

An Antagonist's View of Parapsychology. A Review of Professor Hansel's *ESP: A Scientific Evaluation*¹

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Professor Hansel is well known as a vigorous opponent of parapsychology. He has already published criticisms of several outstanding experiments in journals in the field (5, 6, 7) and his criticisms have been answered there (12, 17, 21). Now he has taken his case to the lay public and published a summary of his criticisms in book form. I may say right now that this is an unsatisfactory book for laymen, but an important one for parapsychologists. It is far too biased to provide the uninformed reader with an adequate view of parapsychology. For the serious parapsychologist, however, it has two merits. First, Hansel does make some useful suggestions for the improvement of experiments and investigations, and secondly, the bias and unfairness he shows in his attacks are perhaps a warning to parapsychologists of what may come from other intransigent opponents in the future.

I find myself wondering how much time we should spend in combating such unreasonable critics. One feels a little like an early settler in the American West. If a settler spent too much time fighting Indians he would never get his crop planted and so starve; but if he did not spend enough time fighting the Indians he could have his crop and homestead burnt down. The analogy should remind us also that the critics of parapsychology, like the Indians, feel themselves armed in a just cause. This land of the mind now being settled by parapsychologists was *their* land, that of the physiological psychologists, and they want no squatters. They feel a mission to resist the settlers just as some of the new settlers feel impelled to push on, ever Westward!

Hansel concentrates most of his fire on experimental work in parapsychology, partly I suspect because he seems to know almost nothing about spontaneous cases and mediumship, but partly also because he thinks parapsychologists have come nearer to convincing their other scientific colleagues with experiments than with other material they study. Briefly, Hansel's approach is to admit the

extrachance scores of several major experiments, but insist that all loopholes to deception and fraud have not been closed. Since deception and fraud *might* have occurred, and since extrasensory perception is so improbable from other evidence (that is, from other beliefs), Hansel feels entitled to favor fraud over psi as explanations of the high scores. His difficulties begin when he tries to devise explanations of how the fraud might have occurred. For here he concentrates on various possible weaknesses and flaws and imagines how the fraud *could* have happened. But he does not provide any evidence that fraud *did* occur, and in fact even his speculations break down on two counts. First, he must suppose that persons such as J. G. Pratt, J. L. Woodruff, S. G. Soal and various associates and witnesses of their experiments were all participants in fraud. This seems to me and others who know these persons much less probable than extrasensory perception. But apart from that, Hansel's proposed explanations of how fraud could have occurred fail when examined in detail because he has had to overlook certain occasions when what he says happened could not in fact have happened. A simple case in point concerns the proposal that the Jones boys (23) cheated with an ultrasonic whistle which the elderly experimenters could not hear. Hansel neglects to mention several experiments with high scores when younger men were present who *could* have heard an ultrasonic whistle.

It will be worth examining in some detail Hansel's criticisms and speculations about fraud in the Pearce-Pratt series of 1933. In discussing this series, his main speculation is that Hubert Pearce came back from the room in the Duke University Library where he had been making his calls and peeked through a window or transom to watch Pratt turning over the cards as he recorded the targets at the end of the run. Pearce was supposed to turn in his list of calls to Rhine immediately after the two runs of the day and if it is imagined that he did not have time to alter the calls to match the targets he had seen, the speculation can include either an accomplice of Pearce who watched Pratt handle the cards for Pearce and gave him the targets quickly, or the possibility that Pearce instead of making calls slipped out of the Library and spent his time watching Pratt.

Now it is perfectly true that Pearce was not watched during the experiment and he might have slipped out of the Library and tried to cheat. But could he anyway? Hansel publishes a diagram of the layout of rooms in the former Physics Building of Duke University where Pratt (for three of the four series of the experiment) handled the cards and recorded the targets. Pratt's desk was near the window in Room 314 (old numbering) to which I shall be referring. Hansel modestly notes that his drawing is "not to

¹ C. E. M. Hansel, *ESP: A Scientific Evaluation*. (Introduction by E. G. Boring.) New York: Charles Scribner's Sons, 1966. Pp. 263. \$6.95.

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scale," but that is an absurd understatement since the diagram is grossly inaccurate. What is the point of printing a diagram of the layout of rooms unless it is both to scale and accurate? Hansel supposes that Pearce or his accomplice looked into Pratt's office through a window while standing in the hall or in a room across the hall. Hansel says he inspected the terrain and later tried unsuccessfully to obtain copies of plans of the layout of the rooms. Through the kindness of Mr. W. G. Roll, I have obtained copies of plans of the layout of the rooms as they were in 1930, 1954, and 1963. Mr. Roll obtained some additional information and then later I myself visited Duke University and with the assistance of one of the maintenance engineers carefully inspected relevant buildings of the University.

There were no alterations in the former Physics Building between 1930 and 1954. The plan of 1963 shows the wall changed in Room 314 which Hansel states obstructed his view when he tried (in 1960) to sight a line from a transom across the hall to the site of Pratt's desk where the cards lay. Now the idea that Pearce or an accomplice looked at Pratt's cards directly through the window in Pratt's office can be discarded quickly. For this window, called a "borrowed light window" because it is intended to carry light from the rooms into the hall and reduce the need for artificial illumination there, is above eye level. The University engineer who assisted me showed me windows in the Chemistry Building where, on the third floor, the arrangements of windows and doors have not been changed since all the original buildings of the University were constructed. In short, the windows, doors, and transoms of the third floor of the present Chemistry Building are now as were their counterparts in the former Physics Building before remodeling. The bottoms of the glass in the borrowed light windows of the Chemistry Building are six feet above the floor. I myself (not quite 6'1" tall) had to stand on tiptoe in order to get even a brief view of the interior of a laboratory through one of these windows. No one could have spent hours on a chair or on tiptoe in that public corridor of the former Physics Building staring through a window without drawing attention to himself.

Could then Pearce or an accomplice have stationed themselves in a room opposite and looked through a transom or window of that room and then through the window, door, or transom of Room 314? Which would be the appropriate room for the spy? Hansel suggests it is Room No. 311 and in his "not to scale" diagram he places this opposite Room 314. It is not opposite 314, but down the hall next to Room 313 which is partly opposite. Room 311 could under no circumstances have served as the sighting point for the inspection of the cards. The same can be said of Room 315,

which is directly opposite Room 314. This room had a borrowed light window and someone stationed behind it might have looked across the hall through the transom of Pratt's office. He would not, however, have seen any cards because the transom is so high as to preclude a view of anything below about five feet in the area of the room Pratt used. The door of Pratt's office had a glass window in it. If this glass had been transparent the cards might have been viewed through this, but the glass was almost certainly of ripple glass then as it is now. All the offices of the former Physics Building now have ripple glass windows in their doors and so do all the offices and laboratories of the Chemistry Building, the third floor of which, as already mentioned, has been little changed since the 1930's and resembles the former Physics Building at that period. Transoms and borrowed light windows had clear glass.

There remains, however, Room 313, also (partly) opposite Pratt's office and between Rooms 311 and 315. This room had no borrowed light window, although one is incorrectly indicated in Hansel's diagram even if we forget about his numbering; but a view could be had through the transom of its door and across the hall through the borrowed light window of Pratt's office to the area where Pratt handled the cards. It is conceivable that someone standing on a chair in this room could have seen Pratt handle the cards and obtained a view of them. But now we have to imagine how this cheating viewer could have got into Room 313 in order to station himself regularly at the height of the transom in order to peek at the cards Pratt was handling. In the plan of 1930 this room was designated for research. I think we can be sure that it was either occupied or vacant and locked. If occupied, the regular tenants would hardly have ignored or welcomed someone standing on a chair at the door looking through the transom. And if the room was unoccupied and locked, access to it could only have been had by special permission and not casually. Hansel's hypothesis of cheating for the three parts of the Pearce-Pratt series for which Pratt used Room 315 is therefore possible, but extremely improbable. I admit, however, that in the end it all comes down to the honesty of the people concerned. Hansel seems to realize this. And indeed he makes it another point of suspicion. He comments on the silence of Hubert Pearce and calls for a statement from him. It certainly is not the business of subjects of investigations to submit a report of what happened in an investigation; that is clearly the job of the investigator and it is a sign of Hansel's desperation for some sign of fraud that he calls for a statement from Pearce. Mr. Pearce, however, is not unwilling to make a statement when properly approached and I asked him for one. He kindly consented and here it is:

In reference to the suggestions made concerning the experiments that Dr. Gaither Pratt and I did at Duke University, I do not hesitate to say that at no time did I leave my desk in the library during the tests, that neither I nor any person whom I know (other than experimenter or experimenters) had any knowledge of the order of the targets prior to my handing my list of calls to Dr. Pratt or Dr. Rhine, and that I certainly made no effort to obtain a normal knowledge by peeking through the window of Dr. Pratt's office—or by any other means.

(Signed) HUBERT E. PEARCE

Does Hansel think I invented Pearce's statement? Well, it was notarized so someone testified that Hubert Pearce actually signed this letter in his presence. But what then are the credentials of the Notary Public? We soon see how absurd this sort of thing can become, and tears begin to turn to laughter.

Hansel clings to any mite of evidence that could support his speculations. While a guest at Duke University he persuaded Mr. Wadih Saleh, then a research assistant at the Parapsychology Laboratory, to run through a pack of cards while he, Hansel, ostensibly made calls in another room. Hansel left Saleh's room and pretended to go to another room to make the calls, but actually peeped through a crack in the door of Saleh's room and noted down the cards he was looking at. Then Hansel presented his "calls" to Saleh. Hansel describes this episode to make it appear as if he had fooled Saleh, but in fact (as Saleh told Pratt later), although Saleh did not know what Hansel had done, he did not think Hansel had demonstrated psi. One is not necessarily fooled just because one does not see immediately how a trick is done. But Hansel imagines that if a trick can be done, then it was done, and that is the way to explain the results.

Before I leave the Pearce-Pratt series I want to draw attention to Hansel's list of inconsistencies in the reporting of the experiments. Hansel has evidently examined very carefully the nine major reports of the Pearce-Pratt series. He has quite properly drawn attention to discrepancies between some of the different reports of these experiments (10, 11, 13, 14, 15, 16). I have gone over some of these myself and there is no doubt that Hansel is right with regard to many of the inaccuracies of reporting he describes. These include such details as the total number of days of experiments in a series, the actual scores made on various days, the number of series in the whole experiment, Pratt's method of handling the cards in his office, Pratt's method of recording the calls, and whether or not Pearce and Pratt conversed before handing in their sealed results to Rhine. None of these noted inaccuracies are critical for the evaluation of the experiment as evidence of ESP

over long distance; collectively, however, they do leave an impression of a certain carelessness in the preparation of some of the reports.

Hansel also draws attention to the fact that no detailed publication of the Pearce-Pratt series appeared until 1954 (16), more than twenty years after the experiments. Evidently its importance seemed to increase with years until it became a classic and Rhine and Pratt then felt the need to publish a full-scale report. This may have been a mistake since Rhine had already published informal reports in two of his popular books and it is doubtful procedure in science to announce one's results first to the general public and then (in this case many years later) present a detailed report for scientists. But in fairness to Rhine and Pratt I must add that *they* would not necessarily put as much importance on the Pearce-Pratt series as does Hansel. It is the English who magnify Napoleon, not the French. Hansel wants the Pearce-Pratt series to be considered the bedrock of parapsychology because he thinks he has broken it into pebbles. But for parapsychologists, including, I feel sure, Rhine and Pratt, the Pearce-Pratt series is just another good experiment.

Nevertheless, by pointing out the discrepancies in the various published reports of the Pearce-Pratt series Hansel tries to make Rhine and Pratt look ludicrous. Note I do not say he *did* make them look ludicrous. For this would only happen if one believes that experiments collapse when errors of reporting occur. If this happens, then the same applies to criticism and Hansel cannot draw this sword without cutting himself. For his own book is riddled with errors of detail. Let me here name a few. "Estabrooks" is spelled "Esterbrooks"; "Denys" Parsons is spelled "Dennis" Parsons; the Ciba Foundation Conference of 1955 is sited in Cambridge instead of at the Ciba Foundation, London, where it was in fact held; Jan Ehrenwald is described as an English psychiatrist whereas he is a Czech living in the United States; H. Forwald of Sweden is identified as "of the Swiss Federal Institute of Technology at Zurich" and although Forwald studied there many years ago he is certainly not connected with that institution now; for the Belgian Stewart-Soal long distance series Mrs. Stewart is located at Brussels whereas she was at Antwerp.

Now these are all quite trivial errors and it would be captious to point them out in reviewing nearly any other book. But over and over again Hansel drives home the point that parapsychological experiments deserve no confidence because the experimenters have overlooked the most simple details and discrepancies. Evidently Hansel believes to err is human for critics, but for parapsychologists unforgivable. Hansel charges Rhine with relying on his memory in writing his popular accounts of the Pearce-Pratt series.

But Hansel must surely have relied on *his* memory when he made many of the errors noted above and for his foolish diagram of the layout around Pratt's office with Room 311 placed opposite Room 314. We all make errors of details at times and we ought to chastise ourselves and make fewer. But there is no need to throw out our experiments or investigations (or those of our colleagues) because we find some errors in the reports. Our task of judgment is rather to discern the main structure and value of an experiment, or of a case report or other observation. These are enhanced by accuracy of detail, but not destroyed if we find some discrepancies.

In pointing out discrepancies in reports of laboratory experiments Hansel may nevertheless have introduced a healthy corrective against a certain snobbery occasionally found toward spontaneous cases. (Not that Hansel likes spontaneous cases, but I will come to that later.) There is a type of parapsychologist who would tell us that everything in spontaneous case material is vague, half-forgotten, and covered with layers of distortion based on the wish to believe. We have been assured, on the contrary, that what happens in the laboratory is always precise, controlled, and wholly reliable. Hansel has torn up the fence along this frontier and reminded us that some experiments may be poorly reported and therefore less deserving of confidence than some spontaneous cases. I am not asserting this of the Pearce-Pratt series, but as a general principle. And the principle is that scientists are as much liable to make errors in the laboratory or at the writing desk as in the field.

I shall pass over Hansel's chapters on the Pratt-Woodruff experiments and the Soal experiments with Shackleton, Stewart, and the Jones boys. For all of these Hansel proposes ingenious, but implausible, almost impossible speculations about how the subjects or the investigators could have cheated. In my opinion his contentions have been adequately answered already in the references cited earlier. Dr. Soal has, however, prepared a special reply to Hansel's book which he has issued in pamphlet form (22) and which I hope receives the wide circulation it deserves.

In a chapter on "Recent Developments in ESP Research," Hansel purports to summarize a number of parapsychological experiments conducted within recent years and mostly after the "classic" experiments to which he devotes whole chapters. Here through employment of Jesuitical phrasing he can sometimes say something which is literally true, but with damaging intent and (on the uninformed) damaging effect. Thus he states about Schmeidler's investigations of "sheep" and "goats" that "repetition of the test by other investigators did not confirm the original result." True enough as to *some* attempts at replication of Schmeidler's results, but quite misleading in that there have also been *some* confirma-

tions (9, 24). And some alleged attempts at replication have not adequately followed Schmeidler's methods, particularly her ways of categorizing "sheep" and "goats."

Hansel describes the classroom experiments of Anderson and White as if they also have never been replicated, which they sometimes have (2). And these experiments were in any case to some extent a refinement and replication of the earlier work, at least with regard to extrasensory perception in school children, of van Busschbach (27, 28), whom Hansel does not even mention.

In drawing attention to some of Hansel's numerous errors concerning names and places, I excused these as being of a type which anyone can make and merely asked that he similarly pardon such slips in others. But when errors accumulate around one topic, a reader wonders just how familiar the author is with the subject he is writing about. For example, in describing the Prague experiments with Pavel Stepanek, Hansel makes nine mistakes in the space of twenty-two lines. These are mistakes, furthermore, of the order of those he condemns in the reports of the Pearce-Pratt experiment, e.g., about how the cards were handled, what results were obtained, etc. It is difficult to believe he has read any of the published reports of this work (19). If he did read the reports, then he has obviously relied on his memory in writing his account of the experiments. If he did not read the original reports (which I think more likely), how dare he comment on the experiments and refer sneeringly to Stepanek as putting on an "act?"

When Hansel comes to consider spontaneous cases and mediumship, his book really becomes unworthy of serious attention. Hansel has evidently enjoyed singling out a few weak spontaneous cases and dissecting them to nothing. He works over the famous Hornby apparition case and shows its well-known collapse after discrepancies between Judge Hornby's statement and facts revealed later (4). (The Hornby case is not closed, by the way; an explanation of at least one of these discrepancies will eventually be published after living persons connected with the case have died.) Typical, it seems to me, of Hansel's unfairness is his destruction of a case in which the Dutch sensitive Croiset participated. I will not defend this case, which I have not studied myself, but I do protest against Hansel's directing his attack at a report of it in a Sunday newspaper supplement by a journalist, Mr. Jack Pollack. The standards of journalism and those of psychical research differ markedly; otherwise there would be no need for psychical research since accounts of ostensibly paranormal happenings abound in newspapers and we could all just accept them as they appear. But why should parapsychologists be blamed for the errors of newspaper writers?

Hansel's discussion of mediumship seems to me equally shallow.

He draws heavily on J. F. Rinn, whose book (18) he evidently takes as a serious treatise on mediumship. Rinn apparently did attend sittings with mediums (I have wondered whether Hansel ever has) and evidently thought he had seen through them all. Rinn must have had either a prodigious memory or prodigious arrogance because large sections of his book report whole conversations in quotation marks as if (in the days before tape recorders) we were given the exact words of everything that was said years before. But Rinn did not have a prodigious memory; quite the contrary, because his book, as Dale pointed out and documented in a review (1), contained an enormous number of errors.

Hansel's bias (or his ignorance of important sources of information) shows in his citation of the confessions to fraud of Douglas Blackburn and Margaret Fox. Blackburn's confession was discredited (8), and Margaret Fox, who was at the time of her "confession" an indigent alcoholic, later retracted hers (3). I am not here arguing the merits or powers of Blackburn and Smith or of the Fox sisters, but Hansel's failure to give his readers a chance to see the other side of these confessions shows how harmful his book could be for uninformed persons.

My reason for suggesting that Hansel has had little or no personal experience with mediums derives from his lack of discrimination between different mediums and his bold assertion of answers rather than questions when talking about trances. He blandly states that "No medium ever studied has been found free of deceit," but fails to cite the evidence of deceit for such well studied mediums as Mrs. Leonard and Mrs. Piper, not to mention many others, e.g., Mrs. Garrett and Miss Cummins, who have also been studied extensively if less than the first two. Hansel further accepts Ehrenwald's interpretation that mediumistic trances are comparable with hysterical dissociations and multiple personality, as if that accounted for everything in trances. Yes, they are comparable; any student of psychopathology who has attended mediumistic sittings must recognize the similarities between mediumistic trances and multiple personality. But if he persists in his studies he will also see that there are important differences between the two conditions, differences which have puzzled investigators for eighty years. The similarities are obvious; the differences remain a problem and one not likely to be resolved by Hansel's insistence on the similarities.

Hansel is not content with denying that Mrs. Piper and Mrs. Leonard showed any evidence of extrasensory perception. He wants to depict all their sitters as foolishly credulous. To this end he cites Mrs. Salter's paper which analyzes some Leonard sittings (20). He draws attention to the fact that Feda (Mrs. Leonard's control) repeatedly offered similar and often rather general descriptions of

deceased young men who, at that time (World War I) might have been killed, and if surviving death, left with an urge to communicate. What Hansel does not tell his readers, however, is that these general statements were not all uncritically accepted by the sitters. Repeatedly throughout Mrs. Salter's article she quotes sitters as stating that Feda's description did *not* fit the ostensible communicator or the person the sitter had in mind.

I said at the beginning that Hansel's book should be taken seriously by parapsychologists, but not by laymen, and I now wish to explain this statement more fully.

In the first place, I think it fair to say Hansel addressed his book to the general public rather than to professional parapsychologists. The book explains elementary points, even telling the reader what a superscript numeral means. Moreover, it uses catchy language, e.g., "salad days at Duke University," in a pejorative manner unacceptable in scholarly publications. One presumes also that Hansel is capable of greater exactitude, but permitted himself more lax standards in a book for the general reader. At the same time, the book has some of the trappings of scholarly work, e.g., some bibliographic references and a suave, commendatory introduction by Professor E. G. Boring. It might therefore impress an uninformed reader as being authoritative. (Indeed, with these appearances and credentials Hansel's book has received some reviews in respectable scientific journals of this country.) Few lay readers are as suspicious as most scientists are when they see the word "scientific" in the title of a book. Laymen need this reassurance that a book is authoritative whereas most scientists find such dignifying of the title of a book repellent.

Despite its defects, however, Hansel's book should serve to emphasize for parapsychologists several matters which they sometimes neglect or in which they could improve their performances. Hansel does in various places offer some excellent suggestions for the improvement of experimental work in parapsychology. He is for me too convinced of the merits of machines for use in experiments, but nevertheless he makes so many useful suggestions, or reiterates others we already know, that I found myself wishing his energies could be channeled toward conducting experiments instead of only trying to destroy them.

Hansel has also, I think, finally destroyed the myth of the fraud-proof experiment. As Thouless (26) and Roll and I (25) have stated elsewhere, there really is no fraud-proof experiment. No matter how many precautions and extra witnesses one may introduce, the determined critic of the Hansel type can always find a place where fraud *might* have occurred. It would be better for us to recognize this frankly. If we give up the idea of a fraud-proof

experiment, we ought also to give up the idea that our experiments are in any way conclusive or can be regarded as proof. Too often parapsychologists have made rash announcements about the excellence of their experiments or the decisiveness of their results, as if to say: "Now we have it. Now you *must* believe us." But it never turns out that way.

And I do not think it will turn out that way even with the repeatable experiment when we have it. (Hansel, incidentally, makes another false statement when he says that "parapsychologists—or at least some of the more vociferous of them—in denying the necessity to confirm experiments by repetition..." I know of no parapsychologist, either vociferous or subdued, who denies the need for repetition.) No experiment in parapsychology, any more than in conventional psychology, is going to be indefinitely repeatable. Sooner or later the subject is going to lose his capacities or his interest in the investigations, and if he does not do either, he will eventually die and have to be replaced. Obviously, then, we need not one repeatable experiment, but many different kinds of experiments and observations and repetitions.

Hansel devotes considerable space to a scrutiny of the motives of parapsychologists and their subjects. This entitles us to consider the motives of the critics of parapsychology. Some of these, including Hansel, act as if they were frightened of parapsychology. They see in it a threat which personally I do not think they need to feel. But here again some parapsychologists may invite attack by proclaiming too loud and too often the revolutionary, science-shaking impact of parapsychological data once accepted. They assert that parapsychology will transform man's image of himself, pacify the world, rehabilitate religion, and introduce a new age to replace our present troubles. Now statements of this order are probably untrue and they are certainly unwise. They are untrue because, if we will remember it, the vast majority of all men have believed and still believe in paranormal powers and survival of personality after physical death. But there is no evidence whatever that in, say, medieval Europe or modern India, where these beliefs have dominated, men live more richly, more constructively, or more peacefully than they do in the skeptical West of today. And such statements are unwise because they are immodest and tactless vis-à-vis our fellow scientists. These believe in the importance of what *they* are doing just as much as we believe in the value of what we are doing. Why should one line of scientific endeavor seem more important than another if all are striving for improved knowledge of man and the universe? Furthermore, our data and our future discoveries do not mean that other scientists will have to throw away their work or, for that matter, most of their beliefs.

Parapsychology threatens no one except dogmatic materialists. But dogmatists will be threatened and will engage us in time-consuming battle if given the opportunity. We can probably expect renewed assaults by successors of Hansel, but there is no need to provoke them. I suggest, therefore, that we let sleeping scientists lie. Hansel and his like seem to have sprung to arms as much to contest the claims of parapsychologists as to contest their results.

Finally, I come to the place of accusations of fraud in the dialogue of science. I know of no other branch of science past or present, other than parapsychology, where innuendoes and accusations of fraud are allowed to appear in print and go unpunished unless the charges are substantiated. No one would think of accusing me of fraud for my work in conventional psychiatry, but obviously I lose this immunity when I work in parapsychology. Why? Perhaps it is because the subject seems so important, or has been made to appear so by some of us, as I have discussed above. Perhaps it is because parapsychologists are still (*qua* parapsychologists) outlaws or at least eccentrics of science and not protected by the same rules and laws whether of simple fair play or of libel. I understand Hansel had difficulty in finding a publisher for his book in Great Britain. Is this because the laws of libel are stricter there than in the United States (which they are) or because English publishers are more discriminating and have better informed readers?

Hansel would have us believe that the early experimenters were naive and perhaps they were, but not in the way Hansel thinks. They were not naive about what constitutes a good experiment. The experiments of Pratt and Soal and many others *were* good experiments, although they had their imperfections. The early experimenters were naive rather in their belief that these experiments would carry conviction to the average scientist. They counted on more interest among scientists than the field receives and more fairness than it usually receives from those who do show an interest. Hansel's book can usefully remind us that if we are primarily working to carry conviction to other scientists, and I do not think most of us are, then we have barely begun.

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