

GENERAL DISCUSSION

MORRIS: We have rated all of the color Maimonides slides for thematic cohesiveness on a scale from 1 to 4. I have been able to send these ratings to Carol Irwin in California and could also send our ratings to any of the rest of you. These were rated by two independent people and you could get a set of them that were rated in a similar way by each of the two.

HONORTON: You have copies also of the rating scales and definitions?

MORRIS: Yes, I think that we could put that in. I have just a couple of minor comments. I have been pretty frustrated at being reminded once again of the interdisciplinary emphasis that we find in parapsychology, the fact that each of us really needs to know a lot more than we do about philosophy, psychology, especially cognitive psychology, biology, physics and so on. Part of our need to be more aware in these areas should also be accompanied by the feeling that we may have a fair amount to contribute in each of these areas, to the problems that those disciplines have themselves. I hope that some of the specifics that have emerged today will help us all to bridge those gaps a little more thoroughly.

Also I have been very pleased with the capacity of the conformance model, to which I don't necessarily subscribe, to generate very good hypotheses and especially to focus attention on the measurements of lability, including physical, biological and experiential lability, in ways that I think are going to compel us to develop some new methodology in all three of those areas, to really make the kind of effective progress in assessing that model that is going to be necessary.

I have one question left over for you, William, and that is, in your presentation this morning you talked about developing some procedures for influencing belief systems. In working on the airport project some time back we found that one of the things that was advocated was that people have a very firm belief system of one sort or another. Would you elaborate a little more on what you meant by belief in that respect? How would you attempt to measure the success of any manipulation of people to influence belief and what's the depth of your meaning of the term "belief"? Could this be a casual, temporary modification of the world view or would this have to be something really heavily represented through out the person's life?

BRAUD: Belief involves not only intellectual acceptance of the reality of psi phenomenon, but also emotional acceptance. It means being comfortable with a world view that provides a place for psi. I have thought about working with that in a kind of therapeutic sense, treating an individual as presenting a psi phobia and working only to eliminate that phobia. It soon became obvious that such a "phobia" is very thoroughly enmeshed with many aspects of a person's psyche and any such attempted "therapy" would have to be thoroughgoing indeed. I have thought, maybe, in the context of an academic course, of bringing in lots of different elements, intellectual arguments, demonstrations, experiential evidence, which can all combine over a long period of time, but I am certainly talking about a deep process. A planned approach will be to show subjects films and video-tapes of impressive psi effects before their test sessions and then attempt to assess whether such exposure influences belief in at least the phenomena being shown and tested in those sessions.

DUNNE: I'm a bit uncomfortable that in our attempts to model the psychic process we may be automatically assuming that it is predominately, or even strictly, a cognitive one. There is no basis for such an assumption other than that we know a little bit more about cognitive processes than other human processes. However, the tendency to reduce all behavior to some form of cognition may cause us to overlook alternative approaches. For example, the transient nature of psi input or imagery could result from a reduction in the ordinary cognitive noise which permits a signal of non-cognitive origins to emerge from between the cracks of our cognitive noise, so to speak, somewhat in the manner of dream imagery. This could account for the psi process having the appearance of being of a transient nature. In the light of the discussion earlier this morning about spiritual disciplines, where the training of concentration and attention could be likened to learning to quiet the cognitive noise to permit some other process to emerge which may have nothing or little to do with cognition, I am suggesting that we refrain, as much as possible, from assuming that the language and labels of cognitive psychology define the psi process until we are a little more familiar with what it is.

STANFORD: When we try to come to grips with this we create concepts and constructs and maybe we need to create quite a few more. I didn't see anything in your remarks about this popping out between cognitive constraints that bothers me in the least. But it is a fact, in terms of how we structure our psi tasks ordinarily, that if a person is going to respond successfully there will have to be some perceptions or cognitions developed to match up with the target and that is a sense in which I feel it is necessary to look at them. Certainly in my own theorization I have always been the last to assume that this is fundamentally perceptual or cognitive

or anything of that sort. I strongly suspect that psi can facilitate a lot of things.

MORRIS: When we use the term “cognitive psychology,” we may mean different things by it. It has taken on a highly formal meaning and because we work very strictly within certain kinds of information-processing models when the term is used in that respect, I would share your concern.

BRAUDE: I'd like to address a question to William Braud. This has to do with a hypothesis you advanced in your paper. This is your version, I take it, of the main tenet of conformance behavior, that under special conditions the initially disordered state of the more labile system will become reorganized so that its final state will more closely resemble that of the structured inert system. The qualm I had about that has to do with the notion of resemblance. It just seems that it is gratuitous and that it causes problems for you that you really don't need to have. Even if I were to accept the mechanistic view that ESP is a process that happens between two brains, you need not assume that there is a similarity of structure between any two brains, since first of all, no two brains need to be topologically identical and since, second of all, what kind of state a brain state is will depend on how it functions, rather than on its structure. Similar brain states, then, considered as cognitive states, are similar in virtue of function, not structure and structure does not determine function. The brain states needn't resemble one another in any sort of interesting topological way at all. And as far as psychokinesis is concerned, I'm not even sure what you mean by saying that the state of the target system comes to resemble the state of the inert system.

BRAUD: I think in determining the presence of psi we use some kind of correspondence criterion. We presuppose in our very measurement operation that there is some degree of resemblance in order to say that psi is even there. So I think that the degree to which that resemblance is facilitated is a very important factor.

BRAUDE: I guess I would challenge that. If our evidence indicated the presence of ESP, for example, we might want to compare a person's response to a target. Now, although we might agree that there is a certain kind of correspondence between target and response, I don't know why we have to assume that it's any sort of resemblance, if all we need to require is a similarity of function.

BRAUD: There are many different sorts of correspondence we can address. Formal correspondence is one. We can talk about semantic correspondences and maybe even higher level kinds of resemblances. I could answer your question in two ways: One there may be some forms of psi to which the conformance model doesn't apply. The second one, if we examine correspondences there are many ways we can go about doing

that and we need not restrict them to topological, formal, physical kinds of correspondence.

BRAUDE: I guess that I would just like to let it rest by saying that I don't want to rule out the utility of all talk of resemblance, but I think we have to use it very cautiously.

BRAUD: Yes, I appreciate your point. An extreme case of that would be a very profound macro kind of poltergeist phenomenon. In what sense does moving a 300-pound piece of furniture resemble any kind of template that's in the consciousness of the ostensible agent. But again we can talk about other formal correspondences and we can talk about dramatization and, in fact, if we looked long enough we might find there is indeed an interesting correspondence.

BRAUDE: But notice, whatever correspondences we find will always be determined with respect to some mapping function or rule projection we are operating with in advance. It never will be a relation that exists intrinsically between any two objects. It doesn't exist intrinsically even between objects in geometry, like a triangle. Whether one triangle is congruent with another will depend on what mapping function we choose, whether a triangle, for example, is congruent only with other triangles having the same interior angles, whether it can be congruent with any triangle at all or even with squares or circles.

ROSEN: Since we are examining the conceptual foundations of parapsychology and are up to the general summary of today's proceedings, I thought I would call attention again to a broader issue than the ones we have been dealing with in the last hour or so, I would like to suggest that people involved in experimental parapsychology, as many of us are, might find it useful to hold a mirror up to themselves and question their role in the work that they are doing, their participation, the input they give to the experimental proceedings. I feel we should bear in mind our reason for doing this work, the practical end it will serve. Does this activity lead us anywhere and if so, where? I'd like to get some comments on these issues.

MORRIS: I think that many of us in the field spend some time holding a mirror up in front of ourselves. The question is, what do we see when we look into it? We may not look long enough or hard enough, but I do think we could at least get the mirrors up and turned towards us.

ROSEN: To pose a closely related question, how do psi researchers view the idea of treating the experimental environment as a totality, rather than examining a portion of this environment and making the assumption that it can be cut off from the rest?

HONORTON: I think that the field has undergone a certain degree of paralysis over the last five years due to a preoccupation with the exper-

experimenter effect problem. I don't think there is an experimenter effect problem. I think the experimenter effect is one of the defining characteristics of what we are dealing with, at least until such time as Bob Jahn or someone else engaged in the hard side of the soft side comes up with some physical boundary conditions on these phenomena. We don't have any limitations. It is purely artificial for us to designate subjects and experimenters. There is no way that we can logically, conclusively, in a study where we are looking at a correlation between say, psi performance and the EEG of an individual subject, eliminate ourselves as experimenters contributing to that result. I think this leads us to a situation where we may have to abandon attempts to isolate these effects. We can demonstrate them. We can study them, but it may be totally inappropriate to try to isolate an individual to the extent that we want to more closely approximate something like a pure effect; we need to remove the experimenter as much as possible from the experiment. That's one of my main sources of interest in the computer game idea. It may be that, as Brian Millar would say, there are only four or five people in the world who have psi and these are experimenters and they can be anywhere. We might even be able to continue to influence results of experiments after death. I mean, what kinds of limitations can we impose? I don't think we can do business on that basis and I think we have to acknowledge, at least temporarily, that we cannot isolate the individual sources.

ROSEN: My motivation for making the comments that I have comes from my work in the philosophy of science. From this point of view, I believe the value of psi research is that it helps us to reexamine some basic epistemological questions, related to that. But even more important, it might aid us in reexamining *ourselves*. Perhaps, in the process of self-examination we do have to go through a short-term period of paralysis, but this could lead to far greater flexibility at a later time.

STANFORD: I more or less agree with the spirit of what Chuck said about the experimenter effect. I don't think we ought to get too over-concerned, over-reflective about that; it may be something intrinsic. We can't get rid of it, but it should not be allowed to paralyze research.

I do think there are a few aspects in which we can tighten up things. I presented a paper at the Parapsychological Association Convention in 1979, about how we might be able to eliminate some of the psi-mediated experimenter effects—not all, but certain types, at least reduce their probability. One can conceive of a number of kinds of valuable psi studies—studies designed to elucidate process—wherein it really would make no fundamental difference to the conclusions even if some experimenter psi were involved. Certain of those studies are made possible, however, because we can eliminate, for all practical purposes, the possible influence

of experimenter psi at certain specific loci in the experiment. It was this latter problem with which the 1979 PA paper was largely concerned. I cannot believe that we have ways of eliminating or even reducing such influence at all possible loci, but not all such loci are of equal importance to the interpretation of a study.

Sometimes, however, we hear or read discussion of this problem which is so nihilistic that it casts any serious process-oriented psi researcher into the role of a Don Quixote. Such nihilism ultimately denies the efficacy of the scientific method.

ANGOFF: Our thanks to all of you, participants and observers, for your contributions to this conference. Ladies and gentlemen, this Twenty-Ninth Annual International Conference of the Parapsychology Foundation is adjourned.