

MORNING GENERAL DISCUSSION

Day Two

SCHMIDT: This is about Susan Blackmore's paper—a kind of non-scientific remark. I was personally delighted with the refreshing nature of your negative approach. I don't care whether you are right or wrong—which Dr. Rao has discussed in detail. But it is something that makes us think and perhaps shakes the sort of complacent state I think we may be in. Concerning the thought that we are on the declining branch of research, I personally share a concern about that, even though I am reasonably successful in experiments. I think there is a very serious threat that we could simply get lost. Richard Feynman, in his book *The Nature of the Physical Law*, speculated about the future of physics and felt as one possibility that physics gets increasingly more difficult such that people just lose interest. I am very much concerned that this might happen in parapsychology. If progress is too slow and too laborious, people will look for more exciting things to study. Thus, there is a big challenge for experimenters to keep things moving. Insofar, your talk in its negative outlook seems most invigorating.

SCHLITZ: I want to go back to Helmut. Yesterday he referred to the potential effect of future observers on the replicability problem. And nobody has dealt with that aspect. I wondered if you would just say a few words about it.

SCHMIDT: The question was emphasized by my theoretical psi model. This model is attractive insofar as psi appears space-time independent and PK and ESP form one unit. But as a side effect, there appears a strong retroactive effect from later observers on a present experiment. To some extent, this effect seems real. It was already observed in the early Feather-Brier experiment. But somehow the model seems to exaggerate the effect to cause conceptual problems. Walker's model turned out to lead to a similar problem. Thus, it seems safe to assume that the models are not quite right. And the role of future observers remains one of the most interesting problems.

HONORTON: This is a general comment but it does apply to Susan Blackmore's paper as well as some of the others that we have heard.

Due to the fact that we recently celebrated the one hundredth anniversary of the founding of the Society for Psychical Research, we have heard a lot in the last day or so about a hundred years of psychical research, but let us be a little bit more realistic, shall we? Up until about twenty years ago there was only one laboratory in the field on a full time basis. And, in fact, the progressive development within the field that occurred in the early stage of development of that laboratory was because there was a full time group that had some degree of funding and continuity. We are only starting to see this spread out a little bit now, to where we can count on two hands instead of one the number of full time research facilities that exist in this country. I think we should talk more in terms of maybe twenty, thirty years rather than one hundred. I don't know how long the American Cancer Society has been in existence, but say twenty years or so, and how many millions of dollars and how many researchers have been involved? Certainly more is spent on cancer research in this country in one year than has been spent in the entire history of psychical research. I really think it is important that we bear that in mind when we decide where time is running out.

SCHECHTER: It seems to me that an awful lot in this discussion depends on what we mean when we say "psi." I am reminded of Marilyn Schlitz' comment yesterday about what might happen if we went around the table and asked everyone to define psi. This is a reaction to your remarks and Sue Blackmore's remarks on the problem of the negative definition.

Negative definition is a problem to the extent that we treat psi as some specific process that is different from the accepted or known modes of interaction. I think it is quite different if we use it as an operational definition, saying simply that we have unexplained interactions and asking what rules they follow, what patterns there are and what theories may fit these patterns? I noticed that Sue used psi and the psi hypothesis interchangeably and I am a little nervous about that. I think it is very important that we make these two uses of the term clear.

BLACKMORE: I did try to be clear about when I meant "psi" and when I meant "the psi hypothesis." I have in fact gone through my paper several times and tried to clarify that one. It would be lovely if we could replace the psi hypothesis with an operational definition. That would do away with the problem of the negative definition. But the trouble is in order to have an operational definition you have got to have some of the kinds of repeatability that you don't

need for doing other things. You have got to be able to say "Here is what I get by doing this operation." And I don't think we can really do that. You can have a statistical operational definition. But I am not happy with that.

SCHECHTER: As I understand operational definition, it refers to the procedures, not the outcomes.

BLACKMORE: Yes, but our outcomes are statistical outcomes. To define psi as "what we get if we do this procedure and it comes out significant at the .05 level," is not going to be a very helpful step.

STANFORD: This is referring to a remark which you made earlier, Susan, in which I believe there is a factual error. You said that parapsychology journals do not publish non-psi research. Well now, this is untrue. Several of us have been studying hypotheses and processes outside of the psi areas that are relevant to the general topic of psi-conducive states. Recently, I published a number of articles on the cognitive consequences of specific types of stimulation during Ganzfeld. Harvey Irwin has been publishing research on cognitive correlates of OBEs in parapsychology journals. And I see that such non psi, but parapsychologically relevant, publication is a growing trend. I do not see a dichotomy. I think we as parapsychologists are going to have to contribute to "normal psychology"; it is necessary for our advancement in psi research.

BELOFF: As an editor of a parapsychological journal, I have occasionally to face this dilemma of whether to publish something that doesn't incorporate any claims of psi or the paranormal. And the way I look at it is that, until it becomes easier for people to publish psi claims in the orthodox psychology journals or other science journals, I ought to devote the very limited space we have at our disposal to those papers that, if not directly about psi, at least bear on the psi hypothesis. That would be my personal way of coping with what I see as a real dilemma, because I quite agree that ideally it would be nice to incorporate lots of other kinds of papers.

BALLARD: My remarks also concern Dr. Blackmore's paper. I think we would all agree that we welcome more and better research into phenomena related to various spontaneous cases. I for one still find the distinction between psychical research and parapsychology to be somewhat useful; parapsychology being the more experimental branch of psychical research. I realize that for many people these terms are indeed interchangeable. I don't think that what we have here is an either/or situation. I think we really need both. I see a strong need for much better scientific work in psychical research per

se. At the same time we need the ongoing research in experimental parapsychology.

Why reject parapsychology? I believe what you are asking us to do is basically to accept the null hypothesis, no psi. Even though technically we cannot accept the null hypothesis, provisionally we can act as though the null hypothesis is true. To that point I would like to point out an article in the *Psychological Bulletin*, July, 1979 by Cook, Gruder, Hennigan and Flay. They say, and I quote, "Provisional acceptance of the null hypothesis depends on assuming all the necessary theoretical countervailing, statistical and procedural conditions for an adequate test of the effect have been demonstrably met." I would argue that we have not met the theoretical conditions and that in terms of our research we have much room for improving the methodological quality, as Dr. Rao pointed out yesterday. In terms of progress, I would like to build on what has been said earlier. We have had a revolution in the past 15 to 20 years, largely as a result of people such as Chuck Honorton, Rex Stanford, William Braud. In my opinion, relative to the past we are really doing excellent research theoretically in terms of psychological theory. I feel that we are making progress. Experimental parapsychology is moving in a new direction, especially in the past decade, which has been very exciting. I look forward to the next 10 or 20 years very much. What we need are funds, well trained people and a renewed commitment to rigor in our research. If J. B. Rhine has a legacy to teach us, it is that we do need rigor in our research and I think that as long as we do that we will indeed make progress. Essentially what I am saying is I see no need to abandon parapsychology as the experimental arm of psychical research.

BLACKMORE: I would like to make it clear that I wasn't suggesting that we should accept the null hypothesis. I quite agree with that quote and that entails quite a lot. I am just suggesting that we shouldn't accept either the null hypothesis or the psi hypothesis. We should be more open minded and just get on with studying the phenomena. I think it is different.

RAO: I don't want to quarrel with exactly what you said now. That is my plea, to keep an open mind, again true to the spirit of the SPR, no presuppositions about whether psi exists or not. With regard to the negative definition, I want to reinforce what Rex Stanford has said, that if the line of research is even indirectly relevant to psi, many of the journals in the field have been open. The *Journal of Parapsychology* published in the last year, three papers

on OBEs which have no direct relationship with ESP. In fact, I like your OBE research; I think you have made a tremendous contribution. I would like to see that kind of research done; I am not questioning that. It is possible to define psi in a non-negative way. As Chuck Honorton said the other day, I think I would say psi is the direct interaction of mind with the environment. The reason why we sometimes use negative descriptors is because we are dealing here with unfamiliar phenomena. You can understand unfamiliar phenomena by contrasting them with familiar phenomena. There is a traditional definition of Brahman in the Indian philosophy. It is defined as *neti, neti*—not this, not that—so that you can understand what it is. In Indian logic there are a lot of discussions about how you could use negative definitions to give an understanding of positive phenomena. Therefore, I see nothing wrong with the way we are describing psi.