

A SCEPTICAL EVALUATION OF 'A SKEPTIC'S HANDBOOK'

An Essay Review of editor Paul Kurtz's *A Skeptic's Handbook of Parapsychology*.*

by ADRIAN PARKER

On my bookshelf there are now two handbooks of parapsychology, both of equal size, similar format, and both claiming to be the ultimate, authoritative work on more than a century of scientific inquiry into paranormal phenomena. One of these will of course be known to readers as the 'Wolman Handbook' (1977) and the other is the above named book edited by the 'anti-parapsychologist', Paul Kurtz. Are, then, these two books with their conflicting conclusions as to whether or not paranormal phenomena exist, the concrete symbol of the failure of the scientific method to achieve consensus in this exasperating field? I think not. I would, in fact, assert that the majority of the contributors to the Wolman Handbook could readily agree with most of the contents of the Kurtz Handbook. The reverse would, however, probably not be true, but then little of the contents of the Wolman Handbook, or the work carried out since 1977, is actually to be found in the Kurtz Handbook. The area of contention is then not so much the existing contents of the book but rather the missing contents which ought to have been included in any sceptical assessment aspiring to be a comprehensive and fair one. However, before specifying these areas of agreement and dispute, some background information about the editor and his approach, seems appropriate.

Paul Kurtz is presumably known to most readers as the fervent chairman of what The Committee for the Scientific Investigation of Claims of the Paranormal (CSICOP)—sometimes known disaffectionately as PSI-COP for their dedication in pursuing 'believers and peddlars in the paranormal'. As the J. Edgar Hoover of the organization, Kurtz has, through CSICOP's broadsheet, *The Skeptical Inquirer*, made vehement statements denouncing parapsychologists for their gullibility. But, lest I be guilty of the same grossness, it should in fairness be added that the magazine does occasionally produce some well reasoned critical analyses. Yet there is little doubt that, since giving up its pretension to carry out active investigation into alleged phenomena, CSICOP has become, like occultism, the object of its hate, a social and religious movement (Hansen 1987). Like other American religious movements, CSICOP would also appear to be big business, so much so, if R. A. McConnell (1977) is to be believed, that *The Skeptical Inquirer* gives a profit of a quarter of a million dollars. Kurtz is also president of Prometheus Books who publish the present volume, along with much more extreme anti-occult literature. Despite this, it would seem that Kurtz has a serious and credible side, and, as a philosopher and editor of the *Humanist*, he has acquired the services of many of the high status names in science. It is undoubtedly this other side of Paul Kurtz that the present book expresses and which actually manages to create the aforementioned area of agreement with professional parapsychologists. Indeed, although the CSICOP contributors' associations are lauded in the listings, some 12 of the 27 contributors are members of either the SPR or the Parapsychological Association. This is of course not to say that Kurtz doesn't also enlist the services of some of the tougher

* Prometheus. Buffalo. 1985. £13.95.

anti-parapsychologists such as Martin Gardner and the self-proclaimed 'enfant terrible', James Randi.

It is, nevertheless, a much milder, more reasonable Kurtz that one meets in this book, a Kurtz who doesn't deny the existence of psi but merely says that because the most stringent evidence is required, the case must be regarded as 'not proven'; a Kurtz who presents a formidable knowledge of the field and one who, in the name of fairness, allocates a section of the book to: 'Parapsychologists Reply'. (With the risk of being a bit ungracious towards this apparent reform, the latter gesture does appear somewhat disconcertingly disingenuous. The four defendants in that section do not appear to have seen the case which they are said to be replying to and one of the four is Sue Blackmore who makes the, by now, conditioned response, of wanting to turn parapsychology into a branch of what can be called 'cognitive pathology'—albeit, as she honestly admits, for her own career needs.¹ Gerd Hövelmann's excellent guide to the literature is further supplemented with a niggardly annotation by Ray Hyman.)

What, then, is the common ground? Approximately half the book is an historical review of psychical research and here there has to be some objective agreement. Naturally, with Kurtz as editor, all the skeletons are out of the cupboard and there can hardly be a case of fraud or alleged fraud in the field that isn't highlighted: the Fox sisters, the Creery sisters, Douglas Blackburn, the Cottingly case, Harry Price, Helen Duncan, Soal, Levy, and Geller. They are all found in this volume and in some cases whole chapters are devoted to their exposures and revelations. Occasionally, the presentation is one-sided, such as in the Sir Edmund Hornby case, which is cited in both the chapters by Simon Newcomb and John Coover as a classic case of fallability of human testimony and spontaneous cases in general. Instead it may have been a simple case of prudish Victorian reticence over admitting pre-marital co-habitation. Whilst some of these chapters make interesting reading, depending on your inclinations, personally, I would have preferred the space to be allocated to more current research, especially in view of the fact that the book contains two very strong historical reviews by Ray Hyman and Paul Kurtz himself which fully cover the seamy-side of parapsychology. But what of the other cases that are paraded by Hyman and Kurtz as obvious fraud: Florence Cook, Daniel Home, Margery Crandon, and Eusapia Palladino? John Beloff in his 'reply' takes a stand along with Inglis (1985) and Braude (1986) in attempting to get the critic to take these, and those of other materialising mediums such as 'Eva C', seriously. It is not often that I have the chance of disagreeing with my former mentor, but, on this issue, my sympathies are, on the whole, with the critic. There surely can be no doubt about Eva C, whatever Geley might have proclaimed about fraud 'not occurring but being impossible' by virtue of seeing the phenomena form before his own eyes, and despite what Richet may have protested by the absence of a trapdoor at Villa Carmen. The photograph of the ignominious phantom, Bien Boa (reproduced in Brandon 1983) says more than the proliferation of reports written by the medium's supporters. But, lest there be any doubt about the

¹ Part of her argument is that paranormal phenomena are being 'normalised', that is taken over and explained by other fields. The treatment of psi in the Wolman-Ullman *Handbook of Altered States* (1986) may indicate the reverse.

genuine article being found in the layers of chiffon and mock-up faces, the reader is referred to Rudolf Lambert's report (1954) on the suppressed evidence of how Eva and Madame Bisson fooled Geley and, indeed, how in the end Geley fooled himself. Many of these cases seem in fact to be of greater clinical than parapsychological significance, in showing how mediumship could be a vehicle for repressed Victorian sexuality. In Eva's case, she seems to have had a peculiar predilection for oral and gynaecological examinations along with regurgitation (to say nothing of being photographed in the nude). Others have commented on the sexual aspects of the Palladino case (McBeath 1985) and sexual titillations also appear to have been at the centre of Margery affair (Tietze 1985). In the Margery case, there surely can be no doubt in accepting McDougall's judgement (as a physician) that the materialized hands were almost certainly sliced entrails.

Surely Brain Inglis's attempt to re-introduce the notion of ideoplasms fails completely to explain why the medium's paraphernalia² which looks and behaves like paraphernalia, shouldn't be in reality, paraphernalia! The Palladino case is clearly more difficult to write off, especially when we have, as John Beloff reminds the critics, not only the testimony of hordes of hard-headed academics, but also that of the sceptical conjurors, Feilding, Carrington and Baggally. Kurtz is evidently concerned by the case and devotes 12 pages to discussing it. In the interest of accuracy, it should be mentioned that, although Kurtz is correct in saying that the follow-up tests in America, and again with Feilding in Naples, revealed only crude fraud, he fails to mention that both Feilding and Baggally endorsed Feilding's later report maintaining that such fraud could not explain the effects that they had earlier observed.

But is the critic obliged to explain every single case of mediumship? Might it not be, as Kurtz says, that she simply outsmarted them on earlier occasions by more sophisticated tricks? No, Kurtz recognizes that the case is not that easily disposed of. Carrington went on to be not only Palladino's convert but also claimed she could transfer her powers to him. Because of this Kurtz is forced to use the critic's ultimate escape clause. Carrington himself must have been in on the fraud! As for Feilding, his subsequent career became intimately linked with the Polish telekinetic medium, Stanislaw Tomczyk (whom he married), and I have in my possession a letter from him in 1914 written to the Swedish psychologist, Sydney Alrutz, describing how both he and Baggally had been convinced by her phenomena (although I must add the circumstances sound far from impressive).

Can single historical incidents ever prove anything? Let us take the contemporary case and suppose that the metal-bender, Jean-Pierre Girard, after having impressed the psi-critic Chris Evans with his performance (which was the case), had then gone on to succeed with James Randi (which was not the case). However unlikely it may seem, suppose then Randi recanted his former beliefs and became a campfollower of Girard, would it make a difference? Certainly, it might startle a few people for a few years but I doubt that in the long run a single case, however impressive, would be decisive for the field, unless it could be continually repeated. But what of the other cases of physical mediumship in this

² In Dingwall's (1922) investigation, the controversy concerned whether or not this was a bit of melted wax.

book, Daniel Home and Florence Cook? Kurtz fully accepts the fraud combined with suggestion theory, proposed by Trevor Hall to account for Home's feats. I have little to add to the debate over this, except to note that if, in the famed Ashley Place levitation, the gap between the two balconies was a mere 4 feet or so (and not over 7 feet as alleged) then the whole event becomes much more mundane.

As for Florence Cook and William Crookes, again, surely the photographs themselves must be incontrovertible evidence of some failing on Crookes' part. Personally, I find no difficulty in accepting Hall's theory of an illicit love affair to be plausible especially given his later sudden loss of interest in the field. The Burt Scandal has surely taught us that, merely because a person is an officer and gentleman of a learned society, this does not mean he is incapable of the most blatant deceit. And, surely, the cases of Geley and Richet teach us that there are no limits to the effects of suggestion and self-delusion. On this score Kurtz and some of his co-workers might have given more credit to the SPR and ASPR since many of the cases they cite in this field were resolved, or partially resolved, by these organizations enlisting the services of conjurors and other experts. Moreover, much to the dismay of Brian Inglis, the SPR has been on the whole quite sceptical of the claims of physical mediumship. Instead of acknowledging at least the diversity of opinion among parapsychologists about claims in this area, the book does tend to capitalise on tales of gullibility. We are for instance once again regaled with accounts by James Randi and Martin Gardner on how easy it is for conjurors to deceive academics. Of interest the first time, but by now, after the 1983 Parapsychological Association resolution recommending co-operation between parapsychologists and conjurors, this is surely a case of preaching to the converted.

With respect to the relation between magic and parapsychology, one of the most interesting personalities in the history of the subject is, of course, the late Eric Dingwall. (It is one of my regrets that I never met the man, although we did briefly correspond.) It is then entirely appropriate that this book should contain a chapter by him. Regrettably—even if somewhat predictably—Kurtz had to choose one of Dingwall's yearly recantations in which he rejects everything and virtually everyone in the field and then announces he is finished with it—only to continue as ever. The chapter (reprinted from the 1971 Parapsychology Foundation Conference) then hardly does service to the man, who, if the various obituaries and his own publications are anything to go on, was a much more complicated individual with an enormous personal knowledge of the field, a knowledge which on several occasions convinced him of the genuineness of at least some of the phenomena in question. Evidently, even Dingwall had his shortcomings as a sceptic. Despite having a literary interest in sexual deviations, it apparently never occurred to him that this could be one of the sought-after motives for fraud in the Eva C and Margery cases, which he himself investigated. Clearly some of the chapters in this volume would have benefited from Kurtz's editorial treatment which he unfortunately restricts to barely two pages of a wide ranging introduction to the subject as a whole.

Although as I indicated earlier, there is relatively little in this book which the professional might find objectionable, there are, however, two chapters that are entirely contentious. One of these is the chapter by James Alcock, who, as author

of *Parapsychology: Science or Magic?* and a contributor to a forthcoming series of exchanges in *The Behavioral and Brain Sciences* on parapsychology, has been known to come forward with some constructive criticism. This chapter is disappointing in that it consists mainly (18 pages) of a motivational analysis of why parapsychologists do research in this field, then on the last two pages, he aptly negates everything he has previously written by agreeing that this is totally irrelevant as to the question under discussion, of the existence of paranormal phenomena. Indeed, psychodynamics knows no allegiance to the user's own belief system and has been used in my opinion with equal validity on critics (Tart 1982) and magicians (Reichbart 1978). Furthermore Alcock's thesis, that parapsychologists use research in this field to bolster their own religious beliefs, is itself highly questionable, given the derivation of the Super-ESP hypothesis and/or of the Observational Theory which avoid such connotations.

The other chapter which I think many parapsychologists will experience as containing assertions of doubtful validity, is that by Scott Rogo, which appears to be a diluted version of the one which appeared in *Fate* magazine and brought him into considerable ill-repute. His assertion is that Rhine considered giving Levy leave of absence with a view to later reinstating him. Such a statement, to be taken seriously, should be supported, whereas in this case it would seem to be directly in conflict with what actually happened. McConnell's new book *Parapsychology in Retrospect* (1987) gives an account of the whole affair. However, as far as giving some of the background to the Levy scandal, Rogo's account is both readable and credible.

What further areas of agreement are there between parapsychologists and their critics, above and beyond that this is a field riddled with claims and counter claims? To answer this I refer to the chapter 'Is Parapsychology a Science?' by Kurtz, and to the chapters contributed by Ray Hyman, John Beloff, Douglas Stokes, Christopher Scott, and Charles Akers. These are undoubtedly the most important and constructive chapters in the book. The common ground as it manifests here, is a recognition that no one piece of evidence or single experiment can be conclusive as regards the existence of psi phenomena (John Beloff's notion of a permanent paranormal object on display may, however, be an exception). Clearly, the critic can, with sufficient ingenuity, always find escape-clauses to explain every spontaneous case or experimental finding. In their defence of parapsychology, John Beloff and Douglas Stokes rightly point to 'global impressions' and 'recurrent patterns' which suggest a natural process rather than a methodological anomaly. Further, John Beloff places the ball in the critic's court, insisting that either they can choose to ignore it, being agnostically disposed to its significance, or if they choose to play, then they are subject to the rule of being required to account for ostensible psi phenomena: that is to accept psi as a working hypothesis or provide a counter explanation.

The point actually appears to have been taken! We no longer find the critic, in the form of Hyman and Kurtz, denying the existence of psi but, instead, maintaining (and in doing so leaving it to the parapsychologist to make the next move) that the case is simply not yet proved. Even James Randi (admittedly in a parenthesis), after referring to the work of Robert Jahn at Princeton, confesses, if this holds up 'ESP may have at last been established'.

It is moreover bluntly obvious to today's critic (which is a clear change of

stance) that there is no simple unitary 'normal' explanation for the collective evidence for psi. He must, as Hyman does, resort to a multitude of varying hypothetical explanations ranging from methodological error to experimenter fraud. Hyman and Kurtz, like many parapsychologists, see the only way out of this impasse is to achieve replicability of findings, and in this vein they readily approve the shift in research efforts away from evidential experimentation, to process studies which are aimed at establishing a repeatable experiment in the form of a psi-conducive procedure. It would seem, however, they want to have it both ways: methodology must be as tight as it is in the evidential studies to be convincing or else the replicability achieved may only represent replicability of errors. The outcome is, in a sense, predictable since much of the current research with these psi-conducive techniques has been purely exploratory and is not intended to meet the demands of methodological elegance. With new techniques, it takes years before all the potential sources of error become apparent. Naturally, all the current contenders fail miserably to meet this standard and in particular Hyman and Kurtz cite the weaknesses of remote viewing experimentation (referring to the debate in the 1980 and 1981 issues of *Nature*) and the Ganzfeld work (referring to the debate with Honorton in the 1985 issue of the *Journal of Parapsychology*). Readers of this journal (July and October issues) will recall that Nils Wiklund and I, in our own survey of the Ganzfeld literature, came independently to the same conclusion as Hyman, although, had we felt more certain about Sargent's work, there would have been grounds for optimism. The debate over the claim of the Ganzfeld to be a repeatable ESP experiment has continued into the 1986 issue of the *Journal of Parapsychology* and engaged such notables on research methodology as Robert Rosenthal. Unfortunately, if we discount the work of Sargent, which I am afraid we must do, now that he has left the field after refusing to supply the original data, then the whole bottom drops out of the Ganzfeld work and much of the debate around it becomes vacuous. In the Kurtz Handbook, a chapter is contributed by Hansel in which he exposes the various weaknesses in the design of Sargent's experiments and supplies the reader with the Blackmore-Wiklund hypothesis plus a few more of his own about how cheating *could* have occurred.

Controversy also surrounds the original remote viewing experiments, and this same difficulty in obtaining and rechecking raw data, is also a source of grievance for Christopher Scott as he relates in his chapter (prepared for The SPR Centenary but I believe still valid today). This experience naturally reinforces his scepticism and leads him to conclude there will always be some positive findings that are unassailable even if the paranormal does not exist. Somewhat ironically, his views are echoed by Douglas Stokes in his role as a defender of the psi-hypothesis: it is easy to seek and gain fame as a psi-conducive experimenter since 'most fraudulent research is not exposed, remember, only a few experimenters in the world are able to obtain significance on any regular basis'. Yet Stokes does not finish there, he considers, like Beloff, that the signs of psi are too numerous to be so easily dismissed, and rounds on the critic, saying: 'It cannot be claimed that psi-phenomena have been scientifically demonstrated to exist. It would however be premature to close the book on the issue; psi phenomena are worthy of further study.' Once again, I think John Beloff is right to challenge the critic to prove otherwise. One of the few critics to attempt to do

so is David Marks and a contribution from him is noticeably absent from this book.

The final consensus that appears to be emerging from this book and other related work is the need for joint experimental projects between critics and parapsychologists. There is no reason why good experimental safeguards should not be commensurate with a positive experimental climate in which success will be expected. For the critic, Kurtz finishes off his contribution with a 'challenge to parapsychologists to bring their findings to the most thoroughgoing skeptics they can locate and have them examine their claims of the paranormal under the most stringent test conditions. If parapsychologists can convince skeptics then they will have satisfied an essential criterion of a genuine science: the ability to replicate hypotheses in any and all laboratories and under standard experimental conditions.' On the face of it this sounds like a more refined version of one of Randi's multi-dollar challenges and not the way to court co-operation. It is tempting for parapsychologists to demand that the critic take a course in social skills training prior to experimentation (afterall, if we conducted experiments on creative inspiration in this manner, it would rapidly be concluded it didn't exist!). However, given a tacit acceptance by critics that some human abilities are affected by interpersonal relationships and that it may be necessary to incorporate this as an essential part of the experiment, then such joint projects should be possible. An obvious solution is that the critic should be responsible for safeguards and leave the running of the experiment to the psi-conducive experimenter. I cannot agree with Kurtz's reasoning (in his introduction) that what is needed is neutral researchers. This is an anachronistic analogy from physical science and is contradicted by the vast literature on experimenter effects. Favourable conditions do not appear to be sufficient conditions for psi, but they do appear to be necessary ones. Moreover such hypotheses need not be *ad hoc* or *post hoc* but can be used, and sometimes have been used, in a precise and predictive way.³

It is of course tempting to close the discussion on this positive note with prospects for co-operative projects in view. With the risk of alienating Kurtz and some of his collaborators, it is nevertheless necessary to now focus on some of the omissions and major errors in the book. In doing so, I wish to introduce and support the use of the concept of *progressive scepticism* advocated by John Palmer (1986).

Palmer is careful to distinguish between the sort of scepticism practised by 'conventional theorists' (I have deliberately avoided calling them sceptics in this article and instead merely used the term 'critics') and 'true sceptics'. True scepticism is in fact a double-edged sword which strikes in all directions rather than a laser sword to be used by the conventional theorist at only those concepts which are the special object of his/her pet hates. Far-fetched, contrived hypotheses are thus never subject to the same empirical scrutiny as paranormal

³ True, no simple, robust effects have been found and one may be tempted, like Anthony Flew does in his contribution to this volume, to conclude psi, if it exists, is a statistical anomaly. The same is however true of most phenomena occurring in the context of interpersonal relationships e.g. psychotherapy, hypnosis, and altered states in general. It may reflect no more than the complexity of factors involved. The relation of psi to altered states moreover suggest it is a natural phenomenon rather than an anomaly.

ones. Nor are they assessed in terms of plausibility. For Palmer, the true subject matter or, if we prefer, 'data base', of parapsychology, is not psi, but 'ostensible paranormal events'. Both normal and paranormal explanations for these should then be empirically tested in terms of plausibility. Conventional theorists naturally evoke the law of parsimony here in giving improbable normal explanations precedence, and this is obviously part of the socio-political *status quo* known as scientism, as has recently been described in this context by McClenon (1984). Now I make no secret of the fact that my own scepticism has increased by a degree or more since the Sargent affair, but I would like the reader to consider how many experimenters and subjects actually are included in this select 'handful' as they are depicted in this book. I make no claims to have produced an exhaustive catalogue but if we supplement the 'classical' and debated early work of Rhine, Pratt, Woodruff, with some of the lesser known, but in my opinion quite valid work of Lucien Warner, Margaret Price, Sharp and Clark, Bernard Riess, Margaret Anderson, Whately Carington, G. W. Fisk and some of Tyrrell's, then the list is long. Add to it some of the well controlled later experimentation as for example the series that Pratt and numerous co-workers conducted with Stepanek and the Ullman and Krippner dream research and then consider some of the contemporary work of the Kreitlers, Lendell and William Braud, Musso and Granero, Schmidt, Honorton (his current work with Bessent), Haraldsson and Johnson (with the Defence Mechanism Test), and Robert Jahn.⁴ We also have the evidential experiment of Schmidt, Morris and Rudolph, which would, on a conventional theory, require cheating by at least two of the authors. From the psychical research field, I would obviously add the vast literature on the Piper and Leonard mediumships to my list. The point I wish to make in compiling this lengthy tedious list, is that surely the collective impression of trying to explain all this work on conventional theories *transgresses any law of parsimony*, or, if you will, simple common sense. It is perhaps symptomatic of the critic's case that virtually none of this work is mentioned in the Kurtz handbook and when it is, it is unequivocally misrepresented. The review of supposedly modern research by Edward and Ellen Girden for instance asserts 'there has never been a reported study in which the hypothetical experimenter effect has been tested under controlled conditions' is in direct contradiction with the numerous experiments cited in the well known reviews by Kennedy and Taddonio (1976) and Rhea White (1977). They further complain about the lack of replication of the Schmidt work when a recent meta-analysis by Radin and co-workers (1985) lists 57 references for reports using binary random number generators giving the combined binomial as $p = 10^{-43}$.

A chapter by Mark Hansel devotes much time to discussing the Ullman-Krippner experiment, apparently relying on a short internal report when, had he consulted the full report in the *Journal of Nervous and Mental Diseases*, the ambiguity would have been resolved.⁵ Further examples of ill-founded false scepticism are given by Child (1985) and Palmer (1986). One of Palmer's examples re-appears in this book (the chapter by Diaconis) as an unfortunate example of how a few

⁴ It is perhaps an interesting reflection of the influence of prestige, that although suppositions have been made about experimenter fraud in the Schmidt work, no such surmise has ever been made to my knowledge about Robert Jahn, Dean of The School of Engineering, Princeton University.

⁵ Fully resolved by Akers (see Child 1985).

condescending remarks, mixed with hearsay and suppositions can not only result in a publication in *Science* but also gain a National Science Foundation grant to produce the putdown!

Had Kurtz considered all these points would his conclusion have been different? In one sense it is of no consequence what Kurtz, Hyman, or myself think about past research (except perhaps with respect to their key role in the release of research funds). What is important, is that there is still current research producing ostensible psi phenomena and this provides the opportunity for experimentation with both conventional theories and paranormal theories. This work might one day provide material for *The Skeptics Handbook of Parapsychology*.

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BOOK REVIEWS

THE SEEN AND THE UNSEEN by Andrew MacKenzie. Weidenfeld and Nicolson, London, 1987. 286 pp. £10.95.

Almost single-handed, Andrew MacKenzie strives to keep up the tradition established by Edmund Gurney and enshrined in *Phantasms of the Living*: the transformation of anecdote into scientific narrative. If eyebrows are raised at the term 'scientific', it needs to be remembered that zoologists work along the same lines. Although they may occasionally be deceived or deceive themselves, nobody rejects their findings out of hand on that account.