

Pearce (1). In short, it is most unlikely that Pearce could have cheated (because of the physical features of the rooms used), and it is even more unlikely that he did cheat.

Yet it is not quite impossible. And no parapsychologist suggests that it was impossible for him, or for many other tested subjects, to have cheated. But if parapsychology has reached the point where fraud by experimenters and subjects is the only alternative to acceptance of E.S.P., then it has come a very long way indeed.

This brings me to the freedom with which accusations of fraud are thrown around when parapsychology is discussed. Sober scientists very rarely impute fraud to other scientists, both out of respect for colleagues and a fear of the laws concerning libel. Why should parapsychologists be required to produce a fraud-proof experiment when other scientists are not? Why should they have to put up with unpunished accusations of fraud? If it is replied that this is necessary because the claims are so extraordinary, I say that this is precisely the point where the unfairness comes in. What if Hubert Pearce, for example, did not cheat? Surely a grave injustice is being done in that case, as well as in the cases of the other subjects and experimenters so casually accused of cheating, if they also did not cheat. Certain phenomena seem to be occurring which, according to the theory of materialism, ought not to occur. Surely this means that something may be wrong with the theory. But we remember sadly that (in the eighteenth century) after the French Academy of Science declared that meteorites could not exist a number of European museums removed specimen meteorites from their exhibits.

Your lack of familiarity with the specialty literature of parapsychology appears also in your praise of Mr. Trevor Hall's book, *The Spiritualists*. Mr. Hall's book reads smoothly, and the uninformed reader can easily be beguiled into thinking he has before him an accurate analysis of the conduct of Sir William Crookes in the Florence Cook sittings. It is only when one knows evidence omitted by Mr. Hall that one becomes aware how often his speculations advanced on one page are a few pages later accepted as established fact and used as the foundation for further conjectures.

Mr. Hall's surmises about Sir William Crookes are largely based on the testimony of two lovers of Florence Cook (Anderson and Bois), both of whom were shown to have told quite different stories at different times (2). As for the "mass of circumstantial detail" which Anderson claimed to recall (after an interval of 56 years), the fact is that his description of the house in which he claimed to have first known

Florence Cook was quite inaccurate. A banister which figures in one scene of Anderson's memories simply did not exist, and other details were equally false. I am certainly not here defending Florence Cook or Crookes's report of his sittings with her; I am merely insisting that critics of parapsychology should adhere to standards of accuracy as high as those they require of parapsychologists.

Fortunately, a field belongs to its investigators rather than to its outside critics. New and better experiments (none of them fraud-proof) are going forward and adding to the evidence for E.S.P. The Parapsychological Association, the international organization of the scientifically-trained professional investigators in the field, now has almost 200 members from 23 countries. Its membership is slowly increasing. May I suggest that you invite some of its well-informed members to assist you in the appraisal of books in the field, just as you obtain expert assistance in reviewing books in other fields? I will be happy to provide a list of the membership for your convenience.

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REFERENCES

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2. — (1963). "Reflections on Mr. Trevor Hall's *The Spiritualists*." *J. Amer. Soc. psych. Res.*, **57** (4), 215-226.

DEAR SIR,

Professor Eysenck says that he has been impressed by the lawfulness of certain events occurring within runs, such as "the fall-off of scores, *which is reported again and again*" (italics mine). I can find no evidence for any such fall-off in scores during E.S.P. tests.

In the early work at Duke University, the DT tests, in which subjects guessed the cards in the pack reposing on the table in front of them, revealed a "U" curve with scores tending to be *highest* at the end of the run. In other tests the published data shows no decline during the run (see for example J. B. Rhine's *Extra-Sensory Perception* (1934) and *J. Parapsychology*, **1**, 141). If we consider the so-called conclusive tests and other experiments discussed in my book, Shackleton's hits, according to Soal, were randomly dispersed over the score sheets. Mrs. Stewart displayed a decline effect in the first column of 25 guesses on the sheet and an incline in the second column. In the case of Glyn Jones, Soal reported that the hits

were randomly dispersed over the sheet and that significantly higher scores were obtained in the second half of each sitting than in the first half. I find that during the first three runs of the Turner-Owmbey series, these being the runs in which high scores were obtained, hits tended to *increase* throughout the run. In the Reiss experiments, the highest scoring rate was on the last five cards of the run and the lowest rate on the first five. The Warner experiment showed an *incline* rather than a decline effect throughout the run of 250 targets.

Professor Eysenck states that Stevenson has found a large number of clear-cut errors in my account and expects Dr. Slater to have read the review and to have drawn attention to it. The particular point raised by Professor Eysenck is concerned with the Pearce-Pratt series. He states that Room 311 is not in fact opposite Room 314 (where Pratt was sitting) as shown in Hansel's diagram, but is down the hall next to room 313. This is precisely where I showed it to be in my diagram. Only by being down the hall and looking through the window at an angle would it have been possible for anyone to have seen Pratt's table. The positions of the window in Pratt's room and the transom, are correctly shown in my diagram. I was unable to obtain architect's plans, in spite of repeated attempts, and suggested that it might have been possible to have got sight of the cards from room 311. Professor Stevenson states that according to the plans this is not so but that sight can be got from room 313 which is next door to room 311 and not as far down the corridor. If Professor Stevenson is correct—it is merely a matter of changing room 313 to room 311 in my description.

Professor Eysenck says "so much for this conclusive piece of debunking". In fact, I suggested two ways by which direct sight of the cards might have been obtained: (1) by looking through the window from the corridor; (2) by going into the room on the opposite side of the corridor, as discussed above. In regard to (2), I wrote "It was impossible to be certain of this point . . . however, there was a good possibility that Pearce could have observed Pratt with comparative safety . . .". Is that an attempt to provide "conclusive debunking"?

The objection brought forward by Dr. West regarding method (1) is that Pearce would have had "to stand on something for hours at a time in a busy corridor". What are the facts? I tested a subject last week to see how long it took him to record a pack of 25 E.S.P. cards. He averaged about 34 seconds. Pratt—well practised at recording Zener cards—had to record two packs at the end of each session. Thus at the most we should expect him to be occupied in this manner for two minutes. Pearce would have

known almost to the second when Pratt would start this operation. It should be noted that the total number of sessions in this building was only 15 and that scores indicating something other than a chance result were obtained at only ten of these sessions.

Dr. West refers to a "busy corridor". I visited it in 1960 and was led by Pratt to understand that it was by no means busy at the time of the experiments. The corridor was on the top floor of the Physics building. Rooms on that floor served as an annexe for the Psychology Department, situated 250 yards away in the Medical Building, and were used by members of the department working on Parapsychology. The corridor was deserted on three occasions when I visited it. I climbed chairs, took photographs, and went into the room across the corridor, which was not locked. The top floor may have been a hive of activity in 1933—but I doubt it. My visit was made during term time, whereas the Pearce-Pratt experiment was conducted mainly out of term and at times mutually agreed between Pearce and Pratt.

If someone had stood on a chair in the corridor we might expect him to have had difficulty getting into position on some occasions, and to have had to get down on other occasions if he heard anyone coming up the stairs. But, as I mentioned in my book, the records are not inconsistent with this having happened.

Dr. Beloff comments in relation to system (2) above "Stevenson found that there was only one room from which a view was possible, and that was designated for research, and would scarcely have been the safe vantage point that Hansel suggests". Dr. West also emphasizes this point; he writes "but this was a research room and likely to have been occupied". We are left in the dark as to who used this room for research. How many people in Universities lock their rooms? Pratt, in fact, told me that he did not lock his office when he left it after the test. A research room containing expensive or dangerous equipment may be locked, but is it necessary to lock the door of a room containing a few packs of Zener cards? In any case, it is possible to borrow a key.

Dr. Slater made an error in confusing the spy holes in the ceiling with the room in the physics building. As Dr. Beloff points out, these spy holes were in the medical building, but, in fact the majority of the tests (22 of the 37 sittings) were held in the medical building (Dr. Beloff's statement "only a quarter of the test series" is misleading).

Dr. West criticizes me for bringing in Blackburn, and Dr. Beloff states that there are discrepancies between official reports and Blackburn's confession. Dr. Beloff appears to think that Pearce did not cheat

because he said he did not, and that Blackburn did not cheat when he said that he did. In his confession, Blackburn remarked how inaccurate were the experimental reports. He pointed out that during one trick, in which according to the report he and Smith made no physical contact, they did in fact make contact on several occasions that being the method they were employing at the time to transmit information. We should expect Blackburn's statement 28 years after the event to contain discrepancies in relation to matters of detail.

Dr. Beloff raises a valid criticism when he quotes Soal "is not Hansel aware that from the end of 1954 onwards, Mr. Bowden and I tested over 60 students at Birkbeck without a gleam of success?" I am sorry about this omission. I was not aware of these tests at the time. I note now, however, that in *The Mind Readers* Soal states (p. 43) that he and Bowden had been testing students at Birkbeck. The only details of these tests emerge on p. 184 where we learn that 76 Birkbeck students were tested and no correlation was found between scoring rate and personality characteristics.

It still seems to me that Soal was remarkably lucky in discovering subjects after 1939. After checking back on the records of two subjects originally tested without success for straight telepathy he found that, unknown to him, they had been displaying precognitive telepathy (precognition) all the time. He retested them and found that one of them continued to display precognition and not telepathy, whereas the second had changed and was now remarkably successful at telepathy but not at precognition. After his unsuccessful Birkbeck test, he tested a Spanish girl who displayed "modest" E.S.P. ability. This appears to have given him the idea that unsophisticated children living in rural communities might make good subjects. He then went to Wales and discovered an unsophisticated telepathic child named Glyn Jones at his first attempt.

Dr. West mentions Medhurst's rebuttal of the "only instances in which Hansel raised the slightest possible evidence that any trick actually occurred". He mentions the case of Mrs. Albert who, it was revealed 20 years after the report, had accused Soal of cheating. He says that I failed to state that the photostats of the score sheets showed no signs of "significant alterations". If he studies what I wrote he will find that I suggested that blank spaces might have been left on the score sheets and filled in during the experiment. In that case alterations would not be evident. In fact, since the *original* score sheets were lost after the experiment, any sort of check is difficult.

He states that "even Soal allowed enough to be done by others to show that the results did not

depend on his presence". What are the facts? In all cases when Shackleton was tested in the absence of Soal, his telepathic powers deserted him. Soal took part in all the experiments on Mrs. Stewart. With the Jones boys he was present except at sittings carried out during the third London visit. Since it is evident that the Jones boys cheated, and not Soal, his presence, however, was incidental.

Dr. West states that I did less than justice to attempts to elicit responses from ordinary people. He refers to group tests in which "individuals have been shown to produce different scoring patterns according to belief, attitude, mood, etc.". I did discuss some of these experiments, and it should be noted that one of the series carried out by the U.S. Air Force was of this type (belief-disbelief). It gave no evidence for differential scoring rates. It appears also that Soal conducted tests of this nature in Birkbeck without success. The results of these experiments are easy to check since high-scoring subjects are not required. But even so, only certain investigators report successful results with this type of test. As Dr. West points out, in one experiment he got results, whereas under similar conditions Fisk did not. This is precisely the type of observation that makes Fisk's result highly dubious.

Dr. West mentions recent work employing measures such as EEG and plethysmograph for detecting non-verbal responses. Having seen the grievous pitfalls into which E.S.P. experimenters can fall when they merely have to count the numbers of hits made by their subjects, I wonder how successful they are going to be using techniques requiring the interpretation of a wiggly line on a recording chart.

Professor Stevenson appears to disagree with what many of his fellow parapsychologists have said in the past when he denies the importance of the "conclusive" experiments. I went to considerable trouble to forestall this type of argument, and can only suggest that he reads again pages 22–26 of my book.

Professor Stevenson asks what right I had to refer to the subject Pavel Stepanek's performance as an "act". I only provided brief details of Stepanek's performance, since it appeared to me to be more like a conjuring trick than a serious attempt to demonstrate E.S.P. Professor Stevenson mentions nine errors of detail but does not say what they were. But as I understand it, Stepanek, when confronted with envelopes containing cards—one side white, the other black (or green)—allocated them to two piles, according to whether the white or black side of the card within the envelope was uppermost, and obtained above-chance results.

I suggested that warping of the cards could have provided a cue and that this was not controlled.

If Stepanek can still perform successfully when cues of this nature and other possible sensory cues cannot be utilized by anyone present during the tests, there should be no difficulty in convincing critics like myself that he utilizes E.S.P. Parapsychologists do not, however, appear to have availed themselves of this opportunity to prove their case.

Professor Stevenson is concerned with the freedom with which accusations of fraud are thrown around when parapsychology is discussed. But fraud is a frequent ingredient of parapsychology, as its history shows, and its possibility can never be ignored. Professor Stevenson says that it is not quite impossible that Pearce could have cheated. Presumably, by this, he means that it is possible that Pearce did cheat. That also is my conclusion.

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DEAR SIR,

Critics of my review have taken me up on a number of points of detail. I am grateful for corrections where I have misstated facts, even though they are, I think, of minor and peripheral significance. It is important that readers should get as fair a view of the field of dispute as possible. To the references quoted in the correspondence there are a number of others to be added. Professor Stevenson's review of *The Spiritualists* should be supplemented by Mr. Hall's reply (1) and by his own rejoinder (2). A full account of the period in the history of the S.P.R. during which the Smith-Blackburn hoaxes occurred has been provided in a work of very careful research by Mr. Hall (3); and the criticisms of J. F. Nicol, referred to by Dr. Beloff, have been answered at length and in detail by Mr. Hall (4). Blackburn had a number of disreputable adventures as a young man; but he was no villain, and he settled down into being a solid and respectable citizen: "It was in mature life and not during the follies of youth that Blackburn revealed that he and Smith had tricked the S.P.R." (Hall, *loc. cit.*)

Medhurst and Goldney, cited by Dr. Beloff and Dr. West, did their best for William Crookes, but in effect could do no more than reach a verdict of not proven. That is not the last word. Their arguments, together with all the other criticisms of *The Spiritualists*, were subjected to a judicial appraisal by Dr. Eric Dingwall (5). Dr. Dingwall is a universally recognized authority; he himself at the offices of the S.P.R. twice interviewed Anderson, the key witness on the question of Crookes's motivations, who has been most under attack. Dr. Dingwall considers that on the

material points Anderson was a truthful witness and his memory is not likely to have been at fault. However that may be, the case for regarding Crookes as the dupe rather than the ally of Florence Cook is, in Dr. Dingwall's judgment, so thin as to be unacceptable. Dr. Dingwall found himself in complete agreement with Mr. Hall's conclusions.

I must return to my own views about these vexed questions. The evidential value of the Pearce-Pratt experiments is annihilated by two circumstances, either of which by itself is final and sufficient: (1) Pearce was not supervised; and (2) the experiments were carried out from August, 1933, to March, 1934, but not adequately reported until 20 years later, in 1954.

It is, to me, very surprising that, on those facts which are not in dispute, parapsychologists should think that Crookes can be defended. Can anyone—do any of them?—now believe that Florence Cook ever in her life produced genuine full-form materializations? Yet William Crookes maintained that she did, and his detailed statements make it plain that he was in a position to know. If the materializations were not genuine, then Crookes told lies about them. If one of the world's great scientists, of such unimpeachable integrity that he is elected to the Presidency of the Royal Society, can go on record with lies about his parapsychological investigations, then the *bona fides* of absolutely nobody (with offence intended to none) can be allowed any weight in the evidential balance-scale. This sounds desperately unfair on the parapsychologist, but if he undertakes to prove a miracle this is the burden that is laid upon him.

As it seems to me, none of Professor Hansel's critics appreciate the strength and solidity of his position. In effect, parapsychologists are claiming that miraculous, and as far as we can see lawless, phenomena are part of the structure of the universe in which we live. Millions of miracles, telepathic, precognitive, etc., are happening every second. Objects are moved without force being applied, and information is conveyed from point to point at a negative velocity. If this is so, then our complexly interlocked sciences of physics, chemistry and biology are rotten to their foundation and the logic of science is a mockery.

Surely, there are just two ways ahead for the parapsychologist. He can either combine thesis and antithesis in a new synthesis, and show us a scheme of things entire in which science and para-science are harmoniously united. Or he can face us with a brute fact and force it down our throats though it choke us. If telekinesis is a fact, then nothing stands in the way of the perpetual motion machine. Let him show us a