

THE SEARCH FOR PSYCHIC POWER: ESP AND PARAPSYCHOLOGY RE-VISITED, by C. E. M. Hansel. Buffalo, NY: Prometheus, 1989. Pp. 308. \$24.95, cloth; \$18.95, paper. ISBN 0-87975-516-4, ISBN 0-87975-533-4 (pbk).

This book is an update of Professor Hansel's previous book *ESP and Parapsychology: A Re-Evaluation*, which itself was a revamping of the original version *ESP, A Scientific Evaluation*. The eighty percent overlap estimated by a reviewer of the previous edition continues here. Whether or not this be considered a measure of progress in this field, this does mean that about one third of the current book can be considered "new" as compared with the original book. The positive aspect of the overlap is that it reflects a continuing interest in the field from a critic who has had perhaps the greatest impact on parapsychology. To his credit, Hansel has followed some of the developments in parapsychology over the years, especially the work with remote viewing, dreams, the ganzfeld, and random number generators. He has also, to my knowledge, attended at least one convention held by the Parapsychological Association (in 1972) although it was my impression at the time that he was not well received (which might explain at least some of the continuing antipathy).

On the negative side, I have to admit being disappointed in the current book, partly because I hoped for something substantially new, not only in the sense of the material it covers, but also in the form of approach it presents. I remember as a doctoral student being inspired by what might now seem to be a naïve notion of finding a high-scoring subject and then demonstrating the performance under Hansel's supervision. (I did find a couple of high-scoring subjects but their performance rapidly declined when I discussed this possibility, although I do not intend to imply any causal relationship.) I still consider cooperation between critics and advocates of parapsychology vital to progress, but at the same time it has become apparent that there are differences in viewpoint which severely hinder active cooperation between certain critics and proponents of the psi hypothesis. Some of these differences become very apparent in the book.

I believe some of these differences may reflect psychology's development as a science during the intervening period. Much greater importance is now given to such previously taboo topics as the role of the self-concept and that of consciousness itself in determining behavior, but it is recognized that these are difficult research areas.

In this respect the content of the book can be considered a monument to an era of psychology whose paradigm was one of perceptual psychophysics and learning theory, an era when repeatable experiments under precisely controlled conditions demonstrated truthful (albeit totally trivial or self-evident) laws. Although the well-known English psychometrist Paul Kline has recently written a critical review of psychology's continued preoccupation with trivialities, I am not sure that this is true of later years. Whatever the case, it is clear that the last twenty years of applied psychology have given rise to the axiom that the more important the phenomena in psychology are, the more they are influenced by subtleties of interpersonal or intrapersonal factors that seldom give rise to precise relationships or final demonstrative experiments. Recently, in reviewing current work in some of the more established research in areas such as cognitive and clinical psychology, I have been surprised by the extent to which the inconsistencies and replication problems exist there. For example, the "Asch effect"—the effect of conformity on perceptual judgment, cited in every psychology textbook as a classic experiment—is today difficult to replicate, and apparently there has never been a replication in France. The same difficulty in obtaining consistent replicable findings is even true of psychobiological areas such as the attention deficit disorder. Nowhere in the present book or its predecessors is there any attempt to put the replication problems of parapsychology in the perspective of those of psychology. The evidence concerning experimenter effects is sufficient to warrant such a discussion even by a skeptic.

In place of the pointless speculation of how fraud might or might not have occurred, I would have welcomed suggestions by Hansel of how some of the above problems might be resolved while maintaining experimental stringency. Some radical and new thinking on joint experimentation instead of the armchair speculations would certainly have been well taken. Instead, it is automatically assumed that if an experiment or a long series of apparently careful work such as that carried out by the Ullman team shows replication difficulties, then this is sufficient in itself to resort to showing that fraud or incompetence *were possible even in the absence of any evidence of such*, and thereby to dismissing it from consideration.

In reviewing the present book it seems unnecessary to repeat what was written by previous reviewers in the parapsychology journals concerning the earlier editions of this book. Regrettably, with the few exceptions detailed below, there has been little attempt to correct or modify the original chapters in the light of even the more

factual errors that have been pointed out during the intervening years. For instance, the fraud hypothesis in the Pratt-Woodruff experiment is presented without any mention of Pratt's 1974 test of differential predictions from Hansel's fraud hypothesis versus that from an alternative ESP hypothesis (the so-called target salience effect). Nor does Hansel correct his erroneous supposition concerning the Maimonides work (which even reappears in the Kurtz *Handbook of Parapsychology*), that the agent could have contact with one of the experimenters, despite the fact that the error has been pointed out on several occasions.

One exception to this, where Hansel does meet some of the points made in the critical reviews of his previous books, concerns the Pearce-Pratt experiment and Appendix B of the book. In the context of the Pearce-Pratt experiment, Hansel replies to Stevenson's earlier rebuttal asserting that the identity or exact location of various rooms is not the point at issue, but rather that it is not impossible that a fraudulent observation of Pratt's target cards *could* have occurred from one of these rooms or from a standing position on a chair in the corridor. He is obviously correct in making this speculation, but it is a speculation that only serves to illustrate the point once made by D. J. West: fraud-proof experiments are impossible since the human ingenuity that invents safeguards can also at a later date think of contrived loopholes, however improbable.

For me, by far the most rewarding part of the book is Appendix B, where Hansel reprints his various exchanges with Honorton, Inglis, and Sargent. Most of these are previously unpublished. It becomes very apparent here more than anywhere in the book that the opponents differ because they have different orientations as to what constitutes evidence of psi, and because the literature of parapsychology is so large that this evidence can be freely chosen on the basis of prior convictions. In historical cases there are of course, with the passage of time, varying accounts and interpretations of the same cases to choose from.

Hansel seeks the final demonstrative experimental proof and focuses on weaknesses in specially chosen straw men while Honorton and Sargent focus on the strengths in general replications and trends in research. What makes the debate intractable is not just the different basis for the evaluation or the fruitless pursuit of historical objectivity, but also the unfortunate derogatory attitudes that surface from both sides. It becomes all too apparent that Hansel regards parapsychologists as a bunch of methodological idiots; and for this reason I suspect many researchers will experience a certain

sense of moral justice in reading the aggressive exposure by Sargent of Hansel's own mistakes, along with a few unfair accusations thrown in. Indeed, it is unfortunate that many of Hansel's otherwise often astute comments can be lost because they are expressed in such an arrogant manner.

The evidence for fraud in parapsychology becomes the central theme in the book. Interestingly, Hansel comments concerning the Soal affair that observers and meticulous control had "little effect on a determined trickster" but does not draw the conclusion that the demand for a one and final fraud-proof ESP experiment is not only an illusory pursuit but can be a dangerously deceptive one. By way of contrast, other critics such as Hyman and Alcock now seem to recognize this and insist on replication as the key to progress. Hansel's viewpoint on this replication issue becomes especially clear in the updated Conclusions section where he comments on the recent (1987) *Behavioral and Brain Science (BBS)* debate. He is unimpressed by any of the attempts cited there which use meta-analysis to show that a level of *general replication* has been achieved and scorns the "hodge podge" of experiments that are included in such overall analyses. This reviewer would, however, have had more sympathy for the author's opinion had he merely stressed the difficulty in generalizing from heterogeneous experiments and had he possibly given some recognition that researchers might be forgiven for (even wrongly) interpreting the results obtained to date as encouraging further work with the psi hypothesis. Because these experiments fall short of the ultimate proof—they were often never designed as such—should not be taken to mean that from the scientific viewpoint they are worthless. It may of course be the case that the consistency shown by them is due to a replication of errors rather than ESP, but the onus is on the critic to show this rather than assume it. Moreover, where Hansel explicitly fails, in my opinion, to fulfil the ideals of skepticism is in supposing that there may have been a sufficient number of nonreported experiments carried out in parapsychology with random number generators to annul the combined probability of results (5.4×10^{-43}). He does this by focusing on the number of experiments that gave significance at the 5% level (or less) and then reasoning that only 98 nonreported nonsignificant experiments would be needed to annul these. His reasoning blatantly ignores the fact that many of the "replications" were significant at far beyond the 5% level, and the figure 98 can only be seen as contrived in this context. This is surely a case where a person's professed skepticism runs amok without reason!

The content of the book that can be considered new concerns Hansel's assessment of the work of Jahn at Princeton, the ganzfeld work of Sargent at Cambridge, and the Schmidt-Morris-Rudolph experiment. Hansel's main complaint concerning the Jahn work is the lack of procedural detail in the published report, but then he bases his entire assessment on one paper (that appearing in the *Proceedings of the IEEE*) when he could have consulted the others, such as the one published in the *SPR* journal. Carl Sargent's work, of course, is easily dispensed with by Hansel on the basis of various hypotheses of fraud, but no acknowledgment is given on this score to the Blackmore or the Parker-Wiklund papers despite the fact that he is known to be acquainted with these. A whole chapter is given to the Schmidt-Morris-Rudolph paper, and Hansel appears impressed by this experiment since the distribution of prerecorded targets to all three researchers is a procedure that constitutes a near fraud-proof experiment. However, the terms for agreement are now changed: a one-off demonstration is no longer simply enough; it must be repeated! (How many times, is unfortunately never specified.) My information is that the first attempt at replication has been unsuccessful, so the status quo is maintained.

As unfairly biased as one might consider parts of Hansel's book, it would be unwise to underestimate its influence. It is still used as a rationale among the scientific establishment for dismissing this field and has even encouraged a wild speculation on whether Rhine or Pearce could have faked the famous complete run of 25 correct guesses.¹ Yet, in contrast to the view presented in the book, it must be apparent to anyone looking at this issue from the outside that the attempt to resolve the debate—and I don't think any unbiased person who has read the *BBS* debate would claim it to be resolved—is a failure, not just of a few parapsychologists, but ironically of the scientific establishment to give any measurable degree of support which would enable the resolution of what (judging by the number of and the distinction of some of the contributors to the *BBS* debate) may well be the most important current issue in psychology. Specifically, it is a failure when public research funding is allocated to see the difference between unrealistic demands that a controversial hypothesis must have prior final proof, and a working hypothesis that has given promising returns in an area where interpersonal factors make research probably no more difficult but certainly no less dif-

¹ By Professor Lennart Sjöberg in an article in a leading Swedish newspaper (*Svenska Dagbladet*, Sept. 1, 1988).

ficult than any area of psychology. Sadly, I think it is a failure to make this distinction that also typifies this book and has encouraged the rise of polarities such as CSICOPS and New Age.

I do not wish to dismiss the book entirely. Witness psychology and the psychology of deception are important spin-offs from parapsychological research. Hansel's first book had a historical value in alerting researchers to these aspects, and many of Hansel's comments written specifically for this edition are considerably milder and worthy of consideration. Also, let's not forget that Hansel has been almost certainly proved right on at least one previous occasion.

I had hoped, however, to understand a little more about the author from reading his book, but I am left with a feeling of puzzlement over what motivates this relentless search of Professor Hansel's which now spans 25 years. Perhaps the new title is revealing. Could it be that the search is not so much for power but for certainty over ESP's existence or nonexistence? Choosing the latter, Hansel's search becomes entrapped in the fraud hypothesis, which, like the ESP hypothesis when terms of absolute evidence are demanded, leads into an endless search because it cannot either be proven or disproven. But the search here is for certainty of the kind one assumes to exist in physics and is one which has not as yet been found in any meaningful field of psychology.

ADRIAN PARKER

Dept. of Psychology
University of Gothenburg
Box 14158
S-400 20 Gothenburg, Sweden

THE ENCYCLOPEDIA OF PARAPSYCHOLOGY AND PSYCHICAL RESEARCH by Arthur S. Berger and Joyce Berger. New York: Paragon House, 1991. Pp. xiv + 554. \$45.00, cloth. L.C. 89-28857. ISBN 1-55778-043-9. 1-55778-358-6 (pbk).

In the past there have been some attempts to compile encyclopedias of psychical research and related topics, such as Nandor Fodor's well-known reference work.¹ Arthur and Joyce Berger's *Encyclopedia of Parapsychology and Psychical Research* is the most recent and up to date of these reference works. The Bergers attempt to present

¹ *Encyclopaedia of Psychic Science*, London: Arthurs Press (1933).