

To the Editors:

For economy of *Journal* space and of the readers' time I shall make my reply cover the three preceding criticisms of my review, and I shall attempt to state only the facts which I think will make my position clear. Since Mr. Banerjee is the person principally involved, the main burden of my reply will be directed to his letter.

Mr. Banerjee's letter raises two sorts of objections to my review, one relating to the validity of my statements and the other concerning their propriety. I shall discuss them separately. At the outset I wish to say that my review does not contain any conjectures or surmises whose truth cannot be established. Every point I made is a statement of fact for which testimony is available. The main points of my review can be summarized briefly as follows:

(1) It is a fact: (a) that the experiments conducted by Mr. Banerjee in Durham "failed to give significant results"; (b) that the results obtained in the presence of Dr. Pratt in India also were not significant.

(2) It is a fact: (a) that the great bulk of the deviation in Mr. Banerjee's experiments was produced by a phenomenally large score on the first two trials of the run; (b) that the data showed an extraordinarily significant tendency to avoid consecutive or double hits in those two first trials; (c) that these are unusual phenomena.

(3) It is a fact: (a) that in the unwitnessed part of the data of the experiment conducted when Dr. Pratt was in India a first call space was found to have been left blank; (b) that this is an extremely odd situation, especially since the scoring on the first trials in these runs confirmed the tendency observed previously in the data.

(4) It is a fact: (a) that items (2) and (3) suggested to the members of the Parapsychology Laboratory, which was supporting Mr. Banerjee's research, the possibility that the experimenter might have made a practice of leaving the top space blank and filling it in later; (b) that the procedure of the experiment as Dr. Pratt observed it did not exclude this possibility.

(5) It is a fact: (a) that having considered these possibilities and looked into Mr. Banerjee's personal record, the Parapsychology Laboratory decided to withdraw its support of him.

In his letter, Mr. Banerjee does not deny the truth of my state-

ments in item (1). He complains, however, that I was unfair in the implication of (a). But in the very next sentence he contradicts himself by agreeing that the results were "not statistically significant," which is all I have said in my review. While accepting (b) as "perfectly true," he attributes the failure "to a general tension in the community at that time (related to the Chinese invasion)." But one wonders why the Chinese invasion did not interfere with the subject's performance when Dr. Pratt excused himself and left Mr. Banerjee and his assistant to work. It may be recalled that the average score per run in the session which Dr. Pratt did not witness was 5.55 hits per run, an average which compares favorably with the 5.46 average run score reported for the last series (the fifteenth) published in the book.

Apparently Mr. Banerjee does not question the truth of my statements (a) and (b) under (2); but he does not seem to agree with (c), that the effects pointed out were unusual. His silence with regard to (b), I construe as his acceptance of its unquestionable unusualness. His reference to the work of Forwald and Mrs. Sinclair does not make (a) any less unusual. Forwald and Mrs. Sinclair were single subjects; but Mr. Banerjee's work involved several hundred subjects, all of whom seem to have shown this effect. What is an even more revealing unusualness of Mr. Banerjee's studies is that he himself did not seem to be aware of the peculiar effects in his data until my review appeared.

Further, it is more naïve to assume that because the members of the Parapsychology Laboratory "consider the psychological conditions of paramount interest," any oddities in the results can be attributed to post hoc psychological variables. I fail to see how "the enthusiastic approach" of the experimenter which we are told "somehow" induced a "state of intense excitement" that "lasted only a few moments" could produce an avoidance of doubles in the first two trials of the run. It may be recalled that in Mr. Banerjee's data there is highly significant evidence that the hits in the first trials are followed by misses in the second trials. This happened in spite of the fact that the rate of scoring on the second trials was also positive. For example, in the data of the last five series sent to us (Series 11-15) there were 394 hits in a total of 1,040 first trials, and 243 hits in the same number of second trials. The first trials

give a deviation of +186 and an average of 9.47 hits per 25 trials. The second trials give a positive deviation of +35 hits and an average run score of 5.84.

Even if there were no ESP operating in the second trials because the “intense excitement” lasted only a “few moments,” we would expect that one-fifth of the number of first-trial hits would be followed by hits on the second trials unless the subjects were psi-missing on the second trials, which of course was not the case. Among the second trials there were in fact 35 more hits than to be expected by chance. Whereas the theoretically expected number of hits on the second trials following hits in the first trials is 78.80 (empirical expectation of hits on doubles would be even higher: 92.04), the actually observed number of double hits in these series is 21, which is equal to an average run score of 1.33 hits. The CR for this effect is better than 7, and rules out the possibility that the non-doubling is a chance factor.

It may be pointed out that in a series of 100 runs conducted by Mr. Banerjee under the direct supervision of the members of the Parapsychology Laboratory in Durham, there were 18 hits in the first trials and 23 hits in the second trials, where 20 is expected by chance in each case. There were three double hits when 3.6 were expected. Thus the experiment which Mr. Banerjee himself does not consider as a failure fails to support in any way the odd effects of his unobserved data.

If Mr. Banerjee’s hypothesis is to explain the phenomenon under discussion, the following things must be assumed: (1) that Mr. Banerjee generates in his subjects, who are children between four and five-and-one-half years of age, “intense excitement”; (2) that this excitement produces ESP; (3) that ESP thus generated has opposite effects on the first and second trials, so that if the subject gets a hit on the first trial, his ESP tends to cause missing on the second trial, and if the subject misses on the first trial, it tends to produce a hit on the second trial; (4) that this excitement lasts only for a few moments—just enough for the subjects to call the first two trials in which, alone, this effect is found; and (5) that this excitement apparently loses its effect when parapsychologists other than the experimenter and his assistant are present.

With regard to my statements in item (3) Mr. Banerjee writes.

"It is possible that sometimes the first call space in the sheets was left blank by mistake." This is a frank confession of incompetence. I wish to point out, however, that his procedure was such that if he had left any call space blank, particularly at the top, it would change the whole sequence of target-call relationships and invalidate all the succeeding calls in that sequence, for he would then be comparing the calls with the wrong targets.

The statements under (4) and (5) relate to the thinking and decisions of the members of the Parapsychology Laboratory who assured me of the truth of these statements. Mr. Banerjee, of course, cannot deny this. What he can deny and in fact does deny in his letter is that the possibility mentioned in (4a) is real or probable. First, he argues that his experimental conditions were such that fraud was impossible without the collaboration of a large number of people with the experimenter. Had I looked into the detailed reports of his experiments published in the *Indian Journal of Parapsychology*, Mr. Banerjee points out, I would have noticed that some of his experiments were conducted in the presence of observers who testified to the authenticity of the results. Who were these observers and what did they observe? The following paragraph from one of Mr. Banerjee's reports is revealing and illustrates why I paid little attention to the witnessing aspect of his tests.

To avoid the criticism that the significant result may be due to fraud, such persons are made participants who were not interested in the success of the experiments. *To be more precise they did not understand what type of result is called significant and vice versa.* [Italics mine.] Those who participated in the experiment are cultured and educated people of the society of Sri Ganganagar (Rajasthan) India. The scores of each subject were calculated before the father of the child and he was requested to testify in writing on the back of each scoring sheet, the scores of his child. In almost all test sessions fathers of the children were present. If in a test he was not present the mother herself recorded her testimony on the scoring sheet.¹

¹ H. N. Banerjee. ESP investigation between the mother and the child. *Parapsychology: The Indian Journal of Parapsychological Research*, 1963-64, 4, 90-91.

If these participants were the observers (as I assume they were since the subjects were children) it is obvious that the observers were too naïve about parapsychology to be reliable witnesses.

In support of his contention that no major defect was found in his technique, Mr. Banerjee quotes the following sentence from a letter he received from the Parapsychology Laboratory: "One of the important developments of Dr. Pratt's visit was the *small* procedural correction introduced while he was there, concerning your technique." [Mr. Banerjee's italics.] Mr. Banerjee asks us to note the word "small," and apparently overlooks the word "important." Yes, indeed, it was a small correction, but an exceedingly important one, one that was directed towards tightening up the experimental conditions. If only Mr. Banerjee had had someone else record the calls or had made it impossible for himself to know the targets before he handed over the record sheet to his assistant, I am sure things would have been very different.

Finally, Mr. Banerjee considers it improper for use to have been made of his data. When he conducted these experiments he was employed full-time by the Parapsychology Laboratory. By acknowledging that Dr. Rhine had sponsored and supervised his research, Mr. Banerjee implied that his work had Dr. Rhine's approval. This left no choice other than supplementing the incomplete information given in the book and clarifying Mr. Banerjee's relationship with the Parapsychology Laboratory.

Since all the statements made by me are true, I have no reason for retracting any of them. However, I would like to leave no uncertainty about the fact that I did not wish to accuse Mr. Banerjee of fraud, even though such a possibility was suggested by the oddities in the data and the sequence of events that followed Dr. Pratt's visit to India. My intention in the review was not one of "exposing" but of clarifying certain aspects of Mr. Banerjee's work and his acknowledged collaboration with the Parapsychology Laboratory of Duke University.

Regarding Dr. Pratt's letter, I must frankly confess I was surprised by it. I discussed with him doubts about Mr. Banerjee's results when we were both in India and also after his return to the Parapsychology Laboratory. I thought we were in substantial

agreement. But now he apparently does not agree with me. He finds fault with my review on three accounts: (1) that the review should have been shown to him and Mr. Banerjee before its publication; (2) that there are certain inconsistencies in my review; and (3) that I take only facts from his reports and do not refer to his personal evaluations.

In regard to (1) Dr. Pratt neglects to state the fact that his reports were official ones made to the Parapsychology Laboratory in the course of his assignment as a paid representative of the Laboratory sent to India to observe the methods in use in Mr. Banerjee's experiments. As such they were not private papers, as he implies, but like Mr. Banerjee's reports, belonged to the Parapsychology Laboratory.

I am told by the editors, a fact which Dr. Pratt, a former editor, should know, that there is no fixed policy about informing authors of forthcoming reviews. In this case Mr. Banerjee might have been shown the review in advance if it had seemed necessary. But as I explained above, the review was written mainly to set straight the impression that the Parapsychology Laboratory had approved his work. No purpose would have been served by a private discussion with him, since he made his results public anyhow. As for Dr. Pratt, knowing his former attitude as already mentioned, I could scarcely have supposed that he would need to see my review beforehand.

The second point of Dr. Pratt's criticism relates to the supposed inconsistencies in my review. Dr. Pratt himself wrote from India, when he was there as a member of the Parapsychology Laboratory: "... it simply does not make sense to me that so many different subjects should almost all show such a strong beginning salience effect." Now he writes from the University of Virginia that the outstanding scores found on the first two trials of the run represent a "very distinctive decline" and accuses me of being inconsistent for not recognizing it as a common parapsychological effect!

The other point he raises in the fifth paragraph of his letter to the editors is not a question of inconsistency but of clarity. As I explained to Dr. Pratt before he wrote his letter, the hypothesis mentioned in the review does assume motivated missing.

Dr. Pratt refers to the focusing effect found with Dr. Ryzl's

subject without describing the circumstances in which Dr. Ryzl's experiments differ from Mr. Banerjee's. Dr. Pratt has too good a knowledge of parapsychology and is too well acquainted with the work of Dr. Ryzl and of Mr. Banerjee to miss the many important differences between the claims of these two experimenters.

With regard to the third point, Dr. Pratt complains that while drawing on the facts from his reports about Mr. Banerjee's work, I did not refer to the "optimistic" comments he made about opportunities for future work in India. He further asks that this aspect of his reports be published. Consequently I cannot be brief on this point.

I deliberately refrained from taking any note of Dr. Pratt's opinions and impressions while preparing my review, for any reference to his evaluative judgments would call for their scrutiny. Further, I carefully avoided involving Dr. Pratt in any of the questions I raised about Mr. Banerjee's work, even though the hypothesis I suggested in the review was first mentioned by Dr. Pratt himself, as he now acknowledges.

Since he himself now thinks that the favorable aspect of his reports should be published, I have no choice other than to discuss the relevant parts of them.

What did Dr. Pratt think of the beginning salience in Mr. Banerjee's data then? "... it simply does not make sense to me," he reported, "that so many different subjects should almost all show such a strong beginning salience effect. One way or the other, it has to be H. N. B!" [H. N. Banerjee.]

The other way, besides fraud, to which Dr. Pratt was alluding here is that "H. N. B. is the 'subject' in these results. If the results are sound, and if my impression is correct, the elaborate preparations taken are just the setting of the stage for his performance." One may wonder how an experimenter who is recording the responses of the subject can himself be the subject in an ESP test. Dr. Pratt explained to me that when the subject pointed out a particular symbol, Mr. Banerjee, who recorded it, might, instead of recording the subject's response, have recorded his own call; and if genuinely significant results were obtained in this way, it would still be ESP since the target for that call had not yet been determined. If Dr. Pratt's "hunch" was correct, not only would we

have to discredit all that Mr. Banerjee had written about his "SEEMI" technique and give a new interpretation to his results, but it would also be necessary to disqualify Mr. Banerjee as an experimenter who could keep accurate records.

With regard to the blank space in the first run of the last sheet which he did not witness, "I am sure," wrote Dr. Pratt, "V. K. [Banerjee's assistant] and H. N. B. noticed that there was no call. I am inclined to think that H. N. B. simply started in the second space in putting down the calls, and that this was purely accidental. And the fact that no credit (or later filling in, whether to give a hit or miss) was claimed when the omission was found gives me added confidence in V K."

But if Mr. Banerjee had noticed the blank space before the records were given to Dr. Pratt, it was his duty as an experimenter to bring this discrepancy to Dr. Pratt's attention and decide as to what should be done under the unusual circumstances. If Mr. Banerjee started in the second position, what then happened to the twenty-fifth call? Where did he record it? Also, how could he check the hits by comparing the first call with the target for the second call and so on, which he evidently did? So, it is likely that Mr. Banerjee either did not notice the blank space; or, if he did, he suppressed his knowledge of it—which is tantamount to a falsification of records since the reported hits no longer represent the actual number of hits obtained. Apparently Dr. Pratt overlooked this possibility.

Regarding Dr. Stevenson's letter, suffice it to say that I did not accuse Mr. Banerjee of fraud. As Dr. Stevenson recognizes, to say that fraud might have occurred is not to say that fraud did occur.

As already mentioned, the experiments by Mr. Banerjee now under discussion were conducted when he was a full-time employee of the Parapsychology Laboratory, and the data were sent to the Laboratory as a part of his obligation. The Parapsychology Laboratory, as a sponsor of his research, is obliged to furnish any truthful information concerning the data in the absence of complete reporting by Mr. Banerjee. Before preparing my review, I was authorized by the Parapsychology Laboratory to make use of the data as well as Dr. Pratt's reports.

I must add that it was with profound regret and reluctance that I wrote the review. It was a sense of responsibility as an investigator in a precarious field that led me into this rather distasteful controversy. Are we not as scientists obliged to speak out when we think that there is something dubious or false in an experiment? Is the reviewer who does not say what he knows and thinks of a work he is reviewing any better than the investigator who suppresses evidence? I feel that my review was very restrained considering the fact that Mr. Banerjee involved the Parapsychology Laboratory and Dr. Rhine as the sponsor and supervisor of his work.

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