

Letters

Radwaste Policy

Luther J. Carter's report of the Keystone radioactive waste management discussion group (News and Comment, 6 Oct., p. 32) has gotten me into some hot water. Some environmentalists are saying we at Keystone sold out. I did not participate at Keystone because radwaste policy-making is "critical to the survival of the nuclear industry." I participated because radwaste policy-making is critical to the survival of humanity, whether the nuclear industry survives or not.

Second, because of the above-quoted phrase, environmentalists are saying the Keystone group's statement on reprocessing is pro-nuclear and pro-reprocessing. We simply said that the Inter-agency Review Group, which is preparing a policy document for the President, should discuss reprocessing and its implications for radwaste policy. To ignore the reprocessing issue seemed inappropriate to us. To favor a discussion of reprocessing is not the same thing as favoring reprocessing, which I personally do not favor.

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ESP Research

Persi Diaconis thanks me for comments on an earlier version of his article "Statistical problems in ESP [extrasensory perception] research" (14 July, p. 131)*, but except for his potentially important contributions to clarifying statistical problems in cases of guessing with feedback, I want to dissociate myself from the rest of his article. As I wrote him in detail about his earlier draft (which is essentially unchanged in its published form), his conclusions about modern scientific parapsychological research are based on a sampling of the field far too small in size,

grossly atypical, and clearly biased toward debunking, and so are quite misleading and a disservice to the readers of *Science*.

There are no legal restrictions on who can call himself a parapsychologist, so many unqualified people claim that title; but Diaconis' article purports to be about contemporary scientific studies of parapsychology, not popular parodies. I estimate that there are more than 600 published experimental studies of parapsychological phenomena in the refereed specialty journals, the vast majority of them using ordinary subjects rather than psychics, having procedures rigidly controlled by the experimenters, not the subjects, and using quite conventional statistical procedures to evaluate hypotheses which were formulated before the experiment was conducted. Instead of dealing with an adequate and representative sample from this large population, Diaconis deals at length with atypical and flashy cases that have attracted wide lay interest, such as Uri Geller's claims of psychic abilities, about which most respected parapsychologists have serious reservations. Diaconis' prime example of what he believes are major problems (multiple end points and subject cheating) in parapsychological research is his description of B.D.'s self-controlled demonstration at Harvard, an event that has no relation to experimental science and that no respected parapsychologist would have regarded as having serious value as data. What was his point in focusing on such an unrepresentative event, especially after the unrepresentativeness had been called to his attention?

After describing several atypical cases like this, Diaconis concludes that fraud and general experimental sloppiness are common problems in parapsychology, even making into an item of faith that while you can't spot the sloppiness and fraud in the published reports, they probably would have been found if a competent observer had been there. There is, of course, no way of disproving such a hypothesis. Such faith in the all embracingness of our currently accepted explanatory system is touching, but not appropriate in a scientific journal.

For the reader interested in accurate and representative surveys of scientific research on the paranormal, I recommend the recently published *Handbook of Parapsychology* (1).

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References

1. B. B. Wolman, L. A. Dale, G. R. Schmeidler, M. Ullman, Eds., *Handbook of Parapsychology* (Van Nostrand-Reinhold, New York, 1978).

Diaconis' article on ESP research, which contains some excellent material on statistics, is unfortunately marred by errors and faulty reporting in his discussion of contemporary research. Specifically, in discussing our work at the Stanford Research Institute (SRI), he references erroneous second- and third-hand accounts published in popular books and magazine articles. We address two of these errors here.

The first error concerns an apocryphal story of a visit to SRI by psychologist Ray Hyman. The claim, repeated by Diaconis, is that Hyman observed experiments at SRI performed by the controversial psychic-magician Uri Geller and reported "sleight of hand performed under uncontrolled conditions, much at variance with the published reports of the SRI scientists involved." The truth of the matter, however, is that when Hyman and two colleagues arrived at SRI with a request to observe experiments in progress, they were denied permission to do so. We had had several such requests per week and had previously concluded that it would be impossible to carry out controlled experimentation under such conditions. As an alternative they spent an engaging 2 hours with Geller themselves, observing the informal coffee-table-type demonstrations which Geller favors, and trying a number of their own (and from our standpoint, uncontrolled) experiments. Therefore, although it is true that Hyman saw uncontrolled experiments at SRI, they were not SRI experiments, and we consider it irresponsible for him or anyone else to assign responsibility to SRI researchers for their own unsatisfactory experiments. Since the early anecdotal accounts of this meeting have been corrected in the appropriate literature (1), it is surprising that Diaconis would be uninformed in this matter.

The second error concerning our work occurs in a section on possible pitfalls of ESP experiments involving feedback. Here Diaconis describes our experiments in "remote viewing" (2, 3) which

*A second group of letters concerning the Diaconis article will be published in a later issue.
—EDITOR