

dished up; but then, there is no sense in discussing one's tastes. Another objection is that only the pro's are put before the reader. Only one or two papers (in the survival hypothesis section) are included that may be said to counterbalance the generally favorable results and interpretations by critically formulating an adverse opinion or showing up the weak points of certain conclusions. The Editor himself seems to have been aware of this gap, for in the Preface (p. xvi) he remarks that, owing to lack of space, controversial publications pertinent to the subject could not be added to the contents of the book.

Those who had the privilege of hearing Dr. Remy Chauvin deliver his paper on the highly significant PK influencing of the radioactive breakdown of uranium at the Parapsychological Association's Oxford Convention (Sept., 1964) will be grateful to Prof. Bender for publishing Chauvin's paper with full details for the very first time, though in a German translation (pp. 475-81). It is to be hoped that Prof. Chauvin's paper will soon be published in English in one of the leading periodicals in our field.

G. ZORAB

*Verdijkstraat 32
Den Haag
Holland*

ESP: A SCIENTIFIC EVALUATION by C. E. M. Hansel. Introduction by E. G. Boring. New York: Charles Scribner's Sons, 1966. Pp. xxi +263. \$6.95.

Prof. Hansel's book was described in the publisher's note as "an important book in an area marked by misinformation, misguided enthusiasm, and prejudice" and as a "dispassionate and comprehensive" survey of parapsychological research. Scientific evaluation at this level is highly desirable; and if the author has succeeded in fulfilling the claims made for his book, he will have made a substantial contribution to the literature of parapsychology.

In the Introduction, E. G. Boring equates ESP research with man's desire to believe in mysterious occult principles. Essentially he makes two points: first, that ESP cannot be scientifically verified because it is negatively defined; second, that certain commonly

accepted aspects of probability theory may be faulty. Neither of these points is maintained by Hansel.

In the first chapter, Hansel surveys the attitudes of several eminent British psychologists, concluding that they regard ESP to be scientifically established. He goes on to indicate the purpose of the book:

A close inspection of the work of the parapsychologists is . . . important for two reasons: if their claims are justified, a complete revision of contemporary scientific thought is required at least comparable to that made necessary in biology by Darwin and in physics by Einstein. On the other hand, if ESP is merely an artifact, it is then important to understand how conventional experimental methods can yield results leading to erroneous conclusions (pp. 7-8).

In the third chapter, "Examining the Evidence," Hansel sets the tone for his evaluation of psi research. He suggests several criteria which the reader is asked to bear in mind when assessing the case for ESP, the most important of which is:

An experiment that has any defect such that its result may be due to a cause other than ESP cannot provide conclusive proof of ESP. In parapsychology research, the process . . . is both hypothetical and a priori extremely unlikely. Any possible known cause of the result is far more likely to be responsible for it than the hypothetical process under consideration.

A possible explanation other than extrasensory perception, provided it involves only well-established processes, should not be rejected on the grounds of its complexity or because it seems unlikely to be the true one (p. 17).

One of the basic principles of experimental method is the need of systematically eliminating extraneous variables that might lead to spurious results, and hence, to erroneous conclusions. This is an important criterion in the assessment of any experimental result. However, there is another element involved in Hansel's statement: that there is an antecedent improbability to be considered when assessing the experimental results obtained in ESP research. Antecedent improbabilities are strange things, because what is antecedently improbable ("a priori extremely unlikely") at one point in time often becomes status quo at a later time. Science, by its very

nature a process of continuing inquiry, is a self-correcting system; and "scientific truth" carries with it the implicit proviso: "until further notice."¹

The great shock suffered by modern physics through the introduction of relativity theory led to the recognition (through operationism) that science must remain as close to the base of observation, and as far away from a priori notions, as possible. Thus, Bridgman, in *The Logic of Modern Physics*, introduced operationism as follows:

The attitude of the physicist must . . . be one of pure empiricism. *He recognizes no a priori principles which determine or limit the possibilities of new experience.* Experience is determined by experience. This practically means that we must give up the demand that all nature be embraced in any formula, either simple or complicated.² (Italics mine.)

Hansel's premise that ESP is "a priori extremely unlikely" must therefore be viewed as a statement of personal judgment and cannot be represented as a valid criterion for the scientific assessment of evidence. However, Hansel places a great deal of emphasis on this premise. He goes on to say, "An ESP experiment can be analyzed in much the same way as one tries to discover how the conjuror performs his trick." and ". . . in analyzing an experiment that purports to prove ESP, it is wise to adopt initially the assumption that ESP is impossible, just as it is assumed that the conjuror cannot saw the same girl in half twice each evening" (p. 19).

Proceeding on this premise, Hansel allows himself a great deal of latitude in the type of "well-established processes" which might be offered to compete with ESP as an explanation of the experimental results in parapsychology. He is mainly concerned with the possibility of fraud (by subject or investigator) and says, "If the result could have arisen through a trick, the experiment must be considered unsatisfactory proof of ESP, *whether or not it is finally decided that such a trick was in fact used*" (p. 18). (Italics mine.) Hansel does not provide any evidence of fraud or of the underlying

¹ Feigl, Herbert. The scientific outlook. *Readings in the Philosophy of Science.* (H. Feigl and M. Brodbeck, Eds.) New York: Appleton-Century-Croft, 1953.

² Bridgman, P. W. *The Logic of Modern Physics.* New York: Macmillan, 1928. p. 2.

motivation that might provide an impetus for cheating. One can, of course, dismiss any piece of experimental research if one is willing to postulate the fraudulent involvement of the investigators. Hansel goes on to say, ". . . trickery is a well-established process whereas ESP is not; therefore no single experiment can be conclusive" (p. 18). One would probably not find much support of investigator-fraud being a "well-established process" in any field, and without actual evidence that fraud did in fact take place, the argument will not likely be taken seriously. As Hansel rightly states, "Repetition after repetition of an ESP experiment by independent investigators renders the possibility of deception or error extremely unlikely and thus, if the original result is confirmed, the probability of ESP becomes increasingly likely" (p. 18).

The central part of the book consists of an analysis of the four experiments Hansel regards as providing the best evidence of ESP: the Pearce-Pratt series (1933-34); Pratt-Woodruff (1939); Soal-Goldney, Soal-Bateman (1954); and the Soal-Bowden series with the "telepathic" Welsh boys (1958). It is interesting that he included the last-mentioned series in light of the fact that it has never been regarded by workers in parapsychology as conclusive. In fact, as Hansel himself admits, it has been severely criticized within the field (p. 129). Hence, its inclusion here would seem to be superfluous. In fact, according to Hansel's rationale, it appears that the entire analysis of the four experiments is superfluous inasmuch as he insists (p. 18) that no single experiment can be conclusive, and the only conclusion he reaches from his analysis of each of these experiments is that they are not in themselves conclusive!

Undisturbed by the apparent inconsistency in his logic, Hansel attempts to demonstrate that fraud (on the part of the subjects in the Pearce-Pratt and Soal-Bowden series, and on the part of the experimenters in the Pratt-Woodruff, Soal-Goldney, and Soal-Bateman studies) is a more likely explanation of the results than ESP. In the Pearce-Pratt experiment, he contends, the subject could have left his library cubicle and returned to the building Pratt was in, carefully stationing himself across the hall from Pratt's room (No. 314) in another room (No. 311) that had been "used by students at the time of the experiment" (p. 78). By standing on a chair in

Room 311, according to Hansel's hypothesis, Pearce could have looked down through the transom into Pratt's office and presumably could have viewed the cards during the time Pratt recorded their order. Hansel includes a diagram of the rooms as he saw them during his visit to the Duke Laboratory in 1960. The diagram is conspicuous for its caption: "Not to scale" (p. 77). While at the Duke architect's office, he attempted to obtain the floor plans as well as the "details of the structural alterations . . . together with the dates on which they had been made, and the persons who had asked for them . . . these details were to be forwarded to me, but I never received them. I wrote again, requesting them, but had no reply" (pp. 76-77).

It is puzzling that Hansel was unable to obtain the plans in view of the fact that Pratt was able, at a later time, to procure them without any difficulty or delay. It also seems strange, at least to this reviewer, that Hansel would publish a plan which, in his own estimation (i.e., "not to scale"), was not accurate. Certainly he would agree that it is necessary for the critic to be as judicious in appraising research findings as the investigator must be in carrying out his experiments. It is unfortunate, therefore, that he published his "not to scale" plan; for while it would allow for the possibility of subject-fraud in line with his hypothesis, the correct scale plans, obtained from Duke by Pratt, do not. The crucial door (Room 311) was displaced so far away from the window in Pratt's room, that there can be no doubt as to the inadequacy of Hansel's hypothesis, to say nothing of the accuracy of his plan.

Hansel's analyses of the Pratt-Woodruff and Soal-Goldney experiments contain similar weaknesses, but it is beyond the scope of this review to consider them in the detail required. Suffice it to say that they involve the fraudulent participation of one or more of the investigators. It is interesting to note in passing that Soal was initially one of the most outspoken critics of the early Duke experiments, having carried out a long and careful investigation with the stage telepathist Marion, on the basis of which he concluded that the results were attributable to sensory leakage.⁸

⁸ Soal, S. G. Preliminary studies of a vaudeville telepathist. *Bulletin of the University of London Council for Psychological Investigation*, London, 1937.

It is also of interest that the Board of Review⁴ of the *Journal of Parapsychology*, of whom Hansel speaks favorably (pp. 65-66), said: "The burden of the proof [of fraud on the part of the investigator] rests with the 'critics,'" and it would be better to "ignore the unscientific charges."⁵

Perhaps the most interesting chapter in the book is the one dealing with "Recent Developments in ESP Research." It is perhaps in this chapter that Hansel's lack of scientific acumen is most noticeable. To those familiar with the research findings in parapsychology it will surely come as a shock to read that Schmeidler's sheep-goat effect has not been successfully confirmed (p. 166); that Kahn's IBM-scored experiment cannot be taken seriously because he was an undergraduate (p. 169); and that the consistently high scoring over a number of years by Stepanek, the subject trained by Ryzl in Czechoslovakia, was an "act, for it can hardly be called more than that . . ." (p. 173). These are certainly curious statements to have been made by a writer who claims acquaintance with the research. Hansel acknowledges the confirmation of Stepanek's high-scoring ability by Pratt and Blom, but goes on to say, ". . . when the experiment was conducted by a psychologist, John Beloff, of Edinburgh University, he [Stepanek] failed to display any clairvoyant ability. Beloff supplied his own cards, which were made of plastic" (p. 173). What Hansel omits mentioning is the fact that Stepanek's deviation in the Beloff series was below chance to a highly significant degree. It seems that he must find it difficult to explain away psi-missing effects. The absence of a discussion of experimental precognition leaves another wide gap which is difficult to understand in view of the large amount of published research supporting the precognition hypothesis. This omission is particularly unfortunate since precognition testing techniques automatically rule out the possibility of sensory cues.

A final example of Hansel's lack of objectivity is apparent in the following passage:

⁴ A group of critical psychologists who reviewed all the experimental papers appearing in the *Journal of Parapsychology* during the years 1939 to 1941.

⁵ Sells, Saul B., et al. Review of "Size of Stimulus Symbols in Extrasensory Perception by J. G. Pratt and J. L. Woodruff." *J. Parapsychol.*, 1939, 3, 246.

After returning to England from the Duke . . . Laboratory in 1960 . . . I recalled how I had expected to find laboratories with white-coated researchers turning over packs of Zener cards but had seen little activity of this sort. I also recalled the regular morning meetings at which there was considerable discussion over coffee. I then thought that it would be of interest to know whether more cups of coffee were drunk than runs made with Zener cards (p. 194).

Altogether, the effect of such selections and omissions as the ones noted above unavoidably leads to the conviction that the book has fallen far short of its objective as a "dispassionate and comprehensive" survey of parapsychological research. One is forced to conclude that if parapsychology is ". . . an area marked by misinformation, misguided enthusiasm, and prejudice" this book has not contributed to the clarification of the problems.

CHARLES HONORTON

Institute for Parapsychology
College Station
Durham, N. C. 27708

ERRATUM

On p. 220 of the September, 1966, number of the *Journal*, Dr. Brenio Onetto and Miss Gita H. Elguin are given as the authors of "Psychokinesis in Experimental Tumorigenesis." Miss Elguin alone is the author, and Dr. Onetto's name should be omitted.