

psi or not" (p. 180), but rather that we should employ "psychological criteria of significance." She does note that ultimately, in order to qualify as a science, parapsychology must "generate objective data" and be grounded in external phenomena. She concludes that at the end of the process of exploring the depths of the soul there is a place for "objective and public verification" of hypotheses.

The 1984 convention of the Parapsychological Association saw a resurgence of calls for the abandonment or substantial modification of traditional scientific methods in parapsychology. Such an abandonment or modification might entail serious political damage and retard the acceptance of parapsychology as a science. Also, as no new alternative method has been clearly articulated, such abandonment could result in a retardation of progress in the field or at the very least a loss of consensus as to what has or has not been established. It is true that certain very important areas of inquiry, such as the cause and nature of conscious experience, have proved largely refractory to traditional methods of scientific inquiry. On the other hand, the traditional methodology of science has resulted in unprecedented progress and consensus in other, often more physical, arenas. For that reason, its abandonment should not be hastily considered. Perhaps by definition any "science" of parapsychology will continue to adhere to that methodology.

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DEVIAANT SCIENCE: THE CASE OF PARAPSYCHOLOGY by James McClenon. Philadelphia: University of Pennsylvania Press, 1984. Pp. xiii + 282. \$14.95, paper.

"The science of psi is not an island."<sup>1</sup> What we do and do not do in research, in publication, and in policy-making is intricately interwoven with the ever-changing currents of our culture. Like other specialists, we often are so engrossed in what is happening in our own little boat that we fail to notice the mighty currents that carry it. And currents are indeed flowing our way: radical methodological innovations are being advanced, psychologists are awakening to epistemological and ontological issues, insights are arising from the history

<sup>1</sup> See J. B. Rhine (1953), *New World of the Mind*, New York: William Sloane.

of science, and we see what Houston-Smith calls the "postmodern mind" in the humanities. *Deviant Science* sprouts from the emergence of an empirically based sociology of science; applying the findings to the understanding of parapsychology is the basic content of McClenon's book.

The sociology of science remained in a stage of speculative, essay writing activity until lately when young Turks decided to sneak into laboratories and do research on the actual processes involved in science. They were not content with the confessed ideals, but looked behind the scene and discovered extra-scientific factors that enter into the process of reaching a consensus and "establishing scientific truth." As in anything new, there was some stumbling and some excess; nevertheless, important contributions were made. Special attention has been given to the ways and means of how new borderline areas are assessed and either accepted and supported, or rejected and starved out.<sup>2</sup> Of course, there was no secret that we parapsychologists were rejected and mainly rejected for extra-scientific reasons. Although our knowledge about this was once impressionistic and speculative, sociologists now offer a version that is grounded in research data. For those who wish to gain further insight and deeper understanding, I recommend reading some basic books on the new sociology of science. One excellent choice is H. M. Collins's latest book,<sup>3</sup> which is one of the rare social science publications to have been reviewed in *Science* (December 13, 1985, p. 1267) despite Collins's obvious sin of being fair to parapsychology.

McClenon started his project as a doctoral student gathering data for his dissertation; this later expanded to a very major effort. The amount of work he applied to data gathering compares favorably with what historians Mauskopf and McVaugh put into the writing of their book, *The Elusive Science*.<sup>4</sup> Naturally, these professors were superior in intellectual maturity, but, in one important respect, McClenon might have an edge on them. Mauskopf and McVaugh appeared as horsemen—riding through parapsychology and looking down at it from atop their mounts, that is, from the mainstream of their history of science background. McClenon, on the other hand, infiltrated our ranks and those of our detractors (e.g., Paul Kurtz's "Committee for the Scientific Investigation of Claims of the Paranormal") humbly on foot:

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<sup>2</sup> See H. M. Collins (1985), *Changing Order: Replication and Induction in Scientific Practice*, Beverly Hills, CA: Sage.

<sup>3</sup> *Ibid.*

<sup>4</sup> See S. H. Mauskopf & M. McVaugh (1981), *The Elusive Science: Origins of Experimental Psychical Research*, Baltimore: Johns Hopkins University Press.

Participant-observation study was conducted by visiting each center (i.e., 13 parapsychological research centers in the U.S.) and staying at least a week at larger ones, by volunteering as a subject in experiments, by fraternizing with parapsychologists, and by attending parapsychological conferences sponsored by the PA and its regional affiliate. . . . (p. 36)

Altogether, he interviewed 102 of us and looked into many of our files, including those of Ted Rockwell, the PA, and the American Association for the Advancement of Science (AAAS). The culmination of his work was an excellent survey of American elite scientists—those who govern the AAAS and represent its sections. McClenon's survey was partially described in this *Journal*<sup>5</sup> and is well worked into his current book.

*Deviant Science* begins with an overview. We learn that authorities such as Thomas Kuhn and Karl Popper—still treated as demigods in some circles—are badly outdated. The deficiencies of their theories are described, as are the systems that replace them. We might think twice before using the “paradigm shift” or the “falsification” doctrine again.

McClenon presents the criteria by which deviant sciences, like ours, are judged. The most important of these criteria are competence of practitioners and conformance with scientific ideology. We have our cardinal sins against the ideological side, mainly its ontological aspects:

Scientism can be defined as the body of ideas used by scientists to legitimate their practices. By necessity, this ideology is somewhat covert, implicit, and latent since science is assumed to be “free from presuppositions” except for the supposition that the rules of logic and method are valid. (p. 2)

He quotes Voegelin's early description of scientism:

(1) the assumption that the mathematized science of natural phenomena is a model science to which all other sciences ought to conform; (2) that all realms of being are accessible to the methods of the sciences of phenomena; and (3) that all reality which is not accessible to sciences of phenomena is either irrelevant or, in the more radical form of the dogma, illusionary. (p. 27)

We are further shown that mechanistic-physicalistic assumptions are the basic grounding of scientistic philosophy. They were adapted since the early developments and triumphs of the “hard” sciences in various modified forms. Though on the surface the epistemological and meth-

<sup>5</sup> See J. McClenon (1982), “A survey of elite scientists: Their attitudes toward ESP and parapsychology,” *Journal of Parapsychology*, 46, 127–152.

odological parts of scientism do not appear obviously to clash with the paranormal, the ontological does.

Over the years we have learned to believe that scientists usually have a tacit impression that psi has outrageous characteristics, such as precognitive ESP occurring over long time spans and independent of distance and that PK can occur in a retroactive mode. Such things are seen by outsiders as misfits in the hard won, unified view of the physicalistic-mechanistic universe, and that happens in spite of the numerous theoretical attempts by parapsychologists to minimize the "otherness" of psi. Even Einstein, who initially accepted ESP, backed off after claims of precognition were thrust at him. Is our assumption about what other scientists think really correct? McClenon compares Allison's poll of PA members with his own poll of elite scientists:

While the PA members evaluated the arguments "Parapsychology threatens the established mechanistic world view . . ." and "Scientists are simply unfamiliar with the present evidence . . ." as most important, the elite scientific group considers these arguments as least important. (p. 151)

McClenon's analyses of other responses clearly show that elite scientists are nearly totally unfamiliar with our work. (Could it be that they consider it an unimportant basis for judgment?) Some of McClenon's data and Collins's analyses of "network theory," which relates to the issues of scientism, convinced me that we also cannot take at face value their second answer (i.e., that parapsychology is not a threat to the mechanistic world view). Parapsychology as an institution might be too impotent to pose a threat, but the previously mentioned "outrageous characteristics" of psi, if asserted within the scientific community, would.

While many parapsychologists, liberal psychologists such as Maslow, and philosophers like Houston-Smith considered scientism as being too narrow and crippling both to research and to fully human life, the author has this to say:

Rather than viewing scientism as a kind of cancerous outgrowth of science that restricts the development of "true" human knowledge, this present study regards it as an inherent aspect of science. Because science is an endeavor undertaken by a community of practitioners, scientism is a necessity. (p. 27)

The stance of scientists, McClenon says, is justified because of a need to demarcate science from other activities, maintaining its coherence and set of priorities for using its resources.

McClenon then states that scientists assert their scientific beliefs by not so glorious means: "Proposition 2. *Scientists engage in a rhetorical and political process by labeling certain belief systems as deviant*" (p. 33). What do we do in response to that? In olden times, that was clear: stand tall, as Gardner Murphy and J. B. Rhine did even under the most pressing circumstances. "Proposition 3. *Scientists labeled as deviant can create a stable relationship with orthodox science by increasing their adherence to scientific orientation, and will tend to do so in response to being labeled as deviant*" (p. 35). Anyone has a right to assert his or her integrity in expressing a philosophical orientation, whatever it happens to be; but changing it to gain prestige and a better paycheck is another matter. McClenon says that he has observed much scientism and some materialism among experimental parapsychologists; he stops before naming those who changed coat, which is in keeping with the sociological tradition of steering clear of ethical issues. Data are presented to indicate that many parapsychologists either heartfully adopt scientism or just give it lip service. Standing tall is no longer a popular response.

Toward the end of the book, when all the data are considered, McClenon states the futility of us standing in the pew of scientism: "A major conclusion of this study is that changes in legitimacy of a deviant science generally are determined, not through the efforts of the deviant scientists, but through changes occurring within established science" (p. 228).

How large is the elite group that judges us? McClenon refers to the literature for some estimates: one source suggests that they number between 200 and 1,000; another, 392. According to his own survey, slightly more than one quarter of them are favorably disposed toward ESP. Our inroads are made, but the majority still say "No."

The author, an outsider, infiltrated our field so well that his observations provide something like "Candid Camera" shots of us, some of which I will describe here. The first case shows that even a *candid* camera can be wrong: one parapsychologist who was interviewed almost succeeded in putting on a better face for McClenon's lens; however, the author was astute enough to label this story "arguable." I am referring to the case described on pages 185 to 186, which details how Rhine's alleged preference for "proof-oriented research" was rebelled against by a faction of his staff who wanted "process-oriented research" and left the lab en masse when they did not get it. Actually, it was common knowledge at the time that a personality clash had occurred between the leaders of the protesting faction and one of Rhine's favorite researchers, whose experiment was opposed by the

others. The man who was attacked later became the department chairperson of a major university, and the three members of the group who left Rhine's lab are now prestigious parapsychologists. Rhine was indeed proof-oriented in his early career but shifted his focus to psi processes long before the members of the rebelling faction were kids in short pants. My impression is that the book *Extrasensory Perception After Sixty Years*<sup>6</sup> marked the end of his push for proof-oriented research, which probably occurred after a give-and-take with the very process-oriented Gardner Murphy, who then cooperated closely with Rhine by editing this *Journal* (circa 1940).

McClenon's "Candid Camera" often caught us when our faces were not made up.

Contact by the public with scientific parapsychology illustrates the irony of parapsychology's strain toward scientism. The reaction of the field to its critics has had the latent effect of alienating those who have experienced psi most frequently. Parapsychologists, adhering to the norm of skepticism, generally cannot validate any particular psychic experience. (p. 208)

I see some truth in that. Courts stick to the positive—innocent until proven guilty—however, we do not. Nevertheless, our support is nearly always sought for and obtained from persons who become interested in the subject because of their personal experiences with psi. "Paradoxically, although the field deals with the paranormal, parapsychologists tend to doubt the authenticity of much that the public attempts to tell them about psi" (p. 212). Do not expect that this outsider came to solve our awesome methodological problems, for example, how to do case research most productively; but do consider a nondefensive look at ourselves through McClenon's "Candid Camera"—it might help to start some fresh thinking.

We are told that research on anomalies other than ours either gains acceptance or, if rejected, fades out in a few years. Why has parapsychology survived 100 years? We are shown that our longevity has depended on our ability to generate public interest and support. The rush toward the shelter of scientism, however, might have resulted in our current predicament of sitting between two chairs.

The parapsychologist's inability to harness the high level of popular interest in psi is a result of the field's scientific orientation. By adhering so strongly to the norms of science, the parapsychologist loses public support yet consolidates his or her role as a deviant scientist. (p. 214)

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<sup>6</sup> See J. B. Rhine, J. G. Pratt, C. E. Stuart, B. M. Smith, & J. A. Greenwood (1940), *Extrasensory Perception After Sixty Years*, New York: Henry Holt.

Why do we then get public support? He says, "It would seem that as long as parapsychology can constitute a threat to scientific orientation or support religious belief, benefactors can be located" (p. 77). I think there is more: at least the issue of human potentialities should also be included. I still remember an early American Psychological Association convention in which a symposium on human potentialities was attended by some 1,500 attentive psychologists; Rhine and Murphy represented parapsychology on the panel. In later years, I also watched a scientifically oriented symposium on parapsychology: just 50 to 70 psychologists attended, many of whom were restlessly wandering in and out.

The "Candid Camera" was also switched on several ex-parapsychologists who had left the field.

Only one rejected the field as invalid, but many were glad to be on a career path that would lead toward financial security. . . . Various former researchers expressed a belief in psi yet a hostility toward the "political games" they felt existed within the field of parapsychology. The field is hostile to innovation, they claimed, and is overly concerned with establishing its legitimacy." (pp. 174-175)

On the competency issue always raised by critics, we receive a reasonably clean bill of health: accusations are analyzed and attributed to rhetorics and unfair "unpacking" techniques.

Parapsychology's struggle with its critics precludes the possibility of a "perfect" experiment proving the existence of psi, since in principle it is always possible to require a further degree of exactitude or precaution. Methodological advances in research oriented toward producing proofs merely evoke new rhetorical strategies for critics to use in rejecting any particular parapsychological study. (p. 184)

Our history seems to justify this statement, but who will ever succeed in teaching a dog not to chase his tail?

Well, our competency looks good but our social conduct seems a bit funny when viewed on "Candid Camera."

Science requires both trust and skepticism to operate. Scientists must trust their fellow researchers since they cannot replicate every experiment. The low state of replicability within parapsychology often leads to suspension of this trust. (p. 180)

and

One parapsychologist notes, "People don't believe what they can't do themselves. That holds for the field also. Parapsychologists don't take each other's work seriously." (p. 181)

So why should outside scientists take us seriously?

"The distrust of colleagues' claims," says McClenon, "thwarts theoretical development within parapsychology since the perfect proof of any hypothesis, although desirable, is impossible in practice" (p. 181). He cites as an example the "unpacking" of Tart's feedback experiment, for which the *Journal of the American Society for Psychological Research* alone allocated 48 pages for criticism and reply. "This and various other efforts to 'unpack' Tart's study have discouraged efforts to develop a means of teaching ESP that otherwise might have been stimulated. This observation is somewhat speculative . . ." (p. 181). Be that as it may, McClenon is not alone. Mauskopf and McVaugh also found the way we disagree on nearly everything to be a sign of scientific immaturity; Brian Inglis saw it corroding the science of psi. McClenon emphasizes again and again that normal argumentation is basic to the scientific process—it is only the excesses and "unpacking" that points to the "ugly American" in us and in our detractors. Of course, every author has opinions that we do not necessarily have to accept.

The strongest and most empirical chapter describes McClenon's extraordinary accomplishment of polling elite scientists in a well-executed survey. There is now extensive literature on poll results, including Wagner and Monet's poll of professors. However, I have found nothing to provide as good an insight as McClenon's own survey, which found that 29% of the elite scientists sampled accepted ESP.<sup>7</sup> What shook me was the finding that even the best of the scientific elite gets acquainted with psi not by reading our journals or books, but mostly by popular print and television sources. Can we finally wake up and face this? The only decisive factor for acceptance or rejection of psi was personal experience. What pleased me? They did not chicken when it came to also describing their apparition and out-of-body experiences, which are taboo even by scientists among us. Their remarks present criticism on various grounds; several pointed out that one deficiency is our too scientific research approach. Maybe Rhea White was right to admonish us in one of her PA talks: let's be ourselves!

Weak spots can be found in this book, as in any other; one example would be statistics— $z$  is incorrectly defined. However, in my judgment, this is a valuable book, packed full of needed information that could be useful when charting our strategies for the future. What outsiders discover is important, especially when it is that which we

<sup>7</sup> Refer to McClenon's survey cited in footnote 5.



overlook ourselves. *Deviant Science* is a "food-for-thought" book, and there is plenty on the platter. But do not expect an observer, no matter how astute, to solve our problems for us.

If our sails are turned so that they catch the winds of our time, we move; if they are turned in a wrong direction, we are stuck. Let us remember the tremendous interest in our field that was evident during the cultural upheavals of the 60s and 70s. At that time, our journals proceeded with "business-as-usual" and failed to provide a forum for a generation of enthusiasts bubbling over with new ideas and "paradigms." Such a forum would have been essential for maturation and growth, turning bubblers into valuable professionals. In spite of the efforts of some, for example, Krippner and Tart, we missed most of the crop. *Deviant Science* could help us to be a bit wiser and more responsive to the various currents of the now emerging "postmodern mind." And, by the way, what is wrong with having a good laugh at ourselves as we watch "Candid Camera"?

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THE PSYCHIC THREAD by Elizabeth Mintz in consultation with Gertrude Schmeidler. New York: Human Sciences Press, 1983. Pp. 232. \$26.95, cloth. (Reissued by the author and obtainable from Box 564, Hastings-on-Hudson, NY 10706, \$18.00 post paid.)

With her book *The Psychic Thread*, Elizabeth Mintz, consulting with Gertrude Schmeidler, has offered a significant new contribution for both psychotherapists and parapsychologists. She addresses the issue of what parapsychological findings and hypotheses, and a transpersonal perspective, can add to the endeavor of psychotherapy.

Early in the book I was reminded of another by Dr. Mintz I read several years ago. It was on a field in which she is an international authority: marathon psychotherapy groups. Like her current topic, that one was very controversial, and vulnerable to polarization and polemics. With a quiet voice, she stepped around all stridency and produced a work that was deeply instructive, balanced, inspiring, and clinically wise. And here she has done that again.

Dealing first with parapsychological issues, Mintz skims some pertinent research findings, pointing out affinities between conditions