EDITORIAL

[*Editor's Note:* Back when J. B. Rhine was alive, he would occasionally publish editorials in the *JP*. Starting with the current issue, I am resurrecting this practice. The editorials will not appear in every issue, but neither will they be isolated events. I envision them as thought-provoking opinion pieces addressing fundamental issues facing contemporary parapsychology. Because I would like our "editorial page" to reflect a wide range of perspectives, most of the editorials will be guest editorials written by other prominent members of the parapsychological community. However, the first contribution should clearly be mine, hence the following.]

Winning Over the Scientific Mainstream

One of the most important goals of parapsychology over the years has been to convince the mainstream scientific community of the reality of psi, at least as a communications anomaly. I think it is useful to reflect periodically on what progress we have made in meeting this goal. My answer is, not much.

In the old days, we attempted to make our case through fool-proof (and magician-proof) "crucial experiments." This approach never succeeded, although the alternative explanations critics proposed were sometimes at least as implausible as psi itself (cf. Hansel, 1989). The whole rationale behind this approach is flawed, as even some critics have recognized (e.g., Hyman, 1981). Nowadays, the replicability of good-quality, if not perfect, experiments has taken over as the criterion of success. In the literature, at least, the battle has focused on the results of meta-analyses of groups of psi experiments of a common type. Success (and replicability) is claimed if the collective result of these experiments is statistically significant and the effect sizes of individual studies do not correlate with measures of methodological quality (Radin, 1997). Such an outcome has been claimed by parapsychologists in several instances. The most important of these is probably the ganzfeld, because it has been debated with critics in a very prominent mainstream psychology journal, *Psychological Bulletin* (e.g., Bem & Honorton, 1994; Hyman, 1994).

In a review of the ganzfeld debate, I concluded that the ganzfeld literature met the meta-analytic criterion for replicability (Palmer, 2003). However, I also noted that the large heterogeneity (variability) of the results illustrates that replicability is by no means universal, and that what replicability there has been is restricted to parapsychologists who are favorably disposed to the psi hypothesis. This latter point is particularly pertinent to the issue of mainstream acceptance of psi. In my view, even open-minded mainstream scientists (I am thinking especially of psychologists) will not jump on board until a critical mass of mainstream scientists who have not been identified with parapsychology in the past obtain positive results in experiments they conduct. The more prestige these mainstreamers have in their own fields, the better. I don't think they need to be skeptics; benevolent neutrality is sufficient. Don't ask me to attach a number to this critical mass, but we clearly are nowhere near it at the present time.

The best recent example of such a mainstream contributor to our research literature is the prominent social psychologist from Cornell, Daryl Bem, who has repeatedly been successful in demonstrating psi in the context of the so-called "mere-exposure effect." As Bem has pointed out himself, his studies should carry additional weight with mainstream scientists because the methodological paradigm is one they use in their own field and is easy to implement. (Technically, Bem must be considered a parapsychologist more than an outsider, because he conducted these experiments after he had already taken a pro-psi position in his ganzfeld paper, and his psi research has not yet been published in a mainstream journal. However, he is close enough to an outsider that his research and general advocacy should carry some weight in the mainstream community.)

However, psi research by outsiders is very much a double-edged sword, as it can be exploited by researchers unsympathetic to psi to damage the credibility of the field even more effectively than the purely armchair critics of the past. Such an effort was recently published in a prestigious mainstream psychology journal, the *Journal of Cognitive Neuroscience* (Moulton & Kosslyn, 2008). I will discuss this experiment in some detail, partly because I want to get my criticisms on the record, but more importantly because it illustrates a powerful tactic that critics can use to damage parapsychology. The study is of particular interest to me because the first author is a former graduate of the Rhine Center's Summer Study Program (SSP). Its credibility is enhanced by the stature of the second author, Steven Kosslyn, Moulton's mentor at Harvard and a well-known and respected cognitive neuroscientist in his own right. He also is a member of the Scientific Board of the Bial Foundation, currently the major funder of psi research. The potential impact of the study really hit home when I found it cited as authoritative in my hometown newspaper.

The study compared brain reactions, as measured by fMRI, to psi vs nonpsi stimuli in unselected volunteer participants. The authors hypothesized that if psi is real, the fMRI should detect differences in brain responses to the psi and nonpsi trials. No such differences were found, leading the authors to boast that they had provided "the strongest evidence yet" against the existence of psi.

Although I have some quibbles with the experiment itself, the main problems are with the writeup and the authors' interpretation of its findings. First, the Moulton/Kosslyn paper was unbelievably deficient in reviewing previous research. The most serious problem was the failure to cite two psi-fMRI experiments that were quite similar to theirs (Achterberg et al., 2005; Richards, Kozak, Johnson, & Standish, 2005). Both studies provided significant evidence of a psi-fMRI relationship. They were published in a peer-reviewed journal a little more than two years prior to the publication of the Moulton/Kosslyn paper, so there was plenty of time for the authors to cite them before submitting their report, and probably enough time to use them as guides for designing their own study. One thing the authors might have noted is that both these studies employed participants selected for presumed psi abilities—paranormal healers as senders (Achterberg et al., 2005) or successful participants in a pilot study (Richards et al., 2005)—rather than the ordinary volunteers they employed. However, the most important point for present purposes is that these two studies put a major dent in the authors' case for the nonexistence of psi, and the failure to refer to them makes their case appear to be stronger than it is.

The second major problem concerns the authors' rationale for claiming positive evidence against psi from negative (nonsignificant) results. They argue as follows:

> Although one can never affirm the null hypothesis, not all results are epistemologically equal. Because this paradigm uniquely minimizes assumptions about the source of knowledge, the kind of processing, or the nature of the mental content responsible for psi, any ensuing null results will be qualitatively more informative than those from behavioral methods. Moreover, we can compare any null results with positive results that reflect other aspects of the same stimuli; thus, conceptually, such null results can be considered part of an interaction, where one variable has effects but another does not. (Moulton & Kosslyn, 2008, p. 183)

Even if we grant the authors their metaphysical assumption of materialism, this argument is nonsense. The fact that the authors failed to find any significant physiological or behavioral evidence of psi simply means either that the psi process was not activated by their procedure or its manifestation was so weak as to be obscured by error variance. In either case, a crucial condition for testing the hypothesized psi-fMRI relationship was not met. This means that their study has the same impact on the psi controversy as any of the many other nonsignificant psi experiments—no more and no less. The existence of such a large number of negative results is precisely why parapsychologists have appealed to meta-analysis to draw conclusions based on the literature as a whole. That the editor and reviewers for a prestigious journal would accept such an argument is disturbing.

Not only is any argument of this type fundamentally flawed, but the authors' points are made in such a vague and abstract fashion that they cannot be readily understood. They do not even attempt to explain how the methodological and conceptual virtues they attribute to their study answer the question at hand. Instead, they seem to be hiding behind a lot of sophisticated-sounding jargon. The average scientist reading the quoted paragraph is not likely to spend the time needed to grasp the argument and will simply assume it is valid because of who made it and where it was published.

When Moulton returned to Harvard after completing (with honors) the SSP, he obtained positive results in a psi experiment that appeared to be methodologically sound. In preparing this editorial, I consulted PsycINFO, the main search engine for finding psychological research, and could not find the experiment listed under his name. Thus, it appears he never published it. If he could not get it published in a mainstream psychology journal (a likely possibility given the results) he certainly could have published it in a parapsychological journal. Ironically, critics often accuse parapsychologists (unfairly) of only publishing results that support the existence of psi and leave the failures in the file drawer. It appears that Moulton has adopted the converse of this philosophy.

Can the behavior of Moulton and Kosslyn be explained by some combination of oversight and errors of judgment and logic, possibly mediated by their skepticism about psi, or by some other relatively innocent explanations I haven't thought of? Or does this behavior represent a deliberate attempt to mislead the scientific community and the general public about the status of the evidence for psi? I don't know—I'm not telepathic. In any case, the *effect* of this behavior is to misrepresent the status of the evidence for psi. All this could be cited by sociologists of science as a case study supporting the philosopher Feyerabend's (1993) cynical thesis that consequential science is less an objective search for truth than political game playing in the interest of promoting the prejudices and prestige of scientists and their institutions.

This lengthy diversion is an example of what we are up against in trying to convince the mainstream scientific community of the reality of psi. Moulton's student status should help dispel any illusions some might have that things will get better once the old generation of scientists dies out and a new generation takes over. As noted above, I think the key problem is the lack of reliability, or what Hyman (1989) properly calls the "elusiveness" of psi effects. The overwhelming majority of scientists (at least scientists of stature—the ones with the greatest stake in the theoretical status quo) will simply not accept statistical replicability as evidence for the existence of psi, however unjustified that conclusion may be. We need something pretty close to replicability on demand.

One thing that should be clear is that even the best psi practitioners are nowhere near up to this challenge. If they were, we could fund parapsychology for decades to come by sending them to Las Vegas and letting them loose in the casinos. The fact that we find the occasional strong psi effects we encounter so newsworthy simply underscores their unreliability. Psychic healers are nowhere near replacing or even significantly assisting medical doctors for either diagnosis or treatment, to give just one of numerous examples of how far we have to go to make psi applicable in any meaningful sense.

Okay, so how do we get there? We can't wait for some super-talent to bail us out; that hasn't happened in the 100+ year history of our field, if it ever happened. Thus, we have to "grow our own," and that means psi training. Our literature records several attempts at training psi using feedback methods, but the results have been unspectacular at best (Palmer, 1978). However, the amount of training in these studies was so small that it would be astonishing had meaningful success transpired. College basketball players sometimes spend hundreds of hours practicing free-throw shooting, which involves nothing more than putting a ball through a hoop from a few feet away with no one guarding you. How much more complicated and delicate a process is developing ESP ability, especially given the high number of chance hits that must be mentally discriminated from the real ones? A better analogy of what is needed might be the years of rigorous discipline that the occupants of monasteries endure. Now, I realize that there are reasons to be pessimistic that such a long-term training program would lead us to the holy grail of reliable psi, even if we had promising participants to train. The answer to this challenge is simple: if we want to convince the scientific establishment of the reality of psi, psi training is the only game in town.

I continue to believe process-oriented psi research has a role to play in parapsychology by helping us to gain a theoretical understanding of psi. However, if we could find a way to make psi effects more reliable, it would make process-oriented research more efficient and productive, so we wouldn't have to do a meta-analysis or such every time we want to draw a conclusion about a relationship. Some of our process-oriented research also might provide guidance in how we should conduct psi training. I don't see any connection so far, but perhaps we could come up with something if we gave the matter some thought.

Finally, it should be noted that such a training program, particularly if it were to succeed, raises ethical issues. The development of more reliable psi will probably create stronger or more powerful psi as a by-product. Thus, the most important of these ethical issues is whether we would like to live in a world in which some of us, and maybe even all of us, had strong and reliable psi abilities. This is a complicated question, but, to put it metaphorically, before embarking on a journey it is a good idea to decide if you really want to reach the destination.

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