

## REVIEW OF A SKEPTIC'S HANDBOOK OF PARAPSYCHOLOGY

BY JOHN PALMER

The *Skeptic's Handbook*<sup>1</sup> might be described as CSICOP's (Committee for the Scientific Investigation of Claims of the Paranormal) answer to the *Handbook of Parapsychology* (Wolman, 1977), in which a number of leading parapsychologists presented the case for psi in a series of scholarly essays. The CSICOP rebuttal consists of thirty essays, eighteen of which appear to have been previously unpublished. The contributors include most of the prominent critics of parapsychology, past and present. Perhaps the most noticeable omission is an article by David Marks evaluating the SRI remote-viewing experiments.

Four of the essays, those written by Beloff, Stokes, Blackmore, and Hövelmann, appear midway through the book in a section labeled "Parapsychologists Reply." This title is somewhat misleading because all these authors are from the conservative wing of parapsychology, and Blackmore clearly comes across as a conventionalist.<sup>2</sup> Only Beloff endeavors to make a strong case for parapsychology, and his thesis is pretty much restricted to a defense of the medium Eusapia Palladino. All this could leave the erroneous impression that parapsychologists collectively do not have much of a case to make. However, this section is still better than nothing and far preferable to having parapsychology represented by "straw men" from the fringes of the field. Perhaps we should be grateful for small favors.

The longest and perhaps the most important essay in the volume is Ray Hyman's "A Critical Historical Overview of Parapsychology." Hyman reviews a number of prominent cases in the history of parapsychology—for example, Crookes's investigation of Home, the

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<sup>1</sup> Edited by Paul Kurtz, Buffalo, NY: Prometheus, 1985, pp. xxvii + 727, \$34.95, cloth; \$17.95, paper.

<sup>2</sup> Elsewhere (Palmer, 1988), I have defined *conventionalists* as those who believe psi phenomena can be adequately explained and best explained by conventional scientific constructs or "artifacts," *extensionists* as those who believe psi can be adequately explained and best explained by paranormal constructs, and *anomalists* as those who believe that psi cannot yet be adequately explained by either kind of construct.

Creery sisters, Soal's research—where initially promising results were shown to have conventional explanations or for other reasons came to be discounted as strong evidence for paranormality. He uses these as examples to make the point that parapsychological evidence has not been cumulative. He uses this fact, in turn, to argue that scientists should not feel required to take psi experiments seriously or make any effort to provide alternative explanations until the research programs in which the experiments are embedded reach some unspecified standards of replicability and methodological rigor. Hyman claims that this approach allows critics to escape the "false dichotomy" of concluding either that psi is real or that the researchers are incompetent. However, because one of his main points is that competent researchers sometimes act incompetently, I fail to see how the dichotomy is overcome in any nontrivial sense. Moreover, I think a careful examination of Hyman's criticisms of modern research reveals that they sometimes imply gross incompetence or fraud on the part of the investigators, without saying so explicitly. Although Hyman argues plausibly that one should presently suspend judgment about how best to interpret parapsychological data, his other argument, that psi research is not worthy of scientific attention, leaves the contradictory impression that the research can be adequately accounted for by artifacts. By encouraging scientists not to examine the plausibility of the alternative explanations such artifacts necessarily imply, he discourages them from considering the very factors that might persuade them to challenge his characterization of the evidence.

Toward the end of the paper, Hyman summarizes his critique of the ganzfeld research (Hyman, 1985) but fails to adequately address Honorton's (1985) rebuttal points. Fortunately, the latter are mentioned in Gerd Hövelmann's annotated bibliography later in the volume.

C. E. M. Hansel, reprising some of his criticisms of prominent ESP experiments, implicitly or explicitly assumes fraud on the part of one or more participants in most of them. Much of the paper is a close paraphrase of excerpts from his book *ESP and Parapsychology* (Hansel, 1980). He continues to avoid correcting factual errors pointed out in reviews of his previous publications. For instance, he suggests that in one of the Maimonides dream studies an experimenter with access to the percipient was with the agent when the latter opened the target envelope, despite that this was shown not to be the case in a review of his book that appeared in *Contemporary Psychology* (Palmer, 1981). If Hansel was not aware of this review, he

certainly should have been. The one major addition is a critique of one of Sargent's ganzfeld experiments in which the four authors alternated roles as experimenter, agent, and subject. To no one's surprise, Hansel finds numerous ways somebody could have cheated.

Edward and Ellen Girden update the former's critical review of experimental PK research (Girden, 1962). The first part of the article briefly summarizes a number of recent process-oriented PK experiments in a manner reminiscent of my own reviews in the *Advances* series (e.g., Palmer, 1978b). Only occasionally do the Girdens actually find fault with one of the experiments, the most notable example being a study by Braud and Schlitz (1983) involving PK influence of electrodermal activity: the Girdens suggest that the results might be attributable to regression artifact. (This interpretation is explicitly addressed by the researchers and appears to be unjustified.) The discussion section reviews a number of standard criticisms of a more general nature, including a not very lucid analysis of the alleged pitfalls of serving as one's own subject. First the authors say that this is an "acceptable practice" if the results are replicated (p. 139); later they say that such experiments are "not 'provable'" (p. 140).

Next comes a reprint of a short essay by Simon Newcomb, first president of the ASPR. The essay explains why he gradually became disenchanted with psychical research and notes some of the methodological pitfalls in the investigation of mediums and spontaneous case reports.

E. J. Dingwall's contribution to the 1970 Parapsychology Foundation conference was obviously written in a state of pique. Dingwall begins by reviewing the horrors of occultism in the Middle Ages, using this as a background to discuss his objections to parapsychology in the modern era. He focuses in particular on the British SPR, with which he was affiliated for many years. This part of the paper is nothing but malicious, undocumented gossip. His ridiculous insinuation that transparent ESP cards were still being used widely in formal experiments up to the time of his paper does not inspire confidence in the validity of his other accusations. It is sad that the editor chose this paper to represent the work of this distinguished scholar.

Paul Kurtz reviews evidence of fraud in investigations of various mediums or psychics, including the Fox sisters, D. D. Home, Smith and Blackburn, Eusapia Palladino, Uri Geller, Suzie Cottrell, and the poltergeist agent Tina Resch. He takes strong exception to the opinion held by many parapsychologists that if a psychic has been

caught cheating this does not preclude the possibility that other phenomena produced by that psychic may be genuine. In discussing Palladino, he describes this view as "the last vestige of a deep-seated faith, a kind of intransigent will-to-believe in someone or something in spite of evidence to the contrary" (p. 202).

Is it really? Admittedly, common sense might suggest that all of Palladino's phenomena are fraudulent, especially considering their bizarre nature, but common sense also tells us the earth is flat. One of the primary functions of science is to prevent us from being unduly impressed by common sense. Also, all the evidence we have suggests that if paranormal powers exist they are erratic and not fully under conscious control. If Palladino was to perform at the level of consistency expected of her, it was almost necessary that she learn conjuring tricks that could duplicate her genuine feats. Thus, Palladino's use of fraud is compatible with both competing hypotheses.

If Kurtz really has a case against Palladino, why did he not explain how the conjuring tricks uncovered in the 1909-1910 American sittings could account for the specific phenomena of the 1908 Naples sittings on which the claim for Palladino's paranormal abilities primarily rests? With one minor exception (the production of a cool breeze), this endeavor is conspicuously absent. As an alternative, Kurtz chooses to launch an ad hominem attack on Carrington, whose conversion to "belief" in Palladino following his participation in the Naples sittings is hardly surprising. I agree with Kurtz that the mere absence of an adequate conventional explanation does not by itself prove paranormality in the Palladino case or any other case, but those who refuse to dismiss Palladino because she sometimes engaged in fraud are not being irrational.

The next two papers present the written confessions of Margaret Fox and Douglas Blackburn, respectively. These are followed by a paper by John Coover, a Stanford psychologist who performed an important and controversial card-guessing experiment in the early 1900s. The essay uses various examples, most notably Crookes's experiments with Home, to argue that one must adhere to formal laboratory methods if one is to observe psychic phenomena accurately. Fraser Nicol then presents several cases of possible or demonstrable psychic fraud by children, including the recent "mini-Gellers." However, he also expresses the opinion that some psychics might be genuine. He specifically refers to Mrs. Verrall as having "an undoubted psychic gift" (p. 282).

The next two papers discuss the most prominent cases of experimenter fraud in the modern era. Betty Markwick reviews the series

of events leading up to the exposure of apparent fraud by S. G. Soal, and Scott Rogo documents the events surrounding the exposure of W. J. Levy. Rogo's paper is actually more of an attack on the way J. B. Rhine handled the affair. It is written so as to put Rhine in the worst light possible, and it contains a number of statements prejudicial to Rhine that are not documented. Rogo had to retract several major allegations that appeared in an earlier paper on the subject as a result of an investigation by the Parapsychological Association (Rogo, 1985). Whatever private reservations Rhine may or may not have had, the fact remains that he fired Levy the day after he learned of the fraud. Also, the controversy about the publication of the subsequent failures to replicate Levy's work were not about *whether* to publish them, but *how* to publish them.

Returning to yesteryear, Trevor Hall cites an interview with Marianne Foyster, a resident of Borley Rectory at the time it was supposedly haunted, to reinforce the conclusion that the alleged haunting was fraudulent.

In the next two essays, James Randi and Martin Gardner argue that parapsychologists should consult competent conjurers when investigating psychics. Although Randi's paper is marred by his usual arrogant and self-aggrandizing style (e.g., "I am proud to know that many of those in my profession include me with Conjurers of the Third Kind [ 'Master Conjurers' ]," p. 343), both articles make some excellent points. I think parapsychologists are coming increasingly to recognize the need for the kinds of consultation the authors suggest. The problem is finding conjurers who, in addition to being competent, are trustworthy and willing to refrain from using their consultation as a public relations opportunity.

I agree with Gardner that it would be desirable to have a consulting conjurer actually present when psychics are being tested, but I am not persuaded that this is *necessary* in cases where the protocol is fixed and forbids improvisation on the part of the psychic. In particular, I think too much is made of the consultant's presumed inability to suggest ways in which fraud might occur in such circumstances without his actually being there. Surely the consultant must have some things in mind, lest he would not know what to look for. If there are multiple ways a protocol could have been violated, let us put them on the table, one by one, and see if they do in fact apply.

The prime example here, of course, is Diaconis's critique of the Delmore research, which is reprinted later in the volume. I do not think it was fair for him to imply that Delmore cheated in the formal experiments without putting up concrete alternatives for eval-

uation. Of course there is always the possibility that some other conjuring method was used, but this possibility cannot be ruled out entirely even if an expert is present (as critics themselves have maintained in the Palladino case). The critic's ability to come up with a viable alternative explanation is clearly relevant to the evaluation of such an experiment, but the reader should not be asked to just take the critic's word for it that fraud was a likely possibility. Such naked appeals to authority have no place in science.

John Beloff uses the Palladino mediumship to challenge critics to come up with plausible counterexplanations of psychic events. I think he plays into their hands by arguing that scientists not trained in conjuring are sufficiently competent to assess feats such as those of Palladino, but he does make the point that this criticism does not apply to the Feilding Committee.

Douglas Stokes's chapter covers a wide range of topics in a scholarly and balanced manner. They include metaphysical and theoretical issues raised by psi, the kinds of factors that might influence belief in psi, and various methodological objections. Although Stokes acknowledges the validity of some of the latter objections, he attacks others, such as Hansel's criticisms of Schmidt's research designs. He concludes that because of the repeatability problem "it cannot be claimed that psi phenomena have been scientifically demonstrated to exist," but the evidence is such that "psi phenomena are worthy of further study" (p. 418).

Susan Blackmore tells once again her now familiar tale of how she became disenchanted with old-fashioned parapsychology and now favors a "parapsychology without psi." I can agree with her to the extent that I think parapsychology should include efforts to test conventional hypotheses of psi or ostensible psi interactions, but the example she gives, studying the phenomenology of OBEs, suggests something a bit different. As I have argued elsewhere (Palmer, 1978a), to treat out-of-body *experiences* as psychic or even potentially psychic is a category mistake. They are relevant to parapsychology only insofar as they suggest possible paranormal *interactions* between the experiencer and his or her environment. Yet Blackmore explicitly excludes these "rare and disputed occasions in which ESP seems to be involved" (p. 443) as irrelevant to her inquiry. What remains, however, falls squarely in the domain of another field, cognitive psychology, and is not a new area of inquiry. I wonder how cognitive psychologists like the idea of lopping off a chunk of their territory and calling it "parapsychology." I agree that parapsychology should be defined by its subject matter and not by what has been mislabeled

the "psi hypothesis," but its subject matter clearly has to be ostensible *psi interactions*, not altered states of consciousness.

Gerd Hövelmann, with the collaboration of Marcello Truzzi and the late Piet Hein Hoebens, offers a very useful annotated bibliography of 63 prominent "skeptical" books, journals, and journal articles. The reviews are well-balanced, and the authors find ways to praise as well as criticize virtually all the publications cited. A particularly welcome feature is the listing of book reviews and other commentaries stimulated by the publications.

More interesting than the bibliography itself is Kurtz's reaction to it. He comments at the beginning that "this bibliography does not necessarily represent the point of view of the editor . . . or of other skeptics" and "P.H.H. [Hoebens] should not be held responsible for any shortcomings or inadequacies the introductory section and annotations may contain" (p. 449). Why is Kurtz so defensive about some conventionalist writers being criticized here? Such comments were not deemed necessary on the admittedly rare occasions when conventionalists were criticized elsewhere in the volume.

Kurtz's discomfort may also be reflected in the fact that Ray Hyman was commissioned to write a short "Annotation" to Hövelmann's bibliography. Although Hyman finds himself "uncomfortable with some of the evaluations" (p. 491), his rebuttal is actually quite friendly. His main criticism seems to be that Hövelmann failed to appreciate the fact that many of the authors criticized were writing for lay audiences. This is a rather lame argument, especially since many of Hövelmann's criticisms could have been met without making the writings at issue unduly technical. I shudder to think how conventionalists would react if a parapsychologist ever dared to defend a pro-parapsychology book on such grounds.

Christopher Scott makes five critical points in a paper most of which was originally presented at the 1982 PA Convention in Cambridge. First, he argues that falsification of paranormal claims requires refutation of every positive psi experiment ever done. This may be true, strictly speaking, but conventionalists could neutralize the problem to a large extent by empirically confirming their own *general* models for explaining such events, as occurs in other sciences (Palmer, 1986). Also, most parapsychologists now acknowledge that the case *for* psi cannot rest on a single experiment, as Hyman notes in his essay. I agree with Scott's second point, that psi is negatively defined, but he then accuses parapsychologists of not investigating the properties of psi (which he distinguishes from the study of its psychological correlates). Here I think he overstates his case; he ig-

nores, for example, research based on models inspired by quantum mechanics. Third, Scott attacks the notion of the experimenter effect, by which he means "experimenter psi," on the grounds that it undercuts objective observation. This argument, in one respect at least, is fallacious. Because paranormal processes are ordinarily unconscious and not linked to the experimenter's observational or critical faculties, the latter can objectively perform his or her duties even if he or she is the "psi source." Scott is right, however, to the extent that "experimenter psi" does create severe interpretational problems that most parapsychologists have yet to fully appreciate. His fourth argument is that psi conflicts with the corpus of scientific knowledge. This is true (if "conflicts" means "transcends" rather than "refutes," and if "psi" means "paranormal"), but that is no reason to reject it or not to evaluate it the same way as any other scientific knowledge claim (Palmer & Rao, 1987). Finally, he appeals to "the constant crumbling of the evidence in the face of skeptical criticism" (p. 499). The conventionalists indeed have had their successes, but Scott exaggerates the frequency with which they have actually proven their case, as distinct from merely coming up with often implausible ad hoc interpretations. Despite the above disagreements, this paper reinforces my impression of Scott as the most cogent of the hard-line conventionalists.

Paul Kurtz reprints his paper "Is Parapsychology a Science?," which was originally part of a debate with J. B. Rhine sponsored by the Smithsonian. Although he discusses the term *pseudoscience*, he declines to apply it explicitly to parapsychology. He reviews a number of standard critical arguments but puts particular stress on the replication issue. I must agree with him that even statistical replication will have to advance farther beyond the rather small group of parapsychologists if the scientific community as a whole is to be convinced by it, but I fail to see why either replication by or conversion of "skeptics" should be a necessary requirement.

Most of Antony Flew's paper is devoted to a semantic analysis of the difficulties with some of parapsychology's terms, such as *ESP* and *precognition*. He also argues that because of the history of fraud and self-deception in parapsychology and the lack of a plausible theory, we should not accept any parapsychological finding that cannot be replicated.

James Alcock asks "why, after a century or more of formal empirical inquiry, and in the absence of undeniable evidence, the search for the paranormal not only goes on but continues to attract intelligent and capable people who, despite the skepticism and even the derision of some of their more conventional colleagues . . . press



the search with great conviction and great dedication" (p. 541). He argues that, in most cases, the reason is a metaphysical quest for what he calls the "secular soul." He supports his case by an analysis of the history of the field.

It would be futile to deny that many parapsychologists have been and continue to be driven by such concerns although, as Alcock acknowledges, that provides no reason to reject their research contributions. However, I might suggest another, more mundane reason for continuing the quest, namely, that parapsychologists have uncovered provocative anomalies that a century of inquiry by conventionalists has failed to explain away adequately. This hypothesis is not compelling to Alcock, who believes that the conventional interpretations are adequate. However, especially in light of the fact that even his colleague Ray Hyman (Druckman & Swets, 1988) acknowledges the lack of plausible alternative explanations for many psi experiments, this position strikes me as rather hard to defend.

The next section begins with a paper by Persi Diaconis, originally published in *Science*, where he cites his reasons for believing that Serios's psychic photography and the Delmore card-guessing experiments are probably due to magic tricks. As I said earlier, I think Diaconis goes too far in generalizing his own informal observations to formal experiments, both the published Delmore experiments and psi research generally. On the other hand, I think he has been criticized unfairly as implying that the improper statistical evaluation of psi experiments using immediate feedback is endemic in the field. (An example of this criticism appears in the article by Stokes, p. 400.) Diaconis's critique was stimulated by a questionable analysis of feedback without target replacement in the early SRI remote-viewing experiments, and he neither states nor, in my opinion, implies that this error has been widespread.

Martin Gardner critically reviews the attempts of some parapsychologists, particularly Harris Walker, to link psi to quantum mechanics. He fails to uncover any internal difficulties in Walker's theory (except the obvious fact that it is speculative, which is true of any theory before it is tested), and he acknowledges that it could solve some of parapsychology's more intractable conceptual problems, like the independence of paranormal processes from space-time constraints. He complains about Walker's use of Forwald's experiments to provide empirical support for his theory, but his only specific criticism of these experiments is that Forwald worked alone.

Gardner concludes by suggesting that parapsychologists "abandon theory and concentrate on devising experiments that can be replicated by unbelievers" (p. 595). But theory can be valuable in

determining the best way to interpret the very kinds of findings that conventionalists like Gardner find unpersuasive. For instance, if certain of Forwald's results can, in fact, be shown to confirm particular, independently arrived at predictions from a paranormal theory like Walker's, especially if they are effects the importance of which the original investigator could not have anticipated, they would render a paranormal interpretation of the empirical effects much more likely, whether "unbelievers" have replicated them or not.

Gardner frequently uses rhetorical hyperbole in his paper as a substitute for rational argument, and he sets up a smoke screen by attacking an admittedly grandiose paragraph about the possible religious implications of psi that Walker included in one of his semi-popular articles. These stratagems might work with Gardner's many "skeptical" fans who take anything he says at face value, but they will not work with those who approach his writings with a more critical eye. Once one sees through all the bluster, this chapter proves to be one of the weakest in the entire book.

Denys Parsons shares some of his experiences from investigating spontaneous cases and illustrates thereby some of the pitfalls in taking the pertinent testimonials at face value. In one refreshing example, testimony favoring a *conventional* explanation of a case was shown to be embellished.

Charles Akers presents a cogent discussion of the limitations of meta-analysis, particularly with regard to the difficulty in objectively coding experiments for methodological quality. Although I do not necessarily agree that "meta-analysis of the ESP literature is premature" (p. 621), if by that he means of no value, I agree that in the final analysis the pertinent controversies can be resolved only by future research.

Piet Hein Hoebens debunks some well-publicized cases where it was claimed that psychics helped the police solve crimes, although he acknowledges one case that remains anomalous. The paper also provides a useful discussion of factors that should be taken into account in investigating such reports.

Gerd Hövelmann provides a very scholarly and balanced critical review of the use of reports of near-death experiences as evidence for survival. I personally find the near-death evidence extremely weak and have been dismayed to see some survival researchers turn to it for proof of survival. One of the reasons why criticism in this area is more compelling than in other areas of parapsychology is that there is a good body of empirical support for the various conventional alternatives.

In the last essay, Leonard Zusne proposes that believers in the paranormal are persons characterized by afflictions such as magical thinking, reification of subjective thoughts, and a wish for transcendence—persons who never quite made it all the way out of Piaget's prelogical stage of development. A tempting hypothesis, to be sure; the only problem is that Zusne cites no evidence for it. Even in his own research, "no version of [his scale measuring 'world-view'] has shown very significant correlation with belief in the paranormal ..." (p. 697). Undaunted by mere empirical disconfirmation, Zusne concludes: "That a person should choose parapsychology rather than psychology as his or her 'philosophy' may well depend on whether he or she is the kind of person who thinks magically" (p. 690).

This exercise in self-indulgent speculation epitomizes "skepticism" at its worst. It is unfortunate, but perhaps revealing, that it was chosen as the anchor piece of the volume. However, not all of the essays in the *Skeptic's Handbook* can be written off as easily as Zusne's. Parapsychologists justifiably take offense when representatives of CSICOP label the extensionist or even the anomalist position<sup>3</sup> as irrational, but we must not fall into the same trap as the conventionalists do. Their position may be wrong, but it is not irrational, even though it is often *argued* irrationally.

The strongest impression I was left with after reading this book is the immense damage that fraud has done over the years to parapsychology and its credibility. This problem continues to the present day, and we must find better ways to deal with it, ways that are both effective and fair to all concerned.

The conventionalists, however, cannot win the day by citing specific instances of fraud, just as the extensionists cannot win the day by citing specific cases or experiments that have yet to be successfully debunked. Only through the development and testing of generalized theories can either side ultimately prevail. The *Skeptic's Handbook*, in the final analysis, merely reinforces the impression that the conventionalists have yet to come to grips with the better psi experiments, or even many anecdotal reports, in ways that inspire much confidence in critically minded persons that they are susceptible to adequate conventional explanations.

Although the quality of the essays in the *Skeptic's Handbook* is uneven, it is the best single source for the reasoned conventionalist case against parapsychology. It is required if not always pleasant reading for those of us who think otherwise.

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<sup>3</sup> See footnote 2.

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