TOWARDS A METHODOLOGY TO HANDLE NON-LOCALITY IN SPACE AND TIME: THE END OF REDUCTIONISM?

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Please allow me to make a brief statement before I present my paper. It concerns the title of my contribution. As you probably know, the title had to be submitted a few months ago and I must confess that I did so without anything except the title, which read then: "Towards a methodology to handle non-locality in space and time: The end of reductionism."

While writing the paper a discontent with this title grew and I decided that the least I could do about it was to add a question mark. However, I would have preferred to change the word "reductionism" into "objectivism" for a number of reasons. The major reason is that my paper does not cover all three aspects of "reductionism." These aspects are:

1. The structural aspect. Or the question: Are all complex (living) systems reducible to a set of (non-living) systems?
2. The theoretical aspect. Or the question: Are biological laws explainable in terms of physical laws?
3. The methodological aspect. Or the question: Is the study of complex systems by studying a few selected parts or descriptors a valid method?

As you will notice it is mainly the methodological aspect that I will discuss. However, I will touch upon the theoretical issues near the end of the paper.

1. Introduction

It has become a cliché to state that scientific research into psi phenomena has shown hardly any substantial progress since its start a century ago. However, I would like to argue that its development, taking into account the limited resources and the difficult research conditions, has been satisfactory. While much of the energy of the researchers has been

spent convincing the skeptics that there even exists a subject matter to do research on, the process-oriented type of research has not succeeded in producing reliable results. This state of affairs is probably what the pessimists have in mind when complaining that little progress has been made. However, in the course of the last decade more and more researchers have begun to realize that the famous "elusiveness" of psi phenomena is not simply a frustrating factor, but the very core of the phenomena. Or in other words, that the unreliability reflects a fundamental property of the phenomena. And because, within the commonly accepted view of science, phenomena that cannot be measured reliably do not exist (are not real), these phenomena must be (if they are nonetheless real) "anti-scientific" in the traditional sense of the term. So the dramatic result of our 100 years of effort is that psi phenomena do not just question certain theories or models, but they question the fundamentals of science itself. With such a result in mind, I would not dare to speak about little progress. Of course, I realize that not everyone is yet persuaded that this result can be accepted.

2. The Basic Assumptions of Western Science

In an important article[1] that appeared in *Scientific American* a few years ago, the theoretical physicist Bernard d'Espagnat formulated the three premises of a classical or natural world view. These are:

"1. Realism: The doctrine that regularities in observed phenomena are caused by some physical reality whose existence is independent of human observation.

2. Induction: Inductive inference is a valid mode of reasoning and can be applied freely, so that legitimate conclusions can be drawn from independent observations.

3. Locality: The third premise is called Einstein Locality and it states that no influence of any kind can propagate faster than the speed of light."

Let us consider a bit more closely these three assumptions, which are thought to be so trivial that they need no proof, and also pay some attention to the practical consequences that they have for the commonly accepted scientific method.

2.1 Realism

The realism assumption is actually a double one. The first part is that regularities in observed phenomena have some underlying physical real-

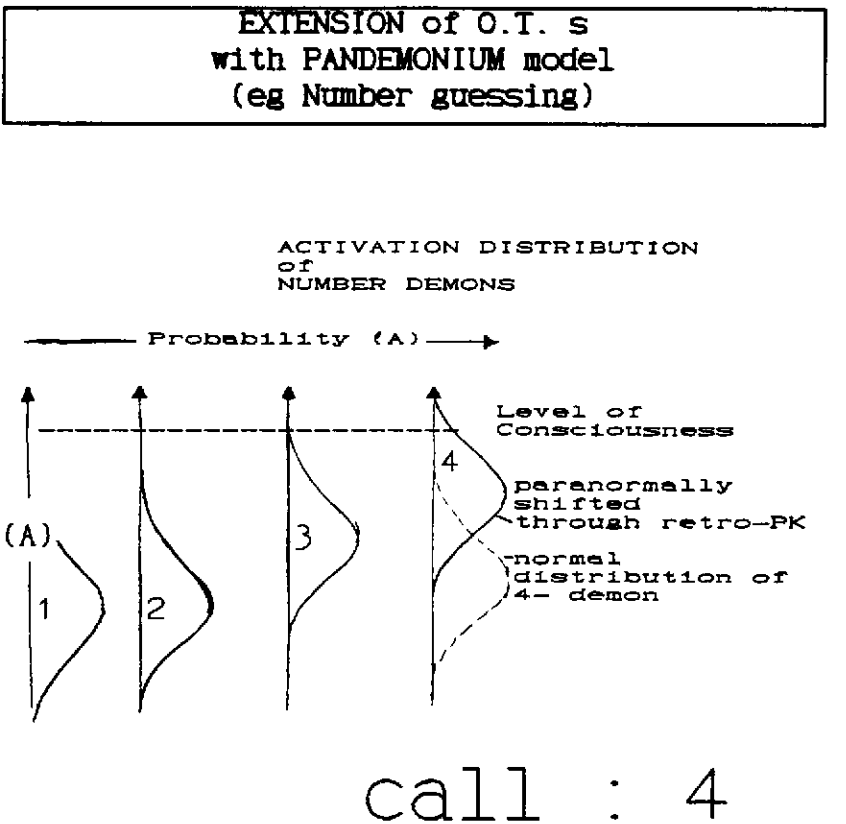

Figure 1

ity. This assumption of course is implicit in any definition of causality. It has resulted in a strong emphasis on the development of methods for research on recurrent phenomena in contrast to single phenomena.

The second part of the realism assumption is that this underlying (physical) reality is independent of human observers. I would like to call this objectivism. It implies, specifically, that the experimenter's contribution can theoretically be reduced to zero.

2.2 Induction

The second assumption that induction is a valid method is of course the essential assumption for allowing one to draw conclusions on the basis of statistical methods.

2.3 Locality

The third assumption, locality, is applied in empirical science through its logically somewhat weaker brother, which can be formulated as follows: The more distant in space-time two events are, the less likely is any interaction. For instance, the role of the experimenter in psychological studies is thought to be reduced by using taped instructions instead of oral ones.

3. Practical Consequences of the Three Premises

The practical implication of realism, induction and locality is that the use of the reductionistic method is warranted. This can best be illustrated by looking at a commonly used statistical technique, the analysis of variance. The underlying model is that an observed phenomenon in a system will show some regular variation depending on the variations of some characteristics (variables) of the system. However, these regularities might be obscured by uncontrolled variations of other variables. The technique thus in the end yields some unexplained regular variation, *which is assumed to be "caused" by these uncontrolled variables.* This last assumption in fact stems from the causality part of the realism assumption. The practical consequence is that for fruitful scientific study, this so called error variance must be, and according to the theoretical model can be, reduced so that the effect of the variations of the independent variables show up in a measurable way in the dependent variable. This idea, that we are in principle capable of describing a system, including the human subject in a psi experiment, by a finite number of variables is closely related to reductionism.

Note that objectivism can be considered as an extreme case of reductionism: the phenomena are thought to be independent of the observer, which amounts to eliminating one of the possible sources of error variance in our example with the analysis of variance.

One other straightforward consequence of the world view governed by our three premises is the demand for reproducibility of a phenomenon as a condition sine qua non for that phenomena to be considered as real. The only way out when faced with the elusive character of psi phenomena in the laboratory, within this world view, is to assume that the error

variance due to uncontrolled variables is so large that it obscures the reality of the psi phenomenon.

This common sense world view has been, implicitly or explicitly, the model used by the Rhinean school. Their explanation of non-reproducibility is that the (unknown) uncontrolled variables "cause" a lot of error variance. However, when the (Rhinean) mathematicians Greville and Greenwood[2] recently expressed their doubt about the mere existence of a distribution of psi, I think that they, in fact, doubted one or more of the premises that are so heavily related to the validity of the statistical method. The notorious unreliability of psi, the occurrence of new theories for psi phenomena which have strong analogies to and sometimes borrow their logic from quantum physics, and the empirical verification of retroactive psychokinesis[3] and future observer effects[4] all converge to a point where it indeed seems justified to question the assumptions themselves.

4. The Reality of the Micro World

In the forementioned article by d'Espagnat it is argued that in the world of the very small at least one of the three premises no longer holds. This implies that the reality of this micro world fundamentally differs from our commonly experienced reality. I will not give you the argument here, but will, instead, focus on the conclusion. After having shown, following an argument originally proposed by Bell, that all local realistic theories (thus theories that assume the three premises to be true) predict empirical results that (under certain conditions) differ from the quantum mechanical predictions and after an examination of the relevant data, he concludes that in the micro world non-locality should exist. Or to put it in his own words: "the violation of locality seems to imply that in some sense all these objects constitute an indivisible whole."

I am not quite sure if the scientific community realizes the danger of this development. Sure enough, by abandoning the least fundamental of the three premises, d'Espagnat avoids being put to the stake. After all, this funny nonlocality correlates atoms, not human beings! Nor does it correlate objects with human beings. Nowhere does d'Espagnat say that a token object might carry information of its owner.

Unfortunately for d'Espagnat and the scientific community not all problems are removed from quantum physics by accepting non-locality. First of all, the acceptance of non-locality implies some form of time independence, as space and time cannot be uncoupled. This time independence has recently received a lot of attention in theoretical approaches to psi phenomena. There, it has been commonly interpreted

as something that might possibly link any future observer to the experiment. I would like to argue that time independence in the micro world is by no means so far reaching as that put forward in the observational theories. To understand this, let us have a look again at non-locality in space. There, two particles A and B seem to communicate information unhindered by spatial distance. It is as if A and B have some link to each other before they are measured, but only after the measurement are A and B uncoupled. Hence time independence also has to break down after the measurement. I think that time independence here should be interpreted as: "It does not matter how long you wait before the measurement takes place, the link will continue to exist up to that moment." In terms of psi experiments this would account for PK on prerecorded targets, but not for extreme future observer effects. Secondly, the reasoning followed by d'Espagnat also implies that quantum formalism gives the most complete description of nature, because local hidden variable theories which could add something to quantum formalism fall in the same class of dismissed theories.

What local hidden variable theories seek to add is information that describes the outcome of the experiment after the interaction of the particles, but before the measurement. Such theories try to remove the problem that "God seems to play with dice," a problem that occurred when classical deterministic causality was replaced by a fuzzy form of it, a kind of probabilistic causality. However it won't work: we have to accept that the most complete theory does not give more than probabilities. The problem is, then, that we don't experience probabilities or fuzzy states. Where does the transition from the fuzzy state (which is a complete description) to a discrete state (which is what we experience) take place? Remarkably enough, d'Espagnat does not even pose the question, but the editor of *Scientific American* answers it! The subtitle of the article reads: "The doctrine that the world is made of objects whose existence is independent of human consciousness turns out to be in conflict with quantum physics and with facts established by experiments." This is, of course, the final blow to objectivism.

So if I postulate that, within the development of psi research, we have come to the point of questioning the fundamental premises of our world view and science itself, we are in good company. However, let me hasten to stress that this does not mean that psi phenomena are "just macroscopic quantum phenomena" or that a minor extension of quantum physics might yield a theory that incorporates psi phenomena. For instance, in the current psi theories that are closest to quantum physics it is not just assumed that it is the conscious observation that turns the fuzzy state into a discrete state (retroactively). This would be a close

analogy to the solution of the "measurement problem" as proposed by the editor of *Scientific American*. But alas, the observational theories add another postulate to this, namely that this process is not a passive, but an active one, during which a flow of information can go from the observer into the observed system. This, of course, is fundamentally different from even the most revolutionary interpretation of the quantum mechanical view. And before, I mentioned that there is also a fundamental difference between the time independence that accompanies non-locality in quantum physics and the time independence postulated in the observational theories.

I agree with some theoretical physicists[5] that we should be extremely careful in discussing psi phenomena in terms of quantum mechanics. Of course using one theory as an analogy for the developments of others is quite legitimate, but we should emphasize that it is only the use of analogy and not the use of quantum physics itself.

5. New Methodologies

So let us assume that locality, objectivism and hence reductionism are invalid assumptions in a world with psi. As all our scientific methods are based upon these assumptions, we have to conclude that we need new methods to study psi phenomena—methods that deal with the possibility that ". . . in some sense all these objects constitute an indivisible whole . . ." This conclusion is by no means a new one. A few times before the need for a new methodology has been expressed.[6] However, that is the easy job. Finding a new methodology is another and more difficult task. When I try to think about it, I find myself again and again trapped in the reductionalistic way of thinking. I have started to wonder if it is even possible for a western scientist to escape the fate that is impressed upon him by his culture.

5.1. An Example

For instance, consider the idea that the experimenter is part of the experiment itself and that there is no way to isolate him: large distances in neither time nor space will help to reduce the system. One could do a series of experiments in which the experimenter is an independent variable in order to control for his/her contribution to the results. In fact such an approach, though mainly based on other arguments, was proposed by John Palmer in his presidential address during the Parapsychological Association convention in 1978. Note, though, that this is by no means a revolutionary methodology. It is just another try to reduce the error variance. Objectivism is lost, but in this approach we

seek to control for it. Somehow there is something wrong with the logic. And it can be worse: let us suppose that future observers of our results do participate in our experiments. What we could do is formalize the procedure of distribution of our results to the outside world; try to control for these future observer effects as far as we can. But again, we fall into an extension of the commonly used methodology and not an essentially different one.

It has become a standard feature in my experiments that empirical data are split in two parts which will follow (in the future) a different observational history. In practice both parts are to be analyzed by different analyzers. If both parts happen to show similar results, one might interpret that as the psi effects being robust against psi contributions by future observers. The weakness of the argument, given that the psi strength of the analyzers might vary in magnitude and direction, is obvious, but again what more can we do? We can only hope that these small steps within the only available methodology, small steps that introduce non-locality in space and time, might be the first steps in the direction of a fundamentally different methodology.

5.2 The Multi-Experimenter Experiment

Currently underway is an experiment that combines these small steps sketched before. Eight experimenters in The Netherlands perform, more or less simultaneously, a precognition experiment. All the experiments are conceptual replications of each other, with the same hypotheses and procedures. After the completion of the data gathering phase, the results of each experimenter are to be split into two parts which will follow a different observational history controlled up to eight subsequent levels. Note that this enterprise is not done to convince critics that we are not frauds; it is based purely on the considerations that I have given above.

6. The Need for a Holistic Approach

6.1 Theories

The fact that the scientific study of the micro world has revealed the falseness of one of the basic assumptions of the natural world view and of science itself seems to be inconsistent. However, the quantum mechanical theory is not dependent on the assumption of locality. And it solved the apparent loss of objectivism by the assumption that the fuzzy state reflects the frequencies with which certain outcomes will be measured upon repeated measurements, giving up predictions concerning

singular measurements. The confrontation with a reality so different from our experienced reality could only be handled by the use of mathematical models. I am convinced that theoretical developments in psi research also will have to follow this route of mathematical modeling.

6.2. Extension of Observational Theories

The observational theories were the first to introduce non-locality in space and time explicitly in a theoretical framework and thus they indicated a reality that is reminiscent of the experiences of the mystics, who speak of a spaceless and timeless oneness. This wholeness, implied by the observational theories, contrasts strongly with the reduced picture of the human element in the theory. The psi source, as this human element is called, is represented by a single (in the hierarchical model

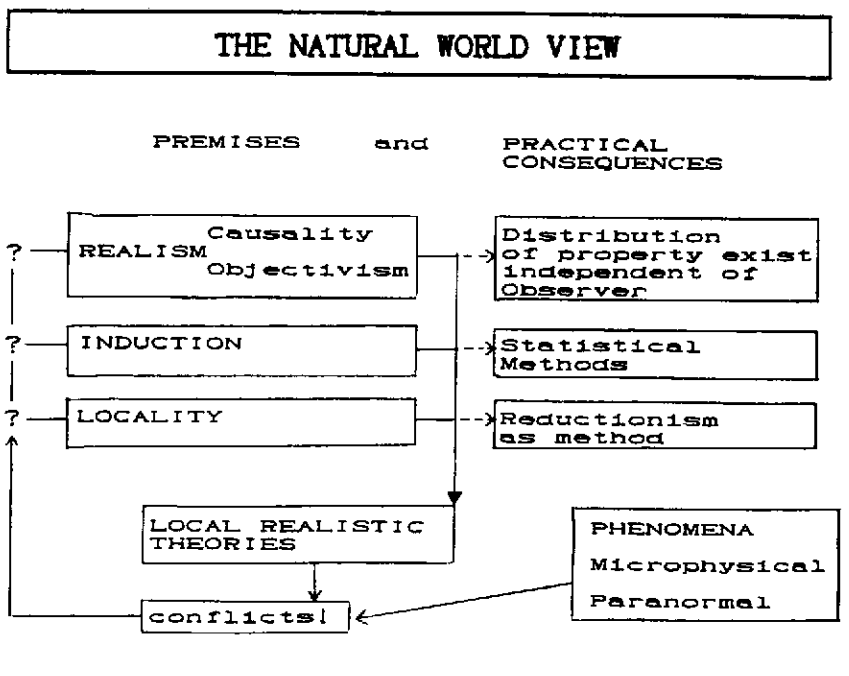

Figure 2

two) figure(s). In no way is the relation of this figure to other descriptors of the human personality discussed. If we want to make any progress at all, I think that it is necessary to extend the observational theories to personalize the psi source. I would like to propose that the class of "Pandemonium" like models of cognition are suitable candidates to link to the observational theories. These mostly qualitative models have in common that our brain functions as a huge number of parallel processors, each designed to "seek" in the input stream for specific patterns. As soon as a more or less similar pattern is detected, this processor, cognitive unit or "Demon" (as we will call it here) becomes more active. If its activity surpasses the threshold of consciousness, the ongoing conscious processing is interrupted to evaluate this highly activated Demon. Recently, I tried to specify quantitatively a set of activation and deactivation formulas. Also I tried to specify the rules governing the development of the representations of the patterns within the Demon and of the associations between them (learning). It turned out that it is virtually impossible to have a working Pandemonium model without the introduction of a random element in the activity levels and in the internal representation of the patterns. Note that this result was not obtained by considerations about human creative behavior and the like, it occurred as a logical consequence in the course of the development of a computer simulation of "learning."

This result sheds a new light on the meaning of the already known random firing of neurons in the brain. It has been generally assumed that these latter random processes were the target of retroactive PK within the observational theories. This could presently be replaced by the more meaningful assumption that psi works directly on the random component of activity levels of the Demons. In the Pandemonium-like models, most of the Demons have an activity level far below the level of consciousness. In this "world" of the subconscious, however, activations are continuously changing. For instance, subliminal perception is an activation just below the consciousness threshold. I propose that psi induces a shift of the distribution of activity levels of a given Demon. The combined theory suggests a research program with ties to subliminal perception research. For instance, I would predict that sensory threshold measurements where no feedback is given would yield different results than measurements where feedback is given (of course, we have to control for a learning artifact). The feedback is necessary to trigger the psi process, according to the observational theories. The random element in the activation level of the Demon that corresponds to the subliminal stimulus is the target on which the psi works (retroactively), according to the present extension.

This is only an example to illustrate the power of a more holistic theoretical approach. This approach is important because it might be an avenue to a better understanding of the specific forms under which psi appears when specific personalities are involved.

6.3 The Place of Psi in the Cosmic Scheme

The scientific study of any subject develops very quickly to a level of specialization where the original problems and questions are lost sight of. Most of the attention of today's psi researcher is devoted to the specific questions dealing with the optimal conditions for psi to appear and with the potential underlying processes. What I want to discuss now is: "What is the purpose of psi?" Those who adhere to the spiritualistic hypothesis would probably answer that there is not really a purpose, but that psi is a byproduct of the spirit world that once in a while interferes with ours. The more abstract dualists do not primarily consider psi in terms of purpose, but emphasize that psi reflects the non-material side of our being. Some researchers have proposed that psi is the vehicle through which the mind controls any original process in the brain. Its purpose is then supposed to be the "go between" of mind and (brain) matter. In this model, psi is quite normal in the sense that it is present in everybody.

None of these considerations really satisfy me. About 20 years ago, Randall[7] proposed that psi played a role in biological processes and most notably in the course of evolution. It is well known that there is serious doubt that the (neo) Darwinian picture of evolution is the complete picture. The major problem is that the probability that a structure which contains as much information as our genes develops by a series of chance mutations is extremely small, even with the most optimistic estimates about the rate of mutations. Randall's proposal, however, was a mere guess, because he did not and could not give the mechanism for this postulated psi contribution to evolution. Following the empirical verification of retroactive PK, we know that chance processes in the past might be biased according to the "wish" of a future observer. Furthermore, it is generally assumed that psi is goal-oriented: in other words, the complexity of the chance process involved (e.g., a mutation) is unimportant. Therefore, it seems natural nowadays to specify Randall's missing mechanism as being "collective retroactive PK." This process should be very weak because large transfers of directed information into the genes would inevitably lead to oscillations in the development of the species. Within the proposed perspective of the purpose of the psi process, laboratory experiments and spontaneous cases should be inter-

preted as anomalies, centered around individuals instead of the group. I must confess that I have no idea what the purpose of such an anomaly (within the anomaly of psi) might be, but neither do I have an idea about the purpose of other "diseases."

It is remarkable how emotionally the scientific community reacts to any suggestion that group interests might play a role in evolution. However, this can be understood, because the real underlying threat factor is the loss of locality that is implicit when groups are considered to be a "causal" factor in evolution.

7. Conclusion

We are in a paradoxical situation: We want to study psi phenomena; we want to do this in a respectable scientific way. But the phenomena themselves undermine the respectable scientific methods. As Millar has put it: "We are like the guy who invented the universal solvent, but found soon enough that there was no bottle to put it in."

I have sketched small steps toward developing a "bottle" that takes into account some of the aspects of the universal solvent. But I have also indicated that there is something contradictory in this approach, that we can only hope to find that the universal solvent is not really universal. And there are, of course—let's be optimistic about it—there are grounds for that hope. I have argued that time independence is not necessarily as radical as originally proposed by the observational theories. Also, if we look at the supposed purpose of psi, its contribution to evolution, I don't see any reason for universal time independence. And last, but not least, we find from time to time some weak forms of reproducibility in our experiments.

I am convinced that at our next centennial celebration all these considerations will be viewed as extremely simple and naive. I hope, though, that people will say: "It was in the right direction, you know."

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DISCUSSION

BELOFF: Well, as a believer in common-sense objectivism, I naturally disagree very strongly with the point of view that the speaker has expressed and I earnestly hope that parapsychology, as a whole, will not be bamboozled into thinking that now they've got to adopt this neo-idealist point of view. The point I would like to make is that, of course, the idea that reality is mind-dependent is a very ancient one indeed. It is known in philosophy as idealism and it just so happens that, in recent times, it has drawn some strength from the study of subatomic particles, and the sort of considerations that Dr. Bierman was referring to in discussing d'Espagnat's ideas, and so on. It's too much to hope that we can really sort out our differences in a few exchanges of this kind, but I would, before sitting down, like to point out how terribly implausible and incongruous is this point of view, when taken into the wider cosmic considerations. I mean, science teaches us that the universe existed long before there were any such things as observers or minds or human beings, and it will probably continue to exist long after we have all wiped each other out. And, much the greatest part of the universe is occupied by matter—but without any form of life at all. In that kind of cosmic picture, to suppose that what exists depends upon what one observes seems to me a real sort of learned foolishness, if I may call it that.

BIERMAN: Well, I think John Beloff misunderstood some of my remarks. I want to stress that this loss of objectivism does not apply to all of our scientific enterprises. It's just in the field of psi phenomena where it seems to be that objectivism is lost and, maybe, not to such a degree that we cannot handle that within our cultural-based methodology. But what I want to express is a serious doubt that there might be a possibility that we cannot handle psi phenomena with the current methodology we have. Now, it might be that there is another methodology which may be as respectable a science as the present one is. But I don't see one and so I have tried to show you my own struggle with this problem. I think we should always try to base our attitude on facts and I think that the main fact here is that psi seems to be, to some degree, time and space independent and one of the characteristics is its unreliability. If we, nevertheless, accept that it is real, then we have a paradox. But I can live with a paradox for some time.

BLACKMORE: As you know, Dick, I don't have much faith in them, but I have a great interest in the observational theories. I don't think they hold out much hope for getting us anywhere in understanding psi, but, given what you've talked about, I would like the opportunity to

speculate a bit. You've talked about the need for a drastically different methodology and it strikes me that one of the things that you could have said, and didn't, was that our science really depends very much on public sharing of information and what the observational theories may be saying is that we can't share that information. We can't give our data to people. We've got to keep them to ourselves. So, doesn't one of the possible methodologies—and perhaps you are actually doing this and you can't say so—involve keeping it all to yourself, a purely private science? One of the reasons why I reject the observational theories, personally, is that, on the one hand, you could collect your data and share them at the bit level with everybody in the world. That would lead to all these future observers. Or, you could simply share them on the P-value level. Or would this be equivalent? I don't know if they give an answer. Would it be equivalent if you simply said to me, "I can't tell you, but I'm convinced I've found it"? Would that be equivalent to giving it on any other level? I don't think the observational theories can answer that, but I would like to hear your views on it.

BIERMAN: Communication of data or results to other people seems to be fundamental to science. I think I would like to stick to that as long as possible. Otherwise, we won't have these nice meetings anymore. But, nevertheless, I agree that it might be that we come then to a situation where the best we can get is some description to give someone of how to get into a state, for instance, in which he can experience psi phenomena. Something like that and that's all. Once you have reached that point you don't need any real scientific conferences anymore.

Your second point is, more specifically, about the observational theories. Do they discriminate between giving information at the specific bit level or at the run score level. Does it make a difference if I show you exactly the results which I have shown to my subjects, or is it different if I only show you the P-values, or give even less information—if I say only "Well, it was a very nice experiment. Thank you. I enjoyed it very much"? I think there might be a difference and the observational theories are still struggling with this. I think Walter von Lucadou was one of the first who recognized that we should talk about meaningful information and try to quantify this. But I think, until now, we have not yet been able to specify how much information can be given to the scientific community before it really becomes "participant" in our experiments.

SERVADIO: Contrary to my old friend and colleague, John Beloff, I was very much in admiration of the paper that we have heard. I only regret that I had not read it before, because my own paper, that you will hear tomorrow, will develop many of these items. The sorry fact is that we are in a dualistic situation, whatever we may do to avoid it, so

that, even when we are reasoning or thinking about some problems, we try to escape this and reach a sort of unity—to by-pass the dualism, we are almost compelled to establish new entities. For instance, this Demon of Dr. Bierman's is already objectifying something that, of course, is not an object; that is, it's just an idea. But, as I said, our tragedy is that we are in a dualistic world and we try with all our efforts to by-pass it.

BIERMAN: That's exactly what I mean when I say that I'm afraid that a Western scientist cannot escape his fate, impressed upon him by his culture. We are struggling to escape this, but I'm afraid we will stay struggling.

RONEY-DOUGAL: Dick, you take the three premises from the *Scientific American* article and you call it a natural world view. Well, I wish to question that premise too, because, from what I gather by my reading, this natural world view is, in fact, a very recent world view. And, it is one that has been promulgated only within the last one hundred years. Up until that time, it was the most *un*natural world view. I would also say that it's a natural world view only for a very few people living in the Northern Hemisphere—farmers, ordinary people. You go to most people living in the third world and, for them, it is still an unnatural world view.

BIERMAN: That doesn't hold for all three premises. I think it certainly doesn't hold for locality. I suppose that also ordinary people think that the more distant in space and time two events are, the less related they are.

RONEY-DOUGAL: You could talk to some shamans, or some Indians or even some physicists and they would not accept that. So then you go to anthropologists, and you look at different societies and how they view the world, and you find it is completely different. So we must take into account what we are learning from anthropology when we devise what we call a natural world view. I fully agree with you that we do need a new methodology. I disagree with you that we must take small steps, because I think that small steps are only for small minds. I think that you have got to be brave and really step out and try radically different methods. We've been too conditioned by our scientific training.

BIERMAN: I've heard this before—many, many times—and I've never heard what these radical steps were. And that's what I mean when I say that it's easy to propose a new methodology, but it's very difficult to find one that makes some sense. I'm very much against throwing the baby out with the bath water.

RONEY-DOUGAL: Right.

BIERMAN: Yes, I would really object to that. Now, with regard to your objection to the word *natural* in "natural world view," I admit that

it's the world view of the last two centuries of Western science and that's exactly what I wanted to discuss. What are the fundamentals of our present-day Western scientific methods? I want to find out whether these assumptions are valid or not.

SARGENT: I just wanted to support Dick against John. As I understand it, Dick is not promulgating some kind of reincarnation of Bishop Berkeley. Neither is anybody else. I thought the idea was not that reality was dependent on mind, but that reality was not totally *independent* of mind—which is a different kind of claim. It may indeed be that this is a learned foolishness, in much the same way in which, as Galileo says, Kepler's idea of the moon influencing the tides was an occult fancy. So, I'm not very impressed with that argument. What I really want to try and argue for is: Why on earth must we continue to have one methodology which we must use? Why shouldn't we suspend judgment and use Dick's methodology with things like the analyzer splits and see what happens? Now my instincts about the observational theories are largely negative, (a) because I don't understand them; (b) because the people who argue for them are strange gnome-like people whom I don't always trust and (c) because, being a psychologist, I'm really not trained to understand the language in which they are couched. So it isn't only that I can't even understand the theories; I can't even understand the language in which they are discussed. But, nonetheless, I am prepared to say, "Well, let's take some of the methodology over from these models." They are suggesting radical and strange lines of experimentation that would never have occurred to me, from the way that I look at the world. Now, I have done this, but I think that most people don't do this kind of thing. I don't think they do tend to do experiments that don't follow from their own natural ways of looking at the world. But, I have done this and such experiments have generated findings that have made me stop and think because they were not what I expected. They were not compatible with the ways that I think and they force me to become cognitively uncomfortable, to have to accomodate new facts and new ways of thinking about facts. If there is one thing that I think is very good for people, it's something that upsets them. It's things that break into their natural patterns of thinking and really force them to stop being comfortable. To be comfortable is absolutely antithetical to any kind of higher mental functioning. Now, this is important, and my argument is that the methodology that Dick is suggesting should be used particularly by those who think that it is of no use for getting anywhere, because the results may force them to really start thinking much harder about what they are doing and why they are doing it. So, I support Dick very, very strongly against you, even though I don't think the observational theories will

get us anywhere. I would support Dick against Sue in the same way. I don't think they will get us anywhere, but I'm prepared to give it a whack and find out what happens. And I think we have to be prepared to do that.

BIERMAN: I'm glad that your prejudice against the gnomes doesn't hinder your being persuaded by the facts.

BLACKMORE: You don't need to defend Dick against me. I'm prepared to give it a try, too. I just want to make the same objections to you—I don't think the observational theories will get us anywhere.

BIERMAN: I think, maybe in the past, some of these methodologies have been used unintentionally or without people knowing what they were doing exactly. There have been analyzer splits, in fact, the Fisk-West studies can be interpreted in that way. There have even been two-checker effect studies, explicitly doing just what I have tried to do in the last four or five years. They have shown the remarkable effects of future observers—future with respect to the moment of the experiment—on the results. I think that there are already some facts which support future observer effects. Maybe the observational theories don't have much appeal for two of our participants here, but honestly, I don't see any other theoretical framework which could explain experimenter and analyzer effects so well.

VON LUCADOU: My point is on reductionism. I thought that you used a very special notion of reductionism, what I will call a classical form of reductionism. But I think, especially in physics and in other sciences, this sort of reductionism is composed of several operations, so to say, such as elimination. For instance, if you say, "a witch is a neurotic person, but a witch does not really exist," this is called elimination. You eliminate a notion. And you say, "This is nothing but . . ." This is the terrible "nothing buttery." And another operation is identification. You say, for instance, "Heat and light are nothing but electromagnetic waves." Another operation is transformation. You can say that, for instance, ideal gas and CO_2 are examples of Van der Waal's equation. So you make a transformation from one notion to another. So this is classical reductionism, and I quite agree with you that this classical reductionism is not useful to explain, for instance, quantum physics or to explain parapsychological effects. But scientists have for a long time used (probably unconsciously) another form of reductionism, which can be understood from the point of view of system theory. I have stressed this in a paper which will appear in the *European Journal of Parapsychology* and I have stressed there that you can replace these old transformations by new ones, and you will again get a kind of reductionism which is more open to our problems. The operations are called idealization, which is really

done in every scientific theory, because you have to omit some features—for instance, to explain something. But this is not the same as this "nothing buttery," because you are aware of your idealization.

The next point is interpretation. Well, this is a very important feature of quantum physics, because you have a formalism and what you are doing is that you interpret it, for instance, in the language of everyday life. So this is an important operation in dealing with formalisms. You must be aware of the fact that you are doing an interpretation, because you might omit information or you might put more information into the system than you really have. So, the last operation, I call classification, and this is known in psychology as the perception of the gestalt. This is important, for instance, in parapsychology. You attribute meaning to a certain phenomenon and you know the meaning is not identical with the phenomenon because probably you attribute some meaning which is not really there. But, if you are aware of this, you are allowed to do so. If you agree with this kind of reductionism you are more open to our problems and you can show that you can solve a lot of paradoxes which seem to appear in the realm of quantum physics or in observational theories in this methodological background. I am optimistic that we really have such a methodological background to describe the phenomena. I think that this has been used in science for several years, but it was not formulated explicitly. So, I think we have gone a step forward and you probably only criticize a position which is already out-of-date for some scientists.

BIERMAN: Your statements bring me to a part of my talk which I skipped, but which is relevant with respect to your idealization interpretation of reductionism. I think that there is also some paradox in the mere existence of quantum physics, because quantum physics showed that non-locality exists. Nevertheless, it's a very respectable science. How can a respectable science show that one of the fundamentals of science is questionable? I think that the answer lies in the idealization, since quantum physics is a mathematical model. If I had to bet on it, I would put all of my money on the idea that parapsychological progress will be made by formulating mathematical models which are able to overcome these apparent paradoxes and can reflect non-locality in space and time. That's no problem in mathematics. So, I really see, and hope for, the blooming of generations of mathematical models describing psi phenomena.