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## MORNING GENERAL DISCUSSION

CUTTEN: I'd just like to say that I was extremely interested in Dr. Spinelli's discussion. He is, certainly, by no means the first one who has indicated better results in telepathy experiments with children than with sophisticated adults. It is also indicated that, quite apart from chronological age, the mental age also has its effect, suggesting that, if we had experimenters of low mental ability, they may get better results than more sophisticated people. It is not very clear whether any actual experiments have been done along those lines, but, if this is true (and I've no reason to think that it isn't), at the same time, we hear a great deal of discussion on the experimenter effect. Now in the usual type of experimentation, it's practically always a highly educated, highly cultured experimenter who conducts the experiments. But, with regard to the experimenter effect, would it not be sensible to have a person of low mental ability conducting the experiments as well?

SPINELLI: Well, perhaps you have just answered the question as to why Sue and I have had different results!

SARGENT: If you want a system for testing young children and you don't want them pulling strings, what you want to use is the natural scanning eye movements. This means an ideal kind of response (you're going to need a laboratory for this one—you can't do it at home). Okay, you have the baby just looking at some kind of blank screen. You have a mother in a different location, just looking at a similar screen, and you produce some very dramatic visual stimulus. The mother's eyes will move and scan it and you want to see if the baby's eyes move at the same time to the same portion of the screen. I think some variant of this kind of system is certainly a very easy way of doing it. The problem, of course, is keeping them awake. It is very difficult, when they are very young, to keep them awake for very long. I think that some variant of this system is probably the best system to work with.

SPINELLI: Some years ago, Jan Ehrenwald suggested a number of experiments with infants about two or three weeks old, along those same lines. I agree with you that, to some extent, it's simple. On the other hand, there are psychological difficulties, not experimental difficulties.

SARGENT: What did you have in mind?

SPINELLI: Well, as I was saying, I think that perhaps the best results might be with two or more young infants. Although, the mother-child interaction is one that's interesting.

SARGENT: I think you probably did this with two babies.

SPINELLI: I can't quite remember her results, but one of the other unsuccessful replicators, Hillary Henegan, did obtain some significant results with mother and child, but I can't remember the age of the children. I think they were about three or four or five years of age.

SARGENT: As I recall that study, it was investigating the hypothesis that in telepathy studies there will be a difference between the scores if their mothers acted as senders and if casual assistants acted as senders. Now, she didn't find a significant difference between the two. As I remember it, the scores were actually significantly above chance for the whole data. Now I'm not really sure, that under those conditions, one would say necessarily that was a failed replication. The effects were weaker than they were in your study, but she didn't replicate the familiarity effect. The results, overall, I recall as being significantly above chance at the .02 level. So I think that one would hardly have said that was a failed replication. Perhaps you are not doing yourself justice.

SPINELLI: Well, only in the sense that she initially began trying to replicate, as closely as possible, my own experiments with two young children. And then, having failed to turn in any significant results in that, went on to her own interests, which was between mothers and infants.

BELOFF: It's very significant, I think, that the one little bit of marginal significance she did get in that thesis of hers, was precisely on the hypothesis that she, herself, had invented, rather than the hypothesis of yours that she was trying to replicate.

RONEY-DOUGAL: I would like to make a comment which, I think, encompasses both Dick's talk and Ernesto's talk. Dick says that we must look for a new methodology. Ernesto says that we must now forget about the skeptics and go to a period of observation and participation and, in a way, learn to incorporate psi within ourselves. I think that maybe this is a new methodology that Dick is groping towards. It is possibly the most immediate way that we can push forward into the second century. It encompasses the experimenter effect in that an experimenter who understands psi from the inside can best design experiments to bring out the psi within people that he is working with.

BIERMAN: When I first became involved in parapsychology—it was around 1972—I met Helmut Schmidt. I had just left a physical laboratory where the clean objective science takes place. And Helmut Schmidt told me that he did self-testing. I found that appalling! Well, during those ten years, I changed my mind radically and now I'm doing a lot of self-testing. I think for any parapsychologist it's essential to do self-testing. So, I agree with that, although I don't agree that this is the radical new methodology which we have to find.

COX: I agree, as you have just now said, about the self-testing. It is very useful and, in Schmidt's case, established confidence, which, as was said a moment ago, is very helpful in anyone. If one is confident, then never mind the experimenter effect theory. We put too much weight on that, I believe. I don't think that it is as responsible for some of our findings as we seem to think. But, the main thing is Dr. Spinelli's remarks about doing tests to find out what we don't understand and also freeing ourselves from habits. You mentioned Schmidt, for example, Dr. Bierman. He has implied that PK and ESP can be separated in his equipment. I disagree with that. I don't know many who share my view; but I remember a year or two ago I wrote a paper on the structure of electronic PK which is a critique on that point (not yet published). It was on the matter of whether we can effect PK on electronic systems, or whether it is GESP alone. I happen to think it is the latter, but that is a question that has yet to be settled. So many people assume as Schmidt appears to, that it is PK when you want it to be PK. This actually is a very good point to test, and I have outlined certain methods by which it can and should be done, for both children and adults; I suggest, therefore, that we also use dice with them—not just electronic Schmidt machines. This will yield even better scores, I believe. The average Psi-Q level isn't as great with Schmidt machines as it is with dice and cards. I recommend using Zener cards, of course.

And I'll say one other thing while I'm speaking—that we should free ourselves from habits, as was said by Dr. Spinelli, and take new steps, for it is always helpful to realize that there is a lot about PK and ESP that we don't know. This also includes numbers of phenomenal types of "qualitative" variety, by which I mean direct PK effect upon static targets. I have been researching these for some time and, in fact, an account is now in print.