MORNING GENERAL DISCUSSION

DEAN: Since this is a conference on experimental methodology, I was very pleased to hear Richard Broughton bring up his control method, which he labeled SCC. I would like to put forward a plea to psi experimenters who are doing computer studies. The computer makes it so easy to do that they give in their results a measured chance score as well as the calculated theoretical chance score. It just makes one feel more confident that the computer's doing what it's supposed to do, but really that the programmer who programs the computer is doing what he's supposed to do. For example, in the study I did on business presidents, we had an IBM card deck of their guesses, then we had a second IBM card deck of the targets and then a computer program to match the one with the other. And we seemed to come up with a significant precognition score, when you use the calculated theoretical chance score. But I was not satisfied with that, I wanted a measured chance score, as well. All we had to do was to take the target deck and make the back half of the cards come in front of the front half of the cards. Then we were matching the guesses against other than the correct targets using the same program, the same experimental deck and the same target deck, but rearranged. We then did come out with a measured chance score. It made me feel much better about the results, that things were going properly.

MCCARTHY: I'd just like to make one comment on Hoyt Edge's proposal for a new methodology. He mentions that parapsychology really doesn't have the laboratories to do the kinds of things that he would like done. Perhaps not all of this sort of thing should be done in a laboratory. The fact is that there are alternative approaches that have been used in behavioral investigations and in other areas that have well established traditions that perhaps can be borrowed from. For example, as Monte Ullman suggested, there is a clinical tradition in psychology and in other areas that has something to offer and there are also phenomenological approaches that have been pursued, so that I don't think there is any need to really totally abandon an experimental approach. I don't think that you wind up completely in no-man's land if you give up some of the familiar trappings and try to look elsewhere for other approaches to knowledge.

EDGE: My suggestion really came out of thinking of traditional ap-

proaches where this has been done, if it's the case that what I'm suggesting takes a great deal of longitudinal study. One tries to think of where this has been done in normal science and one place that it's been done has been in the biological rhythms area. And in that kind of research you've got to have someplace where you can stick people for three months so that light can't get to them and so forth. What I was suggesting is that in some kinds of research that may be interesting; it takes laboratory facilities which are quite expensive.

RUDERFER: In regard to Richard Broughton's paper, the remark was made that computers never make any errors and humans do. I'm a little surprised that nobody picked up on this, so I will. If a computer makes no errors, why can it not be caused to make an error, for example, based on Schmidt's work or any other hypothesis you want? In other words, there can be an experimenter effect with a computer as well as with a subject. In fact, if we look at it objectively, it might be a lot easier for any psi mechanism to work on a computer than it would on a human brain. The human brain has about 10^{10} neurons and maybe about 10^{15} synapses. It is necessary for any psi phenomena to work up into the physical aspects of the brain in order to get a response from the experimenter. So why cannot that same process be applied to the computer elements, which are much fewer in number? They're all man-made and maybe much easier to manipulate from whatever mechanism you want to call psi.

STANFORD: I'd like to make a couple of observations in relation to Ruderfer's remarks. The remark that I'm specifically concerned with is that it seems as though it ought to be easier to affect a computer than it would be the human brain. First of all, even if we could equate a neural system, in terms of it's complexity, with the computer, I think it would be an empirical question for which there are different types of theoretical answers as to whether or not that would be easier to influence. There are people, for instance, who claim that synaptic connections, because of quantum considerations, might very readily be influenced. It depends on your theoretical perspective and of course it's an empirical question. Second, in bringing up the tremendous complexity of the human brain, one should not ignore the fact that the brain is a system with a tremendous amount of redundancy. There are many alternate ways in which the psi factor might encode or produce an appropriate response related to the target. We see many examples of this in actually doing psi research. That may be why it seemed for awhile as though ESP research was a favorite type of psi research and PK was dying on the vine. It may be that the brain has this built-in redundancy that allows it to be readily

influenced by psi. This may be a factor that you didn't consider that may be extremely important in why people are sometimes as psi-sensitive as they are. And then, of course, there's what William Braud calls lability. I have never come up with a satisfactory word for it, but it's the capacity of the elements in a system to be ready to change. We're getting some tentative kinds of evidence that this may favor the occurrence of psi. That may be much greater in a nervous system, let's say, than in a computer. I see it as an empirical question at this stage.

RUDERFER: My main point was that the experimenter effect cannot be eliminated by the use of computers and whether it's easier or not for an ESP mechanism to influence a computer over a brain is really secondary and, in any case, subject to test.

HONORTON: I think what Broughton meant is that computers don't often make errors, but they are capable of malfunctioning. There was an article in *Scientific American* about a year ago talking about the effect of cosmic rays on computers under certain conditions. The memory in the computer can be influenced by extraterrestrial events, not in any exciting way, but these are things to bear in mind. Computers are not infallible devices. They have a lot of redundancy built in and that's why they are so relatively error-free. But computers are not totally infallible. We also have had some curious events. We have a number of machines in our laboratory and on several occasions two or more of them have gone haywire in the same way at the same time. I would raise the question, simply for our future consideration, as to how, if there was a PK influence on a computer, aside from the random generator aspect, how that would, in fact be detectable by us. One final point in relation to Richard Broughton's continuous control theory—I was never one to do that because you can't make psi start and stop within a few milliseconds. But another way of controlling against side bias with random generators is to oscillate the target bits so that you cancel out any gross error.

BROUGHTON: Certainly, I am well aware of how computers can go wrong and how much they cost when they go wrong, too. Generally when a computer goes wrong, which it can do quite frequently, it is in a very obvious way. There may be subtler ways. If, for example, there were PK effects on computers in other than the RNG component, perhaps interlab reliability might help us to isolate that. You have a number of Apple computers there now, Chuck. It will be very interesting if you discover that at night when you are away they talk to one another by themselves!

The idea of alternating target directions is another aspect of computer control. We have done this almost routinely in one way or another in

the computerized work in which I have been involved. I didn't mention it because it is rather basic. When, for example, we wanted the computer to make a decision for a zero on one target, we would let the computer do a sample or a number of samples, then select the opposite alternative as the target, just to control for bias. As you say, one can alternate it. There are a lot of ways of continually checking on biases. But I must agree with you, that computers can go wrong. If they start going wrong in ways which are not as easily testable as they usually are now, we would probably have to start considering the possibility of a PK effect on the machine. Right now, with micro-PK, it doesn't look terribly likely, but if we are to accept as valid certain macro-PK findings, then anything's possible with the computer.