ROUND TABLE DISCUSSION

LeShan: I'd like to start by referring to Dr. Ehrenwald's paper. I think, by the way, that this is a tremendously important theoretical paper, and will have a very profound influence on our field in the future. But beyond its crucial theoretical concept, it helps us get past a problem that has arisen in our field and been demonstrated in our journals and has been very much present at this conference. This is the conflict between those who felt that only the spontaneous experiences were really important and those who felt that only laboratory experiments were really worthwhile. You can watch the behavior of the people as the discussion goes on. One group listens to a discussion of spontaneous experiences, personal experience, with a rather supercilious bored air. The other group listens to discussions of laboratory experiments with not a supercilious but rather a dazed bored air.

The depth of the schism was shown yesterday by Carl Sargent's remark that life experience is a poor substitute for experimentation. I think this shows how deeply the conflict has bitten. Ehrenwald's paper seems to make it very clear that there are valid methodologies and crucial importance for both, that both types of work are necessary and only by taking them together can we develop a coherent field. It's only by transcending the conflict and competition that we can see our data with both eyes, see its richness, see its value for human beings.

Sargent: Yes, I agree with Larry LeShan's sentiments, but the trouble is we are a small field. We don't have much in the way of resources and we have to decide what is the best way forward; noble sentiments tend to go to the wall when one is put in that position. It's just a pragmatic question of what we feel we ought to pursue, and I feel that we ought to pursue the laboratory studies and not expend our limited resources on the areas of research which throw up an enormous amount of *suggestive* material and interesting parallels and possible correspondences, as they have done for years and really not get much further than the original insights of Myers and Gurney.

LESHAN: I think we all have to be a little careful in our thinking and realize that our acceptance of one side or another of this conflict, is based more on personality than it is on the intellectual decisions you seem to be thinking you're making. The clarity of this is shown by your personal fantasy of how nice it would be to be able to study ESP for yourself, with no other human beings involved, just machinery. I think what we're dealing with here are the real personality factors and we have to have a respect for each other's views. Chuck is one of the few people, I think, who transcends both sides of it—who sees both the forest and the trees.

HONORTON: I think you cannot separate experiments and life experience. They're both necessary in order to understand the processes we're dealing with. I do think it's very important to make distinctions in terms of what kinds of conclusions can be drawn from what kinds of data. I feel a separation at times from people who are case-oriented or oriented toward large macro-effects that cannot be completely controlled. To the degree that conclusions are reached on the basis of that kind of data, they simply are not appropriate for the level of observation. And that is not in any way to diminish the importance of case studies, of reincarnation studies, of metal-bending studies, or work with mediums—this is all essential. We cannot divorce experimental research from life experience. Thirty-five years of card-guessing has shown the futility of doing that.

HILL: I'd like to bring up something about information and channels and processing of psi-information. These are the concepts, terms and ideas that have been bandied around during the last few days. Don't take this as a criticism of this conference. It's true everywhere, and nobody seems to have come to grips with it, so I'd like to. The idea, of course, has occurred to many people that we can model ESP as some kind of information transmission and processing of information. Some of the people here have made fumbling starts in that way, but I don't see that it's really gotten anywhere. Ten years ago, Charles Tart presented a very nice diagram in one of his papers (ten years later, these diagrams are still there!) about a channel with noise added to the signal, but what is the signal to noise ratio? What are the characteristics of this signal? What are the characteristics of the noise? What is the data transmission rate? As a former engineer, you should supply these if you're going to use these models. The same applies to Chuck Honorton, I think, because he talks about signal detection but hasn't talked about the signals, and again, I'd like to ask what's going on? As you know, I think information is involved, not just in ESP, but also in

PK processes. I won't go into the details of our theory here, but the idea is that if you can pump in information, you can also produce PK effects. Now, Larry LeShan has said that we have to reject our current scientific paradigms; that they're not going to get us anywhere. I say, where is the evidence that our paradigms don't hold up? We should take them to the limit, and see if they hold up, and then perhaps we have to get rid of them—perhaps not. About 1965, a very brilliant radiophysicist in Moscow, Dr. Ippolit Kogan, made a review of the literature on ESP. Now he is a very tough-minded scientist; he wanted to know what was going on and if, perhaps, electromagnetic waves could be involved in the transmission of information via ESP. He reviewed sixty years of the literature and he found only four reports of experiments which contained the data he needed to analyze this situation. As it turns out, one of these was a hoax. Even though he presented his paper in the United States, it fell on deaf ears. Now, there are a lot of psychologists here at this conference. I would like to make a plea to you. Please, even though you don't think it's important, report this kind of data: How much distance was there between the agents? What kind of shielding was there? How quickly did the subject make a response, or how slowly? Then we can calculate on these kinds of data, try to fit them to the model and see if they work out. If we push these paradigms to the limit, that's the only way we can find out whether they'll hold up to scrutiny. I'm perfectly willing to get rid of our paradigms, but let's first see if they work.

EHRENWALD: I feel a little like a person who wants to shoot at a moving target, or a stammerer who says, "l-l-l-ook at the birdie," but the birdie is gone. I think one of the problems is that in our discipline two different types of phenomena are lumped together. The result is that we speak different languages: *A* thinks that the "other guys"—the *barbaroi*—speak some unintelligible mumbo-jumbo. So does *B* on the other side of the fence. Thus, one of our responsibilities is to find a common language to do justice to both the experimenter's and the clinical observer's contributions. Indeed, I believe whatever progress we have made in this direction is one of the most rewarding aspects of this conference. It was more than just a confrontation between the two camps. It amounted to a true encounter.

SARGENT: I think you are right to some extent, Doctor Ehrenwald. I think the trouble has been caused by my saying, in a moment of temporary exasperation, irresponsible things like "experience is no substitute for experiment," and perhaps your making provocative statements like, "experimental evidence is largely based on meaning-

less, if not irrelevant evidence." I feel that possibly we're off to different ends and I think our languages are tailored to different fundamental objectives. So I don't agree with you entirely, but I do think that perhaps we have been a little more confronting than is necessary.

I said earlier that when I tried to get to grips with the psychometric literature, I didn't even have such variables as the mean and range values for personality factors to play around with when I tried to review a model of anxiety I was trying to build. I didn't even have those data. And it could just be easily appended maybe in two notes at the end of the paper, perhaps meaningless variables such as the time of day and what you had for breakfast, but that you never think of as important. Or at least you can have them on file and keep them somewhere, so that somebody might come along with some theory that might sound crazy, but you'd have that data there. I would make a plea to investigators that even if they don't necessarily put it in their papers, that they keep on file as much information as they can get without intruding too much into the subject's privacy about what went on in a given session.

LeShan: I think the concept of a transcending language is critically important. One of the things we have learned from physics (Bridgman pointed this out many years ago) is that when we have two systems, the closer they come to each other, the more the measurements approximate each other, the more they tend to come to the same answers. As in quantum mechanics and Newtonian mechanics, as the systems get larger, the statistical variations tend to produce cause and effect and to measure identically with them. I think we can devise more and more language with meetings like this that meet these criteria. And the second point about what Scott Hill was saying, I would regard this Russian study that you refer to as a complete waste of time. To try to put psi on the basis of electromagnetic waves is ridiculous today. We've been demonstrating the existence of precognition for lo these many years and you're not going to get any electromagnetic wave arriving before it was sent. Leaving out the Faraday cages and everything else, it's simply nonsense to try to go back to this kind of thing.

Honorton: I'm surprised. I thought you and I have been involved in this field long enough to be so totally confused that we wouldn't be too quick to say that anything was ridiculous. I don't think we have a strong or consistent enough data base to be able to totally rule out some of these physical theories and I, for one, am all in favor of pushing the current paradigm as far as it will go. I tend to agree with you that I don't think we're going to find the answers to the psi channel in this way, but we certainly cannot say, on the basis of the data that's been gathered so

far, that we can completely rule out that possibility. Going back to Scott Hill's comments, I'm sympathetic to a large part of what you said, Scott, but when you said that ten years ago Charlie Tart published a block diagram showing signal noise paths and so on, and where have we come since then—read the literature. We've come a long way. Ten years ago we did not have procedures that were producing between forty and sixty percent replications across laboratories. In our own work, in terms of the efficiency of psi, we have cut down the length of a session from eight hours in dream studies to approximately thirty-five minutes in the ganzfeld and relaxation studies. In terms of delineating signal and noise sources, I think we've come a long way toward indicating at least what some of the noise sources are, to the extent that the studies we're doing following these models are succeeding in producing increasingly reliable and stronger psi effects—I think that we have made some progress. We have a long way to go, and it would be interesting to have the Parapsychology Foundation have this conference again ten years from now and look back over the preceding two decades and see what has happened in the meantime.

PLAYFAIR: On this obsession with repeatability of experiments, *no* event can ever be repeated exactly. The Italian chemist Giorgio Piccardi discovered that standard chemical precipitation rates vary both according to the solar cycle and according to whether the tests are shielded from cosmic radiations or not. Now, surely, if you're dealing with a weak and elusive signal like psi—whatever that means—is it not likely that this will also be subject to the same mysterious extraterrestrial forces? These are perfectly real, not mystic speculations. I noted that Dr. Danest mentioned having had a clairvoyant experience just before a thunderstorm. Sargent might not find that interesting, but I find it very interesting. The electrical conditions just before a thunderstorm, as we know, are quite strange, with all sorts of atmospherics and ELF waves. After a thunderstorm they're even better. You have a lot of negative ions flying about and these have all sorts of effects on people about which we could go on for a long time. But my point here is that the exact repetition of an experiment is impossible, and therefore hardly worth attempting.

TART: I'm reminded of a Mulla Nasrudin story where the Mulla was made a judge. The plaintiff came in and made his case and Nasrudin said, "I believe you are perfectly right." The bailiff and the attorneys were terribly upset. The judge is supposed to listen to both sides of the story before making a judgement. So he listened to the defendant's side of the story and again said, "I believe you are perfectly right." And both

attorneys attacked him, saying "That's contradictory!" Nasrudin said, "I believe you are perfectly right." I enjoy hearing these various sides here. Some days I think I'm just terribly uncritical, but I prefer to think of myself as a synthesist who likes to take all of this in. I carry out a case study occasionally, and I love to work with my laboratory procedures and out of it all something sometimes emerges and sometimes dead ends emerge. On two more specific things, I'd just like to comment on Scott Hill's statement. My block diagram has expanded, with some of the specific details filled in. The discovery of what I'm calling "trans-temporal inhibition" is, to me, a major step forward in understanding exactly how noise is filtered out after it's received over the channel. I'm very excited by the implications of that. As for data getting lost, I share this concern. One of the things I'm trying to do with the Parapsychological Association this year is to get a Parapsychological Association Data Bank set up, where the raw data from experiments done in this field by members of the PA will be deposited and so can be withdrawn by other members for later analyses. My main reason for this is that I'm convinced that while our basic kinds of analyses are valid, they're actually too conservative and weak. There's an immense deal of data lying around that's going to get lost in attics or on trains. If we can begin to get this stuff scored, a lot of it can be put in some convenient (probably computer readable) form and we will be able to do a lot more with the data we have put effort into collecting. Let's face it: there aren't a lot of us collecting data.

We are a young, poor, confused field, but we've gotten somewhere. Again, I recall the Foundation's conference ten years ago on altered states. We've made some major steps forward since then. We're still very confused and speculating, but in a much more specific and sophisticated manner than we were ten years ago.

DIERKENS: I wish to present you with an experimental setup and ask your opinion about it. You put in one room two random generators and a computer taking the results. You leave them just alone without anybody (Grey Walter already proposed such an experimental set-up). Then you only put an animal in that room or a subject, trying to guess or to produce PK effect. Do you think that we could have what would be Sargent's ideal? No human being, just two machines? And then progressively put living beings and consciousness in it. That would be, perhaps, interesting. It's just something I propose.

SARGENT: That's always been something which I favored very strongly, and that is to start with your simplest possible setup, find something stable going on, then maybe you can start to build in further

elements one at a time. Probably the system will be disequilibriated for awhile and then it will settle down. It's just like bringing in an observer when somebody has been scoring well—down it goes, but it comes back up. Add your complexities bit by bit. I think in principle what you're doing is exactly what I'm going to increasingly be trying to do in the future.

Guy Playfair's point about Piccardi in replicability is a complete *non sequitur* because Piccardi was showing slight effects of cosmic variables—data from chemical systems which were known to be replicable in the first place. If they weren't replicable in the first place, he wouldn't have been able to discern the effect of cosmic variables on the system, so if he hadn't had repeatability in the first place under ordinary conditions, he couldn't have shown an effect. The second point is, of course, that there are millions of people who have psychic experiences that have nothing to do with thunder storms, so only one instance is not very interesting. But to go back to your point—yes, I agree with you. I think that's a very good setup, and it's one I want to pursue.

PARKER: I don't really see any return to univariate experiments as being useful; it's a sort of return to the 1940s research. There was a lot of research going on with animals, with testing of single ESP subjects, with machine testing, etc., but it never really got us anywhere. I do admit there are some experiments left to be done of the sort that Dr. Dierkens mentioned, but I don't think they should be a major sort of re-orientation of research.

SARGENT: One thing which I specifically propose is that experimenters should act as their own subjects a great deal and then you've eliminated the experimenter/subject interaction, which is something that Rhine never did. You can eliminate fraud by putting a lock on the file; put the data in a computer and then have the data released by the experimenter or somebody else around. That's one thing. Then you've got just one person; you don't even have to worry about the greeting when you're coming in. This is not an attempt to dehumanize the situation. Far from it. I don't want to do that. But I feel that by simplifying it we've got our best chance of getting stable within an experiment, within subject effects. To take a ludicrous example, which I'm not trying to parallel for a minute, the Skinnereans don't even need to use statistics when they have a single operant subject and a very easily assessable procedure. We're never going to get that, but I think we'll get closer to it with that sort of design.

PARKER: I don't think that's a major point of disagreement. I think we all should test ourselves much more. We should experiment on ourselves before we experiment on subjects. But I think most of us do this anyway.

SARGENT: How many people report it?

PARKER: Only the ones that get results.

HONORTON: On this question of eliminating the human being from the experiment, this reminds me of a goal that Evan Harris Walker described to me not too long ago. He wants to build a machine that, when you turn it on, the first thing it does is say, "Thank you." I would suggest that if you want to get results in that setup, before the human being turns on the equipment and leaves it, he should bless it. When we do control studies on our random generator without an observer present, our normal procedure is to cross our fingers and leave the room, because, given the increasing evidence for nonintentional psi, it becomes very difficult to think that by leaving the room, by going into another building or perhaps even to another city, you're eliminating your own influence or what Jule Eisenbud has referred to as the "mind print" of the investigator.

TART: I believe Ed Cox is already doing something like that. He builds one of his new, complicated psychokinesis machines with shifting targets and he simply says to the machine, "I want you to succeed," turns it on, leaves for a few days and comes back and finds he has significant results.

STRAUCH: To comment on the controversy of clinical versus statistical approach, this is a rather old controversy in psychology, as you know, and I feel it's a dead-end road. And if we think of our own work, we always have to take into account the knowledge accumulated from those extreme ends of various approaches. How could you ever think of designing a quantitative experiment without considering the knowledge which has been gained from clinical case studies?

SERVADIO: I'd like to make a few remarks about what Jan Ehrenwald said before regarding semantics. For many years I've heard laments about semantic imprecision in our research in parapsychology as well as in other disciplines. I think it was a French philosopher, Condillac, who said that science is a well made language. But we go on using very approximate terms and today we have had examples of our semantic difficulties. But nothing is being done about it. We should start doing

something and not lamenting every now and then that our semantic approach is difficult and we use terms like "extrasensory perception," which is not extra and not perception and not sensory. Like the holy Roman empire; it was not holy, not Roman and not an empire.

HILL: I'd just like to correct some misimpressions, if I may. Chuck, I like your experiments very much. I wasn't criticizing them. I think Tart's experiments are excellent. What I am saying is, where is the model? Where is the theory? I mean, do you have one, or don't you? If you have a model, let's see the numbers, let's see the predictions. That was my point. About Larry's comment on whether or not electromagnetic fields have anything to do with ESP, I can't understand why you made this statement, knowing Eileen Garrett as you do and knowing that Andrija Puharich amassed a great amount of data indicating that electric fields *do* have something to do with ESP. What I'm saying is if they don't, where is the evidence? Kogan looked for it, he couldn't find it. It simply wasn't reported. I looked for it; I have studied the literature. It simply isn't there.

HONORTON: Scott, let me give you a copy of my paper since you probably don't recall it. I presented a fairly detailed model of noise reduction. I think we have to differentiate two levels of theory building here. We have the real parapsychological problem which is, how does information in one point of space or time get to the subject? I consider myself totally incompetent to deal with that question. I will give you physics boys the benefit of the doubt for a few years to see if you can come up with some way of dealing with that. My concern and my area of competence, involves the question: once the information is available to the subject, how is it processed? How is it outputted? That's what I'm limiting myself to. I have absolutely no idea how the information gets from point A to point B, what the channel is.

LESHAN: You spoke of the Garrett experiments in the Faraday cages. I was the one who dug those out of the files and had them published, so I can speak on them. They proved just the opposite. They proved that the electromagnetic waves had nothing to do with it. I'll tell you a brief story about that. She was in one of these cages—they were made of one inch pine surrounded by electrical shielding. They were airproof. She could only stay in them thirty-seven minutes, so all experiments were held to about twenty minutes or thereabouts because the air ran out after that. And she did much better working inside these cages than inside of another cage and the outside. And once I asked her, "Eileen, how come you did so much better?" And she said, "You know, it's so

cozy and nice in there. I just snuggled up and it was a pleasure." So much for your electromagnetic waves.

GERSTEN: I think it is very important to know this, otherwise we can not be sure that telepathy is related to new unexplained phenomena. By definition, physics is the science that deals with the fundamental laws of nature and parapsychology has to rely more and more on physics. We should not assume that our knowledge of physical laws is complete; new fundamental laws may yet be discovered. If in parapsychology there are hidden new fundamental laws of nature one has to learn them. It is very important to select new ways of experimentation, to suggest new ideas, new extensions and new realms of possibilities. That's why it is important to make experiments with the Faraday cage in order to eliminate some phenomena and to concentrate on new possibilities. I think physicists should make theoretical guesses, but at the same time they should suggest new experiments to test their ideas on what is behind the paranormal phenomena and what are the new physical laws which govern them. Of course, physicists cannot themselves deal with all these phenomena because we are dealing here with human beings. Therefore, there is a need for an overall cooperation of people representing different fields of science such as psychology, physiology, physics . . . everyone can contribute here. I think the future parapsychologist will have to be familiar with all these subjects. I think that it would be very advantageous if meetings between people interested in paranormal phenomena, representing different branches of science, would take place more often. I do not think there will be a serious breakthrough unless the physical, physiological as well as psychological aspects are understood simultaneously.

HONORTON: I agree very much with that. I suggest that we not only give up the term "paranormal," but that we give up the prefix "para-" and start talking about "psychophysical interaction." That's really what we're dealing with. Psychophysics, I think, is the most appropriate way to describe the field that we're dealing with. A parapsychologist can be a physicist, he can be a biologist, he can be an anthropologist, he can be a psychologist or he can be a school inspector, as F. W. H. Myers was. But what ties it all together, is that we're dealing with a relationship between internal mental processes and aspects of the external physical world and we're looking for the source of interaction between the two. Now, going back to Fechner's original conception of psychophysics, he differentiated between inner psychophysics and outer psychophysics. He was more interested in inner psychophysics, which is really what

we're talking about—the relationship between mind and body, between internal processes and the external environment. What psychophysics has become in psychology is outer psychophysics, a very limited area of sensation and discrimination. I think there are probably more of us than there are of them, or there soon will be. I would really strongly suggest that we consider giving up the term "parapsychology,"—particularly, since all manner of occultists, since the AAAS adopted us as an affiliate a few years ago, have been calling themselves parapsychologists—and start using the more accurate descriptive term "psychophysics."

TART: I will put out a deliberately challenging statement and I hope all you physics boys will prove me wrong. I mean, some of my best friends are physicists. At the present state of our knowledge, I don't think anyone can name a physical variable that has any consistent effect on paranormal phenomena, yet we can name a lot of psychological variables that have at least occasional effects on psi performance. As for the psychological part of parapsychological work, we know something about the processing of psi information once it is received. What happens in the "para" part, how it gets from there to here, or here to there, I don't think we know anything definite about. I hope that in ten years the physicists tell me they've got it all figured out, but, meanwhile, I don't know what to do with the physics part of it except listen to physicists talk. They have such wild ideas about the nature of reality that it's very stimulating.

JANIN: Just a brief comment on Honorton's comment. I am in full agreement with what you just said, namely that we could drop "paranormal" or "parapsychology" and use instead "psychophysics," since parapsychology is, in the full sense of the word, the discipline which studies psychophysical interactions.

SARGENT: I think adoption of the term "psychophysics" is likely to lead to confusion since there is a well established branch of science which already claims that name. I agree that we ought to drop the name "parapsychology." Why don't we just call it "psi research"? That would seem to me to be a better bet. It's a term that's already there, so we don't have to use psychophysics, which is confusing, or paraconceptual, which is just going to be a good stutterer. Why don't we just stick to psi research?

EHRENWALD: I think that it is not a question of what name we should call the baby, but what sort of person he should grow into; where should he go and what frame of reference he should fit into. What I can

see is that the laws of modern, post-classical, relativistic or quantum physics are fully capable of accounting for one half of the phenomena we are studying. That half requires a probabilistic approach and a loosening of causal laws. Such a paradigm gives us a modicum of understanding of experimental, micropsychological aspects of the phenomena. The other half has to be geared to macropsychological aspects. It has to do justice to spontaneous, holistic aspects, geared to human personality as a whole. Such a model has to be designed by the psychologist or psychiatrist, not the physicist.

I agree with Charles Tart in that respect. What is needed is a new paradigm, a total revision of our concept of personality as a closed, self-sealing system, operating in Newtonian space and pre-Einsteinian time. This traditional, classical concept of closed personality has to be abandoned and replaced by a new post-Freudian, post-Newtonian, non-Euclidean paradigm. Yet I'm afraid this can only be done if we are ready to do some pretty difficult exercises in mind-bending and mind-stretching. We have to take a new look with the third eye, as it were, to listen with the third ear—or with the right hemisphere—to an entirely new "music of the spheres" or hemispheres.

HONORTON: I agree with you Dr. Ehrenwald, but if you have a baby girl and you call her Hugo, it's going to have a significant effect on what kind of a person she's going to turn out to be. I think that this is an important question—not, perhaps, one that we should spend too much time on now, but one that we should keep in the back of our minds.

SARGENT: I think we ought to stick to psi research. It's relatively neutral and it's like calling the baby Vivian and then you can change the spelling as you like when he or she grows up.

CLOSING REMARKS

ANGOFF: I will revert to the poet in these last few moments, and so I will say our revels now are ended, but you, our actors, were not spirits and you did not melt into thin air. Indeed, you are not only here after ranging so far over and within the provinces of man's changing states of consciousness, but your words are still with us—in the machine, to be sure, and on tape—but soon to be put in black ink and later in type, and later than that, in a book which, with your cooperation—and that's most important—you will all receive about ten months hence. We shall

first send you copies of your remarks. You will have an opportunity to correct, to alter, to delete, before you submit to posterity your views on psi and states of awareness.

The Parapsychology Foundation has again been very pleased to have brought you together, and the Foundation thanks you for coming from so far, and for some of you on a little metro ride from so near.

Ladies and gentlemen, this twenty-sixth International Conference of the Parapsychology Foundation is adjourned.