GENERAL DISCUSSION
DAY ONE

MAY: I am glad you like chaos because I am about to instill some. I have a number of comments that span all the talks this morning, principally addressing the issue of the conference in general. It seems to me that we have a direct problem to face methodologically in that we do not have a definition of our phenomena. ESP is what happens when nothing else could. One of the direct consequences is that we are confused as to who our target population is that we wish to convince. First off we have a problem with ourselves. We are arguing here whether something is real or it isn't. The next population that we seem to be in my view over-committed to are the skeptics at the other end of the distribution. What seems to be falling through the cracks in all of this is the vast middle group of very competent scientists who ask very difficult questions of our own work. I think our methodology would be better off if it aimed at answering their questions. That is certainly the approach that we are looking at at SRI. So for me that is a general problem. And a second comment that I want to make is if (and it is a big if) psi really is considered to be an ability and considering Jessica's comment that therefore it will be normally distributed in the population for a variety of reasons, then it just seems to me, when I look at some of the methodologies used by our colleagues, that summing across individuals to search for a psi effect when you have no indication at all that you are selecting your population from the right-hand half of that distribution is absolutely crazy. To give an example of that, if you locked myself and Itzhak Perlman in a room each with a violin and asked us to sum our results, you would conclude that violin playing is absolutely and utterly impossible. That is not the way we should look at exceptional behavior. It is just wrong to look at exceptional behavior by summing across people. So there is some chaos.

RAO: I think what Dr. May is saying is that if psi is normally distributed, then you cannot really test unselected subjects, because pooling their results would only confirm the normality of the distribution. This argument, I think, misses the central point of much of process oriented research in parapsychology which by attempting to separate hitting and missing scores in psi tests is really aimed at discovering the discriminators that would show whether a subject belongs to this side of the bell or that side of the bell. In working with unselected subjects under various testing conditions we are trying to identify those variables that would throw light on hitting and missing and what circumstance would enable, would highlight, would enhance the ability and what would inhibit it. So in that sense I do not think it is so crazy to work with unselected populations as long as you have ideas of how to discriminate them.

WICKRAM: I would like to comment on two papers—the one on ability and the one on the experimenter effect. The conceptualization of psi as an ability seems quite heuristic to me. I want to take objection to two concepts—one that all abilities have to be voluntary and two that they have to be adaptive. For example, human intelligence is generally regarded as an ability but there are certain conditions under which IQ may be used to induce pathophysiology and, in fact, psychopathology. In other words, people may use their intelligence for self-destructive purposes. So though an ability may generally be used for the purpose of survival, there may be special conditions under which it is used to produce pathophysiology and psychopathology. The other notion is that all abilities are voluntary. For example, there are conditions under which the ability to have erections will in fact induce detumescence rather than tumescence. Attempts to control certain abilities will in fact make them non-adaptive. The next comment is about the experimenter effect. My mentor was Hobart Mowrer. As those of you who are psychologists know, he was an experimental psychologist and a learning theorist. One of the things that he would always tell me is, "Ian, if you want to know which experimenters get positive results in running their rats, you have to ask only one question—do they have to use gloves when they handle their animals?" Those who have to use gloves when they handled their animals usually get more negative effects. So love may be one of the important conditions for producing positive experimental outcomes. Now, of course, the experimental psychology of love is a different issue.

BROUGHTON: I would just like to reply as a little of that was addressed to me. I love that analogy with gloves. I think that Charley Tart really picked up that idea too, on whether we approach psi with gloves on or not. As to your comments about the psi ability, I thank you for your support of my own vague notion of ability. It certainly may not be voluntary and indeed does not have to be voluntary. That is what I have been trying to stress in my own approach to it. As for the adaptive and maladaptive nature of psi, I think that is something which we really have to take into account. It relates to what Dr. Schouten mentioned in his question to me. It could be maladaptive. Keeping that in mind, however, just from a practical point of view why don't we worry about the adaptive uses first and take care of our maladaptive uses later.

STANFORD: With regard to something that Ed May said, I am not at all sure that psi is exceptional. I certainly think there are vast individual differences. I firmly am convinced that in almost any situation there are going to be individual differences in the ability to manifest psi. But I think that maybe one of the reasons we think psi is exceptional is because we are asking our subjects to do things in our experiments that, if you will, are ecologically rather ridiculous. They are not in the kind of context in which psi normally operates. I might make an analogy here to asking a poet, "Give me a poem; give me a poem right now!" This kind of thing is not going to happen with a good poet. I think we had better consider the kind of tasks that we put before people when we ask them to use, for utilitarian purposes, an ability that does not work in that kind of conscious or volitional way normally. The second point I want to make, before we get too far from John Palmer's paper, is that when we talk about experimenter psi we have got to recognize that one of the chief things that we need to worry about is experimenter concerns. Sometimes the most important, fundamental and obvious things are left out of a discussion. And I really can hardly think of anything that is of more concern to a scientist doing research—one who is worthy of the name scientist—than the desire for truth. What role does that play in experimenter psi?

HONORTON: It seems to me that the real importance of replicability is not in convincing critics of the reality of psi. Replicability is important for one reason only. As scientists we can only build on what we can reproduce. Now for 30 years the Rhine school promoted a series of key experiments that were done in the 1930s. These were like great works of art that were hung on the wall and admired for 30 years. Well those experiments long since ceased to be of scientific interest. They were works of art, of historical interest only. To the extent that we are interested in learning what is going on with psi or learning how to apply it, we have to be able to replicate our results. We can not build on quicksand. The other point that I want to make—and this will probably introduce further chaos into the proceedings—is that in talking with a number of people who regard themselves as having been less successful than they would like to be in this area my impression is that they simply have been unwilling to modify their behavior as experimenters in a way that, at least to me, would seem to be more productive in terms of producing results. It is all good and well to work with unselected subjects because they are convenient, because it is easy to do, but that is not where the action is. I do not see, on the part of any of the people who have consulted me about doing ganzfeld experiments, any serious effort to take my advice into consideration. So I am somewhat frustrated as someone who is labeled as a successful experimenter and who feels that more is needed than simply to consult with somebody who has been successful. You have to take what is said seriously and modify your procedures and see whether in fact they are right.

SCHOUTEN: A few comments on the points Dr. May raised. I would be inclined to say indeed we are not so much talking about phenomena, but about paranormal experiences of people. We should try to explain them. That is quite a different thing. What bothers me often in parapsychology is that it is a bit turned around. People have paranormal experiences, therefore we assume there is psi and therefore we study psi. I always found that a bit peculiar, but that is apparently the way it is. Another comment I would like to make is about your suggestion to take the subjects from that part of the distribution where you really find successful subjects. Well, of course, who would not agree with that? But there are two "ifs" in it: one is if it is an ability and the other is if it is normally distributed. I have seen many attempts to select subjects based on that model. We have tried it at Utrecht. I guess there are many experiments where we have tried to select subjects somehow and as far as I know most have failed. The next point I want to make is about Chuck's comment about the unwillingness to modify experimental procedures. Here again I think it is not so much a black and white situation. Most experiments which have been carried out in Utrecht which were not successful have been carried out by enthusiastic students and those students were always encouraged to bring their friends. It is not true that you have this on the one side and that on the other side. It is much more complicated in reality. So I think before we really go into this sort of thing we should know better how exactly successful experimenters did it. I am curious to learn, I am certainly willing to modify my behavior, but you see I can not find it in the literature how exactly to do it.

WALKER: There seems to be a great desire to communicate with other scientists, to convince other scientists, as John Palmer said, to let parapsychology be judged on the standard of psychology by psychologists, by critics. And this goes back to what Adamenko said with regard to replicability, about the great passion over replicability. I certainly understand that as scientists we should try to improve all the aspects of our science, but that is not what is going on with regard to replicability. Instead we want to convince other scientists that we are legitimate. Our preoccupation with this has become almost a pandering to other scientists, other sciences. This is not how other sciences work. A few months ago I was asked to write a commentary for BBS. I made the statement in my reply that if psychologists were to come to the community of physicists, to ask us whether psychology is a science, physicists would say thumbs down. If psychologists went to physicists for their justification, the result would be absolute zero. Yet it is a science that even a lot of physicists are interested in. If psychologists had tried to build their science by continually going to physics and saying, "Are we there yet, are we there yet?" they would not be there. They would never get there. My feeling is that you have to build your own science without any reference to most of the other sciences. They do their work, come to their conclusions and speak *ex cathedra*.

MAY: Harris, I agree with what you said. One aspect that I heard here today was that we have excessive concern for tight experiments or tight methodology. That to me is an oxymoron. You can not have excessive concern for that. I have grooves an inch deep in my back made by my fellow physicists when some of our work fails to take into account an excessive concern for methodology.

WALKER: I understand that.

MAY: You are not going to get the somewhat skeptical mainstream scientist to pay attention to us if we have even the slightest flaw in our methodology. I would not be convinced myself. As many of you know, I am a very strong skeptic about the whole field of psychokinesis, simply because I am aware that the methodologies, in my view, have not been as tight as they could be.

RAO: I think that there is not excessive concern about methodology, but there probably is excessive concern to shield psi from other modalities. The idea is to have adequate, sophisticated, sustainable methods that would give you a larger effect when psi functions in unison with other abilities. Nobody is talking about loose conditions. Nobody is talking about drawing conclusions that do not follow from the data. Good methodology is collecting data and interpreting them for what they are.

PALMER: I want to make a couple of clarifications about my answers to Dr. Adamenko's question. First of all I entirely agree with what Chuck Honorton said about the need for replicability for the purpose of having stable findings that we can build upon. If it had occurred to me, that would have been one of the points I would have made as well. Secondly, I want to go back and defend what I said about the importance of replicability in convincing mainstream scientists. I do not think it is appropriate to call this pandering. Science is a community activity and truth is defined by the consensus of that community. Science is also an integrated body of knowledge. Even though I have argued elsewhere that it does not need to be as integrated as some people think it does I still think much integration is required. So I think it is very important that we get the support of mainstream scientists and not try and go off on our own. There is also a very practical reason for this. We need the logistical support of the scientific community. For example, if we had the support of mainstream science we might be able to have more people teaching parapsychology courses in universities and when they are in universities, have them actually be accepted and not simply tolerated. There are all kinds of interpersonal interactions and resources that would be available to us if we had that support. So I think it is very important; it is not pandering.

WALKER: I think Adamenko is the only person who spoke today who made reference to J. B. Rhine's doing experiments to show psi is real and PK is real. There is such pandering in parapsychology that we are willing to almost chuck the whole episode of Rhine's work because of the critics. We want to appeal to these critics, so when they raise some question about Rhine's work we say, "Well, we are doing better experiments now." Chuck in his article with Hyman [*Journal of Parapsychology*, *50*, 1986, pp. 351–364] almost shoved a lighted match up his rear end with his comment that we do not have any experiments that are fool-proof or adequate. He essentially handed Hyman just what he wanted on a platter. Our moderator here made the statement this morning that we have no theories. This is stated in order to appear to our critics to be as incredulous about the facts of parapsychology as they are. This is pandering to them.

HONORTON: I think that the main thing that we have to focus on in the future is convincing ourselves. If we convince ourselves then we will not have any difficulty convincing other people.

WALKER: That is my point.

EDGE: And I suppose I can say very briefly that I *never* stated this morning that we had no theories in psi. I am not going to give you a chance to respond, either!