had first a clear basis of knowledge as to the possibility ~~one could~~ or impossibility of clairvoyance. She therefore designed

a test to see whether any considerable number of ordinary people possessed a clairvoyant faculty, at least in a slight degree. I think she grasped, and this also seemed to me enlightened, that for evidential value a slight but constant measure of successes among a large number of subjects was much more important than evidence of a knock-out star conjurer's performance done once under very special conditions and not in any sense reproducible.

What she asked people to do was to turn up five cards in succession from a well-shuffled pack, noting in each case what they thought the card was going to be, and, after turning it up, what it actually was. She thought that five cards at a time was the right amount, but asked each person to do this five times so as to provide the results of five guesses or attempted clairvues. I made out for her a system of scoring partial successes, as when you guess the King of Clubs and the Knave of Clubs turns up, ~~framing~~ framing the system so that the average score was zero, and the ~~and the~~ standard deviation for random guessing, I think, ten points,

so that the total of 25 guesses had a standard error of 50, and the mean of 25 scores had a standard error of 2.

She got to work and persuaded no less than 240 people to co-operate, finally producing 6000 records for examination. Before I saw them she had scored the whole lot and found positive scores well in excess of negative, and in the aggregate very significantly so.

Of course, some of her subjects may have cheated, and possibly those who reported the most improbable successes should be discounted, but, even if one sets these aside, the preponderance of small positive scores among the remainder requires an explanation. I was led to think, by my own experience that, though all but one or two of the collaborators may have been entirely honest, yet they may have violated the results just as much as if they had sent in false returns. I had myself received a set of forms to fill in, and, when first I had them, sat down, did 5 guesses, and filled in one form. I was not successful, no interest was evoked, the forms went into a drawer and were forgotten.

Later, further correspondence induced me to fill in a second form, but I never completed the five. What I think is that if a chance success had come my way in the first five trials I should have been interested, completed the set (unless later experience was too disappointing), and sent in my return to be included with the others.

I was able to show that in fact the large body of data collected had been vitiated in just this sort of way, and the confirmation is important, for, of course, it is just as easy to make a hypothetical and unfounded objection to an experimental result as it is to make a false experimental claim. It happens that, when people guess cards in numbers, certain preferences begin to show themselves quite strongly; red cards are guessed more often than black, odd numbers are guessed more often than even, and so on. In 6000 guesses each card of the pack should have appeared about 120 times, but very wide range, from about half of this amount nearly to 200. This, of course, proves ~~nothing~~ ^{nothing}.

except that you could make money in the right company by offering slightly better than the calculated odds to anyone who will guess the card in your hand, provided you make sure that, on the whole, unpopular cards are the ones you hold. What is really informative about the card-guessing data that I'm speaking of is that, after tabulating frequency of choice of the cards guessed, one could quite independently tabulate the frequency with which each of these same cards was drawn. Here at least one might expect the frequencies to be in accordance with chance, namely equal numbers of Black and Red, equal numbers of Odd and Even, and so on; but the frequencies of cards drawn were also disturbed by some factor other than chance, and what I think was enlightening was that these frequencies were in fact a close reflection of the frequencies appropriate to cards chosen. The differences were not ~~so much~~ half as great, but they were regularly in the same direction. Consequently I think that one must infer that a large number of the cases in which unpopular

cards were drawn have somehow been eliminated from the record in just such a way as would be brought about by the suppression, by non-completion, of trials started insuspiciously.

It seems to be one of the ways in which faith moves mountains !

Yours sincerely,