

"In the second edition of my book . . . I critically deal with some questions of parapsychology from the point of view of natural sciences and materialist philosophy. Those American parapsychologists who fail to understand that at present scientific questions may be interpreted in this manner only, do harm not only to us, but also to themselves. In fact, the overwhelming majority of American and British scientists will never acknowledge parapsychology as a science as long as parapsychologists tend to interpret their findings in the occultistic and idealistic spirit."

The great interest aroused by Professor Vasiliev's publication is borne out by the fact that after the first edition of this book (100,000 copies), which was out of print within a brief period, a new edition of 175,000 copies has come off the press. This reviewer is pleased at the opportunity of expressing his conviction that Professor Vasiliev's work (both as an experimental asset, and, perhaps to a greater extent still, as a simultaneous organizational endeavor) is sure to prove an important landmark in the history of parapsychology.

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FIVE YEARS REPORT OF SETH SOHAN LAL MEMORIAL INSTITUTE OF PARAPSYCHOLOGY by S. C. Mukherjee. Sri Ganganagar (India) : Seth Sohan Lal Memorial Institute of Parapsychology, 1962. 130 pp.

This book is the quinquennial report on the activities of S.S.L.M. Institute of Parapsychology in Sri Ganganagar, India. The Institute was established in 1957. Since then Mr. H. N. Banerjee has been its Director, and he has done much to make parapsychology known in that part of the world.

According to the *Report*, the Institute organized or partially sponsored five seminars and discussion groups at various places in India, launched a program to start parapsychology clubs, started a "parapsychological book procurement service," and established scholarship grants for parapsychological research. In addition, the Institute conducted researches on various projects.

The most significant part of the report is the one dealing with Mr. Banerjee's ESP researches with mother-child groups. In view of the fantastic results reported here for the first time in print and because of Mr. Banerjee's acknowledgment in the preface to Dr. J. B. Rhine, "who has directed the experiments on Extra Sensory-Perception and sponsored them," a detailed review of these experiments may be in order.

In a series of 15 experiments, nearly 300 children from four to five and one-half years of age were given a total of 2,950 runs (of 25 trials each). The reported results give a cumulative positive deviation of 2,069 hits and a critical ratio of 19.05.¹ Each of the 15 series gives independently significant results.

The first five series were carried out in India. Then, in an attempt to repeat his results, Mr. Banerjee was invited, at his request, to the Parapsychology Laboratory at Duke University, where 100 of the runs with mother-child groups were carried out. They failed to give significant results.

Mr. Banerjee then returned to India and carried out further research on mother-child groups. He was supported in part of the work by the Parapsychology Laboratory. His continued success under conditions which seemed to safeguard against possible errors (except deliberate fraud) excited the Laboratory's interest, and, at the suggestion of Dr. Rhine, he sent his record sheets to the Laboratory. This reviewer is one of those who went through them to find out if any of the common effects such as declines and U-curves, which have come to be regarded as valuable internal checks of the data, would show up. The data gave no significant evidence of any of those common features of ESP. On the contrary, two very strange effects were found. First, most of the deviation was contributed by a phenomenal scoring on the first two trials of each run; and, second, the data showed a highly significant tendency to avoid double hits in the first two trials. One would expect that one-fifth of the hits on the first trials would be followed by hits on the second trial. But in Mr. Banerjee's data such consecutive hits on the first and second trials were conspicuously missing.

In January, 1963, Dr. J. G. Pratt went to India to work with

¹ This result was computed by the reviewer from the data given in the report.

Mr. Banerjee. Dr. Pratt witnessed 40 runs. The scores on each sheet of 10 runs were 49, 50, 59, 49 (with 50 expected by "chance"). In these 40 runs there were only 11 hits on the first 40 trials (with 8 expected). Then Dr. Pratt excused himself and left Banerjee and his assistant to complete 40 more runs by themselves, unwitnessed. The results of the tests done in Dr. Pratt's absence were 57, 54, 58, 53 for the four sheets (that is, 22 hits above expectation). The effect of high scoring on the first trials showed up strongly. There were 20 "first trial" hits out of 40 possible first trials, as against the 8 expected and observed in the trials conducted in Pratt's presence. While it is possible that the presence of an observer may have caused a decline in the scoring, it does not seem reasonable that so many different subjects should show such a strong beginning salience—at a level never before reported.

In addition to this, Dr. Pratt noted in his reports to the Laboratory that he observed another odd fact. In the first run of the last sheet which he did not witness, the first call space was blank. This is highly unusual. In the record sheets of the fifteen series of the experiments sent to the Parapsychology Laboratory, there were no such blanks. This raises the question whether the experimenter may not have made a practice of leaving the top call space blank (or occasionally the second one), allowing it to be filled in after ascertaining the target for it. Were the conditions such as to have allowed this to have occurred?

The procedure of the precognitive experiment conducted in the presence of Dr. Pratt at least fits into this hypothesis. As the subject pointed out the symbols, Mr. Banerjee wrote down the calls. At the end of the run his assistant found an entry point in the random number book, showed it to Mr. Banerjee (who recorded it), and read the first three numbers aloud before taking the record sheet from Mr. Banerjee to fill the card column. So it was possible for the experimenter either to get a quick glance at the entry number while the assistant was engaged in locating it, or he might simply have listened for the first digit and quickly written in the first call. If this were the case, it would explain the fantastic beginning salience as well as the conspicuous absence of double hits on the first two trials.

Having considered these possibilities and looked into Mr. Banerjee's personal record, the Parapsychology Laboratory decided to withdraw its support of him. The only way parapsychology can be helped in any part of the world is by solid scientific work with high precautionary standards, and India is no exception.

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LETTERS TO THE EDITORS

To the Editors:

The review, in your March 1964 number, by K. Ramakrishna Rao of the "Five Years Report of Seth Sohan Lai Memorial Institute of Parapsychology" involves some delicate and far-reaching issues. Since I have been conspicuously involved by the reviewer and since my name and information from reports that I submitted to the Parapsychology Laboratory were used without my knowledge, I would appreciate an opportunity to clarify my own position on this matter and to call attention to certain inconsistencies in Dr. Rao's review.

The facts Dr. Rao cites regarding my observation of some of the experiments of Mr. Banerjee and his associates in India in 1963 are correct. But I did not reach any conclusions from the facts. Rather, I pointed out to all concerned that the experimental procedure involved weaknesses that would have to be corrected, and I directed my efforts toward this objective. Thus my reports (as well as my comments to the Indian research team) were critical about the past research, but they were optimistic regarding the opportunities for worthwhile work in the future in the situation developed by Mr. Banerjee.

I think it is only right that this aspect of my reports should be published to correct the impression that a reader of Dr. Rao's review is likely to form that I favored the interpretation of fraud in the research. Indeed, I did point out this *possible* interpretation; but this is a far different matter from taking the position that deliberate falsification of the records actually occurred.

Dr. Rao says that the records Mr. Banerjee sent to the Parapsychology Laboratory showed none of the common internal effects, such as declines. Yet the same paragraph mentions the outstanding scores found on the first two trials of the run, an effect that represents a very distinctive decline in scoring within the run.

Dr. Rao also points out the very striking deficiency in the number of double hits on the first two trials of the run. He then goes on to say that if the fraudulent practice had been followed of

leaving one of these call spaces blank until after the target was known, this would account for the beginning salience in the run as well as for the deficiency of double hits on the first two trials. But it is not clear how this hypothesis accounts for the latter peculiarity in the data. Merely filling in a blank call space to give a hit would not produce a deficiency of hits on the adjacent trial. The explanation would need to involve occasional motivated missing.

It seems to me that experimental results should not be condemned *merely* because they show striking and unexpected peculiarities in the data. Surely parapsychologists have encountered many times very striking effects in the data which were not anticipated, a recent example being the phenomenal focusing of ESP upon particular targets found in the results of Pavel Stepanek in Prague.

Is it not a recognized principle of ethical criticism that such a review should be shown before publication to the people most directly involved? This would not only give them the opportunity to clear up some or all of the questions to the satisfaction of the reviewer, but it would also make it possible for them to comment upon remaining issues in the same number of the publication in which the criticism appears. I was surprised to see that this was not done in my own case, but I was shocked to learn that it was not done for Mr. Banerjee, the one who is so vitally concerned.

This letter is not offered as a defense or justification of Mr. Banerjee. Rather, it is a protest against a manner of criticism that seems regrettable. *At the very least*, by adding to the difficulty of reaching a sound judgment regarding the issues it defeats the basic purpose of scientific discussion.

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To the Editors:

Kindly permit me to voice a vigorous protest against the review of "Five Years Report of Seth Sohan Lai Memorial Institute of Parapsychology" signed by Dr. Ramakrishna Rao and published in the March, 1964, issue of the *Journal of Parapsychology*.

This review insinuates the occurrence of fraud in the experi-

ments conducted by H. N. Banerjee. Although the reviewer's wording is somewhat ambiguous, the effect on the reader is clearly defamatory as regards the experiments. Dr. Rao does not mention other possibilities which might account for the unusual results reported. My opinion that there is an insinuation of fraud in the experiments is shared by several other parapsychologists who have read the review.

If now Dr. Rao can name witnesses who saw cheating occurring, he will deserve the thanks of his colleagues. If he cannot do so, he will deserve their severest reproaches, for the review is clearly unethical unless he can produce such evidence.

Dr. Rao's review is additionally unethical for two other reasons. First, he used in an ostensible "review" of a report, score sheets and data submitted by the experimenter not to him but to another parapsychologist, without notifying the experimenter (Mr. Banerjee) of his doubts or his intention to use these data publicly. Secondly, he used notes and other information furnished by another parapsychologist (Dr. J. G. Pratt) who had been a guest of Mr. Banerjee at his laboratory in India and without consulting Dr. Pratt or asking his permission.

In calling on Dr. Rao to support his allegations with evidence or withdraw them, I am not defending Mr. Banerjee or the competence of his conduct of his experiments. I am merely insisting on a careful discrimination of issues.

I should like other parapsychologists to know that as soon as I learned of Dr. Pratt's concern about the competence with which Mr. Banerjee had conducted his experiments, I urged a correspondence between him and Mr. Banerjee to clarify the details and gradually eliminate various possibilities. Unavoidable circumstances delayed this exchange, but it had begun and, at the time of Dr. Rao's review, was developing satisfactorily in a manner which would have been fair to Mr. Banerjee and also to other persons interested in the possible contribution his results suggested.

If an accusation of fraud is made or insinuated, the accuser must be prepared to show that it did in fact occur. It is not enough to say that it might have occurred, for it might have occurred in any of many experiments we all respect. And to allow judgments about an investigator's "personal record" to justify public accusa-

tions of fraud in experiments simply invites all kinds of doubts about experiments conducted by other persons who have not led blameless lives in other matters. The law of libel makes a clear distinction between surmise and evidence in public defamatory statements. Can parapsychologists afford to subscribe to lower ethical standards ?

Recently we have had a rash of irresponsible accusations of fraud in parapsychology. I have gone on record elsewhere as protesting against this, believing that unless we discipline our tongues and our pens we shall spoil the field for the next generation of parapsychologists, those who, we hope, may make a breakthrough. Any such breakthrough is going to include very unusual data, and who will have the courage to publish these, or even show them confidentially to other parapsychologists if he thinks he might become the object of an unfair attack? Unethical criticism is a form of tampering with facts and does the field as much disservice as tampering with experimental data.

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To the Editors:

I am thankful to the Editors of the *Journal of Parapsychology* for enabling me to reply to the review of the "Five Years Report of the Seth Sohan Lai Memorial Institute of Parapsychology" of which I was the Honorary Director a few months back. In his review Dr. R. Rao has made certain comments on the ESP experiments with mother-child groups of the undersigned which have prompted this reply.

On going through the review, one is reminded of the statement of I. J. Good in his book: *The Scientist Speculates* in which he says :

"Pity the poor parascientist. When his results reach the one per cent level of significance they are not believed, and when they are much more significant than that he is suspected of fraud or incompetence. His results will be generally accepted if they become readily repeatable by other experimenters."

The pioneers of parapsychological research whose experiments are considered crucial have sometimes been unfairly treated by critics of ESP research and the validity of their experiments has often been questioned. Dr. Rao by questioning the authenticity of the results of my ESP experiments has made me share the same fate.

Although Dr. Rao has brought into his review other information not printed in my report, he has apparently overlooked the detailed reports of the experiments published in the *Indian Journal of Parapsychology* in which it was stated that the experiments were conducted under the direct supervision of responsible persons who were mostly professors of the State College, administrative officers and other officials of the State Government. These observers had testified to the genuineness of the results of the test. I am willing to provide the names and addresses of these observers who witnessed the experiments and who can substantiate that the experiments were properly conducted and that the results reported were honestly obtained. If the authenticity of my results is questioned, this will mean that more than fifty persons of the area were persuaded by the experimenter to testify to a wrong statement. I was aware of criticism levelled against other parapsychologists about the validity of their experimental result, so I took precautions to safeguard myself, in case my results were questioned.

It is possible that sometimes the first call space in the sheets was left blank by mistake. I deny, however, that I ever filled in this space after the targets were identified. Further, I think that although some of my observers were attending to other matters in the experiments, at least some of them were watching me sufficiently closely so that they can testify that I did not do this. Since reading Dr. Rao's review I have asked four of the observers particularly about this point and they have assured me that they watched me throughout the experiment and are willing to testify that I did not fill in any calls after the child stopped making his calls and the identification of the targets had begun.

Dr. Rao in his review has said that I attempted to repeat my experiment at the Parapsychology Laboratory but failed. About this experimental attempt, it is unfair to imply that the attempt completely failed. Had it failed completely I would not have received letters, as I did, appreciating it and pointing out factors which may have influenced the results of the experiment. The experiment

had given a positive score, but it was not statistically significant. But there were psychological reasons for the fall in the results which were admitted in a note I received from the Parapsychology Laboratory.

I visited Moscow on my way to Durham in 1962. In Moscow I conducted ESP experiments with mother-child pairs under the observation of Soviet scientists interested in parapsychology, and the results were very good. During this particular visit to the U.S.A., I was in Durham—only for a week because I was keen to return to India to join my College and to resume the experiments there; so I had to conduct the experiments at Duke University Parapsychology Laboratory hurriedly. About this, a letter from the Parapsychology Laboratory acknowledged that the rush I was in might have affected the results.

Dr. Rao further says that I could not find any common effects in my experiments. But if this is so, how could it be claimed, as a letter to me from the Parapsychology Laboratory stated: "Dr. Freeman pointed out an interesting similarity or two in the curves of your two halves of the sixth series, the American and Indian. We asked him to do some further analyses. This may turn out to be an aspect of further interest."

About the unusualness of the results I may add that in my experiments I emphasized the psychological aspects of the tests and gave special importance to the enthusiastic approach of the experimenter, which to my mind, was the chief factor influencing the scores. In my reports I pointed out that the scores of the test varied according to changes in the mind of the experimenter. To me this explains the unusual parts of the experimental results. It is known in parapsychological research a state of intense excitement can have a great effect on the results of experiments. It is possible that I was somehow able to induce such a state of intense excitement in the participants, but that it lasted only a few moments.

Incidentally, I may mention that there are other instances of high scores on the first or early calls like those of my experiments. Forwald, for example, made some calculations of PK effects based on the first throw only of some of his runs. And Mrs. Upton Sinclair in her experiments with drawings noted that if she tried groups of drawings, she did better with the first three than with the others of the group. If my arguments in favor of the enthusiastic ap-

proach of the experimenter are taken into consideration, a satisfactory explanation may be found for the unusualness of the results. This was admitted to me in another letter I received from the Parapsychology Laboratory which I quote, as follows: "As you know, we consider the psychological conditions of paramount importance. . ."

It is perfectly true that results obtained in the presence of Dr. Pratt were without significance. This, however, might have been due to a general state of tension in the community at that time (related to the Chinese invasion), and this factor was admitted as a possible explanation in a letter which Dr. Pratt himself wrote to the Parapsychology Laboratory and of which he gave me a copy. Furthermore, after his visit, Dr. Pratt suggested only a small procedural correction in my method. That Dr. Pratt found no major defects in my technique, or at least reported none to me then, is brought out by a letter which I received from the Parapsychology Laboratory and which I also quote briefly: "One of the important developments of Dr. Pratt's visit was the *small* procedural correction introduced while he was there, concerning your technique." (Please note the word "small.") Had there been any serious defect, as has been suggested by Dr. Rao, it should have been pointed out to me, but this was never done.

I should like to say further that I consider it very unfair that the Parapsychology Laboratory did not communicate directly with me at any time about its suspicions of fraud before publication of Dr. Rao's review. I further consider it unfair for use to have been made in Dr. Rao's review (without any request to me) of data which I submitted to the Parapsychology Laboratory for the purposes of consultation and advice.

Finally, since Dr. Rao has insinuated fraud on my part, I think I am entitled to ask him to state clearly whether he has positive evidence that fraud occurred. Surely it is unfair to insinuate fraud only because I reported unusual results. I feel entitled to call on him to make his evidence more explicit or retract his insinuations.

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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 naive 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

¹H. 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 naive 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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