MORNING GENERAL DISCUSSION

Day One

BRAUD: We should temper all the methodological techniques that we take from other disciplines with the unique characteristics of our own discipline and not take these into our research and attempt to use them wholesale without considering whether they are appropriate. The whole idea of control groups, for example, is something that we might look at rather carefully. We are putting artificial limits upon the capabilities of psi. It may well be that some techniques that work very well in other areas are simply not appropriate here.

One reason for our failure to replicate may be that we are attempting to replicate the wrong thing. There is an idea that has been haunting me for several years now and that is: What does psi do best? We have been forcing psi into a sensory processing mold—we say psi exists to the extent that it can provide information that our regular senses provide. It may well be that just as our regular senses are not redundant—audition provides information that vision does not provide and so on—maybe psi is telling us things about the world that our conventional senses are not telling us. Perhaps we can think about that and maybe develop some methodologies to assess some entirely different aspects of reality about which psi might supply unique information. If we can look at something and entirely know that something through our conventional senses, psi doesn't add anything. However, if we consider larger *relationships* into which events may enter, information about histories or about futures, perhaps dimensions other than those revealed by formal physical properties, perhaps those are the places where we can find some unique contribution of psi and maybe then our replication rate will increase. It is an unusual idea in that we are forced to use our conventional senses at some point down the line for validation, but there may be some round about way to get at this issue.

RAO: I think your first point is well taken. I can't agree more because, while the methodological questions are relevant and important, they can't be accepted when they contradict the nature of the phenomena you are dealing with. So definitely we have to adapt them to our needs, to the characteristics of the psi phenomena. But with regard to your second point, I am not sure I quite understand.

We are working with a dominant paradigm in parapsychology and what I've said here is relevant to that paradigm. At the same time I cannot say that this is the only paradigm and you may not be able to do psi research with a different kind of a paradigm. But I would like to know from you or from others who seem to think that certain approaches might be useful, to tell us what those methods really are. There may be something to it; in fact J. B. Rhine, toward the end of his life, was talking about peculiar parapsychological methods. He said we have been using physical methods so far and to some degree psychological methods to study parapsychological phenomena, but if you really want to understand them you have to use what he called psi methods—real parapsychological methods. I questioned him several times, but I was not able to get an operational kind of answer that I could implement in the research. But if someone can come up with a research idea that can deal with parapsychological phenomena in a nonsensory way—short of doing research in the other world when we leave the body—I would be very, very interested to know how this kind of research can be done. In order to do science we have to go through the process of learning how science is done. And this is, I hope, the kind of training we have all had to some degree. If I am a Buddhistic psychologist I would say that if you want to understand my psychology you have to go through the process of trying to understand yourself and your 128 different states of consciousness. Unfortunately, I can't do it. All that I can reason with is the logic Aristotle gave me and the other kinds of scientific methods and procedures with which I am familiar. Now, if someone can come up with a research technique that we as educated students of science can understand and communicate I would welcome it. But, on the other hand, if it will be the kind of technique that would involve my practicing another discipline for 15 years to train myself to understand it, I'm afraid it is too late for me in this life.

TROSCIANKO: I can't help feeling that parapsychology's critics have made researchers in parapsychology run around in circles and chase their own tails. I don't think this has been done on purpose. I think it is because the critics themselves haven't sufficiently thought through what they were saying. Your rather light dismissal of the question of replicability seemed to me to miss the point somewhat.

Where replicability helps me, as someone who practices psychology, is that it gets me worried if I do an experiment and it can't be replicated or if somebody else does one and I can't replicate it. Now what does this worry do? It forces me to think about the reasons for such a lack of replication and such thinking results in progress. In other words, it says "Okay this was wrong, or I didn't control for such and such a spurious variable which then gave me a spurious result," and so on. The answer is that some form of replication or lack of it allows you to make progress. Now, in parapsychology what seems to happen is a lot of people do studies which are more or less related. Then somebody takes them all together and says there is this level of repeatability, this should satisfy our critics. But the point is that nobody uses this information to develop methodology or to further an understanding of models of the underlying mechanisms. So that repeatability is of a rather sterile nature: it doesn't do anything useful. I think the critics of parapsychology would be silenced far more effectively if people within parapsychology started using their results—global results if you like if the effects are weak—in order to make progress. This progress does not seem to be made at the moment and that is why any thought that repeatability should not be important is depressing to me.

RAO: My only wish is that the people who ask us to do some things in parapsychology would first study some parapsychology. I think that is very important. Someone who carefully read the parapsychological literature in recent years, would not say that parapsychologists are not learning anything from the failures to replicate. For example, why is it that we are paying more attention to Ganzfeld than to some other kind of personality research that was done some time back? It is because here you find a greater replication rate. What is Honorton doing putting all these studies on his computer and trying to see what are the optimal conditions for getting the effect in his Ganzfeld studies? How did he get his 47 number instead of 25 or 35 or whatever number that he gave here? To say blankly without going into the literature that parapsychologists continue to make the same mistakes, they don't learn from their experience, they don't learn from their failures, I think is an expression of one's non-acquaintance with the way the experiments are done. I would agree with you to the extent that I think we should pay more attention to it. We need to make more reviews of our studies and more analysis and comparisons of the conditions so that we can pinpoint which are giving us maximum effect. This is what I think we intend to do and that is what we are doing. Each of us has a major vision of what it is we are looking for. When we fail we attempt to learn from that failure and do the next experiment a little bit better. I don't agree with you at all that most parapsychologists do just one short experiment and then give up. There are those who are continuing in the field over a period of time. If you follow them, you can see significant patterns of learning. When a research project is not very helpful they abandon it and go on to something else, which is a matter of learning.

HONORTON: I think I agree in general with the point you made, Tom. Very often it is not the people who are doing the reviews of the literature who are using them in this way, in terms of attempting to satisfy the critics. I think if you look at most of the global reviews of the literature that have been done, you will find that they have not been oriented toward the critics. They have been oriented toward attempting to develop at least primitive models that can be useful in developing research, rather than attempting to say, "Here, look we have a zillion studies and half a zillion of them reach this level and therefore that should satisfy the critics." I don't think you could find a statement like that in any of the literature reviews of the type that you have referred to. But certainly there are people in the field who have used those reviews in that way.

BERGER: I have a friend who came to my house for dinner and brought a dessert that was so good I asked for the recipe. When I tried to reproduce the treat my tastebuds told me that it was an unsuccessful replication—it was terrible. I asked my friend what I had done wrong. I tried again. Again it was terrible. Finally, my friend came over and supervised by third attempt. "Oh," I was told, "I forgot to tell you to add this!" Not only was my original recipe missing a vital ingredient, it turns out that the order in which ingredients are mixed is also critical!

The process of science is much like the process just described. When we talk about the problem of replication in parapsychology, we are talking about both replicating methods as well as results. It seems to me that we have learned a few of the major ingredients for psi, but are a long way from the complete recipe.

I want to raise a question about the probability of replication being related to the effect size. I must ask "What do we mean by the term *size of effect*?" Technically, it is a statistical measure, specifically the difference between means divided by the standard error, but I don't think that is the way it is being used here. I think the prevalent model for many laboratories is that psi is normally distributed in the population and any random sample should show psi. This model may be totally incorrect.

If the distribution in the population is badly skewed, the random sample may have an effect size of 0 in a given experiment in which a highly selected sample may show an extremely large effect size.

RAO: I think the first part of your point is well taken when you say the size of the effect is related directly to the experiment you are planning to replicate. If your first experiment is with selected populations with certain characteristics, so the size of the effect is peculiar to that kind of a population. Therefore, your replication attempt takes that into consideration and you would not go into a random sample to validate your results. So you do expect that in the given sample the distribution is normal. You have to make that assumption.

SCHECHTER: I think that if we come away with nothing else from these two days, we will have a long list of definitions of replicability. I would like to return to Dr. Rao's comments on statistical replicability and emphasize something you used in describing it, but didn't make as explicit as I'd like to see it. That is the notion that statistical replicability is not the same as the statistical significance of individual studies. Chuck Honorton has already alluded to one variation on that theme. I would like to point out another one that you made use of in your descriptions. You talked about the number of studies where the difference between two conditions went in the same direction, regardless of whether the particular sizes happened to be significant or nonsignificant. In this approach, we take the study as the unit of statistical analysis; this is doing statistics on statistical results and there are new questions that can be asked. For example, rather than simplify counting the number of individually significant studies, we could ask what proportion of the studies was in the same direction when we would expect by chance to have equal sets of results in the various possible directions, rather than a head count of individually significant studies. When we study replication, our data are the studies themselves.

RAO: I agree there are certainly two ways you can talk about it and I think that both of them have their own place, depending on what it is you are attempting to prove.

HALL: Toward the end of his life when J. B. Rhine talked in Dallas on the question of research on postmortem survival, one of the techniques he was considering was whether it would be possible to train a medium to recognize differential sources of information. That is a psychological kind of question, very similar to what one deals with in psychotherapy or psychoanalysis of a person who recognizes different parts of himself which appear with different motivations, even sometimes different language choice. It seems to me that in the question of replication we are always dealing with some state of the psyche or the mind in relation to some measurable event. Where we have the least accurate measurement is on the state of mind which, as we all know from introspection, is continually shifting. What we correlate with are simply gross measurements of the mind and not very fine measurements. I really liked what Dr. Rao said about Michael Polanyi because that structure of focal tacit knowing suggests very strongly that what is tacit in the recipe may be the essential ingredient. My grandmother Hall replicated marvelous biscuits for thirty years until she tried to teach my Aunt Grace, her daughter, how to do it. Instead of picking up flour with her hands she picked it up and measured it and wrote it down and made the worst biscuits that she had ever made. She could not pass that skill to her daughter. But there was obviously a skill and it obviously could be replicated.

Polanyi talked about three different reasons why something is of scientific interest. Parapsychology comes out high on two of those. The first is intrinsic, human interest. I think that that is wide spread about parapsychology. The second is accuracy, which replication would be part of. The accuracy of a measurement that has no human interest is useless. We could measure the average rate of flow of the river out here and no one would be terribly concerned about it even if we were exactly accurate on it. The third of great importance is systematic relevance and that is where theory construction comes in and where small numbers are not too heavy a problem. I have wondered about Ilya Prigogine's talking about dissipative structures that occur at a distance from equilibrium and their application to psi, because in the psychological condition that is set up for an experiment you are simply asking a person to not deal with the usual state of the psyche. It is trained in order to deal with the external world. You are asking your subject not to do that, which throws the psyche out of equilibrium. It may be that the reestablishment of a structure far from the usual state of equilibrium is the point at which psi is measured. I think we may have to find a way to get at these fine movements in the mind of the subject before we can get replicability.

Years ago, Perry Bentley furnished a Bible carried by his grandfather in the Civil War when the grandfather was shot. It was associated in his mind with a lot of things including death. It was an object in a psychometry experiment done by Gaither Pratt in which the subject was Eileen Garrett. She gave in the transcript a whole string of associations moving toward the idea of death, which would have been a hit. And then she said it was like DEG, stopped that whole line of association and went off in another direction. Now, DEG in Bentley's mind meant his friend DeGaullia who had committed suicide. So that it seemed as if there was evidence in the typescript of the sensitive having the right information, but avoiding it probably because of not wanting to deal with the idea of death. Now, it is that sort of thing that I think we will have to come up with somehow. I have no real suggestion about how.

WALKER: The points that I had it in mind to bring out have to do with the law of small numbers and the controversy between Dr. Blackmore and Dr. Rao. Dr. Rao implied that we have to do long experiments in order to get replication of results. Instead we should think of repeatability as being part of our strategy. We accept the fact that we aren't always going to get replication in the experiments, because we don't want to do the very long ones that kill off the effect. We want to do the shorter experiments. We recognize when we do this that we can occasionally miss the effect. Therefore, we adopt a strategy that is perhaps special to parapsychology, just as almost any science has its special little techniques, that improves our overall research effectiveness, but has a reduced repeatability as a consequence.

The second thing that I wanted to say something about was Collins' work and the TEA lasers. This interests me because of the fact that where I work there was interest back in the early 70's in replicating that experiment. There was a lot of interest, money and some of the best people in the world put to work on this. Collins didn't realize that the Ballistic Research Laboratory was another place where replication was attempted and failed. I can tell you in a couple of minutes how you make one of these things. You take a plywood or plastic box, i.e., an insulating block. You put nitrogen and carbon dioxide in the box with a little bit of water vapor in there in proportions that are given, but they are not very sensitive at about atmospheric pressure. You have a whole bunch of electrodes that transversally arc through this gas to stimulate it. At one end you have a 100 percent reflecting mirror and at the other end you have somewhere between 50 to 90 percent semi-reflecting mirror. You switch this on and you will get laser light out of the thing. How can you go wrong in replicating something like that? And yet, with the resources of the military behind it Collins said everybody failed to replicate the first time. In the end, after a great deal of effort, 50 percent went away not being able to replicate this phenomenon and he didn't mention the work done where we were where there was failure to replicate.

SCHLITZ: I had two points that I want to make. The first is in response to Dr. Rao's paper. I applaud this idea of an experiential study of psi. However, you made the distinction between yourself as a laboratory scientist on the one hand and those people who are interested in experiential studies on the other. I would advocate the idea that those two can be integrated and that an experiential approach has an important place within the laboratory setting.

My second point involves a definition of psi. Because we don't know the boundaries of what psi is or isn't, it becomes very difficult to say when we have it and when we don't. I would say that we might define psi as some type of information exchange that is obtained without aid of the known senses. But that doesn't quite address the question of what we mean by outside of the known senses. Where do we draw a boundary? We have people in a laboratory setting using statistical results to try and define when they have psi and when they don't. Well, some of these people don't get results, but yet they continue in their pursuit. What keeps them going? How do they know what psi is? Well, they say, "Maybe I have had an experience in my life and that is enough to make me sure." So you have statistical psi on the one hand and you have experiential psi on the other hand. Then we have the idea that maybe it is something that needs to be validated. That is fine, but what about people who have had an experience that was very impressive in their life, but we didn't have them in a laboratory setting? We don't have objective proof that, in fact, their experience was real. Does that deny the validity of their experience? I think we can all agree that there is a common consensus of what psi isn't, but in terms of really nailing down a workable definition it gets to Tom Troscianko's point about a progressive research program. Do we have an agreed upon definition of what psi is, so that in case we don't have it we can discard that line of research and continue somewhere else?

HONORTON: A person's experience is valid whether psi was involved in it or not. But I think we have a fairly reasonable operational definition of what constitutes psi, which requires, at the present time at least, something reasonably approximating a laboratory situation to confirm. I think also we might be able to go beyond this whole issue of psi being negatively defined if we would agree that there is such a thing as a mind. Because if we define ESP as mental communication, you know this is a negative definition only because we don't know what mind is.

RAO: I would agree with you that we can in some sense blend an experiential situation with laboratory testing. I think we are using experience here in two different senses. If by experience you mean a kind of manipulation, such as keeping someone in the Ganzfeld or asking someone to meditate and have proficiency in meditation and then test him, that is fine. I think that can be fitted very well into the laboratory paradigm. It is a variable, it is a manipulation. But if by experience we mean creating a situation where, unless everybody who wants to understand what this process is enters into this kind of experiential situation, you cannot have an understanding of the phenomenon. That is a different kind of an experience. In that sense I would disagree, even though I do think that you can make a science of it, but not in the same sense I am talking about. In the limited sense of an experience being a manipulation to create a state that is conducive or counter conducive to an expected result, that is entirely testable in the existing experimental laboratory paradigm.

STANFORD: In a discussion of replicability it is really important whether we talk about a replication rate across studies in general or whether we are talking about inter-laboratory and inter-experimenter replicability. Now, say 40 or 50 or 60 percent "replicability" sounds quite impressive, but it is really not quite as impressive as it seems at first blush because there really wouldn't be any controversy about psi phenomena, at least amongst social scientists, if anyone who wanted to get the phenomena could go into the lab, have this recipe we have been talking about and come out with 50 percent replicability. That satisfies most psychologists, but it is the fact that some experimenters go in several times and can't get it that is disturbing and creates part of the controversy. So as we discuss the topic, let us keep in mind that some of us don't get 100 percent, but maybe we get 40 to 60 percent. That is wonderful, but for those who can't get it, it is a different ballgame entirely. We have got to pursue this kind of distinction very strongly.

RAO: I think the distinction is valid to some extent, Rex. But inter-experimenter replicability is not necessarily the basic condition for replication. It is a good rhetorical way to convince people. If a number of people replicate it, then it is easier for people to accept it. But consider a hypothetical example. Let us say that there is an X experimenter in parapsychology. He succeeds every time he does an experiment. Nobody else succeeds. He tries to communicate, he writes all the research papers in the best possible way he can, but nobody else replicates. Now, is this kind of replication by X valid replication, useful replication? I think it is. It is not a useful replication if you are talking about using replication as a rhetorical device to make other skeptics accept your findings. It is useful if we consider why it is that we are interested in replication, given the reliability of the phenomenon. Now, what is the indication of the reliability of the phenomenon? Let us suppose this man is doing research by means of which he can have assured information about the stock market. He could give us assured information about somebody who was murdered. He can successfully guide us to find an oil well. It does not matter if any other person is able to replicate his work or not, as long as this person is continuously successful in employing his knowledge or his experiment to arrive at the kind of data that would enable you to make a prediction which can be cross-validated. This is not a subjective feeling. It is an external objective kind of verification. I think we have a solid replication in that case. There is nothing to doubt about it. But, on the other hand, if there are questions about this man's work and there are no kind of objective data that you can share with him and you still have a lingering doubt if this man had faked his results, yes, to that extent that replication is weak. If he has solid data, even if one man is able to consistently obtain results that would not be otherwise normally attained, I think you have a good case for replication.

STANFORD: I think that the distinction between what is subjective and what is objective, in the sense that Dr. Rao just used it, gets incredibly cloudy. Science depends upon the ability of scientists to reach some kind of convergent conclusions. If we buy this kind of criterion as being of value to science where one person alone can replicate it a hundred times and no one else can get it, it seems to me that science turns into a form of solipsism and nothing more.

RAO: This is a very important point. We are confusing real issues here. The man who placed so much emphasis on this intersubjective observational replicability, Karl Popper, said even one instance is enough. I'm talking here about the father of all this falsification business. It is not necessary for you to have many observations. One observation is good enough if everybody else can share in that observation. If somebody had seen a raven of a particular color, and if there is intersubjective validity to this even tomorrow when this raven is no longer there and you are not able to see another raven of that particular description, that observation is still valid. I'm not talking about a datum that scientists cannot agree on, something scientists cannot communicate. Communication is important. If the result that this experimenter presents is such that other people cannot agree, then we do not have a replication. What I'm talking about is one single instance, one single experiment in which the data were collected in the way they were supposed to be collected, provided you and I who read the report can understand it, provided that the prediction comes true, I don't see how anybody could question it simply because it is a unique event and cannot be replicated.

BLACKMORE: I just wanted to make a comment about what Tom Troscianko said. If I understood him correctly, he was trying to make the point that in many sciences we use unrepeatability as a spur to progress and that this seems not to happen in parapsychology. I think I agree with him that it doesn't, but I think there is a very good reason for this in that in most areas of science which I am familiar with there is not much unrepeatability so you can use it. The trouble is that unrepeatability is too free and cheap in parapsychology. We have too much of it to do the kind of thing he was talking about. And, therefore, that means that it is quite reasonable to do what we seem to be doing and that is trying to get enough repeatability so that the unrepeatability becomes usable.

TROSCIANKO: Sue Blackmore says there is too much unrepeatability and therefore no progress can be made and Dr. Rao said earlier that progress is being made. I'm going to deflect the question. I don't know much about the literature, so I'm going to deflect it to someone who does, namely John Beloff. You said that you are doing some Ganzfeld research and one would conclude from Dr. Rao's answer to my previous question that there has presumably been some progress as a result of consolidating everyone else's Ganzfeld research. Conclusions have been drawn. Dr. Beloff, do you think that the experiments that you or your students are doing now are in any way different from experiments say, two years ago? If they are different, do you think your chances of success have been raised by the differences that have been introduced?

BELOFF: Well, it is not a clear cut question. When one arrives at the post mortem stage of an experiment one makes plausible suggestions as to what might have interfered with getting the positive result. But the cases that I had in mind were not slavish replications of something in the literature. They were attempts often with an extra twist to them. Therefore, it might have been the twist that was wrong, it might have been a dozen things. The trouble is that there are too many possible explanations as to what might have gone wrong to be terribly confident that you know the next time it will come out right.