

misses estimated; perhaps Cohen's h greatly underestimates effect size when very low probability events (less than 1 in 50 for heart attack in the placebo condition and less than 1 in a 100 for aspirin) are involved. I'm not a statistician and thus don't know if there is a relevant literature on this point.

Comment

Frederick Mosteller

Dr. Utts's discussion stimulates me to offer some comments that bear on her topic but do not, in the main, fall into an agree-disagree mode. My references refer to her bibliography.

Let me recommend J. Edgar Coover's work to statisticians who would like to read about a pretty sequence of experiments developed and executed well before Fisher's book on experimental design appeared. Most of the standard kinds of ESP experiments (though not the ganzfeld) are carried out and reported in this 1917 book. Coover even began looking into the amount of information contained in cues such as whispers. He also worked at exposing mediums. I found the book most impressive. As Utts says in her article, the question of significance level was a puzzling one, and one we still cannot solve even though some fields seem to have standardized on 0.05.

When Feller's comments on Stuart and Greenwood's sampling experiments came out in the first edition of his book, I was surprised. Feller devotes a problem to the results of generating 25 symbols from the set a, b, c, d and e (page 45, first edition) using random numbers with 0 and 1 corresponding to a, 2 and 3 to b, etc. He asks the student to find out how often the 25 produce 5 of each symbol. He asks the student to check the results using random number tables. The answer seems to be about 1 chance in 500. In a footnote Feller then says "They [random numbers] are occasionally extraordinarily obliging: c.f. J. A. Greenwood and E. E. Stuart, Review of Dr. Feller's Critique, *Journal of Para-*

The above objections should not detract from the overall value of the Utts survey. The findings she reports will need to be replicated; but even as is, they provide a challenge to some of the cherished arguments of counteradvocates, yet also challenge serious researchers to use these findings effectively as guidelines for future studies.

psychology, vol. 4 (1940), pp. 298–319, in particular p. 306." The 25 symbols of 5 kinds, 5 of each, correspond to the cards in a parapsychology deck.

The point of page 306 is that Greenwood and Stuart on that page claim to have generated two random orders of such a deck using Tippett's table of random numbers. Apparently Feller thought that it would have taken them a long time to do it. If one assumes that Feller's way of generating a random shuffle is required, then it would indeed be unreasonable to suppose that the experiments could be carried out quickly. I wondered then whether Feller thought this was the only way to produce a random order to such a deck of cards. If you happen to know how to shuffle a deck efficiently using random numbers, it is hard to believe that others do not know. I decided to test it out and so I proposed to a class of 90 people in mathematical statistics that we find a way of using random numbers to shuffle a deck of cards. Although they were familiar with random numbers, they could not come up with a way of doing it, nor did anyone after class come in with a workable idea though several students made proposals. I concluded that inventing such a shuffling technique was a hard problem and that maybe Feller just did not know how at the time of writing the footnote. My face-to-face attempts to verify this failed because his response was evasive. I also recall Feller speaking at a scientific meeting where someone had complained about mistakes in published papers. He said essentially that we won't have any literature if mistakes are disallowed and further claimed that he always had mistakes in his own papers, hard as he tried to avoid them. It was fun to hear him speak.

Although I find Utts's discussion of replication engaging as a problem in human perception, I do always feel that people should not be expected to carry out difficult mathematical exercises in their head, off the cuff, without computers, textbooks or advisors. The kind of problem treated requires careful formulation and then careful analysis. Even

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after a careful analysis is completed, there can be vigorous reasonable arguments about the appropriateness of the formulation and its analysis. These investigations leave me reinforced with the belief that people cannot do hard mathematical problems in their heads, rather than with an attitude toward or against ESP investigations.

When I first became aware of the work of Rhine and others, the concept seemed to me to be very important and I asked a psychologist friend why more psychologists didn't study this field. He responded that there were too many ways to do these experiments in a poorly controlled manner. At the time, I had just discovered that when viewed with light coming from a certain angle, I could read the

backs of the cards of my parapsychology deck as clearly as the faces. While preparing these remarks in 1991, I found a note on page 305 of volume 1 of *The Journal of Parapsychology* (1937) indicating that imperfections in the cards precluded their use in unscreened situations, but that improvements were on the way. Thus I sympathize with Utts's conclusion that much is to be gained by studying how to carry out such work well. If there is no ESP, then we want to be able to carry out null experiments and get no effect, otherwise we cannot put much belief in work on small effects in non-ESP situations. If there is ESP, that is exciting. However, thus far it does not look as if it will replace the telephone.

Rejoinder

Jessica Utts

I would like to thank this distinguished group of discussants for their thought-provoking contributions. They have raised many interesting and diverse issues. Certain points, such as Professor Mosteller's enlightening account of Feller's position, require no further comment. Other points indicate the need for clarification and elaboration of my original material. Issues raised by Professors Diaconis and Hyman and subsequent conversations with Robert Rosenthal and Charles Honorton have led me to consider the topic of "Satisfying the Skeptics." Since the conclusion in my paper was not that psychic phenomena have been proved, but rather that there is an anomalous effect that needs to be explained, comments by several of the discussants led me to address the question "Should Psi Research be Ignored by the Scientific Community?" Finally, each of the discussants addressed replication and modeling issues. The last part of my rejoinder comments on some of these ideas and discusses them in the context of parapsychology.

CLARIFICATION AND ELABORATION

Since my paper was a survey of hundreds of experiments and many published reports, I could obviously not provide all of the details to accompany this overview. However, there were details lacking in my paper that have led to legitimate questions and misunderstandings from several of the discussants. In this section, I address specific points raised by Professors Diaconis, Greenhouse,

Hyman and Morris, by either clarifying my original statements or by adding more information from the original reports.

Points Raised by Diaconis

Diaconis raised the point that qualified skeptics and magicians should be active participants in parapsychology experiments. I will discuss this general concept in the next section, but elaborate here on the steps that were taken in this regard for the autoganzfeld experiments described in Section 5 of my paper. As reported by Honorton et al. (1990):

Two experts on the simulation of psi ability have examined the autoganzfeld system and protocol. Ford Kross has been a professional mentalist [a magician who simulates psychic abilities] for over 20 years . . . Mr. Kross has provided us with the following statement: "In my professional capacity as a mentalist, I have reviewed Psychophysical Research Laboratories' automated ganzfeld system and found it to provide excellent security against deception by subjects." We have received similar comments from Daryl Bem, Professor of Psychology at Cornell University. Professor Bem is well known for his research in social and personality psychology. He is also a member of the Psychic Entertainers Association and has performed for many years as a mentalist. He vis-