'For analysis purposes we have created little pigeonholes, but psi refuses to be pigeonholed. Its quicksilver process evades our analysis as we reach for it and desperately try to grasp it. Like the chain that bound the Norse god Loki it is invisible yet a bond of steel. And in its grip we are as baffled as he.' As a statement of our predicament it would be hard to improve on that.

JOHN BELOFF

ESP AND PARAPSYCHOLOGY: A CRITICAL RE-EVALUATION. By C. E. M. Hansel, Buffalo, New York: Prometheus Books 1980. 325 pp. Index.

For all its blatant bias, this updated version of a book first published in 1966 (*ESP: A Scientific Evaluation*) is more scholarly than many of the attacks on psychical research that appear from time to time. The attempt to demolish the paranormal completely may appear irritating and exaggerated, but Professor Hansel does have some comments of substance which challenge any complacency one might feel about standards of evidence in our subject.

What is needed, of course, both to further knowledge and to convince scientists, is some straightforward and potentially repeatable psi effect that can be obtained by any competent and diligent worker. Some people believe that this is about to be achieved, by the Ganzfeld technique in ESP tests (1) or by the implantation of strain gauges in metals for PK testing (2). Hansel mentions neither of these topics. Instead, he concentrates on criticizing a selection of famous researches and is able to demonstrate that the perfect case, or the definitive experimental demonstration, which will prove the existence of psi for all time, does not exist. This is no surprise. So long as the only available evidence depends upon some particular event or experimental set up that cannot be duplicated the sceptic can always fall back, as Hansel does, on the possibility of fraud.

In some of his examples (e.g. Smith and Blackburn, the Creery sisters) fraud was established, in some (e.g. the Soal-Shackleton series) there was strong evidence of falsifications, in others (e.g. Pearce-Pratt distance tests) Professor Hansel can produce no more than unsubstantiated, and some may think implausible, suspicions. Even within the restricted context of a review of the more celebrated experiments Hansel's selective approach gives a poor impression of the true strength of that kind of evidence. Several notable examples, such as the work with the gifted subjects Harribance and Delmore (both of which are listed by Francis Hitching in his recent SPR pamphlet on *Psi in the Laboratory: 12 Crucial Findings*) receive no mention.

Some of the criticisms of the methodology of particular experiments, such as the Targ-Puthoff remote viewing research, Brugman's pioneering chequer board experiment and the early dice throwing at Duke, are certainly justified, and indeed have been made by other parapsychologists. Hansel does not, however, always give proper consideration to the answers to criticisms that have been made, for example, by Targ and Puthoff. A particularly glaring instance is his repetition of criticisms of work with the special subject Pavel Stepanic which appear without reference to Pratt's replies (3). He also implies (p. 271), wrongly, that Stepanec's results depended upon Pratt's presence. It would be profitless, and require much more effort than I could give, to produce a detailed critical evaluation of all Hansel's comments. One can readily admit that the flawless experiment does not exist.

What I think Professor Hansel does do successfully is to point out that parapsychologists have not always been as careful as they should, and that in the past experiments have been accepted much too readily as being virtually fraud proof or evidentially incontrovertible. The Soal *debacle* provides a striking example, but there are many other less dramatic instances. The Pearce-Pratt distance tests, one of the 12 'crucial' experiments cited by Hitching, was never reported in adequate detail at the time it was carried out, and Hansel has a fine time listing all the discrepancies between the various descriptions of these experiments published over the years by Rhine and Pratt, which apparently extend even to some doubt as to the actual scores and number of runs.

The weakest point about the Pearce-Pratt series and other similar demonstrations is the impossibility of repetition. Pearce was reported to have lost his powers almost immediately after the experiment. As Hansel is at pains to point out, failure to repeat is more suspicious in its implications if the subject is still supposed to retain psi powers. At the Stanford Research Institute Uri Geller is reported to have been successful in every one of eight attempts to specify the uppermost face of a die concealed in a metal box. Hansel comments: 'Since it is incomprehensible that, after this result, the experiment should be dropped completely it would appear likely that Geller refused to participate in any further tests.'

Subjects other than experimental ESP are dealt with only briefly, but here again Hansel finds examples of cases frequently cited and widely accepted as well substantiated which no longer appear so when original sources are critically examined. For instance, in checking up on one of the cases in which Gerard Croiset was said to have applied his clairvoyant faculty to good effect in helping the Dutch police with their detective work, Hansel shows that in actuality the performance was much less remarkable than was suggested by the reports published by Tenhaeff and Pollack.

Provided his wilder pronouncements and sweeping denunciations can be ignored, Professor Hansel can be credited with giving us good cause to reflect on our standards for evaluating evidence.

D. J. West

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Water Witching USA. By Evon Z. Vogt and Ray Hyman. University of Chicago Press, Chicago and London. Second Edition, 1979 (First Edition, 1959), 260 pp.

The authors of this book are sceptics and argue their case powerfully though unfanatically, for example quoting with approval the work of Francis Hitching, although his position and viewpoint are very different from theirs, and fairly playback no sign of radio interference then this is most unlikely to be a problem during efforts to receive the EVP. Incidentally, I should also point out that since by far the greater number of EVP voices picked up by most people using this simple 'microphone recording method' are non-tonal in character, i.e. 'whisper voices' they are totally dissimilar from broadcasts from radio stations.

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Madam,

Dr. D. J. West in his fine review of C. E. M. Hansel's *ESP and Parapsychology: A Critical Re-Evaluation* (1980) (*Journal* No. 787) seems to accept too readily the implications of Professor Hansel's alleged discovery of discrepancies in the reporting of the Pearce–Pratt experiment in various places. Since the Pearce–Pratt experiment is one of the highly evidential studies we have in parapsychology and since Hansel is apparently successful in creating the impression—even among such unbiased scientists as Dr. West—that there was something seriously wrong with it, I wish briefly to examine Hansel's arguments and his credibility as a responsible critic. The points made against the Pearce–Pratt experiment are: (1) that it was not reported in adequate detail at the time it was carried out; (2) that there were discrepancies in its different published versions; and (3) that the experimental conditions were such that the subject, Pearce, could have cheated in a number of possible ways.

Let us consider the fraud issue first. Neither Hansel, or anyone else for that matter, presented any evidence or circumstances that suggest even remotely that Pearce did cheat. The best Hansel (1980) was able to produce was his concluding statement in the book, 'A further unsatisfactory feature lies in the fact that a statement has not been made by the central figure, Hubert Pearce. The experimenters state that trickery was impossible, but what would Pearce have said? Perhaps one day he will give us his own account of the experiment' (p. 123). This statement does not tally with the facts. Contrary to Hansel's remarks, Pearce did make a statement in which he unequivocally asserted that he did not cheat (Stevenson, 1967). Pearce is now dead, and therefore will not be able to make another statement more to the liking of Hansel, unless Hansel believes in the ability of the deceased to make statements!

The hypothesis of fraud to explain away the results of such experiments as the Pearce–Pratt series is essentially sterile and non-falsifiable. As I pointed out elsewhere (Rao, 1981), the argument that it is more parsimonious to assume fraud rather than the existence of 'impossible' phenomena such as ESP is as logically false as it is historically untrue.

Much was made of the fact that the original report of the Pearce–Pratt experiments did not give all the details of procedure and experimental conditions that we now consider necessary. West and some other parapsychologists appear to be ready to blame Rhine for this failure. Stevenson (1967), for example, writes, 'Rhine had already published informal reports [of the Pearce–Pratt experiment] 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' (p. 259). I believe these accusations are unfair.

It is not the case that Rhine announced his results first to the public. The results of the Pearce–Pratt experiment were first published in *The Journal of Abnormal and Social Psychology* (Rhine, 1936) and were only subsequently mentioned in his popular books. (The first of these, *New Frontiers of the Mind*, appeared in 1938.) *The Journal of Abnormal and Social Psychology* is a respected journal in mainstream psychology and Rhine had no editorial control over it. Does this not clearly imply that the additional details that we now consider necessary were not considered so then by the psychologists who refereed his paper and the editors who published it? If the *Journal of Parapsychology* was in existence then and if Rhine published his report in it with inadequate details, we might have had some reason to blame him for not giving them all. The truth is that details of the sort that we now require of parapsychological reports were simply not found necessary then. When it became increasingly clear that further details of the experimental procedure were called for, Rhine and Pratt published a detailed report in 1954.

Now, the more serious of the criticisms relates to the discrepancies between various published accounts of the experiment. Several of these are trivial and none is sufficient to call into question the veracity of the experiment or the credibility of the experimenters. Interestingly, Hansel makes more errors in his very brief review of the experiment than do the authors. Here are some examples.

He writes, 'The scores published in the *Journal of Abnormal and Social Psychology* disagree with those in the *Journal of Parapsychology*. They give total hits for the four subseries as: A, 179; B, 288; C, 86; D, 56. The individual scores quoted are also in a different order for subseries B and C from those given in the *Journal of Parapsychology*' (1980, 120–121). Here Hansel gives the total scores as reported in one journal and not in the other. Therefore, the reader does not really know the magnitude of the discrepancies. More significantly, neither report actually gives the total number of hits in each of the four subseries as Hansel implies. These totals, it appears, are computed by Hansel from the footnote on page 222 of *The Journal of Abnormal and Social Psychology* (1936). He found they differed from those obtained by adding up individual scores as given in the *Journal of Parapsychology* (1954) report. I did the same and came up with different figures. Hansel gives the total hits for subseries A as 179. Actually, the total score that one would obtain by adding up individual scores given in footnotes in both reports or by computing from the average and deviation scores given in the main body of the reports is 119. So Hansel in his computation makes an error much larger than anything that he finds in the reports he criticizes. Again, as far as this score is concerned, there is no discrepancy between the two reports.

As for subseries B, the individual scores as given in the footnotes add up to 288 and 295 in the 1936 and 1954 reports, respectively. Recall that totals are not given in the reports, but can be computed by us from the footnotes as well as from the results presented in the main body of the reports. In the table on page 222 of *The Abnormal and Social Psychology* report, we find that for subseries B there are 1100 trials and the average score for 25 trials is 6.7. From this, it is clear that even in this report the total number of hits for subseries B is 295, the same as that given in the *Journal of parapsychology* report. So there is no discrepancy here.

It would appear that a few of the individual scores as given in the footnote for the 1936 article were misprinted and that one score was inadvertently left out. The footnote gives only 43 scores when there should have been 44.

Hansel leaves the impression that Rhine and Pratt were unmindful of the errors in the first report. This was not so. A footnote in the *Journal of Parapsychology* article (Rhine and Pratt, 1954) reads: 'In the two reports . . . in which the run scores of the series were published, the scores of subseries B and C were not given consecutively, and there were two other minor errors. It seems worthwhile, therefore, to list the complete run scores in chronological order here' (p. 171). Here is the explanation of the discrepancy in the sequence of the scores as given in the 1936 and 1954 reports. Surely Hansel cannot be unaware of this: he gets the individual scores from this footnote only.

While it is regrettable that there were errors in the first report, though inconsequential in themselves, I wonder how many of us can honestly say that we make no such errors. As I have pointed out, Hansel himself commits a few. To give a few more, reference 8 on page 119 which has to do with *Extra-Sensory Perception after Sixty Years* refers on page 123 to (The) *Reach of the Mind* (incidentally, *The* was omitted); reference 9 to *The Reach of the Mind* on page 119 is listed in the notes on page 123 as *New World of the Mind.* On page 121 Hansel mentions *Frontiers of the Mind* by J. B. Rhine. He obviously means *New Frontiers of the Mind.*

In evaluating Hansel's critique, we should bear in mind that the records of the Pearce–Pratt experiment are still in existence, and that they were examined in the past by others and re-checked by Stuart, Greenwood and Murphy. Again, Hansel himself was at Duke with Rhine and Pratt and they would have easily clarified these matters, if Hansel had raided them then. Hansel (1961) did not refer to these discrepancies in his first critique of this experiment published in the *Journal of Parapsychology.*

In summary, then, Hansel's criticism of the Pearce–Pratt experiment is not entirely reliable. But the fact that his words have been taken seriously by such persons as Dr. West makes me wonder whether there is some truth in the saying that if someone shouts long and loud enough he will be heard without regard to what he says.

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Madam,

I have just returned from a lengthy trip to discover that Prof. C. F. Osborne, of the Caulfield Institute of Technology in Australia, has misrepresented my tests of dowsers in the pages of your *Journal.*

I hardly know where to begin. First, I at no time referred to my tests as 'experiments'. They were tests, agreed to completely in form and content by the subjects and all other participants. The object was to determine if the subjects could perform *in accordance with their firmly stated claims*, in which case I was prepared to surrender my cheque for the sum of US$10,000, offered for any demonstration of a paranormal ability within stated limits. Osborne's claim that I must now forfeit my cheque is made in spite of the fact that everyone knew what the terms were. The terms were widely publicized. I cannot understand why Prof. Osborne ignores this fact.

All the dowsers claimed results of 85 per cent to 100 per cent success, and all fell miserably short of these estimates. Thus, none deserve the prize. That is an incontestable fact.

All participants agreed that dowsing for *any* substance is part of the same phenomenon. That is in the record. Thus, all agreed in advance that the results of any one test were to be included with all the rest. I, too, agreed to abide by this rule. If we cannot take the opinion of the dowsers themselves, whose opinion are we to accept?

I did *not*, as Osborne reports, '(dismiss) dowsing as a genuine phenomenon'. Until I have seen *all* dowsers, I cannot make that statement. Similarly, confronted with such miserable performances as I have seen in all the years I have investigated these matters, I have no reason at all to accept the claims. My statement is only that dowsing does not seem, on the basis of present evidence, to be a genuine phenomenon. That statement was made in the TV film from which Osborne quotes. No significance level was quoted in my tests, since that was not their purpose. I displayed no 'ignorance of scientific method' as claimed by Osborne. *He* was not ignorant of the design and purpose of my tests.

Dr. Osborne tells readers that I was able to 'obtain successful results'. Really? I am at a loss to discover them. Contrary to what Osborne claims, I *did*' (avail myself) of the scientific expertise of members of (my) experiment . . .' (again, his use of that term, 'experiment' is incorrect) and consulted with Prof. Persi Diaconis of Stanford University, California, Department of Statistics. Dr. Diaconis points out a fact that escaped Osborne: *None of the dowsers, in any tests, performed at a significant level! None of them!* Diaconis, Stanford's professor of statistics, says that Osborne's techniques point to the need for 'a good introductory course in the statistical facts of life'. He goes on to say that such *ad hoc* methods were discontinued in parapsychology long ago.

To select out some data and prove a point with them is not a scientific