ROUND TABLE DISCUSSION

ANGOFF: I should explain this discussion period. It will provide an opportunity to look back at our two days of deliberations, to express your general comments, and to ask your remaining questions which have some relevance to the papers presented. You may address your questions to particular persons, or you may make some general observations. But we do have to have a bit of organization, so I'm going to ask the participants and observers once again to address all requests to the chair. Let us start with Professor Whiteman.

WHITEMAN: The point I have in mind is not exactly a general comment. I would like a question answered, one which I think very important. The question is, whether Professor Chari's "Comments," which were not read but were circularized, are going to appear in the *Proceedings*? They include, I think, four specific charges against my paper, in good spirit, of course, but I think these charges ought to be answered if they are going to appear in print. Otherwise people who buy the book might be left with wrong impressions concerning my paper.

ANGOFF: Yes, Professor Whiteman, it will appear, and you are free to make your comments on it now.

WHITEMAN: Good. I am sure my friend Professor Chari, with whom I have had very amicable correspondence, will not take it amiss if I say that I think he must have felt a bit hurried when he made these interesting comments in time for us to read them. There are four which I propose to answer.

First, he says, "It seems to me that Professor Whiteman makes no significant advance over the usual Copenhagen interpretation by talking about potentiality." I would like to list five specific advances that my papers make over the Copenhagen interpretation. One is that I reject the four redundant postulates of von Neumann, especially the projection postulate, as being incompatible with the phenomenological analysis of quantum theory. Thereby, as I maintain, nearly all the paradoxes fall away.

Secondly, I adapt this modified Copenhagen interpretation to quantum field theory, so that there are some further significant changes. Thirdly, there is the theory of "backward extrapolation," which is explained for the first time in my paper published in *Quantum Theory and Beyond.* Here it is insisted that the state vector must be operationally defined, and that it can only be so defined by working backwards from a statistical classification of observations. The conception of two interactions and a backward extrapolation between them does away with the EPR paradox and other obscurities.

Arising from that is the question of completeness, the establishing of which was the chief object of Einstein and his collaborators in devising the EPR paradox. The question was whether quantum theory is consistent in providing for a latitude or uncertainty of events, according to the ideas of Bohr and Heisenberg. In some of my papers I deal with this problem by distinguishing between overall potentialities and small-bias ones, and I think this is also an original idea.

Fifthly, there is the introduction of an "ideal-purposive" level necessary in my mind in order to permit boundary conditions to be established, without going beyond the bounds for a scientific theory. These five points, I think it will be agreed, make advances over the usual Copenhagen interpretation.

The second objection which I want to deal with concerns precognition. Professor Chari says, "I doubt whether the hypothesis of a precognized event in cosmic time can take care of precognition in all its experimental ranges," and he remarks on the case of animals.

Here I will merely comment that I did not make known these ideas on precognition until I had tried out the theory with every kind of difficulty I could think of—the intervention paradox, free will, and so on—and satisfied myself that the theory would hold water.

Thirdly, Professor Chari says, "Whether all the spaces and times hypothesized by him exist or not, they may describe the phenomenology of some ill-understood out-of-the-body experiences."

It is not correct, however, to say that I have "hypothesized" such things. The method of the paper is analytico-deductive, and therefore the possibility of nonphysical spaces and times is put forward in the first place as resting on evidence. The fullest evidence is of course from out-of-the-body experience, and in reference to the assumption that this is "ill-understood," all I can say here is that it is surely unlikely that when someone with a scientific background has been experimenting with techniques in a certain subject over a period of forty years, the subject remains so ill-understood by him that all ideas he has on it are unscientific. The subject, I maintain, can be put on a scientific basis when current misconceptions are exposed. And in addition to work I have already published I have other papers which will appear in due course, embodying material which I think a student of the subject will need to study carefully before taking a stand.

Now we come finally to Professor Chari's first point. I put aside the references to Gödel's theorem, since this is a matter of one-level symbolic logics. The rest of the criticism seems to be an objection to my bringing sayings of the Upanishads into relation with a scientific theory. This puts me in some difficulty because it would take rather a long time to answer the criticism at all lucidly. But the "challenge" has been made, and must be met.

Near the end of his first paragraph of comments on my paper he refers to the *parāvidyā,* or higher knowledge, and says that since the higher knowledge "stands over and against all mathematical systems . . . all means of knowing are but the lower knowledge, the *aparāvidyā*." The suggestion seems to be that what is referred to in the Upanishads is so much a higher knowledge that it makes no contact with knowledge in any scientific sense of the term.

On this matter I should like to remark that the classic reference to the distinction between the *parāvidyā* and the *aparāvidyā* is the Muṇḍaka Upanishad, in which the branches of the lower knowledge are actually listed, and include such subjects as pronunciation, grammar, prosody, and astronomy. These are passed over as preliminaries. Elsewhere in the Upanishad the *parāvidyā* is described as Brahmavidyā, or knowledge of God, and the whole Upanishad is in effect a treatise on how this higher knowledge can be arrived at. You will notice also in my quotation from the Bṛhad-Āranyaka Upanishad the plain statement, "He who knows this," that is, the cycle of 16 *kalās,* "is himself that Ruler of Creation with 16 parts." The theme running through all the Upanishads is that these things, referred to in my paper, can be known and taught.

Professor Chari also says that the Ātman is "the theme of the higher knowledge" which "stands over and against all mathematical systems," and he asks, quoting a famous phrase in support, "By what means can the Knower be known?" In popular theosophy Ātman is the first principle of all, usually rendered as Self. In the Upanishads, however, it is subsidiary to Brahma or Puruṣa, that is, God or Divine Man, archetypal humanity. Ātman, literally Spirit, is the principle of creative consciousness and life, which "is moved and moves not," as the Īśa Upanishad says, and releases us into the Divine when known. Thus Ātman begins with the unchanging ideas of the Godhead, of Brahma, and paradoxically carries us into created life, or conversely.

On the one hand, therefore, there is Ātman "beyond duality," which is spoken of in Professor Chari's quotation. On the other hand the

Upanishad reiterates, in the verses preceding this, that "When Ātman is seen, heard, reflected on, and known, then all this is known" (B.A.Up., IV.5.6).

"All this" (idaṁ sarvam) means the entire system of the world, thus stated to be knowable when released in the Spirit.

KOESTLER: I would like to make one specific and one general comment. The specific comment is on Professor Feinberg's memory of the future. I have to put my comment in a simplified form. I wrote a paper on memory called "Abstract and Picture Strip," in which I distinguished between two types of memories—the long term memory which abstracts, encodes, skeletonizes, and classifies memories; and the vivid detail which remains like a picture strip, even if the context is gone.

Now, your model could work on short term memory. In long time memories, leaving aside the vivid detail type, in order to work, it would have to satisfy the following condition: When we remember a play, a movie we have seen, or a book we have read, as we progress in time, this memory must become more and more a summary, abstracted, classified, cross-referenced, and so on. Long term memory is a dynamic process, not a tape recording. And the more remote from the original impression, the longer the time elapsed, the more abstracted it becomes.

So your model would have to satisfy the opposite condition. This means that if I predict an event, which will happen to me three years from now, this prediction would be as shadowy as a three-year-old memory of my past. In two years time, it might become more vivid; twenty-four hours before the event, it would become very vivid and immediate.

So it would have to satisfy this law. However, I do not see any evidence in the literature on predictions that they become more vivid, colorful, precise, or detailed, the closer they are to the event predicted. That is my specific comment.

I also have a general comment . . .

FEINBERG: May I make a remark? That is a very interesting point. I haven't explored the question in general of what other things you could predict about precognition, if the model I suggested was right. You are saying that short term memory has other aspects to it besides being short term, and therefore, if precognition is an analog to that, it should share those aspects. I think that is a very interesting point which should be followed up by looking at the predictions which have been made and seeing what characteristics they have.

KOESTLER: May I proceed to my general comment? Last year I suggested the subject of this conference to the organizers.

ANGOFF: Yes, I would like to confirm that. Arthur Koestler was the first gentleman to say, "Quantum physics and parapsychology would make a good conference," as it has, I think.

KOESTLER: But I only mentioned it because what was in my mind at that time was a sort of general feeling which is in the air, that the apparent absurdities of quantum physics, I say *apparent* absurdities of quantum physics, make the apparent absurdities of parapsychology a little less preposterous and more digestible.

But this is a purely negative analogy. Both quantum physics and parapsychology go against common sense, go against Newtonian mechanics, conventional space-time models, and so on—but that's a negative alliance.

So the question is, can one discern signs on the horizon for some more positive gettings together?

Various models have been produced at this conference. They are either purely phenomenological models—there is the very honorable refusal of Puthoff and Targ to try to give any explanation of these distance-target experiments; or they are micro-models.

The big step, the big problem is, how do we get from the micro to the macro? How do we go from Schmidt's machine to the poltergeist which allegedly pushed chests of drawers weighing 80 kilos?

Now, I see, this is a big step, of course. That is the central problem. But I see one danger, which came out a little in Puthoff's presentation, when he said, "We are not offending, we are not violating physics, only the laws of probability. . . ."

This seems to me very dangerous, because it begs the question. If I put a teakettle onto the gas stove, or to be more realistic, if my wife does it, and then I forget to strike the match, and the tea nevertheless boils, then I do not care whether I only violated the laws of probability and mobilized Maxwell's demon to direct the Brownian motions, or whether I violated the laws of physics.

I could not care less about the anti-probability results which one gets in some series, whether they are 10^{-26} or 10^{-36}. I call it a miracle, when that happens. So that seems to me no way out.

The second danger is that of putting all one's money on the laws of probability—Johnny von Neumann called probability "black magic." Probability itself begs the questions of causality. Nobody mentioned, characteristically, the term, "the law of great numbers," because everybody feels very uneasy about what these laws are and what their foundations are.

So we are trying to get rid of one headache by buying an equally big one or an even bigger one, if we play on probability theory alone.

So my feeling was, and several participants shared that in private, that the models and theories presented deserve the same censure or the same reproach which Niels Bohr once directed at Wolfgang Pauli. He said, "Your model is crazy, but the trouble is, it isn't crazy enough."

So there is now a radical wing in parapsychology, a sort of Trotskyite wing, of which I am a member, with Alister Hardy and others, who are trying really radically to break away from causality, not only paying lip service to the rejection of causality, or confining this rejection of causality and determinism to the micro level, but who really wonder whether a completely new approach, indicated by holism, Jung's synchronicity, and so on, might not be theoretically more promising.

The real trouble with changing to this new approach, which I do know a lot of people would like, is that there isn't even a beginning of an equation, an *Ansatzpunkt,* as they say—a starting point, for any quantitative approach; and the second trouble I think is reflected in a passage by Piaget, "Two children are talking to each other, and one says, 'Why doesn't the sun fall down?' The second says, 'Because it's yellow, silly.' "

It is our nature to ask why, warum, pourquoi? It is one of the first abstract words which appear in the baby's vocabulary. And this "why?" is asking for "becauses." And because we are always looking for becauses, we are captives of this traditional way of thinking, even if we think we have broken away from it.

PUTHOFF: There are certain ways in which I in fact agree with you, that probably did not come out in what I said, because I only went so far. When I made the statement that I do not think it is physics that is being violated, but maybe rather probability, I was talking about the mechanism of the apparent violation of macroscopic observables. But I did not go the next step, which in fact I have my own ideas about, from observing the phenomena, and also talking with the people who do so well. This is basically the idea that, when I say probability is affected, that really is an effect which comes from a more senior cause. And if I had to state it in a single sentence, hoping no physicist would read what I had to say, it would be that basically I see the universe moving much as expressed by Professor de Beauregard, in that you have a number of monads interacting, and at that level, it is entirely a causal creation by individuals or beings, or however you want to say it; and furthermore it is only their neglected causations which result in statistical probability. Therefore, when one of the monads declines to neglect his creations quite as much as he usually does, that gets expressed in an apparent shift away from the probabilities as we usually see it. So, in fact, I see the probability laws themselves strictly as a fallout, based on the law of large numbers, of a whole number of creative acts by monads, which, by and large, are not very focused. So when I back up from physics and say, "Well, it's the laws of probability," I have really only gone half way. If I were to say it in a forward direction, I would say, "You have the monads creating at a certain level generally, and their neglected interactions result in probability, and then the law of large numbers and probability results in physics as we know it. And then the creative act of, for example, producing a psychokinetic event is an act in which one of those monads takes a little more creative action, and therefore reduces his contribution to the statistical interaction which generally results in physics."

So it is not just dickering with the laws of probability around the edges. It is really that I see the probability that we generally observe, which finally results in the physical laws, as themselves being totally derivative of this more basic creative activity, much in the way that de Beauregard speaks of it.

TARG: Just very briefly—I sympathize with Arthur Koestler's comment that we do not see many equations being proposed for the explanation of psychic phenomena. But we have come out of the state of the sleepwalkers. That is to say, we have managed to turn on the lights in the séance room. But before Kepler could write his equations explaining how it all worked, it was necessary for Tycho Brahe and Copernicus to make fifty years of measurements. And I think we are still in the measurement taking stage. I have felt that my function here, although I am not a quantum physicist, is to match physical theory, to make sure you have some physical data so you can check the things that you understand in physics with the things that you consider observables in physics experiments. Maybe in another two years, we will have an adequate data base so the data itself will allow the formulation of equations.

KOESTLER: May I just comment? Yes, I think that is entirely satisfactory as a declaration of intent. My only objection is to the term "monad." Leibnitz's monads had no windows, whereas these entities are nothing but windows.

SCHMIDT: May I come back to a more down-to-earth question? Professor de Beauregard had promised to give a specific example of how the Einstein-Podolsky-Rosen experiment could be used in parapsychology, and I think several of us find that this might be a good thing to get, to link physics and parapsychology specifically.

BEAUREGARD: My feeling is that the problem is merely one in the interpretation of quantum mechanics, systematically using the intrinsic symmetry between past and future. I am convinced we do not need any more equations to understand the specificity of parapsychological facts.

KOESTLER: I would like just to correct one misunderstanding. I did not say, there are not enough equations. I said, I belong to a school, this synchronicity type of thinking, which, alas, has not even the beginnings of an equation. There is a difference. It was not directed against . . .

BEAUREGARD: Perhaps we agree. It is a proposal. Of course, the first big question with the EPR paradox is, if we increase the distance between the two recording apparatuses, will the distance correlation drop or not? I agree with Professor Piron that perhaps this might be the case. Perhaps, but not certainly, however.

PIRON: Experiments have shown if the distance is very large, the two particles will scatter and afterwards data correlation will diminish.

BEAUREGARD: Yes, so I believe the experimentation that is going on should be pursued. Suppose that this correlation occurs at a distance which can be sufficienty large, say perhaps one meter, or so. I want to test the point whether this correlation goes straight along the space-like vector (which I do not believe), or rather along two timelike vectors, in the region of space-time where, let us say, the atom opens into parts: It emits a positron on one side and an electron on the other. So, I propose to test if really living beings can excite and produce advanced actions, which will result in an observable correlation between this point and that one. I have corresponded with Dr. Schmidt in this respect and I think I have a better proposal now than the one I made before.

Let us assume that plants do have parapsychological ability, as has been proposed by some people. I think this could provide a simple experiment. I have a source of light which can be made extremely weak, a semi-reflecting mirror dividing the beam, and then, on one half beam, the plant, and, on the other, a spectrometer or a photocell, something to measure the intensity of light, and maybe its spectral distribution.

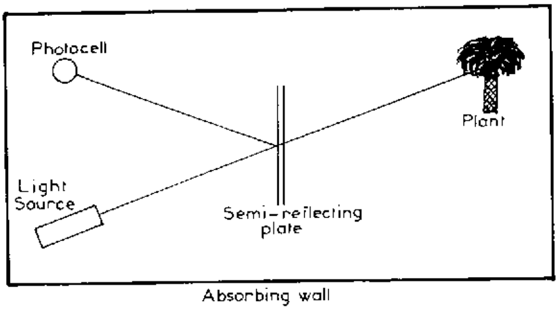

Figure 1. Proposed plant experiment for testing advanced waves.

I assume that the plant likes receiving photons, in order to induce photosynthesis, and it will not be pleased if the light goes down too much.

So I will decrease the intensity of the source, going further and further down. Suppose this semi-reflecting plate is set at half and half. Normally you would have one-half of the intensity here, and one-half there. My proposal is to determine if, when the intensity goes very low here, whether the plant will react by sucking in advanced waves from the plate, *so as to reduce the intensity of the other beam.*

WALKER: Could I make a comment? This is another version I believe of the earlier experiment you were talking about, using rats or similar animals, that would live or die depending on the outcome.

An experiment of this sort has been done. It was reported in 1972, at the convention of the Parapsychological Association in Edinburgh. But I do not think I should presume to report in any detail the specific data that was obtained. There was slight positive significance, rather consistent with the general precognitive levels. It was done with animals. But the level was down around 1%.

BEAUREGARD: Plants are not essential.

SCHMIDT: Yes, certainly you would not need plants; you could have human subjects. But how does this relate to the EPR paradox?

BEAUREGARD: Well, it is not distinctly the quantum correlation in itself, that is to say the spin interference between the two states. It is essentially a determination whether the correlation works along two timelike vectors rather than along one spacelike vector.

SCHMIDT: What would you look for in your measurement?

BEAUREGARD: I reduce the intensity of the initial beam and suppose I measure accurately the intensity of the beam comprising the cell.

Of course, the whole thing must be enclosed in a dark room. Let us assume that you normally have one-half of the intensity in each beam. My proposal is, if the intensity of the source drops very low, then you will no longer have one-half and one-half, but more intensity in the beam comprising the plant, because the plant likes to receive photons.

SCHMIDT: Yes, but I feel that this would also hold true in the classical case that is not related to the EPR paradox—say, if you have a ball, which would reflect this 50%.

BEAUREGARD: This approach is related to my belief in the existence of advanced waves. Also, as I have explained, because it is quite essential to my ideas that we live in a probabilistic world, where the intensities of waves "correspond" to the occupation numbers of waves, that is, where the intensity is interpreted in terms of probability.

FEINBERG: I wonder whether the experiment you described does not occur everyday when the sun sets? When the sun sets, the plants on the side of the earth where it is night are cut off from their primary source of illumination, and one might then think that they would start receiving the advanced waves which are heading in toward the sun, rather than retarded waves coming out from the sun. If that is the case, the effect is certainly very very small, because one knows that the ratio of daylight to night light is down by some factor of 10^{10} or so, and I would therefore guess that the effect here would have to be very small also, and in that case it might be hard for us to measure.

FIRSOFF: I only want to make a small comment. I wonder if that experiment could be improved, if you used polarized light, or a magnetic field?

BEAUREGARD: One can imagine many improvements. This is the experimentalist's problem.

WALKER: I just want to mention a couple of things.

First, seeing how there was some question about a lack of equations, I should not let you go away without having at least one on the blackboard. This was shown in one of those slides that appeared so briefly.

$$W_\psi \, \Delta t = -\log_2 P \; (\Psi_0 \rightarrow \psi_i)$$

[see my paper, p. 15, Eq. (28)]. Here ψ_i is the target state where Ψ_0 is the initial situation, the initial state. The quantity P is the chance expectation for the target state to occur. The time interval is given by Δt and W_ψ is a subset of the W which has roughly a 10^4 bit/sec rate.

The actual value of W_ψ is calculated from a knowledge of the signal-to-noise ratio, W/C. Generally, there is so much noise from the consciousness that you only get a small value for W_ψ, but under certain conditions you can have W_ψ nearly equal to W.

What I wanted to show here is that, if we work with somebody like Geller during a test time period of about, say, 1000 sec, the probability of a target state which can be achieved comes out on the order of 2^{-10^7}, which means a very unlikely sort of event can be achieved.

But if you have the subject in a condition where motivation is pretty high, but more especially, where the interfering consciousness is reduced, as through sensory isolation, then the W/C is increased improving the signal-to-noise ratio. This increases W_ψ; you would, therefore, expect subjects who can isolate themselves mentally to be characteristically good subjects.

Finally, I want to draw attention to my chapter in Edgar Mitchell's book *Psychic Exploration,* and to a footnote that I have in that chapter. There I give an expression for calculating the magnitude of PK effects that you would expect for an extremely good subject like Geller.

I believe that Ted Bastin's paper gives some time periods, and the amount of displacement for some of the objects that were affected by Geller. Is that right?

BASTIN: It just gives a vague idea of how long it took to happen.

WALKER: At any rate, I would expect that the values you give can be used to check that equation.

BLOESS: I would like to come back to the remarks of Arthur Koestler, which I liked very much. But I wonder, if you drop causality, whether it does not at the same time mean that you drop science altogether?

KOESTLER: Well, quantum physics *has* dropped causality, and replaced it by probability.

BLOESS: Yes, if you keep probability, there is no objection.

PANATI: I have been quiet during the conference, perhaps for the good reason that I am in the unique position of writing a paper on all of the other papers. I will get a chance to voice my opinion, and without rebuttal, within the next three weeks to a month.

However, I would like to make one comment. It is not on any of the papers or what has been discussed, but it is on one of the points touched on by Hal Puthoff and Russell Targ. If anything has been obvious, I think, from the conference, it is that the problems in parapsychology are horrendously complex. They are not going to be solved by a few physicists or even by a large number of physicists. The only way, I believe, that the problems are going to be solved is through an interdisciplinary attack involving physicists, psychologists, and neurobiologists. If that is the case, it means that more quality minds will be needed in the field, and of course more money.

The idea struck me listening to Puthoff and Targ, that they might have, in one of the experiments they described today, what I would call *the* cogent experiment, that is the magnetometer experiment with Ingo Swann.

You have something there that no magician would dare to claim he could duplicate. You have a subject in Ingo Swann who is sort of pristine, not tainted as Geller is from all of the publicity that he has received and, the publicity hound that he is, from his desire for more publicity.

Also that experiment does not suffer from the fact that it in any way looks like a typical magician's trick—despite the integrity of your clairvoyance experiments and telepathy experiments with cards in envelopes, and the balls and sugar cubes and tin cans, they do *look* like feats performed by magicians. But this experiment seems so pure, so novel, and so convincing to any physical scientist, that I wonder about the possibility of a joint or international effort, where several prominent scientists, from several institutes throughout the world, would get together to witness Swann influencing a magnetometer?

Now, if he does it with the frequency and repeatability—with the accuracy you say—such a thing would not be hard to set up.

That would be a quite different situation (it would be sort of a sledge hammer blow, in terms of credibility) than having everyone disperse and go back to his independent laboratory, and slowly grind out his own little experiments and attempt to get them published.

Have you attempted to undertake such a large joint effort, or large joint observation? And what do you think of the idea?

PUTHOFF: I think it is a good idea. We have not attempted that. We have borrowed the magnetometer, or borrowed use of the magnetometer, from Stanford University, and it was only available to us for a short time.

PANATI: What physical scientist, in seeing that happen before his eyes, would not be absolutely fascinated, if not completely convinced, that there was something definitely there to be investigated? As a matter of fact, he would be delinquent if he attempted to go back to his laboratory and work with physical equipment, having seen that humans can interact and affect this equipment, without investigating the possibility further.

BASTIN: There are a lot of delinquent people about.

PANATI: Yes, exactly, and that is the very reason I am saying, it is not going to take a little personal pick, chopping away the ice, to get to them, but it will require something of a more concerted international effort, something very convincing. I think it is a highly significant experiment.

BEAUREGARD: I will say something that I think is extremely trivial in parapsychology: *you must keep in mind the interaction between observers* of one and the same quantal event. If you have many skeptics among the observers, the demonstration will come out as zero. So I think I do have a better suggestion. Why not just walk into a beautifully equipped laboratory comprising some delicate apparatus and a ready sophisticated experiment, bringing with you some unknown young undergraduate, or postdoctoral fellow, who in fact would be a psychokinetic agent making everything go wrong or crazy?

My guess is that after some repetitions of this sort of unannounced parapsychological experiment a few nonbelievers might well begin to turn into believers. . . .

FEINBERG: Well, I only want to remark that the man who built the magnetometer, who is a rather prominent physicist, apparently paid no attention at all to this report. He shrugged it off. The graduate student who was specifically working with it at the time when Swann did this thing, whom I saw six weeks later, said it was interesting, but he is going to go on doing what he is doing.

PANATI: But of course, that was not a joint effort to demonstrate the phenomenon. That was the phenomenon being demonstrated at a particular location, by two individuals.

FEINBERG: I know the distinction you are making. But I only made my comment to indicate that people are not necessarily going to react to seeing this thing before their eyes in the way that you suggested.

PIRON: In answer to your suggestion, you must realize that if somebody notices that a phenomenon exists, this does not prove that he accepts it. And if you go into a laboratory, you will see this kind of effect. This phenomenon is known for example as "Murphy's law," or "If something can go wrong, it will." It is well known in French as "La théorie de la tartine."

PANATI: No—the point I was trying to make, I think you are missing. It's not that the report, as it comes out now, will bear the names of two individuals and one organization, that of Harold Puthoff and Russell Targ and SRI. They have received both good and bad publicity from the press, and the report will be looked on in both lights.

However, if a report were to come out, signed by several very prominent physical scientists, not a single organization but a collective number of organizations, and not just in California or the United States, but throughout the world, that type of report, I believe, would have much more weight. It is much harder to conceive of an international conspiracy than of a single laboratory conspiracy.

PIRON: Yes, but I would still say that almost all experimenters know this kind of phenomenon. They have seen it happen one time or another in their laboratories when making experiments. The experimenter sees that when he has noise or certain fluctuations, he has a chance to see something. We may know very well about the existence of the phenomena, but this does not explain them. Psychologically, it is very difficult to accept them and to have the courage to confess to other people that one believes in phenomena.

Then, the problem is not to demonstrate the phenomena to others, to other physicists and other people generally, but to find a way to go up against this psychological barrier.

BASTIN: I looked cynical at what you said, only because I was at that meeting three years ago when people first heard about Geller, and Andrija Puharich was in Israel investigating him a fortnight later. Gerald Feinberg was there. There were several other distinguished physicists and all of them swore—taxed by Andrija Puharich—that if they could actually see with their own eyes some of these phenomena about which we heard, their lives would never be the same again. However, Gerald is the only one of them here today.

FIRSOFF: My comment is semi-jocular, but I believe it was Max Planck who said that new ideas gain acceptance, not through their cogency, but because their opponents die out.

KOESTLER: Well, I think Panati's suggestion is excellent. And I cannot understand the objections to it, because if the objection is "Whatever you do, however watertight your experimental conditions, they will still not believe it," the conclusion is that we can close shop in general.

FEINBERG: I would like to respond to Koestler and remark that your conclusion does not follow from your premise. That is to say, it does not follow that if, as I believe, a demonstration of the kind Panati suggested will not convince people, then nothing will convince people. Indeed, I think there *are* things that would convince people of the validity of these phenomena. But these do not involve a demonstration, no matter how many famous scientists you have sitting around. What is required is rather a real understanding of them.

That is to say, when Targ and Puthoff or someone else have analyzed these phenomena in enough detail that one can begin to relate them to the rest of the world, rather than have them shoved aside as an isolated instance, and they present the results, then I think there is a chance that scientists will begin to take them into account.

Otherwise, I simply do not believe one is going to get the kind of scientific attention we want.

KOESTLER: This is an important point. I do believe that in the history of science, you very often have acceptance of phenomena without explanation. There were no explanations for electromagnetic phenomena, starting with Oersted's compass needle. Even gravitation, implying action at a distance, met absolute incredulity when it was postulated. Explanations came only later. If we wait for explanations, we can wait for Doomsday.

FEINBERG: It was not explanations I was referring to, it was more detailed study and more understanding. Oersted's results were accepted when lots of people could duplicate them, and when they could see how the magnetic field and the current varied in strength. The same I think is true about x rays and all the other spectacular developments. It is only when someone begins to understand the parameters and define them, not in terms of a fundamental theory necessarily—that may come generations later—but at least in terms of being able to get some kind of picture of what is going on, rather than just having a phenomenon which you demonstrate.

KOESTLER: Do you mean predictability and repeatability?

FEINBERG: Yes, right. That kind of thing.

PANATI: Replying directly to Gerald Feinberg, the very reason I mentioned the idea was that it is preferable to have science proceed in a logical step by step fashion, and to build up a theory, rather than go on and do an amazing demonstration which does not fit into a theoretical structure. But the reason for making the suggestion was to perhaps convince, or maybe a better word would be to entice, more quality minds, such as yourself, who was convinced apparently rather recently to look into this field, and to get them to turn their attention away from some of the conventional problems they are dealing with now, and look at parapsychology.

It is true that an international effort to show people that Swann can interact with a magnetometer may not increase by one iota our understanding as to precisely how he does it. But it may bring another dozen, if not more, quality people into the field.

WALKER: Essentially what I had in mind has already been said by Gerald Feinberg. Until there is a way of understanding these phenomena within the accepted pattern of physics, or until that is overturned, or until there is a stronger theory that gains more support, people are going to look at parapsychology as a curiosity at best. They will not spend their time, which is already taken up in other problem areas, to be very much involved in it.

However, I think that this is truly a remarkable experiment. It comes closer to being *the* demonstration than any other that I know of.

Professor de Beauregard warned about skeptical minds. I saw this effect at Berkeley, when Geller was trying to perform. You could have cut the tension with a knife. He was trying to use the faculty members as subjects, and they were going to have none of that. They were also powerful minds, with powerful wills, and they were directed in the opposite direction. And there is a cumulative effect. Geller did essentially nothing.

However, I would suggest that if the demonstration is held, one should preface the critical experiment with a discussion or display or something to get the people into a nonantagonistic state of mind, because it is a group phenomenon, it is not a single individual who demonstrates this.

TARG: Hal and I were the participants in what was supposed to have been the definitive experiment. Uri Geller was brought to us with the alleged ability to bend metal in somebody else's hand, and that seemed like a very clear description. We thought we knew what all the words meant. And we tried very hard for six weeks, shot off 30,000 feet of color film, and nothing even close to that particular thing happened.

While in the course of our work, we have found a number of fairly reputable experiments with very good repeatability, as I think most of you accept, that we can demonstrate for visiting scientists.

In our experience, people who are not predisposed to believe in the existence of the phenomena do not believe in things, first of all, that surprise them in somebody else's laboratory, but even more remarkable than that, they do not believe in the reality of things that you do in their laboratory.

We have gone with a number of subjects on a promenade to various laboratories in the San Francisco Bay Area and elsewhere, to demonstrate a variety of PK experiments, primarily with magnetometers, but also with other instruments, in other people's labs. And Koestler, who is a proponent of the ink fish, should really be the most sympathetic with us—that people will not believe what they do not want to believe, even if they have designed the experiment. But if it is your man in his lab, then he will say that there is something the matter, independent of what happens.

So there is an interesting dichotomy, which I even hesitate to mention, that has been showing up across this room at this particular point, between the people who are working physicists and the people who are presently working writers. The physicists have the day-to-day experience of not being believed, when they describe what they have seen and what they have done, unless each person can go home and do it for himself with some sort of regularity.

So I think that Uri Geller could walk from Berkeley to San Francisco on the water, under the bridge, and there would be a substantial number of people who would say, "Oh, that's the old walking-on-the-water trick."

PANATI: As I said, though, Uri Geller is a bad example, because of the adverse publicity that he has attracted in the last couple of years.

BAUER: While I would support the opinions offered by Dr. Walker and Dr. Feinberg, if you consider the history of psychical research, we have always been confronted with the same problem. There was the famous presidential address by Henry Sidgwick in 1883. And for example, the experiments by Dr. von Schrenck-Notzing about materialization phenomena in Germany of the twenties or thirties always involved the same problem. When Dr. von Schrenck-Notzing took a group of scientific celebrities to his laboratory to witness the phenomena, what were the results? When the scientists came out of his lab there was a sort of *Schwundphänomen*, a fainting phenomenon. And I do not believe that inviting large numbers of famous scientists would gain support for the credibility of psi phenomena. It would rather require a sort of theoretical or sociological analysis, such as was, for example, uttered in Thomas Kuhn's book about the structure of scientific revolutions. The fact is, we have merely encountered certain anomalies that do not fit into our present paradigm. Hence, I am of the rather pessimistic opinion that this sort of conference would not make any important contribution to the sort of paradigm we seek in parapsychology.

BEAUREGARD: Coming back to the famous Planck statement Mr. Koestler has recalled, I can say that I personally know quite a few young physicists, both in the United States and in France, who are very much interested in parapsychology. I would not say this of elder physicists. So I think we should not bother. The thing will come out by itself.

FEINBERG: Well, I want to make a comment, as a scientist who has had very little interaction with parapsychology in the past—and perhaps won't have too much in the future either.

I think in some sense that the sin of parapsychologists, of the people working in parapsychology, is trying to get too many others involved in what they are doing, rather than doing their own thing.

It seems to me that what you should try to accomplish is not to get Professor X and Dr. Y and Mr. C to drop what they are doing and come work on parapsychology. As I understand the history of science, very rarely does a new field progress by that sort of procedure. Rather, it seems to me that what you should be doing is, to go ahead and study these phenomena as best as you can; figure out what things they vary with, what things they do not vary with, make models of the kind that Helmut Schmidt and others here have progressed, and after some time, if there is something to be understood, you will understand it. You do not need a great mind, in the sense of someone who has already established a reputation in another field, to do that. If things are understandable, then they can be understood by most people who are prepared to do it.

Therefore I can only remain a bit skeptical about the type of analysis which says, "Well, we have gotten up to this point, we have established the existence of something—now it is time for somebody else to come in and explain it or study it further."

Frankly, that seems to me to be a rather strange way of proceeding. I would think that if you found something, you would very much want to work it out for yourself.

So my parting blessing to you, if you like, is, do not worry too much about what the rest of the world is doing; go on and do what you can, and from the standpoint of eternity, it will work out the way it works out.

ANGOFF: Thank you, Gerald Feinberg, and thank you for your blessing. We have now come to the end of the Parapsychology Foundation's 23rd International Conference. These two days have been, we can all agree, stimulating and provocative days. And if you will permit me to say, my good colleague Mr. Robert Coly has made them very pleasant days in other respects.

In any event, we of the Foundation thank all of you for giving us this opportunity to sponsor your contributions. You have reached a modest audience here in Geneva, and again with your continued co-operation, we shall reach a much larger audience, about ten months hence, when the volume comprising all the formal papers and all the discussions will be published by the Parapsychology Foundation.

Again, our thanks to all of you. This conference is adjourned.

[*After reading the transcript of the Round Table Discussion, which was sent to him in India following the conclusion of the Conference, Dr. C. T. K. Chari submitted the following final "Comment" on the Discussion.*]

I suggest that my observations which were circulated among the participants in the Conference, though not read, may remove some misapprehensions. Let me say in reply to Professor Whiteman that I am *not* thinking of a simple linear extrapolation of current information theory using "bits," "nits" and "Hartleys." I am envisaging a *non-Boolean* extension of current probability theory as well as of current information theory. I do not claim to have formulated rigorously all the requirements of a non-Boolean logic applied to quantum mechanics or parapsychology. In quantum mechanics, I think, a beginning has been made with non-Boolean logic in Jeffrey Bub's *The Interpretation of Quantum Mechanics* (D. Reidel Publishing Co., Dordrecht-Holland, 1974). I hold that the "paradoxes" of quantum mechanics arise, not from any holistic interaction of the observer and observed, with "hidden" channels and potentialities, but from our requiring a non-Boolean structure to satisfy the conditions of a Boolean logic, e.g., one introduced by a Lindenbaum-Tarski algebra F/H, where F is the set of formulas and H is the semantical relation of equivalence of sentences provided by a Gentzen-type of axiomatization of the full predicate calculus. Obviously the Wigner-Yanase, Araki-Lieb, Robinson-Ruelle, entropic inequalities do not go far enough towards the kind of reconstruction I contemplate. For estimating information in biology, we may well have to go beyond our notions of combinatorial rarity or unexpectedness.

On the question of "verification," I maintain that major "paradigm shifts" in T. S. Kuhn's sense lie beyond the Popperian "testing" and "criticism." I am persuaded that most of the proposed models in parapsychology carry little promise. R. B. Partridge, in a recent experiment devised to test the "advanced effects" posited by the Wheeler-

Feynman absorber theory of radiation, found that absorption along the "future cone" is complete to better than one part in 10^8 in an expanding universe [Nature **244,** 263–265 (1973)]. Theories of "hidden" variables resort to nonlinear generalizations (the Bohm-Bub version, discarded later by Bub, is the simplest case) and presuppose Boolean axiomatics. I reject this axiomatization as quite inadequate. Parapsychology, in my view, requires more than a reconstruction of quantum mechanics. It calls for a major "paradigm shift" in our notions of probability and information, especially in biology.

C. T. K. C.