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