

## GENERAL DISCUSSION

ROE: Both presentations have drawn an analogy between the kind of connection that is hypothesized to exist between people and the kind of entanglement that has been observed with microscopic systems. But it's difficult for me as a non-physicist to see how this analogy is viable or even useful; how can we meaningfully say that the kinds of macroscopic systems that we're interested in represent entangled systems?

ACHERBERG: Well, people who have looked at this research have looked at the initial documentation of these macro systems and describe them as acting in an entanglement fashion.

ROE: So, practically, how could you go about 'entangling' two people?

ACHERBERG: Through consciousness.

ROE: So having them simply spend time together or engage in conversation would be sufficient?

ACHERBERG: They did not have to engage in conversation in my research.

WALACH: Let me re-phrase the whole thing; I think it is a mistake to try and use physics to understand what is going on because it is not a physical situation—it is a general systemic situation. By virtue of us being human beings we are necessarily correlated to some extent and the stronger you make connections between certain people, through rituals and interactions the stronger that gets. Very simple.

DOBYNS: If I try to raise all of my questions about Dr Walach's presentation we would be here the rest of the night! Therefore I would like to just provide one counter-example to the no-signaling theorem, which I notice is inserted into weak quantum theory as an assumption rather than something that can be derived from first principles, the way it is in ordinary quantum theory. I expect everyone knows the story of Russell Targ's first application of associative remote viewing (or ARV) in the early 1980s. It was initially very successful, raising hundreds of thousands of dollars in the Silver Futures market, then after a while it stopped working. This is a pattern seen over and over again with many people who have tried to do this with ARV; they have initial successes that sometimes are quite impressive and then things peter out and get more difficult. This is not the way the quantum entanglement no-

signalling theory works. You do not get to send signals for a few dozen rounds until the paradox monster wakes up and says 'Ok, we're done!'—it just does not work at all. So this tail-off phenomenon is part of the non-replicability issue but it cannot be accounted for by the no-signalling principle and in fact it violates it.

VON LUCADOU: I tried to explain this, it depends on what you conceive as a signal so you have to specify this notion—there is a mass of different notions of 'signals'. To explain this, one has to go into details and to put it simply I would say we use the term 'signal' in the following ways: if you have done several experiments and if you take the information from these previous experiments to make a clear-cut prediction about the outcome of future experiments, then this theory says you cannot get the same degree of certainty as you would from classical signal enhancement theory. If you go into the detail it is very complicated because there are so many entangled systems all going on and being linked with signals, so one has to be very precise. Conversely, the advantage of this no signal-transmission axiom is that you can easily compare it with the thermodynamic principle that you cannot use energy a second time. So this is a very general principle and you can apply it in certain situations, but in this situation you have to define it very precisely. Then this principle allows you to decide very complicated systems and to make predictions. However, the remote viewing experiments are so complicated in their details, using different complex structures as targets and decoys and having different ways of describing and comparing them, and this gives so many different levels of signals present that it is impossible to make a clear prediction in this case. But if you take a very simple experiment such as a PK experiment, then the theory fits perfectly, making a prediction that fits 100% with the result of the meta-analysis of Bösch and Boller. It is utterly untrue that you cannot apply this model to real experimental situations, but first you have to define exactly which type of information you will describe as a signal. I would like to say if compare for example the remote viewing experiments to the early experiments which have been done by Sinclair and his wife, you can see very clearly the same structure that the structures fit perfectly, which shows there is entanglement, but if you go to the interpretation of these structures then there is no effect at all. I have many examples in the remote viewing case where I have the original pictures from Ed May and I have discussed the matter with him; you have no signal but you have correlations.

SHOUP: I really like these talks; you have both put your finger on the right ideas but from very different points of view. The metaphor of 'it's just a dance' is really perfect, and I shall talk more about this tomorrow in my talk. Harald, we agree on so many things and yet I have to part company with you when, as many do, we try and generalize the idea of entanglement and say that people or minds are entangled. It may appear that way, but I don't think we can play with quantum mechanics in quite that way. Unfortunately, that turns away many mainstream scientists who have a special meaning attached to entanglement. What we need to do is find a way to get from what we observe that appears like entanglement—or special relationships between human beings—to what is really going on in physics and make that connection. If we can do this in a plausible way we can make a connection with mainstream science.

WALACH: I would partially agree with you, if we can really make the connection with physics then that would be preferable to what we do here. It is very important to understand that what I have presented is not physics; it is a systems theoretical approach using theories and ideas that are available in physics and extrapolating them to a more general situation. It is just turning the whole thing around, and I understand the misgivings of people who have certain definite meanings attached to the term. I understand that perfectly and if I had a better one I would use it, but I don't. It is, of course, the attempt to use the structure and the formalism of a physical theory and apply it to general systems, and I don't think that is prohibited. You have the right to challenge the idea that we should eventually make connections with the physics of it all; that may be one step too difficult at this point, and therefore another suggestion is to use a generalized form of the formalism to see where that leads us, and it appears to give very similar predictions, which I find intriguing.

MULACZ: I think the importance of quantum mechanics and its application in this field is that we are actually forced to change our frame of thinking, that we are forced to do away with Aristotelian logic. We have learnt this from childhood, maybe it is innate, that there is *A* and *not-A* and nothing else. Apparently we have to amend this and learn that there are some situations in which there is *A* and *not-A* at the same time, and only *A* and *not-A* gives us a complete description. Actually, you already find this notion of complementarity in the

writings of Jung and others. The most important effect is that we are forced to change our way of thinking.

ACHERBERG: To state the obvious, because I have been trying to defend my data for some time, I think we feel compelled to have a known mechanism for an effect. One of the reasons editors and reviewers give for rejecting this kind of research is that there is no known biological mechanism for the reported effects. That's the death; the article will not get published and will not be heard. Even though quantum mechanics is itself very controversial it does provide us with a small doorway into public awareness, and I feel we're justified in doing something with it even if it turns out to be not totally correct or to have been applied obliquely in a way that may or may not fit, since it provides us with a known mechanism to get the point across.

JOSEPHSON: The connection with physics was actually made in a paper by Pallikari and me in *Foundations of Physics* and Valentini in *Physics Letters*, which have both been ignored by the mainstream. We used Bohm's statistical interpretation of quantum mechanics to show that if you assumed a different kind of probability distribution then you could get action at a distance. What Valentini showed by a piece of mathematics was that there would be a tendency to go to equilibrium so he said it was like a quantum analogue of heat death. The usual situation is quantum heat death, where you have gone to an equilibrium distribution and quantum mechanics apply, but you might get non-equilibrium distributions before you reach heat death where you can get action at a distance. This fits well with what Walter was saying about how you can extract energy from a heat source to work with, but you are using up that resource and it disappears. In the same way, using a source that lets you communicate directly at a distance may be using it up and going towards equilibrium, which would explain why you could get an effect at the beginning but it would fade away. In the context of my talk this morning this would mean you discovered some source in your consciousness and you can only use it a certain amount of times before it would run out, heading to equilibrium and heat death.

VARVOGLIS: Harald, I would like to take issue with your 'parapsychology is dead' announcement, and like Mark Twain would say maybe that rumors of our death have been exaggerated. There are two ways of defining parapsychology; one is in terms of methodology, a process of study in a certain area; the other is in terms of its object of study. If the object of study is psi phenomena—or whatever attribution

you want to give it, such as nonlocal correlations between complex systems—then you cannot say it is dead, because you are continuing in that direction. There is a lot to be said for the idea that as long as such phenomena seem to keep appearing in one way or another then we will continue to study them. If we abandon all pretence to attempt a systematic study then parapsychology *is* dead, but as long as you introduce some kind of systematicity in your approach then I don't think you can say it is dead. What might indeed be dead is the attempt to squeeze parapsychological phenomena into a certain methodology, which was initiated in the late 19<sup>th</sup> century and came to its clearest definition with Rhine and his successors. We may indeed be seeing the end of the era of that approach to parapsychology. It would be interesting to tone down the 'parapsychology is dead' message because depending on who is hearing it, the message may be interpreted in very different ways.

WALACH: I am glad of the precision that you bring in your comment, because this is exactly what I want to say; I think I actually said 'classical parapsychology is dead'. And by that I meant exactly what you said: the approach of squeezing the phenomena of parapsychology into a framework of classical experimentation that is only meant to extract causal signals. Because that is what we have done *ad nauseam* and it didn't help, did it? As for your other comments, I could not agree more.

VAN DE CASTLE: I want to add to the comment that was made earlier about the work done at SRI using remote viewing to speculate on the Silver Futures market. One of the big people for that had been Keith ('Blue') Harary, and it is true when they started out they were able to do very well in predicting the Silvers market, but then it went down. You can look at that in two separate ways. One is in terms of one of the most robust findings we have in parapsychology, which is the decline effect. Most of the time we find effects that start out strong but then goes down, and frequently it is because of the dullness of the task; for example, guessing 'star, circle, star, circle' and so on is very boring and the scores rapidly decline unless you can add fresh novelty to it each time. The second explanation would be to look at it in terms of Harary getting very egotistical about it; they were using it for their own selfish needs and making big money, and some psychics would say that if it all becomes too commercial then they lose the gift. But there is an interesting way to get around that. Out at the Monroe Institute they

have a lot of people who have gone through at least a week's training, and they take groups of people out to Las Vegas to apply what they have learned in the Casinos. But each group has to agree before they start that any winnings will go to a favorite charity, so that they will not personally benefit except in the sense of the love connection, or agape, that it feels good to be able to do this for my fellow human beings. It's not yet been published but I believe that for the last 10 years or so they have always ended up making money for the charities.

ACHTERBERG: You have triggered some thinking for me with respect to the prayer studies that have been discussed at this conference, which have started so well but the last few have gotten abysmal negative results. I was wondering if that is not a similar diminishment of results, perhaps because more experimental controls have been added, which could do it, but maybe because the study of the phenomena has a natural lifespan.

VON LUCADOU: Thank you very much for the comment on the stock market experiment. It illustrates exactly what Harald was saying about global variables and local variables. In the context of this experiment, I would say that novelty is part of the pragmatic information; it is a global variable because it can only be considered from the whole system, whereas replicability—to get the same again—is a local variable. You say novelty is a psychological variable, a condition of what is going on, and therefore is always changing. The difference in this paradigm of treating this as a psychological variable is only that in the model of pragmatic information this would be considered as a system theoretical variable—it does not depend on the psychological state but it depends on the state of the whole system. There are many examples where you can show that these are different ways to describe things, but I believe the psychological description provides a kind of language to describe the systems theoretical structure behind it. In many experiments—not only in parapsychology—you find this pattern. For instance, in therapy research if you have a new method then it contains a lot of novelty so you get big effects, and after a while you get the decline. This of course does not mean that the therapy is worthless because if you wait for half a century another therapist will come along when we have forgotten about this therapy and people will think what they have is new, so you have gained novelty in the system. The model is not, as some have maintained, nihilistic; on the contrary it entails the whole dynamic of the system, where novelty and replication

play against each other and so this makes the dynamic much more interesting than the pure signal model, which says everything is stable. Nothing is stable in self-organising systems, and the model of pragmatic information takes this into account.

ROLL: A related point of view has been presented by Michael Persinger with respect to replicability, and that is if you lay out the same road again with no reason except to replicate then the brain has no interest in pursuing that, it just shuts down with respect to that activity.

ACHTERBERG: I like that, because you do get tired of the same activity.

VARVOGLIS: I have a question for both Harald and Walter. I am trying to think in terms of how there is this split between microscopic and macroscopic referentials that say you cannot apply the same rules, 'as below so above'. How you make the transition from micro to macro is not clear to me, but I am sure there are equations somewhere in physics that say—other than from thermodynamics—why all of a sudden there are no non-local phenomena occurring in macroscopic systems. Now in the weak quantum theory I understand that the claim is that there *are*. So we have these systems that spring out once in a while with these non-local type correlations but they can't last once you try and turn it into a structure, and these run alongside systems that seem to be operating in a normal structured mechanical and reliable way. So my question is: what is the distinction between these two systems? Why are there some situations where entanglement occurs but it is very fleeting and goes away whereas with other systems mechanically reliable causal events occur?

ACHTERBERG: It is a wonderful question and I am following the work of Pizzi in Milan very closely because they are reporting what look like entanglement effects in macro-systems made of human neurons, so I am watching the evolution of that research. To my knowledge that is one of the few demonstrations in what would be considered a macro-system, and it's exciting research.

WALACH: I think this is an essential question but I don't have an answer to it. I think it is complicated because we would have to understand how this entanglement situation actually condenses into something that is stable all the time. I am not even sure that for quantum physics that is quite clear and it is certainly harder to understand in the generalized situation but I am not a physicist, maybe Brian knows that.

JOSEPHSON: As it happens I had a student who got a PhD on the subject of the boundary between quantum and classical but it was all buried in mathematical models and it could not be explained without looking at the thesis, which he has had trouble in getting published. On the other hand there are things involving Bose condensates where people are getting entanglement with more and more macroscopic systems.

SHOUP: I just wanted to say that entanglement disappears when it is measured and so it may be that some of these effects are simply due to using up the entanglement that has been built up in the personal or experimental situation. The opposite case would be if we had some way of generating entanglements which naturally happen when things interact—it's kind of a romantic notion that once we've interacted sufficiently intimately we are never truly separated again. But that can be destroyed by measuring it, and so may have some relation to what kinds of experiment we do on those effects.

VON LUCADOU: The mistake we have at the moment is that we say causality and entanglement are things that fight against each other. In reality, they help each other. If you have self-organizing systems, internally it is entangled; if you open it, it becomes disentangled. In the system itself there are also causal processes, so nature has elements that need to work together, much like causality and entanglement. What we do in experiments is separate them to study the properties of entanglement. You have to do very complicated experiments to see this. Normally we look only to the causal elements. These are very simple as you have one variable and another which depends on it. So, as we have to make nature simple, in experiments we have made this separation, when nature itself does not.