

## EDITORIAL

Recently, I was chosen to be one of forty individuals invited to submit a short essay for a special issue of the *Journal of Parapsychology*. The topic we were asked to address was “Where will parapsychology be in the next 25 years?” This challenge encouraged me to reflect on the history of psi research and the continuing debate over its merits and results, and I found it to be a useful exercise. I also found it to be somewhat depressing, and I don’t believe that was due merely to an uncharacteristic spasm of negativity. In any case, it led me to wonder what my SSE colleagues working in other areas of anomalies research would say about the future of their own respective disciplines. For example, can we speculate competently, based on the history of UFO research, where that research is likely to be in a quarter century? Or the quest for new and reliable sources of energy, including “cold fusion” or LENR? Hypnosis or altered states research? Are we seeing any notable and sweeping advances in any of these areas—say, increases in understanding among the few who do the research, or in the impact that research is having in the public at large? Can we predict competently which of the latest cutting-edge or trendy theoretical proposals (e.g., in earth science, astrophysics, or survival research) are likely to be genuinely fruitful?

So, in the hope that my reflections on the future of parapsychology will encourage similar exercises among *JSE* readers (in addition to eliciting predictable cries of protest), what follows is an expanded and (freed from severe space limitations) rather less curmudgeonly version of my little essay for the *Journal of Parapsychology*, reproduced in part with the kind permission of editor John Palmer. Perhaps it would be interesting to have a roundtable discussion along these lines at some future SSE conference.

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I don’t believe I’m a pessimistic person, but I find it difficult to be optimistic about the next 25 years of parapsychological research. That’s because when I consider the field’s successes and failures since the late nineteenth century, certain patterns stand out starkly for me.

First, skepticism about the reality of ESP, psychokinesis, or the evidence for postmortem survival, has always been intense, especially in scholarly circles, and it has quite often been vicious, recalcitrant, and dishonest. Granted, over the years, some open-minded scientists and others have dispassionately (or otherwise) reviewed the evidence and found themselves persuaded either about the reality of the phenomena or at least the value of doing additional research. But these people clearly comprise a very small minority, and parapsychologists have clung anxiously to their most prominent members in order to tout their endorsements or support. To

take just one example: we're too often reminded that Brian Josephson—who does no psi research but who actively and effectively defends it—is a Nobel-winning physicist who takes parapsychological research seriously. Don't misunderstand me; I also welcome Brian's vigorous support, his willingness to contribute to the theoretical dialogue in parapsychology, and his many—time consuming—efforts to combat shoddy skepticism. But personally, I'm embarrassed by parapsychologists' frequent and dialectically shabby appeals to his authority and prestige. Partisans from all sides of the psi debate can point to prominent scientists who share their points of view, and respected, intelligent people can say foolish things. It's both pathetic and irrelevant—and it does appear desperate—to cite Brian's (or any supporter's) credentials. What matters is what they say.

The fact is, the resistance to the entire field of psi research hasn't diminished significantly in more than a century, and the tactics employed to discredit the field or its major figures have remained the same as well. Critics have all along feigned certitude about the worthlessness of the data while betraying their ignorance of what the data actually are. Detractors (or deniers) still employ fallacious argumentative strategies (e.g., ad hominem or straw-man arguments) they would be quick to detect and denounce if they had been the targets of those arguments instead. And not surprisingly, the tone of these criticisms often reveals an intensity of emotion inappropriate to what should be an open-minded empirical inquiry. Indeed, it looks conspicuously like a fear response. A contributing factor, of course, is that somewhere along the line (but undoubtedly beginning many centuries ago), scientists and scholars allowed ego, pride, or self-interest to interfere with the thrill of discovery. Why aren't more of us excited to learn that we might have been wrong, so long as the discovery brings us closer to getting things right?

Second, it's clear that parapsychology's gradual adoption of more relentlessly and sophisticated quantitative methods of investigation has made almost no difference to the course of skeptical opposition. On the contrary, it's simply opened a new and fruitful—and largely technical and specialized—playing field for glib or dishonest criticism. So instead of concentrating on allegations of mediumistic fraud, biased observation, sloppy reporting, or faulty memory, critics now focus (for example) on allegedly questionable statistics, the proper criteria for conducting meta-analyses, or other methodological flaws (real or imagined). In that respect, J. B. Rhine's so-called “revolution” of moving from mediumistic case studies to quantitative lab experiments has been a complete failure. Overall, neither the public at large nor the subset of academic detractors has been more convinced by quantitative research than they were beforehand by anecdotal reports and mediumistic case studies.

Of course, all sides in the psi debate (believers, doubters, and deniers)

are guided by some combination of intuition (or “passion”) and reason. Nevertheless, spontaneous case studies have always been, and continue to be, more impactful—and in important ways more clear-cut—than a study whose conclusions rely on controversial and very abstract reasoning, either about statistical presuppositions, quantum weirdness, or the nature of causality. Significantly, not even all psi researchers consider themselves convinced about the reality of the phenomena they study, and I believe it’s true that most of the doubters (or fence-sitters) assume that conviction can only come from applying some version of probability theory to lab experiments and from determining ostensible paranormality solely by the numbers.

Now if only there were a growing or robust trend in current parapsychology to focus more on field work or exceptional subjects, and to try to get a handle on the phenomena’s role in life, there might be reason to think we’re finally starting to get somewhere. We might then have a better idea of what it is, exactly, we’re trying to study experimentally—not to mention whether (or to what extent) experimental methods, confidence intervals, and *p*-values are even appropriate to the phenomena. And then maybe we’ll have a better grip on how to elicit the phenomena with greater reliability (or at least a better understanding of why the phenomena are doomed to remain somewhat elusive). But that’s not happening, and overall the dialogue between critics and defenders of psi research (and to some extent the conversation among parapsychologists themselves) hasn’t budged significantly in many decades. It continues to center too often on difficult-to-resolve alleged methodological shortcomings or statistical errors—at least when critics aren’t merely flaunting their ignorance of the data or else echoing the old skeptical mantra about the supposedly intuitively obvious impossibility of the phenomena.

Actually, some critics (and even a few seasoned parapsychologists) continue to make the more egregious error of thinking that we first need a well worked-out *theory* before we can admit that the phenomena are real. But of course it’s completely obvious that we can know *that* something is the case without knowing *why* it’s the case. Yet this flawed objection, like the other lame critical strategies mentioned earlier, shows no signs of disappearing from the field of debate.

Another discouraging trend (revealed even more clearly from my privileged perspective as a journal editor) is that too many people publish (or try to publish) books and articles about parapsychology (pro and con), or conduct their own experiments, with little or no grounding in the field’s extensive literature, both empirical and theoretical. In my editorial for *JSE* 23(3), I lamented, for example, how presenters at SSE conferences, with a solid background in conventional scientific research, try to conduct parapsychology experiments (say in healing) in apparent ignorance of two related and well-known methodological problems: (a) the “source of psi”

problem about the extent to which a controlled experiment is even possible in parapsychology, and (b) well-documented experimenter expectancy effects in behavioral research. It should be obvious that a background in some mainstream scientific discipline is not, *by itself*, qualification for publishing opinions about parapsychology or conducting one's own experiments. But this form of naïveté—if not outright hubris—is regrettably quite common, and I encounter it repeatedly in manuscripts submitted to the *JSE*. Of course, as a result, simplistic and ignorant opinions (pro and con) about psi research spread and perpetuate. This can only impede the dissemination and recognition of sensible and informed work in the area.

I know some will disagree with my bleak assessment and point to apparent inroads here and there within the scientific community. But of course there have been scattered successes. Some formerly intransigent skeptics have adopted more moderate positions; some who had previously opposed all things paranormal now display degrees of sympathy or support; and occasionally a paper on psi research appears in a respectable mainstream journal (usually accompanied and followed by a chorus of outrage). But that's always been the case, and I'm still awaiting evidence suggesting that the optimists have identified a lasting trend and aren't simply ignorant of the field's history or otherwise empirically myopic, or (equally likely) inductively challenged. In the meantime, funding remains scarce and modest, educational opportunities and stable research positions are few and far between, and the academy remains a generally hostile environment.

I'm not saying this will *never* change. After all, I do believe in the inexorable (though not smooth or steady) advance of human knowledge, and I'm actually confident that humankind (if it persists long enough) will eventually progress to points at which psi phenomena are generally accepted as real and in which the phenomena get incorporated into one or more widely established conceptual frameworks. But this will be a huge and deep change. After all, people don't relinquish old habits and entrenched beliefs without a real struggle, and in fact it's very difficult to reason people out of positions they haven't been reasoned into. So I wouldn't bet on major progress or success in parapsychology happening any time soon.

Now as far as I'm concerned, that doesn't mean that researchers should throw in the towel, or that the *JSE* should stop publishing good quantitative (or qualitative) papers in parapsychology. Although I don't think we can look forward to rapid progress, there's always room for good research (both in the lab and in the field), and in fact there's a continuing *need* to accumulate such research. That's the only way the truth will out. So I'm pleased that this journal and the SSE can play a vital role in the process, and I'm proud that the *JSE* so consistently delivers research and essays of the highest quality in many areas of frontier science.

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