

AFTERNOON GENERAL DISCUSSION DAY TWO

MAY: I want to return to a really pragmatic situation. We are worrying about methodologies and all these complex issues, yet maybe this is not correct. It seems to me we are ignoring, not beyond just lip service, two very important methodological issues that address any kind of process-oriented research you would like to try to do. One that I myself have looked over, that Chuck pointed out earlier today, is that if it turns out to be true that in free-response experiments there is a great deal of target dependencies and you do not acknowledge that in your research, you have to solve that problem first; because, if that is true, you will have chaos in terms of understanding any of the other process-oriented research. The second one, which is one of my favorite themes, is that you can do experiments in what we will call a precognitive methodology and, if that is true and I certainly believe that it is, again you have a problem of method, of process-oriented research. You have all these complex conditions that you are doing experiments upon. If you really are simply by-passing all that by some precognitive mechanism, you have another difficulty on your hands in interpreting the process-orientation aspect of your experiment. And these are two fundamental methodological issues that I believe have to be solved if we are going to make some really significant progress in process-oriented research.

HARTWELL: It can not be denied that we are making progress. Never before have we used such space-age microphones, clear mark of progress. Sybo, I was grateful for your breaking the field's history into the three sections that you did. It seems to me that we did just as you described. We began long ago casting about and looking out there in a very phenomenological way. Then came the period we credit the Rhines with, where we tried to frame hypotheses very tightly and that was done by-and-large in the context of forced-choice experiments. Then we went to free-response experiments where the goal was in the main to try to cast a broader net, not to press things into such a tight box. It seems to me that the real progress that we have made there was brought forth in Dr. Utts' paper yesterday and in the discussion which followed. The common theme of the questions addressed her

was "could I answer the following?" and her response was always "yes, if you construct your bit list in that way." And so the nexus is in bringing together the broad net that one wishes to have (the more life-like situation in which we wish to place the subject), and the scientific need to tightly frame a hypothesis. The idiom in which we couch that today is "how you construct your bit list." Ed May came right back saying these were the parameters that guided us in constructing the bit list for a particular experiment. And it seems to me that that really does constitute progress. There is some way in which the present state we are at does try to bring the best from our historical phases and see us approach scientifically the sort of real world situation that gave birth to it. So I thought Dr. Utts' contribution was excellent and summarized exactly where we are at in some progressive way.

HONORTON: Most of the methodology that we are using today did not exist in 1968 when the Parapsychology Foundation had its earlier conference in methodology. The free-response work was just getting started, the random number generator work really had not quite yet gotten started. I do not think anyone 20 years ago forecast the extent to which computers would be everywhere, cheap, easy to access and would provide ways of controlling certain aspects of experiments that had not been possible before that time, in addition to doing analyses and looking at pattern recognition possibilities and so on. If you look at the books on parapsychology up through the mid-60s, let us say, you can take a book in the early 60s, a book in the 50s and the 40s and the 30s and you could pretty much interchange them. There really was not much methodological movement at all going on. That is no longer true. I am in the process now of reviewing the Edge, et al. book which was published three or four years ago, finally getting reviewed in JASPR, and that book in my opinion is badly out of date in a number of respects. The meta-analytic perspective had not yet come into its own, there were lots of changes that had occurred in that short period. I would like to reinforce an aspect of Schouten's paper that did not get mentioned orally and that has to do with the importance of bringing spontaneous cases back into the picture a little bit more directly. After all the free-response research had as its primary impetus the idea of developing a more naturalistic experimental approach. One that was more compatible to the way in which the phenomenon seemed to occur in everyday situations. And I suspect that the more we can model experiments on trends that we can find in the collections of spontaneous cases, the more progress we are likely to make. The spontaneous case material I suspect is likely to provide better predictors of laboratory

success than many of the other kinds of predictors that we have used up until now.

ROLL: There is something that we have not covered here and that is the suggestion made a good while ago by Charley Tart that the state-specific approach should be part of parapsychological methodology. That is, the researchers should be able to enter the parapsychological realm in a personal, immediate way as a means of gaining insight into the processes that they are exploring in their experiments. I feel that some respect should be paid to that idea and that is something that it is still worthwhile talking about in a conference dealing with methodology.

BRAUD: Let's open the issue even wider and rather than saying state-specific just extend that to subjective. We can learn a great deal from our own subjective experiences, whether or not we enter altered states. And that was really the thrust of my analogy, that we probably are carrying around in us right now a great deal of tacit knowledge that we are not sharing with others or perhaps only privately. If we look inside, I think we will find a lot of lawfulness and a lot of useful information that is being ignored. At the Mind Science Foundation, we typically participate in our own experiments as subjects, influencers, or agents to get a subjective feeling for what is happening. Those experiences drive our research. Well, yes, I think it is a very good idea. In its loose form, we can do that today; in the more strict form that Charley suggested I think that might be difficult. I am always wrestling with the idea of how to verify things that are not immediately evident to the senses. A lot of parapsychology has to do with that. If it is sensory information, that is rather easy to verify, but in entering this state several researchers come up with content. How do they put that content into words once they have left the state and how do they communicate that to those who are interested only in sensory experiences?

RAO: Yes, I think we have made progress. The proceedings of this conference is a clear testimony of the many-faceted progress that the field has registered in the last several years. This is something that we all can be very proud of. But at the same time I do not think it helps very much, except perhaps it gives us some additional motivation, to say that psychology has not made any better progress, that we are doing better than psychology with fewer people. It is probably comforting to think we are doing better. Perhaps we are. But we should not ignore the fact that today we are too few. The funds that are available for our research are too meager. As Ed said the question is survival. So it is not going to help just to say that we are doing better than psychology. It is not going to help us to say that our effect is

comparable to at least some meager effects in some soft areas of psychology. The fact of the matter is psychology is flourishing. There are more people studying psychology, there are more funds psychologists are able to attract and parapsychology is not making commensurate progress in terms of attracting people, attracting funds in proportion to the practical and philosophical significance of the phenomena we are studying and relative to the high standards of our research. So I think we must ask "Why?" Why is it that after 60 years of continuous experimental inquiry into the field, we are still struggling to survive? I hope the discussions that we have had on methodology here during the last two days have given us some insights into the new directions we must take, new approaches we must make so that whatever success we have achieved would meet with necessary encouragement and necessary support. Without that I think the field is going to be in very bad shape.

HONORTON: I agree with you, Ram, but I think there is one very important sense in which this comparison is more than just morale boosting for us. That is that there has been a very strong tendency for a very long time for people to suggest that the slow progress in parapsychology was due to the intractability of our subject matter. And it is not.

STANFORD: I would like to address two topics briefly. One of them relates back I think to the question that Bill Roll asked. I am not really going to comment on state-dependent science, which I do not think of as a conceptually viable endeavor for reasons that I will not go into here. But I have a feeling that the individuals who are having considerable success as investigators in psi experiments are persons who have personally experienced these phenomena. I can't prove that. I do know, though, that many years ago Charley Tart and Burke Smith did a survey of parapsychologists asking their opinions about the extent to which they had seemingly been able to elicit the phenomena. They found a definite correlation between people's reports that they had had personal experiences and whether they had been able to elicit the phenomena in their own experiments. I have not seen any evidence that that situation is any different nowadays. I frankly believe that some of our most successful investigators are those who have had a number of personal experiences that may be psi; obviously we can't evaluate an individual experience. William Braud was talking about implicit kinds of knowledge that the investigator might have. I quite agree with that. One way we might boost the yield in this field is if individuals who have not been having success in getting psi could find a way to open themselves up to have some experiences themselves. It

might bring some yield. I do not know if this is plausible or not. I mentioned this on a number of occasions, but it is something we might want to think a bit more about. Finally, I think it was Sybo who said we really need to make some advances in certain areas of psychology that can help us in parapsychology. I alluded to some of those in my talk and more explicitly in my paper. Now there are some glowing opportunities for parapsychologists to make contributions to psychology. There are various reasons to do this. One is because we need those substantive contributions. We need those methodological contributions to benefit our own research. But I would suggest that there is another reason we need those things. It is for public relations within the scientific community. I know from experience with my colleagues in psychology who have no special interest in parapsychology that they tend to be open to what you have to say, to respect you and listen to you if they know that you are able to deal in the realm with which they are familiar and that you can make contributions and work there. They can no longer push you off as someone who is a bit too strange to consider seriously. So there are solid reasons why we need to get to work and make some contributions outside of parapsychology. I believe we can do it at the same time as we are doing our psi experiments in many, many instances.

EDGE: It would be interesting to hear from sociologists now.

McCLENON: I am in a strange position because I have attended a lot of these conferences here in America, I watch parapsychologists. I have also seen psychical researchers at conferences in Taiwan and in Japan and in the People's Republic of China gather together and talk to each other. People from different cultures seem to be on different wavelengths. The people in Asia seem to be, to a degree, still in the observational stage. But at the same time they use computers and videotapes and they perceive of themselves as making a lot of progress. It would seem that they *are* making progress because, they getting very, very robust results and they perceive themselves as progressing. They appear different from us in that they have a greater perception of the importance of social networks and the importance of belief. William Braud noted this is something we know we *should* do, but they are seemingly better at doing it. Our response to this is that they are naive and their subjects are fooling them. But ironically the skeptics say the same thing about us in many cases. So it seems to me that maybe we are congratulating ourselves a bit too much. There does appear to be progress, but it seems that the methodology itself restricts the theorizing. I wonder if some of the panel might address that question.

MAY: In a sense that is partially what I wanted to say a moment ago.

I think it is a testimony to the enriching aspect of our discipline that there is room for psychologists and engineers and physicists and medical people and our attention. This is addressed to something that Bill was talking about a little earlier. I am frankly not interested from the nose inward. That is too hard a problem for me to solve. I have to leave it to you guys to solve that one. There is a lot that can be done once you have a stable receiver of psi (and fortunately we are lucky enough at SRI to have that) to begin asking about what aspect of nature in the physics realm allows information to get from point A to point B. So here is a case where the methodologies you have been hearing about primarily are not restrictive in terms of the physics modeling that might go on. So I do not think we should restrict ourselves only to the psychological methodologies or the psychological models. They are important to do, no question, but I think the field has grown to the point to where there is more room for physics and more room for physiology, more room for biology and the more traditional, harder kinds of sciences which are really easier.

MORRIS: Well, this really follows up on both points. I think one of the main things I heard you say there, Jim, was that it would be instructive to look at some of the possibilities of things that they may be doing a bit better than we are. A focus in part was on some of the social network aspects, some of the ways of really sustaining and maintaining enthusiasm. I think this goes along with having us reflect more on just what does go on within the system of our own labs, of our own research endeavors. At Edinburgh part of what we are trying to do is to survey some of the different labs to try to pull from people some of the deeper lore of what goes on in things such as target selection and target usage, free-response judging procedures and so on, and yet even that is pretty shallow. We intend to study psi training procedures and are ourselves subjects in some of the early informal work. We do try to pay much more attention to ourselves as system components and get ourselves to the point at which we are more inclined to regard ourselves as active participants in the subject matter we are dealing with. But that is going to be hard.

PALMER: This is pure speculation, but I have a feeling that somewhere down the line we are going to come up with an idea, a concept, a theory, whatever you want to call it, that is going to be radically different from anything we have now and is going to lead to a breakthrough that will make the current levels of effect-size and replicability seem rather trivial. The point I want to make is that I think it is very important that we be open to new ways, perhaps even radically new ways, of looking at our phenomena, provided that these can be logically devel-

oped and articulated and that they are testable. I am coming increasingly to believe that the observational theories are wrong, but I think it is very good they exist, because they have allowed us to look at our phenomena in new ways. I think Rex Stanford's Conformance Behavior Model has served a very similar function. And as I said in my talk, I think taking experimenter psi seriously is another example. I also believe that systematic development in areas that look promising now is important and should continue; that is really the bedrock of our science. But at the same time we should not allow ourselves to get into a conceptual rut. There may still be some very exciting avenues out there that we do not know about at this time, and we need to be open to them.

ROLL: In relation to some of the remarks made by several people here, including Sybo, and connecting them with the concept of the significance of the goal-directed aspects of psi, I feel that one realm of methodology is in the realm of applied parapsychology. Improving the condition of clients, society, somebody in disease who is having problems, that general purpose pulls you ahead and enriches your work, enriches your attitude and in this case would enrich the field. So what I would like to see is further consideration about the possibility not only of psychic healing, but also the realm of counseling parapsychology or clinical parapsychology or whatever we call it. This is an interdisciplinary realm, this is a realm where our skills, our knowledge have to meld with the fields of medicine, psychology, psychiatry, neuroscience in particular. It is a field where we might do some good, make some significant discoveries and help to place our subject matter firmly in this world.