

EDITORIAL

The May SSE Conference in Charlottesville, VA was, as I'd expected, interesting, informative, and often entertaining. Charlie Tolbert did a superb job of handling the local arrangements; the conference venue was delightful (and Charlie contributed periodic juicy nuggets of information about the campus's rich history); the program was varied and nicely balanced; and there were many opportunities for stimulating conversation, exchange of ideas, and of course reconnecting with old friends, making new acquaintances, and meeting people I've known only through correspondence.

But as I later reflected back on the conference, I realized that something troubled me. It concerns some striking differences between an SSE conference and most other scientific or scholarly conferences I've attended. In particular, in a typical academic or scholarly conference, one can count on speakers sharing a general body of assumptions specific to their field(s) as well as a general background of knowledge about the history of their discipline and the discipline's key issues and problems. Of course, that won't happen in a group as diverse in its membership as the SSE. One reason, naturally, is that SSE members are drawn from different branches of science (physical, biological, and behavioral) as well as the humanities. And another reason is that what attracts them to the SSE are issues and areas of inquiry that push the received boundaries, or challenge the usual assumptions, of one or more of the familiar scientific or scholarly disciplines.

Not surprisingly, this has its good and bad points. The good is that the cross-pollination of an SSE conference (and the *JSE*, for that matter) works against the insularity to which all of these disciplines are susceptible. It reminds us that concepts aren't isolated or isolable entities and that our apparently diverse interests actually have many points of contact. So it encourages healthy communication and exchanges of information, it promotes novel and potentially fruitful collaborations, and it suggests new and sometimes quite daring research agendas.

The downside, however, is that speakers at the conference often betray an ignorance of key issues and data bearing on the research the speakers are presenting. That's not surprising, of course, because despite their relevance to the research under discussion, those issues and data may fall outside the speaker's primary mainstream area of expertise. This sort of thing doesn't happen (or at least I've never seen it happen) in a mainstream academic or scientific conference. In those cases, conference submissions failing to display the minimal background knowledge expected of a professional in the field simply don't get accepted. Granted, it would be unreasonable to expect SSE conference presenters to have

a professional-level grasp of all the issues and research bearing on their courageous—and typically interdisciplinary—efforts to push the boundaries of scientific knowledge. And one of the virtues of an SSE conference is that presenters have an opportunity to learn precisely what the lacunae are in their broader scientific education.

Nevertheless, it remains the case that SSE conference presentations are sometimes (and maybe often) weakened or undermined by these gaps in the presenter's background knowledge. And as I listened to the presentations at the May conference, it seemed to me that a number of speakers were simply unaware of two related, general, and very important methodological concerns. The first has to do with the very possibility of conducting a controlled parapsychology (or remote healing) experiment, and the second concerns well-documented experimenter expectancy effects in behavioral research.

The parapsychological problem is straightforward, obvious, and surprisingly neglected. In fact, it's neglected (or at least underestimated) even within parapsychology, probably because membership in the community of serious psi researchers is as methodologically and professionally diverse as in the case of the SSE. At any rate, the problem is this. If we're taking psychic functioning seriously enough to test for it, that is, even if we're treating it simply as a working hypothesis, then we're testing phenomena which, *by hypothesis*, can subvert all conventional experimental controls. For example, in the case of PK (including remote healing) there's no way to determine conclusively who's causally responsible for the result (significant or insignificant). It's not as if we can go around with a PK "meter" to detect the presence or absence of PK before the presumed effect is detected. But then for all we know, it may be someone other than the official subject whose PK is causally relevant. For all we know, it could be the experimenter, the onlooker, or worse still, someone we consider remote from the experimental set-up. Moreover, if ESP is possible, and accordingly, if interested outsiders (skeptics and sympathizers) can have psychic access to what's going on, that last scenario—farfetched as it might seem to some—can't be treated as inherently less plausible than the others. For one thing, there's no reason to think that psychic distance corresponds neatly or at all to physical distance. But more generally, given our considerable level of ignorance as to which psychological or other situational variables are causally relevant, and considering our inability even to track, much less modify, the most likely suspects among them, we're in no position to rule out any of these options. Similarly, if ESP occurs, then there's no such thing as a genuinely blind or double-blind ESP experiment. The ordinary control procedures in these tests block only normal or recognized channels of information.

So as far as we know, psychic functioning might be sneaky and naughty all or much of the time, and there's really not a damn thing we can do about it. This means that process-oriented experimentation, experimentation in which we try actually to learn something about the phenomena under investigation, may well be a methodological pipedream. Nevertheless (as Jule Eisenbud once noted—Eisenbud, 1963, 1992), many psi researchers proceed as though everyone

connected with a parapsychology experiment will adhere to a kind of absurd gentleman's agreement. They act as if subjects will use only the psychic ability being tested, that they will use that ability only after the experiment has begun (and then only according to their appointed role in the experimental design), and that others (experimenters, judges, onlookers, remote ill-wishers) will use no psi at all to influence the experimental outcome. But in fact, undertaking a parapsychological experiment is opening a Pandora's box of unidentifiable and uncontrollable potential influences. The most we can hope to do with any degree of confidence is something we arguably have already done quite enough—namely, merely accumulate evidence that *something* ostensibly paranormal has occurred.

And here's where the second issue, concerning experimenter expectancy, enters the picture. The possibility of both uncontrollable telepathic leakage and telepathic influence might go a long way toward explaining some of the more puzzling evidence for experimenter expectancy effects, that is, evidence that the experimenter's expectations concerning research results affects the experimental outcome.¹ For example, in one famous series of tests, experimenters were provided with groups of rats that they were told had been bred to be maze (or Skinnerbox)—bright or maze (Skinnerbox)—dull, and the experimenters believed that their tests were designed to confirm the success of this selective breeding. But in fact, that was false; the rats hadn't been selectively bred for their dullness or brightness. On the contrary, the groups of rats assigned to the different experimenters were selected so as to *minimize* differences between them, and which groups were to be labeled dull or bright was decided randomly. Nevertheless, the rats believed by their experimenters to be bright outperformed those believed to be dull.

Another study compared the performance of brain-lesioned rats to that of rats who received only a sham surgery in which the skull was cut through without damaging brain tissue. The rats were labeled as either lesioned or nonlesioned. But randomly, some of the really lesioned rats were labeled accurately and some were falsely labeled as nonlesioned. Similarly, some of the unlesioned rats were randomly and falsely labeled as lesioned. The results again clearly indicated the effect of experimenter expectancy. In the case of genuinely lesioned rats, those mislabeled as nonlesioned outperformed those labeled as lesioned. And for the genuinely unlesioned rats, the correctly labeled rats outperformed those falsely labeled as lesioned (Burnham, 1966).

Rosenthal and Rubin clearly appreciated the importance of this. They noted, if investigators interested in the effects of brain lesions on discrimination learning had conducted the usual two-group experiment without keeping the experimenters blind to treatment condition, the results would have been seriously misleading. (Rosenthal & Rubin, 1978: 384)

And more generally, they observe,

For investigators interested in assessing, for their own specific area of research, the likelihood and magnitude of expectancy effects, there appears to be no fully adequate substitute for the employment of expectancy control group designs. (Rosenthal & Rubin, 1978: 384)

Not all expectancy effects noted in the literature are as potentially exotic as these. Needless to say, it's an open question how the experimenters' expectations were conveyed to (or otherwise influenced) the rats. (And obviously, if appealing to psychic interactions is one of our explanatory options, then we're again faced with the Pandora's box noted earlier.) At any rate, it's reasonable to think that some observed expectancy effects will be explainable in terms of relatively mundane interactions between experimenters and subjects. But whether the processes involved are ordinary or exotic, the literature on these effects should be required reading for anomalies researchers conducting formal experiments, as should Rupert Sheldrake's *JSE* paper on the almost shocking neglect of blind methodologies in most scientific disciplines.²

All scientists, and anomalies researchers in particular, must assume that their interests or expectations might be causally relevant to their experimental outcomes. They certainly can't pretend that, as experimenters, they're merely neutral participants in an objective search for scientific knowledge. The only truly emotionally or conceptually neutral scientist is a dead one. Like everyone else, scientists are teeming cauldrons of interests, fears, and grubby predispositions. To some extent these are part of our everyday psychological baggage, and to some extent they're connected intimately and inextricably with the specific research in which the scientists are involved. So it's naive to think that the experimental results reported at SSE conferences can be presented as if the experimenters' and others' intentions, expectations, and interests aren't a potentially crucial component of the underlying causal nexus.

I'm pleased to announce yet another addition to my team of Associate Editors: Michael Ibson. Michael is a senior research physicist at the Institute for Advanced Studies at Austin, and a former member of the PEAR Lab at Princeton. His scientific expertise and familiarity with the data and methods in parapsychology will be obvious assets in this role. Welcome aboard, Michael.

Notes

¹ I've discussed the so-called Rosenthal Effect more fully in Braude, 2002. See also Martin, 1977. For the data, the essential sources are Rosenthal, 1976, 1977; Rosenthal and Rubin, 1978.

² Sheldrake, 1998.

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