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 Interagency 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 involved a list of 100 San Francisco Bay Area target locations "chosen to be as distinct as possible." A team of experimenters visited the locations in random order, and a subject tried to give a description of where they were. In the context of the article, the discussion carries the implication that post-trial feedback to the subject during the experiments provided information which helped him narrow down the field of target possibilities in later trials. Diaconis' statement concerning the distinctness of targets is incorrect, however. The target pool was carefully constructed to contain several targets of any given type—that is, several fountains, several churches, several boathouses, and so forth—specifically to circumvent the strategy of "I had a fountain yesterday, so it can't be a fountain today" (*4*). Since we brought this misunderstanding to the attention of Diaconis last year in a letter after we had seen an early draft of his study, we find the lack of correction in his accounting of such an important methodological issue an exceptional faux pas.

As researchers in the field we welcome the kind of insights Diaconis can provide from his own area of expertise; however, we deplore the lack of attention to detail and the reliance on anecdotal sources regarding the broader aspects of the field.

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References and Notes

1. B. O'Regan, *New Sci.* **59**, 95 (1973); R. Targ and H. Puthoff, *ibid.* **64**, 443 (1974).
2. H. Puthoff and R. Targ, *Proc. IEEE* **64**, 329 (1976).
3. R. Targ and H. Puthoff, *Mind-Reach* (Delacorte, New York, 1977).
4. In (*2*) see, for example, fountain targets in tables IV and V and in figures 7, 8, and 15; churches in tables II and III; boat marinas in tables II and V and figure 14; and so forth.

The main point of my article on statistical problems in ESP research was that poorly designed, badly run, and inappropriately analyzed experiments abound in ESP research. Reading published records is not enough—when professional statisticians, psychologists, and magicians are allowed to view these experiments they often spot devastating methodological flaws. Puthoff and Targ provide a fine case in point. Since they take me to task for using secondhand sources, it is worth reporting that I spent a day at SRI viewing one phase of their research. Briefly, in one room a strobe light was flashed at a sending subject either rapidly, slowly, or not at all. A receiving subject in another room tried to guess the strobe light pattern. An electroencephalogram (EEG) of the receiver was monitored in the hope that changes in the EEG could be correlated with the strobe pattern. The account by Targ and Puthoff of this experiment (*1*) gives a feeling that it was tightly run. Unfortunately, my direct observations tell a different tale. For example:

1) When I asked a lab assistant how the patterns for the strobe light were generated (for example, whether they were randomized or carefully designed), she told me that she just made them up. This is a well-known error. Humans cannot generate random patterns.

2) Although electronic equipment was used to record the EEG's, many crucial details, such as the actual guesses made by the receiver, were handrecorded by a very busy lab assistant.

3) The final analysis of the EEG data was based on techniques I did not understand. I questioned Targ and Puthoff about them and concluded that they didn't understand the techniques either. As statistical analysis of EEG's is a very tricky business, I suggested that they consult one of the SRI statisticians. Targ said to Puthoff: "Do you notice how statisticians are always trying to make work for one another?"

4) The listing of the strobe light patterns to be sent was lying around for several hours before the experiment, accessible to anyone. I copied them down and during the experiment was toying with the idea of pretending to go into a trance and reveal the patterns.

The above points are typical of many other methodological problems I saw that day. It is unfortunate that such problems are impossible to recognize from the published record. It is this experience, together with reports from other skilled observers who have seen how this research was conducted at SRI, that led me to conclude it was impossible to determine what went on during these experiments.

Puthoff and Targ say that a criticism I make of their remote viewing experiments—internal cues resulting from feedback could be used to guess targets correctly—isn't relevant. In a recent study (*2*), psychologists Marks and Kammann used actual transcripts obtained from the SRI experiments and showed conclusively that, because of available feedback information, there were enough internal clues to guess every target correctly without visiting target sites and without ESP.

Puthoff and Targ begin by trying to set the record straight on Hyman's visit to SRI. They should have included a reference to Lawrence's rebuttal (*3*) to their letter to the *New Scientist*. Lawrence accompanied Hyman on the trip and completely supports Hyman's account.

In the first letter above, Tart reemphasizes many points I made in my article. To answer his one question, my purpose in focusing on B.D. was to report that a subject who has been used in widely quoted ESP experiments has been observed using sleight of hand. The similarity of the descriptions of the controlled experiments with B.D. to the session I witnessed convinced me that paranormal claims involving B.D. should be discounted.

The examples I reported in my article are a small and surely biased sample of modern parapsychological research. As indicated by the example described above, they are typical of all the ESP research I know of.

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References

1. R. Targ and H. Puthoff, *Mind Reach* (Delacorte, New York, 1977), pp. 130–134.
2. D. Marks and R. Kammann, *Nature (London)* **274**, 680 (1978).
3. A. Lawrence, *New Sci.* **66**, 595 (1975).

The Numbers Game

The recurring suggestion that a person's contribution to science can be measured by the number of published papers or the frequency with which they are cited by others (News and Comment, 29 Sept., p. 1195; 20 Oct., p. 295) brings to mind Dorothy Parker's cogent observation (*1*):

> There exists, especially in the American mind, a sort of proud confusion between [talent and industry]. A list of our authors who have made themselves most beloved and, therefore, most comfortable financially, shows that it is our national joy to mistake for the first-rate, the fecund rate.

Her critical assessment, in a review of a lesser-known novel by Sinclair Lewis, evidently can be extended to include authors of nonfictional works (and not just scientific ones). No doubt it was only a matter of time before quantitative estimates of unquantifiable values would be used to predict winners of the three annual Nobel prizes in science. By the way, Sinclair Lewis received the Nobel Prize for Literature in 1930.

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References

1. *New Yorker* (16 March 1929), reprinted in D. Parker, *Constant Reader* (Viking, New York, 1970), pp. 108–112.