SOME SUGGESTIONS FOR METHODOLOGY DERIVED FROM AN ACTIVITY METAPHYSICS

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Parapsychologists sometimes see themselves as forerunners of a new science in which the data of parapsychology force normal science to rethink its discipline. It has become commonplace in parapsychology to assume that the data of parapsychology will force a radical rethinking of the nature of the world for normal science and in that sense parapsychology will be the bearer of a new paradigm. While I think that parapsychology has much to offer normal science, I am much less sanguine about the possibility of our forcing normal science to change in fundamental ways. One reason that this is so is that normal science is so intransigent; however, I suppose I am much more pessimistic, looking at our past, about producing data that are so convincing and radical that they will force a paradigm shift. In fact, the point of this paper is to say just the opposite: parapsychology has always been much more influenced by normal science than the reverse and I am suggesting that we ought to continue to be so. Methodology that we have accepted as traditional has been taken over from normal science, particularly behavioristic psychology and just as there are new winds blowing in normal science toward new experimental methodology, so we ought to enjoy the breezes and learn from them.

What I propose to do in this paper is to outline the historical roots of the traditional methodology accepted in psychology and parapsychology and point toward another research exemplar which yields some interesting methodological conclusions. The historical and philosophical considerations, which comprise the first part of the paper, will attempt to show that Western thought has generally held two positions, to a great extent assumed in discussions, and these have affected our methodological considerations. The two points are: a foundationalism, which urges that there is ultimately some absolute foundation to knowledge and an entity metaphysics, which urges that the world is made ultimately out of discrete things or entities. I want to show how these assumptions have developed in Western culture and how they are assumed in much of

contemporary methodology. Finally I will suggest trends which are not based upon these assumptions and examine how these trends may affect experimental design.

PART I

Historically the first building block in the traditional Western view is seen clearly in Plato. While the *Republic* is ostensibly dedicated to outlining an ideal state and telling us what justice is, the heart of that book is the elucidation of Plato's ontology and epistemology, Plato argues that there are Forms, non-material, eternal and absolute entities which are known through the mind and the objects of knowledge must be these Forms. Other objects, in particular physical objects, are only veiled representations of these ideal Forms and our knowledge of the physical world can only be mediated by the Forms. This separation of the Forms, which are known by the mental, from the physical world is the basis for Plato's distinction between reality and appearance. Only the Forms are real and physical objects can have reality only insofar as they somehow "participate" in Forms. It is interesting to note that this ontological and epistemological distinction also forms the basis for Plato's ethics, so that the Good, which is the Form of Forms, is in the realm of the mental, while the physical world is therefore less valuable. One can see the implications of this doctrine in traditional Christianity and I believe that it is to a great extent Christianity which perpetuated these assumptions in Western culture.

To recapitulate, then, what Plato has argued for is that there are Forms, a different and distinct Form for each mental object, and that these Forms are ultimately known through reason. Thus begins, I believe, the assumption that reality is ultimately composed of entities and there is an ultimate authority for knowledge, which is reason.

Rene Descartes, the Father of Modern Philosophy, perpetuates the same assumptions in his works. For Descartes what is known first in time and best is the existence of the self, which is radically simple and unitary. In Cartesian language it is a substance, which makes it completely independent of any other thing (except God) in order to exist or continue to exist. After proving a non-deceiving God, Descartes proceeds to prove the existence of the material world, which is a substance whose attribute is extension. Mind for Descartes is diametrically opposed to matter, making the separation between mind and matter even more distinct than we find in Plato.

If we step back from the arguments themselves and ask why Descartes may have wanted to propose such a philosophy, several points seem obvious. Descartes, himself a scientist, wanted a realm in which the ad-

vances of a mechanistic science could be safely felt, while still saving freedom and purpose for the realm of the mind. Wanting, however, to save the priority of mind, it was mind that was known best and even the physical world is best known through reason. Reason for Descartes became the ultimate foundation for all knowledge and his substance view of mind and body perpetuated the entity metaphysics.

It is an easy transition to go from the rationalist tradition, represented by Plato and Descartes, to the empiricist tradition, represented by John Locke and B. F. Skinner. Locke retained the entity metaphysics of tradition in his description of the mind as being composed of ideas. Just as Newton envisioned the physical world as being composed of minute, indestructible bits called atoms and the empiricists accepted this description of the physical world, so the mind was composed likewise of indestructible bits called ideas. Just as it was conceived that everything in the physical world could be explained by referring to these basic atoms and the laws of how they associate, so it was argued that all mental phenomena could be explained by referring to ideas and their laws of association. Thus entity metaphysics is retained.

Foundationalism is also retained, but the authority changes. It is no longer reason that serves as the foundation of knowledge; it is sense experience that does so for the empiricists. The mind is a blank tablet at birth and all knowledge ultimately stems from sense experience which writes upon that tablet. What is interesting about the empiricist tradition is the belief that this basic sense experience gives us immediate and unexpurgated access to reality such that one cannot be wrong about this basic experience. Unlike Descartes, who argued that our senses can lead us astray, Locke viewed perception as being like a camera taking a picture and the camera does not lie. It mirrors what is actually there. John Locke was adamant about this point, saying such things as "simple ideas are not fictions of our fancies, but the natural and regular productions of things without us, really operating upon us."[1] Such basic experience forms the Given of knowledge and all knowledge must be based upon it. Where we go wrong in our knowledge is that we either fail to restrict our knowledge statements to what has ultimately been experienced or we may fail to name it adequately, so that our language fails to mirror what has been given to us in this prelinguistic awareness. Foundationalism and entity metaphysics form the basis, therefore, of the empiricist tradition also.

PART II

What is happening on the contemporary scene? I hope it will not be too gross an oversimplification to say that there are essentially two thrusts

traditionally in psychology, a humanistic thrust and a behavioristic thrust. Each of these can be viewed as stressing one aspect of the dualism of Descartes or the other, although the separation between the mind and body may not be so great. Insofar as humanistic psychology asserts the self as the primary fact in psychology, it accepts entity metaphysics with its implications.

Behaviorism, which lays stress on operant conditioning, can be represented by B. F. Skinner. If we keep in mind the historical tradition which I outlined, we see that Skinner is a modern day Lockean; he simply talks about the laws associating behaviors rather than ideas. On the other hand, he took seriously the Cartesian dualism of mind and body and simply ignored one-half of that ontology. After all, Descartes had created such a radical separation between mind and body so that it would be possible to have an objective, deterministic science of the body, so if one wants to do science, why should we even consider consciousness? I will not belabor the traditional philosophical difficulties with dualistic interactionism, but simply point out that Skinner's position follows directly from a Cartesian position. Given such a radical separation between mind and body, it is altogether natural for the scientist to do exactly what Skinner suggested. But consider what this implies: it means that Skinner seems to have accepted the entity metaphysics of Descartes and just eliminated a mind entity from it. If one takes the assumptions of entity metaphysics along with the linear mechanics which influenced psychology, we have operant conditioning. We have the view that there is an organism, a separate entity from the rest of the environment, which is affected by the environment. Just as Descartes viewed the world, including the body, as a machine, Skinner's is a mechanistic model, which therefore stresses structural components. There are parts of a machine and you can affect some parts of the machine so that you get different results. Hence you have the idea of a dependent variable, which is easily isolated from the environment (since the world is essentially composed of parts) and the independent variable affects it in certain ways. If we are dealing with different entities, different parts, we ought to be able to manipulate the environment in such a way that we can control all of the factors except one, which is the independent variable. In essence operant conditioning becomes the idea of an isolated and isolatable factor affecting another isolatable factor, while all other factors are being held constant. This causal relationship takes place linearly over time so that it is always the independent variable which gives rise to changes in the dependent variable. Since we are dealing with structurally independent entities, experimentation is fairly straightforward. What one does is to run a baseline, then manipulate a variable, measure the effects on the dependent variable, take away the stimulus and essentially perform an-

other baseline experiment. Thus the organism serves as a control against itself. Because operant conditioning viewed this process as so straightforward, behaviorism has always argued that statistics should be used only minimally.

There is a statistical branch of methodology, however—one which parapsychology seems closely akin to in its methodology—which urges that statistical analyses are necessary. What is important to point out here is that the metaphysical assumptions of the statistical approach are precisely the same as the behavioristic approach, but there is merely disagreement about how much one knows and can manipulate. The proponents of the statistical approach will argue that the operant approach assumes that we know all of the relevant factors to control the environment such that it is only the independent variable which affects the dependent variable and that these factors are easily controlled. Not so, argue the statisticians. There is too much background noise, particularly in organisms as complex as human beings, and the only way that we can tease out what the relationship is between the dependent variable and the independent variable is to run a great number of experiments and statistics will eliminate the random background noise. In other words, the statistician urges humility before what is known and suggests that if we throw a barrage of experiments into the analysis we can tease out what we want to learn. The disagreement between these two approaches, therefore, is only about how much noise there is in the system and it is not a disagreement of a fundamental sort.

I would suggest that virtually all experimentation in parapsychology falls into this classification and therefore assumes an entity metaphysics. What I would suggest in its place is a view in which entities are not viewed as primary, but rather seen as functional designations within a flow of experience or within a system. If there is a clear philosophical predecessor, it is the pragmatism of William James in "Does Consciousness Exist"[2] and of John Dewey in his analysis of "experience."[3] For Dewey experience is more like a flow in its ultimate constitution (not the mental act of mental images directed toward physical objects) and we create parts of experience based upon what we want to do in experience. We find that what we can refer to as entities are pragmatically designated parts of experience which serve some function. There is no inherent or pre-existing separation in experience, nor is there any inherent meaning.[4] What Dewey calls "an experience" turns out to be a separation that is placed upon the flow of experience and the criteria for separating one experience from another seem to be almost aesthetic. It is a felt closure that separates one experience from another, not a pre-existing, structural separation.

If we look at the contemporary scene in normal science, my sugges-

tions fit most neatly into a systems approach to knowledge, but I am unwilling to accept all of the assumptions of this approach. For instance, a key item of a systems approach seems to be a cybernetic view, one in which elements mutually affect each other through feedback and manipulation. Although I have no objection to viewing some kinds of systems as working this way, it strikes me as mere prejudice to assume that all systems must work on this model. The cybernetic component of the systems view strikes me as relying a bit too heavily on an entity metaphysics and thus it may represent what the Marxists call a downward pull in the dialectical progress of our knowledge. The cybernetic view is certainly right in urging a mutual interaction among the elements of a system, but it may be that the view stresses a bit too much the fact that the elements may be pre-existing elements and not simply elements that have been picked out as functionally interesting for a particular analysis. In the past I have called my view "activity metaphysics." It has close ties with the systems approach, but let me call it a "field" approach since I am not sure I accept the excessively cybernetic assumptions of traditional systems theories.

PART III

A. There are two aspects of the field theory which are of methodological concern. The first is the structural dimension and the second is the functional dimension. The first deals with the simple structure or mechanism of the system or field. One must be concerned with the structural components of any system; one cannot understand a television set without understanding the individual structures within the television set and how they are causally related. The structural component can be viewed as the mechanical aspect of the system. The functional dimension, on the other hand, is the element that too often has been forgotten in traditional methodologies and it is concerned with the implications of one part of the system for other aspects of the system which share the same organization. Traditional operant conditioning was totally unconcerned with the functional dimension. What is important about the field theory is that neither the structural nor the functional accounts are adequate in themselves nor is it really possible to give a structural account without paying some attention to the functional dimension and vice versa. The point is that in a field there is no purely structural element nor is there a purely functional element and it is not that one can simply fail to describe an important element; it is that no element can be described without taking both structural and functional dimensions into account. There exists a fundamental complementarity between the struc-

tural and the functional descriptions in psychological phenomena so that no psychological field can be described without referring to both. It is the recognition of this factor that led J. R. Kantor, traditionally allied with operant psychology, to criticize that branch of psychology for its excessively mechanistic and structural concerns.[5] Kantor suggests that even for an operant psychologist "interbehavior" ought to replace "behavior" in methodology. I must admit that I feel somewhat squeamish about the word "interbehavior" since it will be interpreted as being too much in the camp of behaviorism, but the stress that Kantor is placing on what I take to be field relationships is correct.

Let me juxtapose traditional psychology once again with the field approach. Traditional psychology seems to have divided into two camps. The behaviorists have argued that it is the environment that affects the organism, while others, such as the humanists, have argued that it is the organism that affects the environment. Both camps are wrong in failing to lay proper stress on the interrelationship between organism and environment, which is what I am referring to as the field relationship. Both of these traditional analyses seem to be unidirectional and linear. They have resulted in a methodology which essentially looks for a linear and unidirectional causal influence, either on the part of the environment on the organism or vice versa. Thus we have research which investigates how certain environmental conditions will affect our behavior. I need not point out the methodological similarity with traditional parapsychological experiments in which we manipulate some environmental conditions to see the effect of that manipulation on psi functioning—the so-called "process-oriented" experimentation.

These approaches, therefore, have tended to accept entity metaphysics, in that organism and environment were conceived of as two distinct things. Philosophers have argued that Descartes was fooled by language into asserting the existence of the "I" since the Indo-European languages are structured in a subject-object way in which one of the subjects can be the first person singular. It is ironic that it is the more behavioristically oriented psychologists and philosophers who have made this criticism and have argued that we have reified certain functions into the existence of an "I" and that faculty psychologists in particular have failed to realize their error. However, *it is the same criticism* that I am leveling against the behaviorists, since they seem so blithely to talk about behaviors as if they were distinct and a priori separate entities so that one could simply view and measure them. In fact, our experience is more of a flow; it is continuous, much as an analog function, while behaviorists view it as discrete, as digital. Perhaps this is because language itself seems to be discrete or digital and we are misled by it into too

easily assuming that our experience is composed of separate things. It should not come as a surprise, having the language that we do and through it having had an entity metaphysics, that it is the digital computer that was designed first. Behaviorists, therefore, in being misled by language, thought that they were dealing with discrete organisms and thus it is no wonder that they focused exclusively on structural elements.

Having accepted this entity metaphysics, it is only one step further, and we see this step easily in the 17th and 18th century empiricists, to argue that the discrete state of an organism becomes the necessary and sufficient condition for the next state of the organism, or that discrete state along with some specifiable, unique stimulus. The search for mechanism was born and the search for causal sequences—that which becomes the necessary and sufficient condition for the next state of the organism. What this view fails to see is that each component is merely a contributing component of a larger systematic interaction which is not unidirectional and to which the term "causation" does not apply. Causal analyses are structural analyses and these by themselves, as has been shown, are inadequate.

B. Another important factor of a field approach is that it is based on a perspectivism. Traditional entity metaphysics is absolutistic in that entities, whether they be mental or physical, are an inherent part of the structure of things and, in order to describe the world adequately, these entities must be described. The field approach asserts that experience is continuous and that the elements of a field or system are picked out as much for their functionality as for their structurality. In fact, what is even considered to be a structure must be conceived of so only in light of the particular function that that part of continuous experience which is termed the system is concerned with. Here one can see the relationship of the field approach to the rejection of foundationalism as discussed earlier. One does not try to mirror a pre-existing structure that is already implicit in nature; one finds or perhaps even creates a structure whose function is something that one happens to be concerned with. Even the function must be viewed as a matter of perspective. What we are dealing with is something that Gregory Bateson in *Mind and Nature*[6] called patterns and patterns of patterns. What science is to be concerned with are patterns or fields or systems which can be construed in a hierarchical form depending upon what the functional relationships are among the patterns one has picked out. What this means for methodology is that the experimental approach should be a great deal more inductive than has been considered before. I will try to relate this point specifically to experimentation in parapsychology in a few mintues.

C. A further important characterization of the field approach is that

of the acceptance of emergent properties. If there are no independent entities which have particular properties, then the elements within a system must be viewed as having the properties that are given to it by the context of the system. Properties become relational. It is not possible to isolate particular elements or parts of a system and view their properties and then deduce the properties of the system. The system has emerged out of the context of a network of systems. This is an important point for parapsychology, for it may seem that psi phenomena should be considered as an emergent property. It may be only in certain interactions, certain relational states of a system, that we find psi. To look for psi as the result of some sort of mechanical or causal process may be to misconstrue field relationships. For instance, to look for an energy which connects the sender to the receiver, or even to assert that since there does not seem to be such an energy we can conclude certain things about psi, is to accept the traditional mechanical model and to fail to understand the emergence of properties within a field. LeShan,[7] in this light, seems to have been right when he asserted that we may have been asking the wrong question in asking what causes the receiver to have some information.

I must admit to always having been intrigued by LeShan's description of the mythic reality.[8] I interpret it in terms of some phenomenon like voodoo in which it may be possible to affect someone physically merely by doing something like sticking a pin into a doll. Interpreted in the field theory, that act may be setting up a special field relationship between the doll and the individual, as well as the voodoo doctor and perhaps a number of other elements. This notion is traditionally rejected by Western thinkers, who are under the influence of entity metaphysics and its attendant mechanism, because we cannot find any causal relationship between the act of sticking a pin into the doll and the purported illness of an individual. Assuming that voodoo works for the time being and not simply for psychological reasons, the criticism seems to be that the structure of that relationship is unknown, although the function of the relationship seems to be clear. Since we have thought it so easy to separate structure from function and to be concerned virtually only with structure which deals with causation, it is not unusual that we would reject a mythic reality. Given, however, a field approach which says that function is just as important and that structure cannot be viewed independently of the function, it is not clear that we would want to reject this view. One thing that the field approach may do is to loosen up our conceptions of what may be possible and what is worth investigating and we may be surprised by the results. Indeed, I have done a couple of experiments based on the principle of the mythic reality and although

my data cannot be analyzed in any straightforward way, my intuitions tell me that there are enough interesting elements in the data to warrant further study.

Let me make one other point while I am on the subject of emergence within systems or fields. All of you know my interest in the conformance behavior model and although I must admit that there has not been an overwhelming amount of data to support the theory (indeed, some of my own experiments do not seem to encourage our acceptance of it), the model assumes a great deal more integrity viewed from the field perspective. Some have criticized the model, I think, because they could find no clear, understandable causal relationship between one element in the model, the disposed system, and the particular outcome. In other words, the model does not seem to fulfill the traditional mechanistic paradigm. As I am coming to see the conformance behavior model, the particular psi results, the biasing of the random system, take place in a rather complex interactional field and it is a kind of emergent event. The whole thrust of Stanford's model is in the direction of pointing out that psi events occur within certain fields, within interrelating elements of a system and must be understood in that way if we are to understand them. I think the conformance behavior model is a call away from traditional behavioristic methodologies toward a field methodology and it thus deserves our continuing serious attention.

D. I have stated previously that a field approach is more of an inductive approach. According to the traditional model, induction plays a limited role in that we may inductively come up with a hypothesis, but from then on the methodology is deductive. The hypothesis is immediately couched within theoretical language, then one deduces certain hypothetical results coming out of the hypothesis and then one tests to see whether or not those hypotheses hold true. The methodology of the field approach cannot be that deductively structured, simply because those theoretical terms are assumed to refer to real pre-existing things. Let me illustrate the difference in approaches by talking about my writing this paper.

One way to approach the writing of the paper would be for me, in an a priori way, to lay out a structure to outline the paper and proceed on that basis. The material that I am dealing with, however, was so new that this became impossible. Rather, what happened was that I came up with idea after idea (most of them in the middle of the night, unfortunately), some of them related to previous ideas and some of them not. When the time came for me to write the paper and I simply had no further time to come up with further ideas, I had to sit down and look at the ideas I had. It was at this point, after reading over all of my notes

a number of times, that I began to see a pattern emerging, which seemed like a natural flow of events in my thinking and which formed the outline of this section of the paper. The ideas then took on new meaning because they were juxtaposed to other ideas and new relationships were formed because of the particular field or section of the paper. But this section of the paper cannot be viewed as standing alone; it makes no sense without the other parts of the paper. And some may say that this paper simply follows the pattern of my thinking coming out of my defense of the conformance behavior model in an article in the *Journal of the American Society for Psychical Research* several years ago.[9] Others might want to call it the conclusion of ideas that I was working with in the paper I gave at my last Parapsychology Foundation conference in Copenhagen.[10] With luck in the future this paper may be viewed purely as an element in a larger system or field which extends beyond this time. This inductive approach is going to have important implications for methodology in psi research, to which I will return after a short excursion into the experimenter effect.

E. The experimenter effect is being viewed by a number of people as a major problem in parapsychology, if not *the* major problem. There are those, I think, who may very well wish not to carry on further research until this problem has been solved. Their thinking is that, until it is solved, we will not be able to become a legitimate science with consistent replicability. I agree that there are aspects of the experimenter effect that need to be dealt with, but I do not agree that the experimenter effect is a problem. The view that it is a problem stems from the traditional assumptions of entity metaphysics and foundationalism. Within this tradition objectivity was viewed as the scientist observing the interaction of objects in the experimental field which he has manipulated. In a psi experiment, the subject and the subject's responses would be thought of as something that could be objectively observed, much as John Locke thought that perception took place like a camera taking a picture. The camera, if it is working, does not affect the object that is to be pictured; the process is simply one of mirroring what is going on. It is clear from what I have said previously that I believe this view is seriously flawed and indeed the experimenter effect is what we should expect given a field approach. It is not possible to separate as isolated, unaffected entities any element of the system, so that it simply is not possible for an observer to watch a subject in an experimental situation as a camera takes a picture. Rather, as part of the field, the experimenter affects and is affected by other elements in the field.

As I see it there have been two approaches to solving the experimenter effect "problem" in parapsychology. The first suggests that we ought

to do all that we can to eliminate the possibility of the experimenter's exerting psi in the experimental situation. Thus, it has been suggested that targets should not be chosen by a random event generator or a random event generator should not even be used to select an entry point into a table of random numbers. This approach, in my opinion, simply does not face up to the fact that the experimenter is already in the field with the experiment. The second approach is to suggest that objectivity can be achieved through intersubjectivity. The idea here is to get as many observers as possible and the agreement among the subjects will be taken as what is objective. Once again, this approach is within the entity metaphysics tradition as well as the foundationalist tradition. What is really being said is that it is possible for one camera to malfunction, but if we have enough cameras we ought to be able to tell what is really objective. Although one camera can lie, surely a dozen cannot.

Both of these approaches are inadequate in my opinion and even the attempt to eliminate or reduce the experimenter effect is misdirected. Rather, we ought to accept the experimenter effect as part and parcel of the experimental situation and deal with it. What I would suggest is that the situation is even more complicated than some people might imagine. Some people have suggested that we ought to have experimenters write into the experimental protocol what their expectations and beliefs are, but there has been virtually no suggestion (at least in this context, besides the sheep/goat effect) that the subject's expectations and beliefs are just as important. We have been concerned about how the experimenter may affect the subject, but isn't it just as important to study how the subject affects the experimenter and how this mutual interaction affects the experimental situation? We have good reason to believe that there is a paranormal experimenter effect and we ought to make that the object of our research. We ought to look at the whole experimental situation, examining so-called psi-conducive experimenters and psi-inhibitory experimenters as they work in labs with subjects. In other words, what I am suggesting is that we ought to begin to see patterns within patterns and that the experimenter effect as it occurs in the interrelationship with subjects in an experimental situation ought itself to become the object of field study. Further, we need to ask: what is the effect of an experimenter being at a major research lab such as the Foundation for Research on the Nature of Man or at Edinburgh or at the Mind Science Foundation? What is it like for a subject to participate in an experiment in one of these labs, which has developed a tradition, as opposed to the subject participating in an experiment at a lab without such a tradition? What are the interrelationships among the experimenters at one of these labs and how does that affect the experimental

situation? I could go on with these kinds of questions, but I am sure that you see the drift of my comments. Obviously what I am talking about is not only a difficult methodology, but it calls for long-term studies. These are not the kinds of questions that can be answered by someone jotting down a momentary expectation and running a subject and then expecting to find answers. This kind of approach calls for major research efforts, but this leads me back on track to the last subject to be handled in the paper: what kinds of experiments does this methodological approach demand?

F. I have already suggested that the approach is going to be a radically inductive one. In general, what I am suggesting is that we ought to observe, observe, observe. Let us not prejudge how the system operates; let us not think that we understand how psi works and set up all sorts of experiments to test these things. Rather, let us view it and measure it, preferably in the naturalistic environment much as one would sit and observe the world as it passes, or as a colleague of mine has done, observe a whale at Sea World for days at a time, 24 hours a day.[11] Let us take those individuals who self-report psychic experiences and simply observe them. Let us take their self-reports seriously; let us see what *their* criteria are for a psychic experience. We may be in the position of those who wanted a measure of intelligence and came up with the IQ test, but after many years we now question whether the test has much to do with intelligence after all. Analogously, our tests of psi may not be testing the phenomena that piqued our interest in the first place. We may not have observed the phenomenon enough in the natural setting, however odd this may sound, for us to know what we are talking about. After all, don't we parapsychologists, particularly the field researchers, talk about how elusive psi is in the field when one e.g., investigates a poltergeist phenomenon: Once again perhaps LeShan was correct in going to the statements of Eileen Garrett and taking seriously what she said about her experiences, when she had them, how she had them and what made them psychic experiences. Just as he learned a great deal that was not expected before this research, we may all be surprised to find that we know very little about our subject area while our subjects know a great deal.

But the faint-hearted may respond, particularly someone who has read a bit too much of the *Skeptical Inquirer*, "Maybe we will be fooled. Maybe these are not psychic experiences after all. Maybe we are just dealing with coincidence and not the real phenomenon. Maybe there is just too much noise in the system for us to learn anything." If this field approach is correct, what it teaches us is that the noise may be an important element within the field and thus an important co-producer of the phe-

nomenon. But further than that, what really is noise? And really aren't we rather presumptuous in thinking that we can control it out? If we do not know what psi phenomena are, if we cannot talk about their structure, how do we know what is an important element in the co-production of it? Are colors of the room important? Is temperature important? Are certain temperatures along with certain colors important? I could go on and on, but I think the point is clear—we do not know what a control is. We do not know enough about the phenomenon.

I am reminded of the introduction to Patricia Carrington's *Freedom in Meditation*[12] where she points out conflicting statements about meditation. She has quotes in which she points out that some individuals say that meditation is best done sitting in a cross-legged position either in a full or half-lotus, while others recommend that meditation should be done while sitting in a chair with a straight back and others say that it should be done while lying on a bed. Further, some warn against meditating at night, saying that it should be done either in the morning or early evening, while others say that meditation is best done at night. From my years of working with meditation practices with others, it has become obvious that meditation does different things for different people. Some who meditate at night find that it gives them so much of a special kind of energy that they cannot sleep, while others find that if they do the same meditation right before sleep they are rested for sleep. Here we have the same structure within different systems and the end result of that structure is going to be different in the different systems. Any view that does not take into account all of these factors is going to fail to be adequate to the situation. Similarly, we are all different processing systems. Some people are more imagistic, so they will naturally do better in ESP tasks which call upon them to use their imagery, while others are more linguistic in their information processing apparatus and they will do better with fixed response/verbal targets. Finally, there are others such as myself who are much more kinesthetic in their processing systems and there seems to be virtually no psi task which takes us into account, with the possible exception of Gruber's "random walk." so that we are usually put aside as displaying no psi ability. I am not recommending that we ought to go out and test individuals for their information processing mode and then give all of them three kinds of psi tasks, as we would approach the question in the traditional mode. All that I am trying to point out is: (1) we may not have noticed some ways of responding paranormally because we have not observed enough in the natural setting and (2) any system is a complex interrelationship among elements, which themselves are systems having a history. Structure and function commingle within any system.

I realize that the inductive technique I am suggesting is a difficult and

arduous one; it calls for experimentation over long periods of time. What it says is that we ought to set our sights on the task of observing. Much as Darwin spent years in simply looking at nature before he finally began to see patterns emerging out of all of the data, so ought we to begin to spend time examining our data over long periods in a naturalistic or relatively naturalistic setting. In doing so we will want to focus on a number of behaviors and not simply one behavior. This methodology leads us away from manipulation, particularly at the onset. Manipulation assumes that one knows what one is manipulating in the primary sense—what is called the independent variable—as well as what one is passively manipulating by trying to hold constant—what is called the control. It may be that after long and serious work we may begin to manipulate things, but what I am suggesting is that we do not know enough about the phenomenon to begin manipulation; we have not performed the long and arduous inductive task of observation. What we need to do is simply keep on observing and measuring until we begin to see patterns emerge. Once we begin to see these patterns emerge and we begin to understand what appear to be the structural and functional elements within the field and how they interrelate, it is only then that we will begin to get enough of an idea about the system so that manipulation will be profitable in experimentation.

I am well aware that what I am talking about is not only a long process, but an expensive process; parapsychology does not have the laboratories nor the money to do this kind of experimentation well. In a certain sense, however, laboratories may not be that important since I am suggesting that much work needs to be done in the naturalistic setting. There may be some kinds of experimentation in the lab and this will call for very sophisticated settings, as we find in biological rhythms research. However, less emphasis should be placed on the laboratory and more emphasis on researchers observing phenomena in a naturalistic setting. Technology can be used to help us in this. For instance, we may want to use videotapes a great deal so that we do not have to depend upon the vagaries of one-time observation. Since I am talking about long-term research and it may be expensive to work with human subjects, it may be that we will want to do more work with animals. However, there are various drawbacks with this suggestion as the typical work with animals is in an artificial environment and we are not sure how much the artificiality affects the system; also with animals, unless John Lilly is successful in his work with inter-species communication, we will not have the advantage of getting the viewpoint of the animal.

I know that what I am suggesting is radical—I am proposing a radical rethinking of our cultural and philosophical paradigm and an attendant rethinking of the methodological program which stems from that par-

adigm. We ought to back away from the behavioristic model. What I am suggesting is that we do nothing less than start at the beginning of the process of exploration, hence the stress upon induction. You may think that this proposal is overly radical or overly strange or simply impractical. It is certainly not for the faint-hearted. But I do not think that my proposal is as radical as it may seem at first blush. Let us take heart in realizing that something like this world view is what the most respected of the twentieth century philosophers seem to have concluded, independently, working out of their own systems. And not only that, but if I am reading the direction of the latest trends in both social science and natural science, I see the same movement. It may not be that parapsychology at this point is going so much to *force* a revolutionary change in thinking, as much as it ought to *accept* the same kind of revolutionary proposals that are being made in philosophy and science. Perhaps we are not on the frontiers of research at all, but in our commitments to dualism, to entity metaphysics and to foundationalism, we are the real reactionaries of science.

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DISCUSSION

STANFORD: I generally concur with the methodological perspective which Hoyt Edge has presented for us; in fact, I think my own personal

history of interest in the field reflects that we have some pretty convergent views on some of these matters, with my interest in spontaneous psi and PMIR. The funny thing is, as I reflect back over my own experience in working in the field, more often than not the very things that I sought to study in my own research were things that I felt that I or my close friends had personally experienced ourselves. We talked about them and observed them in a nonexperimental, more passive sense for a period of time. I believe this is an approach which we certainly need to take to heart in this field. Now, I'm not ready by any means to attempt to abandon the experimental method. I concur especially with the latter remarks of Hoyt's, when he says this may not be quite as radical as it appears more or less at first blush. For instance, even if we had a field type situation, there are fields and there are fields, and different field situations function differently. What we need to do is to find out more about the factors that influence the function or the flow within those interrelationships. I think we can do that. I really believe that the experimental method is going to continue to be useful in doing that kind of thing. I certainly agree that it ought not to be used naively, to imagine that we can artificially isolate the experimenter from the experiment. At the same time, with regard to the experimenter question, I think there are things that can and should be done. I think Hoyt implied that we ought to look at that in some respects rather than ignore it. I would suggest that there are things we can do. I've indicated some of those in my paper on shamans or scientists. My purpose here has been sometimes misconstrued. I was not in any way suggesting that if we do things such as using fixed random number sequences to get our ESP targets rather than RNGs, we're going to somehow magically eliminate the experimenter completely from experimental settings. What I hope we could do with such approaches is to transfer the locus of the psi effect in some respects, and we can examine the effects of trying to do that. I suspect that we will find some rather different results when we control for such factors. It can be very instructive. I think some persons have misconstrued my intention in the shamans or scientists paper, but I do think it very important to look at factors of that kind rather than just to make assumptions about them. This does not imply that we don't have a field situation. I really think we do. But it may cause a readjustment in the dynamics within that field and I think that could be quite useful. One thing that nobody mentioned today is the problem of cross-lab replication. In the end, a lot of the validity we can bring out in this field is going to come from cross-lab replicability. I think that trying to do the same experiment over and over again in a particular lab is a bit fatuous, yet if we're talking about extracting the signal from the noise, we certainly need cross-lab replication. I would suggest that what's going to

happen in the end is a kind of a synthesis, where we're going to see that the experimental, manipulative method is quite useful and can be applied in light of field considerations. I really think that that can be done. My final point is that we need to bear in mind in this discussion two contexts regarding science. One is that of discovery and the other, the context of verification. The kind of field-type observations that you're talking about are very, very good, absolutely necessary; in some sense I think they prove some things to some people's satisfaction. But we really cannot demonstrate, in terms of the normal meaning of scientific demonstration, without the attempt to manipulate parameters. In my opinion, we will have to move from the context of discovery to one of verification, but it's certainly going to be made more complicated by the kind of consideration that you've been addressing today.

EDGE: I basically agree, I think, with everything you said and I certainly would not want to get away from some manipulation and controls. I am a pluralist in research. It seems to me that we ought to try as many methodologies as possible and let them all flourish and see what happens, but I agree that even within a systems view you can have kinds of controls and manipulations, even in terms of the experimenter effect.

GREGORY: I'd just like to make one or two more theoretical, philosophical observations, the experimental ones having been made. I'm completely in sympathy with your view that the subject's reactions are of prime importance and that we have absurdly neglected them in a spuriously scientific, snobbish way which really has no justification at all. But at the same time, I'm very uneasy about the sort of radical empiricism which you just describe. In my view this is not a feasible undertaking. One always approaches everything with a theory of some sort, explicit or implicit. The very structure of your language has theoretical implications. One thing it includes is the theoretical structure that we impose by deciding what we're going to select and what we're going to reject.

EDGE: I entirely agree and I think my own considerations really come out of that. I think the philosophical foundations that I was using are the same foundations that lead Kuhn to say what he does and what you're saying is a Kuhnian approach. In reading I left out a part of the paper on the problem of language. What I accuse the behaviorists of is being sucked in by language just as much as they accuse the Cartesians of being sucked in by language. You cannot simply go to nature and observe without some preconceptions. The question is whether you recognize this and have some flexibility regarding your biases.

HONORTON: I also agree, in general, with much of what you have said. I would like to amplify just a little bit on my earlier comment. I was not at all suggesting that we ignore a serious problem. I was sug-

gesting, as I think you have, that we seriously consider the possibility that that is not a problem at all, but one of the defining characteristics of the phenomena that we're studying. Looking at it from that standpoint, we're able to do business in this field more effectively than we can by always looking over our shoulder at ourselves, so to speak, to see to what degree we can eliminate our own participation. On a more concrete level, but in the same area, I think it's always a sign of progress in the field when we can eliminate helpful suggestions. I think we can eliminate Rex's suggestion in the shamans paper concerning the advantages of using prepared random numbers, as opposed to momentarily generated ones. We can reduce, at least, the likelihood of experimenter effect on the basis of the work that Schmidt has been reporting over the last year or two, where only the seed for the entry point to the random number sequence is generated through a live random source. I don't think that proposal is any longer one that can be seriously considered, unless you have some way of specifying the degree of probability that the experimenter is not influencing the seed of the random process.

STANFORD: I have never proposed that the "seed number" approach, namely using an indeterminate—hence, psi influenceable—number to enter a fixed number sequence, will eliminate the kind of experimenter psi influence with which I am concerned. Quite the contrary. I have warned against that approach in the shamans paper and have advocated, instead, another method described in that paper.

VARVOGLIS: I find the idea of a field appealing from a theoretical viewpoint, from a conceptual viewpoint. What concerns me from an experimental viewpoint is how you would propose to define the boundaries of this field. You referred to "the system." But since, as part of your assumptions we don't really have an entity kind of ontology, but a process ontology, then, if you apply your assumptions consistently, you can't refer to "the system." You can't really "close" the system and say OK, now I know what the interactions between the members of this system are.

EDGE: With definiteness, yes.

VARVOGLIS: Also you mentioned the function and the structure of the system, and juxtaposed the two. But unless you can find some way of defining the structure or boundaries of functions, or of saying that functions obey some kind of lawfulness other than that they're simply useful to someone, I don't think you really have an interaction between function and structure. You just have functions, or purposes.

EDGE: What I would urge is that the criteria for functionality not be defined a priori. It could be itself an experimental question, but before we start out, the only criteria I would place on it would be pragmatic

criteria. Let there be as many functions as people want to try to place, let them flourish, let there be experimentation, and let's see which one survives.

ROSEN: As I see the issue that Hoyt Edge has raised, we're faced with following either an entity approach or a process approach. Normal science, as Hoyt has characterized it, seems in some way to go beyond an entity approach and he points to systems theory and cybernetics. My own impression is that systems theory and cybernetics do not go beyond an entity approach; they just describe larger entities. When we go *further* into normal science, to the point where we realize it isn't quite so normal—into the work of theoretical physicists David Bohm and Henry Stapp, for example—we realize that what's called for is a *radical* process approach, not one that ultimately gets reduced to an entity approach. This radical process approach would call into question our methodologies and epistemologies in a much more fundamental way than current systems theory does. Therefore, I don't see the solution being as easy as it is sometimes portrayed.

EDGE: I agree with that. The reason I did not call the approach a systems approach, although I saw some relationship to it, was that I saw that the systems approach was excessively cybernetic, in the sense that the elements of the system seemed to be a little too much like entities to me. It was not really the relational approach that I wanted. So I feel a discomfort with the systems approach, also, as it stands, although I feel uncomfortable saying that, in the sense that there are experts on this and I certainly am not.

BROUGHTON: I am a bit unhappy that the field approach would be reduced ultimately to some kind of entity metaphysics, but that wouldn't be surprising. Normal science as we know it is dependent on our language, perhaps tricked by our language, as someone said. We know from brain hemisphere research that our language is largely dependent on the fact that it is lateralized to the left hemisphere, perhaps because we have used our right hands to manipulate objects. So, in a sense, one whole tradition has brought us to an entity metaphysics. Now, my feeling while listening to your paper was that it is not really necessary to create a dichotomy between the field approach and the entity approach, because the two have always really been with us in science. What I mean is that, as human beings, we have our entity processors. We think things through logically. We think them through with our linguistic structures. But we also have with us our parallel processors which, as you say, observe, observe, observe on all levels, take in information, make sense out of the world in ways that are below our conscious levels. I would not want to say that this is necessarily right hemispheric, but there is a precon-

scious, preattentive level which functions in all of us, and I think this enters in the field approach. Activity metaphysics is with us and enters into science in ways which are not too often discussed by the philosophers of science, with the exception of ideas such as Polanyi's tacit dimension. It is essentially intuition and insight, but it is very much a field approach, a wholistic approach, which varies in great degree. Sometimes it's less successful than others. In the most successful scientists it comes out as an Einstein idea.

EDGE: I would not totally disagree with that and I certainly would not want to indicate that I am coming up with something that is unique. I think science has used aspects of a field approach and has done it for a long time. Perhaps what I'm saying is that as human beings we work in much more an analog way than we do in a digital way, but what happens is that we tend to come to experience (because of language and other factors) with an entity approach. We have these great preconceptions with which we come to our settings and I couldn't disagree that there is this process going on unconsciously and perhaps even consciously. On the other hand, I want to say that our basic experience is a field approach. I have to agree that in some ways we have to have known that, we have to have experienced that. What I'm suggesting is that what we have *formalized* is not that basic field experience and how we consciously go about doing business is not that way.

ULLMAN: I am grateful to you, Dr. Edge, for clarifying my own philosophical odyssey. I think perhaps the dream work, in it's relationship to telepathy, is something of an example of what I think you're trying to put across to parapsychologists. Because, starting out as an analyst, I was into a field approach—at least I think every analyst should be into a field approach—and looking at what was going on in the field created by the patient and me that was psi conducive. Then I became a scientist and tried to test it out in the laboratory. But what we came out with was just another statistical result in favor of psi. I think perhaps it's something of a lesson, not against the experimental approach, but against using the wrong experimental approach in relationship to the problem at hand or restricting it in an experimental approach. I think, for example, what happened in relationship to the grant we got from the National Institute of Mental Health was a prime example of how the whole thing was so tied down and so tightened up that absolutely nothing happened. When I left the lab I didn't leave my interest in the relationship of dreaming and psi. I have been involved now in the kind of experimental approach that you described so beautifully, because it starts from a basis of total ignorance about what psi is. We don't know anything about the relevance of the concept of a target or the concept of an agent or the concept of

an experimenter. What we're doing is simply establishing a small group of three or four people that has been meeting now for about three years—because we have a longitudinal perspective in mind—and all we do each week is share the dreams we had during the week. When we have time, we go a bit more deeply into the information in the imagery. Essentially what we're trying to do, really, in a most spontaneous way, in a playful way, in a way somewhat, perhaps, analogous to the Philip experiment, is to create the kind of field in which we may then discover what the target was, what the agent was and what the psi effect was. And it really is one in which we learn from the person experiencing the effect as to where and how it came into being.

SCHECHTER: I find myself with a clash between my metaphysics and my pragmatics. I like using a process metaphysic in trying to make some sense of the nature of mind. It helps me understand some of what I see as the more troublesome aspects of the mind-body question. From a practical point of view, however, I'm not comfortable with the idea of really shifting fully to a process approach. Comparing process and entity metaphysics does remind us that we can get ourselves into trouble by creating "entities" unnecessarily. But to focus *only* on the constantly-shifting patterns—is there anything stable there to grasp? I suspect that we need some stability if we're to make any sense of it at all.

I think that, in the end, we'll need to think in terms of both entities and processes. The hard part is to keep the balance, to avoid overdoing either approach.

EDGE: In some senses, I feel this discomfort myself. Thinking of where I would like to go, experimentally, I still find myself doing things or suggesting things that may not follow from what I just said today. Perhaps, however, I am more optimistic about what could be found. One thing that I am suggesting is that this approach calls for longitudinal studies, it calls for a great amount of data collection. We can do it now with the computers, whereas we could not do it before.