AFTERNOON GENERAL DISCUSSION

Day Two

WALKER: I want to return to Dr. Braud's table, which I feel is extremely important here. I feel that this now really gives the data. One can argue about these data, but I don't think they should be dismissed. The data indicate that quite an adequate level of repeatability has been attained. Moreover, taking into account what Dr. Rao said with regard to this issue of belief in small numbers, if we looked into it in those terms we might find indeed that the problem of repeatability is even smaller than the table would indicate.

Now, when I said that earlier, Chuck Honorton jumped on me and said that when you are doing the random number generator type of work you have a million trials. Well, in reality I don't think that there are a million subject events, real efforts by the subject a million times in one of these studies. I don't think that that is a reasonable thing to assume.

But more than that, after I sat down Chuck gave his talk in which, when Hansel criticizes Chuck on this point, Chuck then references Rao and the same issue was brought up. I am very puzzled now as to what Chuck's real position is with regard to that. I am rather confused, because I had the feeling that first Chuck was stating that I was wrong and then turning around and using the same thing that I was saying in order to justify himself in the light of Hansel's criticism.

HONORTON: I did not hear the first part of that, except that I jumped on you and I certainly did not intend to do that.

WALKER: The point that I was making is that you brought up the question of correcting this table at some time with an appropriate serious research effort to see what happens when once again you go into this and correct for the issue of small numbers. If you do and you find that, for example, in the first one of 33 total 27 were significant, we can understand that we really should not have expected more than 27 anyway, so there is no repeatability problem. When we come down to the random number generator work we have a worse situation there. Now, you raised the issue about a million trials. I say that that isn't meaningful. You said the same thing in criticism of Hansel.

HONORTON: I didn't say that a .20 percent effect in a million trials isn't meaningful. I said it isn't a very big effect.

WALKER: I am trying to abbreviate the argument once again. I made the statement and you said that when you go through that process of correcting this table, when you got down to random number generator studies you don't have a situational belief in small numbers there, there are a million numbers being generated. And, of course, those are large numbers. But my feeling is as far as the subject was concerned there were not a million acts of will, independent efforts; indeed, the person probably slumps 90 percent of the time.

HONORTON: That is possible. That really is an empirical question. The statistics in the table are based on trials.

WALKER: That is true, but I don't think that has anything to do with the whole matter of the belief in small numbers. It may be that when you get down to this particular instance you have to throw up your hands and say "Well, I don't know how to correct for this issue of small numbers other than if I take the small numbers to mean I only had a few subjects." We didn't say what the small number is to refer to. Is it a small number of trials, a small sample of subjects? Some subjects can bring in a 2 percent, a 1 percent and .01 percent, and that still would bring you to significance. But unless you bring in one subject who can give you some effect, you get nothing out of it and that can account for this. So you are dealing with a small number effect.

HONORTON: Random generator studies generally have more trials than any other kind of study.

WALKER: But it isn't meaningful with regard to this issue that Rao brought up. I don't think that particular number is meaningful.

HONORTON: When I said that some of these estimates would have to be revised, I had in mind not just for the number. Rex originally raised the issue of the different numbers of experiments in the various areas. But going through the Ganzfeld review and going back and forth with Ray Hyman, it became necessary to lower the estimate of success. In fact, for Ganzfeld now I would not claim more than 40 percent of studies being significant with corrections for multiple analysis. I haven't, for example, seen the review of the remote viewing studies yet, but I would suspect that at least in some of these areas these are preliminary estimates and that the analyses have not gone to that stage yet. I would argue with you though on a more substantive level. I think this is very encouraging, but I don't think that we should cheer too hard for this to be an adequate level of repeatability. As long as Blackmore can't get her results and as long as none of Beloff's students can get results we are not in a good position in terms of replicability.

WALKER: We referenced Collins appropriately. In light of Collins, I told you how to build a TEA laser that the village idiot should be able to do and yet highly qualified and monetarily backed scientists did not replicate. There is another example. Isaac Newton took a prism, put it in a beam of sunlight and saw a display of colors. If you go to the *Proceedings of the Royal Society* you will find that that was not replicated! That precise result failed to be replicated.

I want to insist on this. This table is our literature now. I think that we should really address this either by saying "This tells us something" or we should write a paper in the future to revise it. We should not just toss rocks at it. Either we write papers to come up with a better estimate or else we accept it and say we like it. Frequently, people do a lot of good work like this. Then because it is very easy for one to throw rocks at it, we don't get anywhere. The result is stagnation. If the people here don't really feel that this is right—Sue Blackmore, I bet doesn't feel it is right—go into the thing and do it right. Until that is done, competently, this result stands as witness to a high level of repeatability.

HONORTON: Harris, I have to make a somewhat emotional statement at this point. I appreciate what you are saying and I appreciate how you feel, but I do not appreciate your holding up a piece of paper and saying let's admire this. Because what you are doing is setting up a new target for somebody to shoot down. Every time somebody comes along and puts up his list of the seven or eight best experiments, all that does is to make the guys on that list spend the next two or three years defending those studies. Don't put it on the wall and admire it. Go and do some more research. Build on it if you can. That is what it is for. It is not to be admired.

WALKER: I am not saying anything other than don't simply destroy research by throwing rocks at it. It is easy to do. I can think of hundreds of counter arguments. We only progress when we take the research and make suggestions for future work. Then if you really feel that the past work is wrong, do a detailed study and correct it.

RAO: I think that the point made by Dr. Walker is well taken, but it is not too well communicated. I think there is simply a misunderstanding. I don't think Chuck Honorton and Harris Walker would disagree at all. If I understand Dr. Walker, what he is saying is here we have a table which is not corrected for the numbers. If we take into account the size of the effect and then figure the expectations, the replication rate might be better than what is seen from this table. I don't think Chuck would disagree with that. It might be possible and certainly worthwhile for someone to do it. I don't really see any difference of opinion between the two of you as far as that is concerned.

HONORTON: My only point is yes, it might be better or it might be worse. What I am saying is when you hold it up in that fashion you are asking for someone to come and shoot it down.

WALKER: I think we see eye to eye on this whole business. But the one added point is that we do need to see how big this replication problem is and this is a good way to do it. Maybe in the future we will have a better measure of this particular thing based on a very careful further improvement.

BLACKMORE: When I brought up this business about the table in the first place I would like to repeat the question I asked, but get away from the table. My only reason for referring to it was to say that when we pick on Ganzfeld and REG as being the most hopeful parts of our field, which many people have done, we are clearly not using percentage repeatability as our criterion. What I am interested in knowing, and I would like anyone to answer this, is what are we using? Why do we all seem to agree that these are good, hopeful areas to follow up?

HONORTON: As far as Ganzfeld is concerned, I believe there have been more independent investigators who have done successful Ganzfeld experiments perhaps than any other area that I am familiar with. That would be my main reason.

BLACKMORE: Is that really a sort of formalized numerical reason or is it something that is more a gut level reaction?

HONORTON: Just as you can't divorce your 32 relatively unsuccessful studies, I can't ignore the fact that I have had eight or ten successful ones.

BLACKMORE: But everyone else seems to agree that Ganzfeld is a hopeful thing and I wonder why.

HONORTON: That is because many of them have gotten the same results.

RAO: I wanted to address this question a little bit. There are a variety of reasons. I haven't done any Ganzfeld work myself. I haven't so far done a single REG PK experiment, but I still feel that these are the two hopeful areas for parapsychology. Not simply because the replication rate seems to be about what I think is critical. You have to have a certain replicability rate. And of all these studies that have passed this critical rate, you want to see which are more feasible, which are easier to control and what are the bugs relating to those we know. For example, if you want me to do a DMT (Defense Mechanism Test) I can't do it because I have to go and learn from Martin Johnson or Dr. Kragh how to interpret it, so I don't do this. Similarly, if you want me to go into meditation, I run into all kinds of problems teaching meditation to people. Post- and pre-meditation comparisons involve all kinds of differential situations. So from all these research strategy points, I feel that among those that have passed this critical level of replication these seem to be the most feasible to work with. A number of people have worked with them and succeeded with them and therefore the generalizability of this kind of an experimental set-up being successful is more likely to be the case.

BLACKMORE: That makes more sense.

EDGE: I, too, was informed by the work on the placebo effect. I had no idea that this sort of work had been done in this large a number and had this kind of statistics. And you look at this and you say "The replication rate is about the same as in parapsychology looking at this particular table and there is no explanation for the placebo effect." I mean, what is the placebo effect? People get well and we don't know why they get well. And then you say "Well, parapsychology is not in such a bad condition because we have the same replication rate and we are in the same boat—we don't know how ESP works either." But, I am a little bit bothered. I think there is a difference: if we use operational definitions of what is science, placebo effect work seems to be scientific—that is, they are getting money to do work! We are not! On one level, indeed, it does appear that there is no explanation and yet on another level I think that is not true. I personally would never think much about going in and doing placebo work—maybe for lack of imagination. But I suspect that, after a while, when we get to know a little bit about physiological correlates some sorts of interesting relationships will arise. We implicitly believe that the placebo effect will be tied to that kind of explanation, whereas I am not so sure that it is going to be in the area of PK and ESP. I suppose that I just wanted to say that I think something else is going on there. And that if we ask why it is that the placebo effect is connected—that seems to be more scientific and we are worrying about why we are not as accepted. There are implicit reasons, I think, which are important.

HONORTON: Pain is a real reason. It is a really good reason. It is hard to argue against. If you are asked would you believe something that gives you seven fewer days of pain, then you are going to believe that because you don't want the pain any longer than is necessary. That is analogous to having a practical application in psi. If the placebo was an experimental effect only, then it would not be in the position that it is. Of course, all the money is going there not because people regard placebo effects to be real science, but because the drug industry is one of the biggest industries and placebo studies are necessary in order to test new drugs. That is where most of the placebo data come from. There are very few placebo studies to date that are oriented toward evaluating the placebo effect per se.

EDGE: I think that it is important once again to come back to the comment that maybe practical application is something that we should be thinking more about.

RAO: It is not completely correct to say that placebo effects have no explanation. In fact, as far as pain reduction through placebos is concerned, I think they have a good physiological explanation. There was an experiment done in 1969 that clearly shows that more endorphins were produced because of the belief. The point, however, is that placebos do seem to do more than reduce pain—where this particular kind of explanation would not satisfy. So is the case with regard to the experimenter expectancy effect. We do know in some cases the reason why the result was biased, but then there are other cases where this reason doesn't hold. There is no one reason to explain this effect and there are some experiments in experimenter expectancy and in placebo studies where no conceivable explanation seems to be adequate. Therefore, I seem to think there may have been some psi in them.

HONORTON: Well, thank you, Dr. Rao, for providing an opportunity to describe a failure to replicate because the study you referred to that involved the drug Naloxone has recently been challenged. It seems Naloxone increases pain with or without placebo and its effect on placebo is unclear. But even if that weren't the case, the issue is how does this get initiated in the first place? That is not an explanation.

BELOFF: One has to be awfully careful here as to where you have got a physiological explanation. The fact that one may discover that in the course of the pain elimination endorphins are produced in the brain doesn't mean that the production of the endorphins is something that itself doesn't have to be explained. Why should expecting that it is going to be a pain alleviator suddenly produce these? It is sometimes suggested with acupuncture that this is the mechanism and that might be so, because that is after all a physical stimulus that is applied—it is not a mental one. I am quite prepared to believe that there might well be a physiological explanation now forthcoming for acupuncture effects. I don't say it is, but that sounds to me much more reasonable. But here, you are called upon to explain a psychological influence expressing itself through physiological means.

RAO: Well, I do appreciate this point. I think it is very valid. The mechanism by which this is done does require an explanation. But this is much larger than placebo; this encompasses many areas. In hypnosis, if you create a blister by means of a suggestion obviously this is happening. If you want to invoke a PK hypothesis, I would have no objection. But the thing is, that doesn't seem to be very parsimonious at this particular juncture in our research.

HONORTON: I would agree with that. The whole point of my bringing placebos into the discussion was not to suggest that PK or psi is implicated in the placebo process, but to illustrate an area outside parapsychology that has extremely similar characteristics in terms of repeatability: experimenter effects, inter-laboratory and even international differences, decline effects and so on. I am intrigued enough by those similarities to want to see some empirical work done to see if there is a relationship there. But I certainly did not mean to imply that there is.

SCHECHTER: I would like to go back to Hoyt Edge's original comment and even to his discussion yesterday on intelligibility. I have a feeling that if there is a difference between the placebo work and the psi work here, it is not the presence or absence of a good explanatory mechanism, for the reasons that Dr. Beloff, Chuck Honorton and Dr. Rao were just discussing. Rather, we may have a subjective intelligibility difference or a comfort difference here. One thing that has been done over the decades in psychology and physiological psychology is to treat the problem of the interface between experience and physiological process rather lightly. We talk at great length about the physiological mechanism and we talk about the subjective experience, but we treat the interface between them by acting as though anyone can see that there is a connection and let it go at that. In this sense, to say that there is a physiological mechanism for placebo effects does not really provide a full explanation, but it does provide a comfortable one. What we are doing in psi research, however, is focusing on that juncture point, on that very uncomfortable point. At the core, we don't really have an explanation for fundamental psycho-physiological interactions. How is my intention to wiggle my fingers related to the neural activity that activates muscles? What is it that causes those synapses to fire in the end? We don't know. But it is comforting to be able to trace a neural path and show the synaptic activity flowing and to say "I have got 99.9 percent of a path laid out and it is only .10 percent that I don't understand." That is okay. But when it comes to asking what makes an electronic RNG act in a way that is related to my intentions, I don't have *any* percent of a mechanism to talk about. I think that in addition to the intelligibility problem, we have a comfort problem that we need to deal with.

EDGE: I would simply say that I included comfort in intelligibility. I think that it is part of it. Perhaps that is the reason why we say that extraordinary phenomena have to have extraordinary evidence. It is just a matter of comfort, there is really no other reason. Maybe on second thought we should strike that from the record. I am sure there are other reasons, too.

BLACKMORE: We have been talking about a variety of difficult-to-explain things which are nonetheless thought to be normal, such as producing blisters by hypnosis or the placebo effect and so on. Now, if there is psi, ultimately we ought to be able to contribute to the research literature on all these effects and say, "Ah, we have got a better hypothesis that will account for these things and produce better results than your 35 percent or whatever it is on placebo results." If we could do that, then I would say we have a progressive research program. And at the moment it is yet another symptom of our problem that we can't really contribute to those research areas even though we know that what we are talking about is relevant to them.

STANFORD: Well, to return from the possibly parapsychological to the conceivably parapsychological—I want to go back to something that is in both Chuck Honorton's and my paper and that is how subjects are treated and interacted with. This is, however, germane to what we have been talking about. It is tied in with the physician/patient relationship. Have you ever been to a surgeon to have a growth removed from your skin and in one instance the physician tells you everything that he is going to do. You are never caught by surprise. It is so much more a gentle procedure that way. But some physicians tell you little or nothing as they proceed. I have had both types of physicians do the same physical procedure. Now, I think this can be generalized to the parapsychological setting, especially to Ganzfeld experiments and physiological experiments. There are a lot of things that we do in the lab, that I think we really must make the subjects aware of before we do it. Think about a person in the Ganzfeld, in an at least partially darkened room, lying splayed out and you are slapping ping-pong ball hemispheres over his eyes. First of all, I think we should tell the subject before we even approach him just what we are going to do. Don't go slapping tape on or grabbing and manipulating the subject without explaining beforehand. He can't sense when that is going to happen, he is going to be jolted. I am simply saying here, that although I have never found any comments on this in the literature, I suspect this is one place where there is much inter-experimenter, possibly inter-laboratory, variability. I really think that a simple rule here is "Do unto others as you would have them do unto you" and that includes not doing what isn't expected.

ANGOFF: The Foundation thanks all participants and observers. Ladies and Gentlemen, the 32nd Annual International Conference of the Parapsychology Foundation is adjourned.