

ESP AND PARAPSYCHOLOGY: A CRITICAL REEVALUATION by C. E. M. Hansel. Buffalo, New York: Prometheus Books, 1980. Pp. 325. \$15.95, cloth; \$7.95, paper.

This is a revision of a book published in 1965 under the title *ESP: A Scientific Evaluation*. Its thesis remains the same, and indeed much of the new book is identical with the earlier one. The principal change is the addition of new sections dealing with research published since 1965.

Hansel addresses the question of whether any single pieces of evidence for psi processes are conclusive. He argues, as would presumably all parapsychologists, that no one piece of evidence is conclusive. The same argument would apply to other scientific problems. Hansel proceeds to argue, however, as he would not for most other problems, that each separate piece of evidence for psi processes should therefore be totally disregarded, since for each one an explanation in terms of error or fraud remains possible.

In the course of justifying his position, Hansel criticizes a number of separate studies. He gives the impression that he has selected studies for attention primarily on the basis of the strength of evidence for ESP that parapsychologists claim they provide. His allotment of space belies this basis of selection. At the extreme, he devotes twenty pages to Soal's report on "the telepathic Welsh schoolboys" (which few parapsychologists, I imagine, would take seriously as evidence for ESP) and only ten lines to all the "sheep-goat" studies (several of which separately might, and the sum of which would certainly, be thought by many to provide strong evidence for ESP).

A reader familiar with the parapsychological literature is likely to gain from these facts an impression of either extreme carelessness or deliberate misrepresentation. Many other facts about the book and its history will tend to confirm this impression. For example:

1. In the earlier edition, Hansel's discussion of the Pearce-Pratt experiment concludes by asserting (p. 85) that a "further unsatisfactory feature" is that the subject has made no statement about whether he engaged in trickery. It is on the one hand hard to see why a serious critic would regard this as an important matter, since lying about one's trickery would seem easier than the trickery itself. On the other hand, it is hard to understand why Hansel lets his assertion stand (p. 123) when a statement from the subject has indeed been published subsequently in one major review (Stevenson, 1967) of Hansel's earlier edition and referred to in another (Medhurst, 1968).

2. Hansel's paragraph about the "sheep-goat" experiments remains identical to that in the earlier edition despite the fact that reviewers of the first edition called specific attention to its inadequacies. As a statement at either time of publication the paragraph is thoroughly misleading. It reports a single article by Gertrude Schmeidler, and then cites two experiments by others in justification of the assertion that "repetition of the test by other investigators did not confirm the original result" (p. 200). The uninformed reader would never guess that Schmeidler's evidence is based on a number of experiments, nor that the two experiments by others that Hansel cites have been selected by him from among numerous experiments, some of which have in fact confirmed the original result. Hansel could hardly plead ignorance of a body of publications which have had so conspicuous a place in the parapsychological literature (see, for example, the review by Palmer, 1971).

3. In his first edition, Hansel made some inaccurate statements in support of his suggestion that Soal was a dishonest experimenter; their inaccuracy, pointed out by Medhurst (1968) was confirmed by another reviewer friendly to Hansel's book (Scott, 1968). Hansel nonetheless repeats his statements verbatim in the new edition, as though the new and quite different evidence for Soal's dishonesty removes all obligation to be truthful in writing about him.

4. It is in general not possible or practicable to design an experiment in a way that permits simultaneous control of all possible alternative explanations of results, especially when those explanations pertain to various possible forms of experimenter dishonesty or carelessness. Hansel seems to concentrate his account of each experiment on a particular alternative not controlled in that experiment without mentioning the alternatives successfully excluded by the design chosen. A prime example is provided by his account (pp. 224-225) of certain PK experiments by Helmut Schmidt. Schmidt had a subject try to influence a single machine, sometimes in a high-scoring direction and sometimes in a low-scoring direction; and at other times as a check on machine randomness he recorded machine performance with no one trying to influence it. Hansel discounts the statistically significant findings, saying that Schmidt should instead have used separate machines for high-aim and low-aim trials, so that departure from chance would be clearly evidenced on the non-resettable counters inside the machine. I have no doubt that if Schmidt had conducted the experiments as Hansel recommends, Hansel would have dismissed their results on the grounds that

randomness of the machines was not assessed, and that for each condition a machine may have been selected that was biased in the direction the experimenter wished.

5. Selective mention of experiments which can be criticized is apparent in Hansel's discussion of "remote viewing." Of Targ and Puthoff's experiments, Hansel presents in detail only the one with Pat Price as subject. The choice seems to be based on two considerations: (a) For this experiment, he is able to follow the statement that the subject "scored at well above the chance level" with a statement that Targ and Puthoff's calculations involved "an elementary statistical error." (He leaves readers to imagine whether the initial statement is completely vitiated by the second one, a point he could easily have clarified.) (b) For this experiment, he is able to point out a procedural defect which is potentially very serious, and he does not mention later experiments in which no corresponding defect seems to be present.

6. Evidence which appears to support Hansel's position seems to be accepted quite uncritically. A good example is his reporting as though established fact ("... it transpires that there were more obvious reasons for the result achieved by Price") inferences by Marks and Kammann (1978) whose implication for the Targ and Puthoff work, when viewed critically and empirically, turn out to be quite different from what Marks, Kammann, and Hansel suppose (Tart, Puthoff, & Targ, 1980).

7. Selective mention extends to details as well as to choice of experiments. In reporting the line of animal research in which an American investigator was caught cheating, Hansel mentions (p. 234) that three comparable and earlier experiments which yielded significant results had been "reported by two 'eminent' but anonymous French biologists." He is inviting the reader to doubt the eminence and perhaps even the existence of the French biologists. The naive reader cannot reasonably guess that the senior author among the two French biologists is indeed a biologist of great eminence, whose name would be known to many readers of biological literature in English as well as in French, nor that he is anonymous only in the special sense of avoiding publicity by using a pseudonym, not in the sense of concealing his identity. If Hansel does not know these facts, he does not have the familiarity with parapsychological journals that would be essential background for the sweeping generalizations he makes or implies about their contents.

Hansel's book, I must conclude, does not meet ordinary scholarly standards of accuracy and objectivity. The first edition was clearly not the "scientific evaluation" claimed in its subtitle; the second edition in

turn is not accurately described by its subtitle, "a critical reevaluation." It is an expressive evaluation, a version of parapsychology as seen and presented selectively under the influence of an apparently unshakeable conviction that it has no subject matter. Since most readers who share this conviction, or who might easily be persuaded to share it, have no detailed knowledge of parapsychology, they may not recognize the propagandistic character of much that seems on the surface to be scholarly.

Deficient though it is as scholarship, the book may be very useful as a stimulus to thought and action in parapsychologists. Here is a psychologist with a long-standing interest in parapsychology who has obviously read with some care a number of research reports and has remained unimpressed. Why? Some gratification might be obtained by trying to psychologize the author. More profit in the long run may be found in considering research—one's own as well as that of others—in the light of the various criticisms Hansel makes. In this way it may be provocative and informative for parapsychologists, even if for other readers it seems designed to misinform and thus to corrupt judgment.

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