BOOK REVIEWS

PSYCHOLOGY AND THE PARANORMAL: EXPLORING ANOMALOUS EXPERIENCE

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David F. Marks is a retired academic psychologist who has been a prominent sceptic regarding parapsychology. In the late 1970s he co-wrote *The Psychology of the Psychic* with Richard Kammann, which in part examined the paranormal claims of Uri Geller and the mentalist Kreskin. The book also contained two chapters credited to Marks, which criticized the remote viewing experiments of Russell Targ and Harold Puthoff, and two credited to Kammann covering various other topics, including coincidences and the roots of paranormal beliefs.

In 2000, after Kammann's death, Marks produced a second edition. Again much of the book dealt with Geller, but there was also wider coverage of research, with extra material on remote viewing and the addition of chapters on Ganzfeld studies, psychic staring and psychic pets. His new book, *Psychology and the Paranormal*, represents a further shift in emphasis towards experimental parapsychology. Although there is still a discussion of coincidences in the chapter on synchronicity, nearly all the material on Geller has gone, and the coverage of experimental work has been extended to include precognition and psychokinesis.

The book's target audience is specified in the preface: "This book is geared towards the needs of teachers, researchers and students of anomalistic psychology, general psychology, parapsychology and consciousness, all exciting, fun areas where nothing can be taken for granted. The book covers topics that many students want to learn, but traditional course structures and stuffy, 'old-school' professors resist putting them in the curriculum" (p. XVII). In keeping with its intended function as a textbook, it provides a series of questions at the end of each chapter to test the reader's comprehension. There is also a pair of "belief barometers" for each topic—one for the author to indicate his degree of belief in the phenomenon, and another for the reader.

The scope of the book is ambitious. In the preface, Marks states his intention: "to cut a path through the vast, tangled jungle of publications about psi with a machete that is sharp and decisive" (p. XI). In the concluding chapter, he claims to have provided "a critical examination of the best scientific evidence for psi from laboratory investigations over the last half-century" (p. 292). What is more, he claims at the outset that his mind is completely open, that he is looking at the evidence with new eyes and that he is eagerly wondering where his investigation will lead. His initial "belief barometer" is set impartially at precisely 50%. (It is fair to note that similar statements about initial open-mindedness were made in the prefaces of both editions of *The Psychology of the Psychic.*)

Marks also explains that, in order to establish dialogue, there are contributions from six psi advocates—Daryl Bem, Stanley Krippner, Adrian Parker, Harold Puthoff, Dean Radin and Rupert Sheldrake—and one ex-advocate—Susan Blackmore. The contributions from the "advocates" are placed after the relevant sections, and Marks responds to them in the concluding chapter of the book.

The book is organized as follows. There is an initial "Basic Principles and Processes" section, in which Marks explains what he means by the paranormal and discusses people's belief in it—its prevalence, its causes and the psychological factors that may predispose to it. After this comes the main section, entitled "Subjective Anomalous Experience Claimed to be Paranormal." It begins with a chapter on synchronicity and then moves on to experimental evidence from studies on remote viewing, psychic staring, Ganzfeld extrasensory perception (ESP), precognition, dream ESP and psychokinesis. Then there is an interlude, comprising three chapters on hypnosis, out-of-body and near-death experiences and (psychological) homeostasis. Finally there is a short chapter of conclusions.

An up-to-date, comprehensive and accurate survey of the scientific evidence about psi, written from a sceptical but open-minded viewpoint, would be a very welcome addition to the literature. But how well does *Psychology and the Paranormal* fit the bill?

One thing to note is that although the main body of the text runs to more than 300 pages, the parapsychological evidence is somewhat crowded out by other material. The introductory section accounts for over 50 pages, the chapter on synchronicity for another 30, and the third section—on topics "usually not … claimed to be paranormal," according to Marks—for nearly 90 more. (The description of this section is apt enough for hypnosis, and while out-of-body and near-death experiences are often claimed to be paranormal, they have rarely been studied in the laboratory. As for homeostasis—a psychological theory proposed recently by Marks himself—it is difficult to see why it is included in the book at all).

There are fewer than 100 pages of detailed discussion of the experimental evidence for psi—and that includes the invited contributions by parapsychologists and responses to them. Considering that the discussion ranges over six different topics, the coverage of each is necessarily limited in its depth. Nor does the book include all the topics that might have been expected to be covered.

For example, the study of presentiment, based on physiological measurements —a popular experimental technique in recent years—is omitted, as is the Global Consciousness Project. This is despite the fact that both have been claimed to provide strong statistical evidence of psi effects.

To assess the quality of the discussion of the experimental evidence, it is necessary to look at the details. Those are discussed in more depth below for three of the topics covered: Ganzfeld studies, Bem's "Feeling the Future" experiments and psychokinesis. But as a general comment, the treatment tends to lean heavily—perhaps partly because of a lack of space—either on published meta-analyzes or on a few selected criticisms of the experiments or the experimenters.

In my opinion, Marks does not succeed in being open-minded or balanced in his discussions, but tends to display a bias against the possibility of psi in the laboratory. And although the comments by parapsychologists improve the overall balance, the format in which they are presented is not entirely helpful. It tends to give the book the flavour of a "sceptic versus proponent" dialogue. I suspect that as a result, Marks has felt less need to be balanced in his own comments. In some cases where an inaccuracy or a misleading statement has been pointed out in these comments, it has been allowed to stand in the relevant section, and responded to only—if at all—in the concluding chapter. And of course this is a dialogue in which Marks has the last word.

The exception to the anti-psi tone is the chapter on synchronicity, where Marks seems to throw sceptical caution to the wind. In *The Psychology of the Psychic*, a chapter on coincidences, originally credited to Richard Kammann, characterized a belief in paranormal coincidences as "Koestler's fallacy." Kammann argued that this was the result of a failure to take account of the large number of pairs of events in everyday life which could potentially produce seemingly unlikely coincidences. In the present book Marks repeats this argument, but he also describes a striking personal experience and estimates the probability of its chance occurrence at 10^{-18}. Later, he multiplies this together with estimated probabilities for four further personal experiences, to obtain a combined probability of 3×10^{-58}. Estimating the probability of coincidences occurring by chance is notoriously difficult, and there is plenty of scope to question these figures. But they convince Marks that Kammann's argument fails, because the odds against this set of coincidences are so astronomical. The result is that Marks ends with a high degree of belief—75%—in the reality of paranormal coincidence.[1]

Marks's overall conclusions about the existence of psi are mixed. He claims that "laboratory ESP is nothing more than a myth" (p. 293). His "belief barometer," which was initialized at 50% in the preface, has plummeted to 10^{-11}—as far

[1]Details of the five coincidences experienced by Marks are available on his website, at https://davidf-marks.com/2019/02/16/reality-or-illusion/.

as experimental evidence of psi is concerned—by the end of the book (though it is difficult to understand how such a low figure could have been obtained by combining the given range of probabilities for different psi phenomena, the largest of which is 3.5×10^{-5}, for psychokinesis). However, thanks to his personal experiences, Marks is much more sympathetic towards spontaneous psi. In fact, in the title for the final chapter he seems to acknowledge psi as real, albeit capricious: "a spontaneous process that cannot be summoned at will in a laboratory experiment." Though admittedly he is more cautious elsewhere, as reflected in the running head: "The psi hypothesis is neither confirmed nor disconfirmed."

This is not a unique position to hold, but it seems paradoxical to me. If the possibility of spontaneous psi is accepted—and if its existence is even judged to be more likely than not—how can anyone have confidence that laboratory psi is entirely mythical? How can anyone argue against it on the basis that it would require a revolutionary correction to "… the whole of physics and science as we know it" (p. 296), when exactly the same is true of spontaneous psi? And if psi were a real but capricious phenomenon, would not the difficulty in replicating experimental results be expected?

Marks seems to be on particularly shaky ground when he discusses work by Wallace Scherer (1948), a student of J. B. Rhine. Scherer found highly significant results when subjects were free to follow an "impulse or hunch" but not when they were told to use "conscious deliberation." This may be evidence that psi is a spontaneous phenomenon, but as it comes from a laboratory experiment it is hardly consistent with the conclusion that laboratory psi is mythical! (It is also worth noting that Scherer used a forced-choice protocol. Marks says that the history of parapsychology could have been radically different if only Rhine had listened to Scherer. However, one way in which parapsychology has evolved radically since that time is that forced-choice protocols with conscious choices have largely been superseded by free-response protocols and studies without "conscious deliberation").

Detailed comments

1. Ganzfeld Studies

Ganzfeld studies have often been claimed to provide strong statistical evidence for ESP. They were previously discussed by Marks (2000). His discussion then highlighted the non-significant findings of the meta-analysis by Milton and Wiseman (1999) and concluded that previous claims had been incorrect because of methodological problems.

Marks's conclusion in this book is essentially unchanged, but his discussion necessarily takes a different line, because the meta-analysis of Milton and Wiseman has since been succeeded by another which, as earlier ones had, produced a highly significant result (Storm et al., 2010, subsequently updated to 2018, as Storm & Tressoldi, 2020). This time Marks has two main criticisms of the Ganzfeld studies. Firstly, he discusses a series of experiments by Adrian Parker and coworkers and argues that the apparently impressive data are the result of "subjective validation." And secondly, he draws attention to a series of experiments by Carl Sargent and coworkers published in the 1970s and 1980s, in which fraud was later alleged.

Marks recounts Parker's summary of experimental results, and then comments that the "results are astonishing," drawing particular attention to their consistency and to the high hit rates (corresponding to a *p*-value of 0.0012 collectively). But then he bursts the bubble: "Six years later, Parker's research team make a complete about-face and describe how remarkable [Ganzfeld] results can be obtained by that perennial spoiler of psi studies—subjective validation." Clearly, if subjective validation had proved to be responsible for the results, that would be very relevant, and would sound an important note of caution about the credibility of experimental parapsychology in general. But is it true?

In fact, the later conclusions of the team concerning subjective validation related only to a single aspect of the studies—an innovative protocol that allowed the impressions of the percipient to be synchronized with the target video for comparison. They did indeed conclude that the "remarkable correspondences" between the two could have been the result of subjective judgments. But that conclusion did not apply to their other results, including the hit rates and the resulting *p*-value (Westerlund et al., 2006). Although Marks's description of the results occupies about a page and a half of the book, it does not include so much as a single reference to the real-time correspondences found in these studies (the extract on p. 123 concerns an earlier study from Edinburgh). Yet he gives the reader the impression that these astonishing results had been disclaimed in toto by the authors.

Marks's second line of argument is even more questionable. It stems from a visit paid by Susan Blackmore in 1979 to the laboratory of Carl Sargent in Cambridge. Blackmore reported that she had observed "errors" in the conduct of the Ganzfeld studies, though, at the time, she was unsure about whether they were the result of cheating or had happened accidentally. Her report was not published until 1987. Later, she became convinced that cheating had taken place, as confirmed by her contribution to this book.

Marks notes that Blackmore has complained that Daryl Bem included Sargent's experiments in "his meta-analysis" of Ganzfeld studies (i.e. Bem & Honorton, 1994), and that he did not cite the source of the data or refer to her published criticisms of Sargent's work. Blackmore also claimed that when she later met Bem and asked him about this, "He simply said it did not matter" (Blackmore, 2018). Marks takes this foundation and builds on it. He accuses

Charles Honorton too of ignoring Blackmore's allegations about Sargent in a meta-analysis done eight years after she published them.

Marks writes that of five meta-analzses of Ganzfeld studies, the four that showed significant results "included the highly contentious studies conducted by Carl Sargent" (p. 131). In fact only two of them did. Some were indeed included in Honorton's (1985) meta-analysis, but Marks miscopies the date of its publication. In fact, it appeared before Blackmore's report was published, not afterwards. In addition, as Bem notes in his response, it would still produce highly significant results if Sargent's work were excluded. Two other studies by Sargent were included in a small group from the 1980s meta-analysed by Storm and Ertel (2001). Bem and Honorton (1994) did indeed summarize the results previously presented by Honorton in a four-page review of previous work, but their own meta-analysis was new and did not contain any studies by Sargent. Nor did the most recent meta-analysis, by Storm et al. (2010), include any of Sargent's work.

While the Blackmore-Sargent controversy may be of historical importance, it relates to a period when Ganzfeld experiments are in any case acknowledged to have suffered from methodological defects, which were later corrected, partly as a result of the dialogue between Honorton and the sceptic Ray Hyman in the 1980s. Blackmore's allegations, which were published more than 30 years ago, are no longer relevant to the debate about the evidence for psi.

2. "Feeling the Future"

Bem (2011) published the results of a series of experiments on precognition, under the title "Feeling the Future." He used standard protocols from experimental psychology which were modified so that, for example, rather than a stimulus preceding a measured response, the measurement was made before the stimulus was applied. These studies appeared to show strong statistical evidence for precognition. As Bem was a respected academic researcher and his report was published in a prestigious psychology journal, it gained more attention than most parapsychology papers, and also attracted more criticism from sceptical commentators.

After briefly summarising the two sides of the argument over Bem's work, Marks concentrates on two published criticisms of his experiments:

(1) A paper by LeBel and Peters (2011). In fact these authors were critics of Bem's work only in a limited sense, because they began by praising "his experimental rigor and the clarity with which he reports procedures and analyzes, which generally exceed the standards of [modal research practice] in empirical psychology" (p. 371). Their reasoning was that because his paper was of "objective high quality," yet they found his results unbelievable ("fantastic"), there must be something fundamentally wrong with the accepted methods used in experimental psychology. As a piece of logic, that is obviously open to question.

But it is also difficult to see much applicability to Bem's work in the specific concerns they raised about research practice in psychology.

Firstly, they were concerned about the tendency of replications to be "conceptual" rather than exact, which gives opportunities for failures to be explained away rather than faced. Obviously that is a reasonable general concern, but it hardly applies to Bem's experiments, because all except one of those presented in the paper produced significant results. Secondly, they cautioned against "insufficient attention to verifying the soundness of measurement and experimental procedures," but gave no examples of this on Bem's part. It is difficult to find scope for any, as most of his experiments concerned a simply binary choice, and the question of adequate randomization was discussed at some length in his paper. Thirdly, they warned against inappropriate null hypothesis significance testing. But the primary reason for their concern was that the null hypothesis "will almost always be false," and they acknowledged that "it might be argued" that it was "theoretically appropriate" to Bem's experiments (p. 374).

Marks outlines the general concerns raised by LeBel and Peters but, like them, he does not explain specifically how they can apply to Bem's work.

(2) Part of a blog post about Bem's work by Ulrich Schimmack (2018a) is included, with relatively minor revisions, as the final section of Marks's discussion. Schimmack went to the commendable effort of obtaining and reanalysing Bem's raw data, and obtained some interesting results. Most notably, he found a marked decline effect, with early participants in each experiment producing stronger results than later participants. On this basis, Schimmack suggested as a possibility that Bem had run a larger number of pilot experiments, and completed only the successful ones, discarding the others. This, he concluded, was the likeliest explanation of the results.

In support of this idea, Schimmack quoted comments attributed to Bem in an online article, suggesting that some pilot data were discarded. Schimmack also said that in his 2011 paper, Bem had posed the question of how much pilot exploration should be reported, but had not answered it. In fact, as Bem points out in his response, he did answer the question, by giving details of the one unsuccessful experiment in the series that was not reported in that paper (but which had already been reported elsewhere). Bem might have added that after the publication of the original blog post he had explicitly denied discarding failed experiments in emails which Schimmack subsequently reprinted on his blog (Schimmack, 2018b). Likewise, in Bem's response to Marks, he denies "peeking" at the data during the course of the experiments in the way that had been suggested. In view of this, it might have been better to amend Schimmack's original words to make it clear that Bem had subsequently denied his suggestion. But instead Marks uses his own response in the final chapter to imply that despite his denials, Bem did indeed do what Schimmack suggested.

Bem denies discarding exploratory data. But even if he did so, is Schimmack's scenario—in which unsuccessful pilot studies are discarded and successful ones are completed—capable of producing results like Bem's? It is very difficult to see how (Phillips, 2021). Firstly, as Schimmack himself acknowledged, although the later results from each experiment tend to be weaker than the earlier ones, the later results remain collectively significant when combined together. Something more than just the discarding of unsuccessful pilot experiments would be required to explain this. Apparently Schimmack's idea was that there was a further stage of the selection process, in which a proportion of the completed studies were also discarded. But even in this scenario, simple model calculations suggest that—in order for this process alone to account for the results Bem obtained—a very large proportion of the experimental data would have to be discarded. Selection on that scale could not be done innocently. It would amount to outright scientific fraud. But if someone were going to commit scientific fraud, why would they do so in such an unnecessarily elaborate and needlessly laborious way?

3. Psychokinesis

After brief comments on early experiments with dice, Marks moves on to discuss experiments on psychokinesis using random number generators. The discussion is based almost entirely on a single meta-analysis by Bösch et al. (2006), together with criticisms of it by Radin et al. (2006). Marks copies two and a half pages of tables and a figure from the meta-analysis and summarizes some of the findings. He emphasizes the extreme heterogeneity of the effect sizes measured in different studies, and the fact that in those with the largest "sample sizes" the direction of the effect is contrary to intention. He then considers in more detail how the effect size varies with different "moderator variables," drawing particular attention to reversals of its direction. Many readers will probably get the impression from the discussion that there is no rhyme or reason to the observed variations.

Unfortunately, there is a huge pitfall in trying to use the meta-analysis in this way. As Marks explains (in a footnote), the method of analysis used meant that studies with larger "sample sizes" were weighted more heavily. But what he does not explain in the text (though observant readers may spot it in one of the tables and a figure) is that "sample size" means the total number of random bits of information generated in each study. This reflects not only the number of participants and the time spent by each, but also the rate per second at which the random bits were produced. In this sense, a "large" study may not be large in terms of time or effort. It may simply be a study that used a high bit rate per second. Many of the experiments included came from the Princeton Engineering Anomalies Research (PEAR) laboratory, and in three of them the bit rate was 10,000 times larger than the standard one used.

In terms of the number of bits, Radin et al. (2006) estimated that these three studies represented at least 99.7% of the data considered in the meta-analysis, and were weighted accordingly. What is more, in these studies the direction of the measured effect was found to be opposite to intention. As a result, there is often a straightforward explanation for the apparently puzzling variations in the effect size and reversals of its direction that are discussed by Marks. Often, they simply reflect which of the three "large" studies are in or out of any particular group of studies considered.

This had been explained by the authors of the original paper. In fact, they included an explicit warning about the interpretation of their results: "it is important not to place too much emphasis on the apparent reversal of direction in any subsample that includes one or more of the three largest studies" (p. 508). The difficulty was discussed further by Radin et al. in their response to the original paper. Although the reversal of direction at the highest bit rate may be surprising, there is no reason to assume a priori that if psychokinesis exists there will be a fixed effect size per bit, regardless of the bit rate, as Marks seems to expect. Given that the studies from the PEAR lab alone used four different bit rates (Dobyns, 2015, pp. 222, 224), this potentially gives rise to a confounding factor which makes interpretation of the data difficult. It is a difficulty that Marks's readers—unlike those of the original debate in 2006—are left unaware of.

Marks says he considers the paper by Bösch et al. (2006) to be the only one of any genuine merit published on the subject of psychokinesis this century. He adds that nothing published more recently has given him the impression that the impasse reached in 2006 will ever change. Apparently other material was rejected because it had appeared in books rather than journals, which are not peer-reviewed, giving the authors "free rein to express their personal opinions independently of the evidence" (p. 193). This seems a strange objection, considering Marks's heavy reliance on a sceptical blog post in Chapter 7. But as a result, the coverage of this extensive field is essentially limited to a summary of the results of a single rather problematical meta-analysis.

Conclusions

This book has an ambitious scope and commendable aims. No doubt opinions will differ about how objective the coverage of the evidence is. But in terms of depth and accuracy it falls short, and it seems unlikely the mixed conclusions about the existence of psi will convince many. It would be difficult to recommend it as a textbook for students of parapsychology.

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