THE PROBLEM IS NOT REPLICATION

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For as long as I have been involved in parapsychology, I have heard both practitioners in the field as well as critics talk about the problem of replication in parapsychology. Obviously, it is a theme which is important and one which deserves attention. Critics have argued that parapsychology fails in an important respect because we have not produced the repeatable experiment, while some parapsychologists argue that we have made great strides in the last decade in producing experiments which are repeatable. For both, the reason replication is considered a problem is that parapsychology cannot be considered a science unless we have achieved a good degree of repeatability in our experimentation. The so-called "problem of repeatability" is really a problem of legitimizing our activities as a science. Indeed, the critics of parapsychology, those that dub us as engaging in "pseudoscience," are the same ones whose charges of the lack of the repeatable experiment are the loudest.

It seems to me that we ought to be concerned about the status of parapsychology as a science, but I doubt that the locus of the problem revolves around the question of replication. The thesis of this paper is that parapsychology does not have a problem in the area of replication. This is not because we have achieved a good degree of replicability (although I think that pattern analysis provides a powerful tool to argue that we have significantly recurring results, as others on this panel will demonstrate), but simply because replication is not the *real* problem that keeps parapsychology from being viewed as a science. Replication is not important in itself; rather, it becomes a symbol for other factors, and it is to those other factors that parapsychologists should turn their attention.

When one looks at the concept of replication, there are at least two factors that seem important in elucidating the concept. The first is the notion of observation. In order for me to designate an experiment as a replication, I must be able to *see* it as such. The second concept involved is prediction. In order for an experiment to be repeatable, one must be able to predict what will happen under

controlled circumstances. Both concepts are basic and each deserves separate attention.

Turning first to the theme of observation as a component of a replication and its role in science, let us take a historical turn and examine the nature of observation at the inception of modern science in the 17th century. Science is nothing other than what it does and in order to understand what is important in science, it is important to understand how science has been practiced and the assumption behind those practices. Therefore, in order for us to try to see why replication has taken on the importance that it has in science, let us take a look at the rise of modern science and at the intellectual milieu of the 17th century. Let me divide this historical excursion into three parts, the first discussing the epistemological sources of science, the second analyzing the nature of the self assumed by this view of science and the third showing how a particular view of replication emerges out of this background.

One can point to two general sources of the new science as it developed in the 16th and 17th centuries. One came out of epistemological rationalism, and its approach was deductive, while the second arose out of epistemological empiricism which was more inductive in its approach. We can take Thomas Hobbes as an example of the rationalist. Hobbes did not engage in much experimentation *per se*, but rather his approach was to set forth a set of postulates and deduce a scientific view from these postulates. He asserted that everything was matter in motion and, using this one principle, he thought that he could explain all things in natural science, in social science and even in ethics. If a person is nothing but matter in motion, then that person is nothing but a machine, an automaton which is to be explained by using laws that refer only to mechanism, only to matter in motion. While Hobbes' rationalistic approach is obviously one-sided, it does point out the importance of deduction in science, and it shows the fruitfulness of the deductive approach. Hobbes' conclusions may appear to be naive because of his language and the rather obvious and perhaps primitive mechanistic analogies that he employed, but, in essence, his conclusions are not far from the contemporary materialist view of the person and the world.

The second historical source of modern science is that of epistemological empiricism, which we can understand by quickly considering Francis Bacon and John Locke. Bacon, too, was a materialist, having had great respect for Democritus, but what Bacon brought to science was an inductive methodology. Bacon considered the mind to be a kind of mirror, one somewhat misshapen by prejudices and tendencies

of passion, and he counseled us to avoid certain "Idols" which forced us to mirror the world in a misshapen way. Once we guarded against these Idols, we could proceed with experimentation, inductively deriving laws from simple observation of particulars in their series and order. Although Bacon knew that science was not merely observation and induction, he thought it to be the main thrust of science.

If we move from Bacon to Locke, we see a full-blown empiricism with an equally great emphasis placed upon observation, because, for Locke, all knowledge must be derived from sense experience. If one could not ultimately reduce the source of an idea to a simple perception, then the idea was meaningless and certainly unacceptable for science. Again, the mind becomes a kind of mirror, but while Bacon fretted about certain Idols distorting perception, Locke did not seem to take this possibility into account. For Locke, observation was pure and unmediated and, as such, the only legitimate source for science and knowledge.

A more detailed examination of the concept of the self that comes out of both rationalist and empiricist traditions brings us to the second point. Bacon and Locke used the analogy of the mirror to describe perception, but the analogy can be expanded somewhat for us to get a better notion of the kind of self that is implicit in this analogy. Although it is true that a mirror is a physical object, the important part of the analogy is that the mirror is an entity which is separate from the process mirrored and it does not interfere with or take part in the process of that which is mirrored. What the mirror symbolizes is the idea that there is a self which is separate from the process being observed and that self merely reflects what is occurring in the process. It is not involved in the physical process that is reflected, nor is it in the empiricist tradition of Locke nor in the more dominant rationalist tradition of Descartes a physical thing subject to the same physical laws as the material processes which are mirrored. The mind seems to be wholly other than the physical and this is one reason that it can mirror without distorting the physical process. Part of the job of the mental is merely to reflect, either internally on its own sources (hence our use of the term "reflection") or to reflect in a mirror fashion that which is outside of and other than itself. At least in principle, the scientist has no prejudice or bias because it is possible for the scientist to act merely as a reflector of the physical process. True observation in science is the ability to look at and perceive without bias or prejudice the physical process which is occurring outside of itself and this perception inherently, as a

mirroring process, does not affect that process. The empiricist tradition has given to science the notion of a self as mirrorer and reflector of nature.

The rationalist tradition agrees with the empiricist tradition that the self is other than nature and is based on laws which are not physically describable. The analogy that the rationalist might use is different from a mirror, but the notion of the self is the same in fundamental respects. Rather than the mind being a mirror, the rationalist would prefer to think of the mind as a calculator. The objectivity of the mind is not found so much in its ability to adequately reflect nature, but in its ability to reason and to deduce. Once again, the mind is independent of physical processes so that the calculating and reasoning abilities of the agent can in principle be totally objective and unbiased. The mind's attribute is to think, to stand outside of the physical process and to reason about it.

What is similar in both traditions is that the mind is viewed as something that is outside of nature, in fundamental ways cut off from nature so that the self really becomes ahistorical. By that I mean that what seems most essential about the mind is something that is in no way fundamentally constrained by space or time or by the particular history of the agent. Neither the mirroring nor the calculating faculties in these traditions seem to be subject to historical conditions. In principle, an agent is able to reason using timeless deductive principles, so that if I reason correctly, which is in principle possible, what I do is unmediated by any locally constraining factor. Likewise, perception is direct and unmediated and it does not matter when or where a mirror is functioning nor what its particular history is. *Qua* mirror, it merely reflects. Both of these conceptions picture the mind as a kind of *deus ex machina,* an all-encompassing entity which is able to drop down in the midst of the world, but which itself is not conditioned by that world. Both traditions admit that our knowledge about the world may be limited, but it is not the method or the process of gaining knowledge that is limited; rather, these are fully adequate to the task. It is probably no coincidence that the rise of science and the rise of political sovereignty occurred at about the same time, as both of them seem to have at their basis a notion of direct and absolute power. And this power is located in an entity or process which is somehow set apart from the normal conditions of the physical world.

The extraordinary powers of this ahistorical self lead to our third point. If one's observation is so clear and if one's ability to reason is so powerful, then surely replication must be a straightforward affair.

If one is able to see clearly and if one is able to reason clearly, it is surely a simple task to evaluate whether one experiment replicates another one. If there is no problem of misperception, if there is no difficulty in knowing whether or not "x" exists or not, if an observer does not affect nature, then there should be no inherent problem in knowing whether a replication is successful or not. Given these notions of the self and of observation, the idea of replication presents no real difficulty. In fact, given the powers of reasoning, replication becomes almost an algorithmic affair. An observer is able to pick out the relevant factors in any experimentation and merely examines another experiment to see whether these same factors are present. The observational process of this ahistorical self does not affect the process that it is observing, since that process is separate and since the self only mirrors it. Therefore, there is no difficulty in describing what a replication is or in observing whether or not a particular experiment replicates a former one. Given this notion of observation and the Baconian emphasis on inductive enumeration, replication became central to traditional science. If the world is so transparent, generalizing from repeated phenomena, there may be difficulty in knowing where and when to look, but the observation presents no problem *per se*.

What is sound in the empiricist and rationalist traditions is that both observation and theory are important. Both are benchmarks of science, which could not proceed without them. But the particular notion of the self is highly suspect and, because of that, the notion of replication is suspect. Ever since Kant, there has been the recognition that perception is not the straightforward affair envisioned by the early empiricists. What Kant suggested and what has been supported by a great deal of experimentation is that all perception is mediated; it is laden with personal and cultural influences which, although they can be guarded against, are not possible to eliminate even in principle. Kant suggested that our perceptual awareness is "filtered" by intuitions and categories of the understanding which he thought were unconditioned by time or history, while the more modern version of this view argues that the filters, in fact, are time and history dependent. To use Heidegger's terminology, we are "thrown" into a particular time and place and we are limited by that fact. The self is no *deus ex machina* whose seat is outside of the stage on which we are playing, but the self is a lead actor in the drama. The self is not like the vision of the playwright who is outside of the drama and is able to perceive or create the limitations of each of the actors, because the self is simply one of those actors. The abilities of

both reasoning and observation are conditioned by historical factors and neither is unmediated or direct or has absolute sovereignty.

If we reject the notion of the self and of the type of observation which is assumed, then we must question the notion of replication which is derived from this tradition. Replication can no longer be viewed as a simple, straightforward algorithmic process in which a calculator with clear vision is able to be clear and certain about replications. In questioning this traditional notion of replication, there are three quick points that I would like to make initially.

The first follows directly from the above and it is that it is difficult to know when an experiment has been replicated; indeed, in the strict sense, replication may be impossible. In order for an experiment to be an exact replication, it must reproduce all of the experimental conditions of the first experiment. It was possible to consider this as an idealized goal when the notion of the self was conceived to be ahistorical, when it was a timeless calculator which merely mirrored physical processes outside of itself; but with the rejection of this notion of self, it becomes in principle impossible to have a strict replication. Even a replication attempt by the same experimenter at the same lab is not possible in idealized form, since the experimenter has additional knowledge and that knowledge necessarily is an important part of the definition of that person who is no longer ahistorical. Previous experimentation is part of one's past and one invariably "sees through" that knowledge. In abstract form, this argument may appear to be awfully nitpicking and not worth taking seriously in practice, but when one is dealing with the social sciences where the complexities of experimenter-subject relationships are incredibly complex, what appears to be an abstract point takes on great practical effect. When it is further noted that we may not be dealing with the same experimenter in replication attempts, but with possibly a myriad of other experimenters in different labs and with different backgrounds, the practical problems increase. Add to these factors the possibility that the subjects are different and one has a potentially overwhelmingly complex situation. Plus parapsychology adds its peculiar problems. If the experimenter effect found in parapsychology is something that we ought to take seriously, the possibility for us to conceive of any algorithmic replication process is nil.

This same point can be made more forcefully by looking at how science actually works, because there is very little replication that takes place in science. It has been only fairly recently that investigators

have been made aware that replication plays only a minor role in science, particularly in the social sciences. The psychologist Seymour Epstein (1980) has pointed out that there is very little replication in psychology. Further, Nicholas Wade has lately become concerned about the amount of fraud which has been uncovered in science. That problem does not concern us here, except for the fact that Wade (1983) points out that replication is considered to be one of the major fraud detectors in science. In practice, however, it does not function that way because scientists rarely replicate each other's work. As a reason for this, he gives the following explanation: "A scientist rarely attempts an exact replication of another's experiment. The reason is that prizes in science go for originality, and replication is by definition unoriginal. When researchers repeat their colleagues' experiments, it is generally with the idea of improving or refining them so as to be able to claim some advance on the original finding" (p. 15). Only rarely will a journal publish a straightforward replication attempt. Because of the actual lack of replication in practice, Wade (1983) asserts that "Replication in practice plays a very different role than that attributed to it by philosophers of the scientific method" (p. 15). Replication, then, does not seem to be as important for science as it is thought to be, since the actual practice of science does not encourage it nor does one find a great deal of replication in fact.

If replication fails to play the traditional role laid out for it, perhaps the idea is playing a different role. In fact, sociologists are suggesting that rather than its being an idea which functions as an objective or inter-subjective criterion for the acceptance or rejection of experimental data, the idea of replication is used more as a tool by experimenters, more of a honorific designation of approval and sign of agreement. H. M. Collins (1976) and Stephen Braude (1979) have argued that there are incredible difficulties in determining whether or not one experiment is a replication of another one, because it is always possible to point out some difference between two experiments and argue that these differences are important. There is no algorithm by which one can decide whether or not an experiment is a replication, even a straightforward replication of another experiment. Collins (1976) concludes an article on the idea of replication with the following quote: "A major obstacle in the way of the acceptance of such a 'tractability' in scientific findings is the belief that *genuine results* evidence themselves by their repeatability, so that the criterion of replicability distinguishes the unique set of genuine results from the set of false ones. I have tried to show that

this obstacle is less substantial than it seems, and indeed, show that it is precisely in 'negotiations' over the replicability of phenomena that one result rather than another might be '*discovered.*' On this reading, replicability should be seen as part of the 'rhetoric of scientific presentation'—a means of *accomplishing* objectivity rather than demonstrating it."

Since charges about the lack of replication are tied, in critics' eyes, to the charge of pseudoscience, it is instructive to note that Roger Cooter has made essentially the same claim regarding the use of the term "pseudoscience" as it is used by critics of a particular discipline. Charges of pseudoscience, Cooter (1982) argues, are more prescriptive rather than descriptive. He says ". . . all post-17th Century attacks on 'pseudoscience' must . . . conserve and protect the ideology imbedded in science" (p. 138). The rejection of the traditional notion of self leads us to a rejection of the traditional role of replication in science and its role has become more prescriptive than descriptive; thus it is less than a totally useful concept for science. Perhaps this point has already been noted by philosophers of science. In preparation for this paper, I examined four standard textbooks in the philosophy of science, concerning both natural and social sciences, and not one of them listed replication or repeatability in the table of contents. Further, I was able to find replication listed in the index in only one of the texts and only two paragraphs were devoted to the subject in that book. Perhaps replication does not enjoy the importance in science that some people think it does, particularly the critics of the field.

Additionally, there are a number of phenomena studied in science, especially in the social sciences, which are important, but which cannot in any meaningful sense be said to be replicable. Such events as clinical outbursts, war crises, eclipses, earthquakes, the birth of quintuplets and certain natural disasters do not lend themselves to controlled repeated observation, and we can never know when their recurrence will take place exactly. Even if we do, the conditions many times are so varied that we may want to question whether they should be counted as a replication. Yet all of these phenomena are still of interest to scientists and they are studied by science. Perhaps this points to the fact that replication in itself is not the important factor in defining science, but some other criterion is at work that is more important than replication.

Let us focus now on the second component of replication, prediction, to see if we can get a handle on what is inadequate about the

charges of the lack of replication in parapsychology, but also what may be true about the charges. If a phenomenon is repeatable, it is predictable, so it may be that prediction supplies that important component to the idea so that replication can stand as a central criterion of science. Let us turn to the topic now.

While I was in Bali the summer before last, a friend told me about a very impressive case of prediction. We were at the most holy temple in Bali on Mt. Agung, which had been the scene of a very destructive volcano a couple of decades earlier. As we were going through the temple, the Balinese friend pointed out the location where a manuscript had been found after the last volcano which described two previous volcanos and predicted the next volcano one hundred years after the second one, which was one hundred years after the first one. This true prediction had not been known before the last volcano, but the prediction was impressive. One is even more impressed with the prediction when one finds out that the largest and most important religious ceremony of the century was taking place in the temple when Mt. Agung erupted and the lava flowed down the mountain until it reached the temple, at which point it divided and went around the temple only to rejoin again at the base of the temple, saving everyone who remained in the temple. Here is a case where the prediction is impressive, but such prediction does not seem to create science.

Let us speculate a bit, then. Let us say that a group of scientists have done a very tight double-blind study of voodoo and have found that deaths in fact occur to individuals who have been cursed, and these deaths do not seem to be due to the psychological factors the target person suffers when he finds out that a curse has been placed on him; rather, the deaths may occur just as easily when the agent has no knowledge that a curse has been placed on him. Let us say that a number of double-blind studies are done and it turns out that in one-fourth of the cases, the targeted person dies, an outcome that we can predict. Can we say that we have replication, or perhaps, more important, can we say that the study of voodoo is a science; let us call it voodoology. I have my suspicions that the scientific community would view such a result with a very critical eye and question whether voodoology is a science. Parapsychology is probably in the same position as my hypothetical voodoology. Parapsychology fails to be a science for the same reason that voodoology would not be considered a science, and I think it is not because of the lack of replication or prediction. Let us say that deaths occur half of the

time or even three-quarters of the time. These results would be impressive, but I still do not think that voodoology would be considered a full-blown science.

We can take an historical example to illustrate the same point. The Babylonians of the 5th, 6th and 7th centuries, B.C. were extraordinary observers of nature. The precision of their observation and the accuracy of their recording are phenomenal. But what is most astounding is that they not only observed nature, but, from these observations, they were able to predict celestial occurrences. Although their record-keeping did not allow them to predict with any accuracy earthquakes or infestations of locusts, they could predict with great accuracy eclipses, retrograde motions of planets, stationary points of planetary orbits, as well as the crescent of the new moon. The baked clay tablets recorded not only what they saw and what they predicted, but also gave the arithmetic steps to calculate the daily position of the planets. They were able to achieve such accurate prediction because of the continuity of their records over such a long period and a reliable calendar. And yet, Stephen Toulmin and June Goodfield (1961) argue that the Babylonians were not engaged in science as we conceive it. The ability to note regularly occurring events and to predict those events is not enough to make a science, no matter how accurate. After all, we in parapsychology are quite familiar with the phenomenon of precognition. Even if we were able to find a psychic who predicted with great accuracy, to levels that are unheard of in our laboratory experiments today, we would still not be engaged in a science. To be a scientist is not to be a prophet. Prophecy and prediction are not science, even if one is able to predict the occurrence of phenomena with almost total accuracy (well above the .05 level). The Babylonians were able to say when events were going to occur and to give the mathematics for their occurrence, but that still failed to be a science. Prediction is not what is at the heart of why people think replication is so important. What the Babylonians lacked was theory. They attacked the problem of forecasting, not of physics. The Babylonians could predict, but they made the motions of the planets no more intelligible than before. Without theory, without true understanding of the phenomena, one can have all of the repetition in the world, but one does not have science. It is not until phenomena are made intelligible that we have science. And it is here I think that we have finally gotten to the root of the quest and the importance of replication. It is really not replication in itself that is important. Events which cannot be replicated, as clinical outbursts and war crises, are brought into science because they are

made intelligible by theory. It is theory that brings investigation of phenomena into the realm of the science, not the ability to predict them or to repeat them, although these qualities are valuable.

It is not a crisis of replication that faces parapsychologists; it is a crisis of theory and explanation. Replication is merely a tool in the arsenal of trying to achieve intelligibility. As Abraham Kaplan (1964) has said, "For a scientist, repetition is a device to improve the quality of observation, but not the only device, and not necessarily the best" (p. 128). What we have failed to do in parapsychology is to make our phenomena intelligible and producing a repeatable experiment in itself will not necessarily help us to do that. Replication is merely one way of providing data which must be made intelligible through theory and intelligibility does not guarantee replicability in any strong sense.

One of the aspects of the traditional notion of repeatability, which is why people have traditionally taken the notion of replication to be so important, is that nature is uniform. What makes this notion so important to science is not that it leads to prediction, but that it should lead to understanding. In discussing the question of replication, Abraham Kaplan correctly points out that intersubjectivity seems to be what is at stake, and he expresses this assumption of science in a classic way. He says "Nature plays no favorites, but exposes herself promiscuously" (p. 128). Because of the experimenter effect, it may be that parapsychologists are making an honest woman of nature, showing that at least in some areas she is less promiscuous than traditionally thought. But the root notion is that nature is open to all to be understood. Science assumes that the world is orderly and what we must do is to understand that order, and only in doing so have we made nature intelligible. It does not matter how many times a phenomenon repeats itself, it does not matter how accurately we can predict its occurrence, if we cannot make it intelligible, we are not doing science.

Michael Mahooney (1976) has written "In its barest essence, science is a search for order; it is an attempt to describe relationships among events" (p. 95). The search for order, the search for intelligibility, the search for explanation are not simply benchmarks of science. They are benchmarks of rationality in any form. Rationality demands order. It demands to see patterns and to make those patterns intelligible.

The November, 1983, issue of *Psychology Today* talks about research which brings into question Piaget's assertion that the idea of causation is first learned at the age of six or seven. The newest research

purports to show that causation is a biologically based concept which even the infant has a primitive idea of. What the research shows to me is less that a child has a specific concept of causation and more that a child has a basic innate or biological urge to understand—to make our world intelligible, to make sense out of it, to make it less mysterious. When the concerned critic of parapsychology seeks the repeatable experiment, what he is really seeking is some way to make the event intelligible, some way to make sense out of it.

This can be done in several ways. One can produce an overarching theory which brings together a diversity of data, or one can connect the data of parapsychology to laws accepted in normal science, or one can even make the phenomena intelligible without the former by showing practical applications of psi abilities. It is beyond the scope of this paper to pursue these avenues, although I do want to assert that we should pay more attention to the last approach to intelligibility. But replication in itself is not a virtue. What we need is intelligibility. Replication may not bring intelligibility (especially of the sort derived from pattern analysis, although this practice is a useful tool; I am not a defeatist) and intelligibility does not necessarily demand repeatability.

There is a danger in saying that our problem is repeatability in that it may discourage other legitimate avenues of making our phenomena intelligible or it may imply that replication brings intelligibility. Both approaches, I think, are wrong. The failure of modern parapsychology is not that we fail to have replication nor is it that parapsychology studies non-existent phenomena; rather, it is that we have not made our phenomena intelligible. That is why parapsychology is not a full-blown science. Repeatability is only a problem insofar as it has become a symbol for this failure.

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DISCUSSION

BELOFF: I fail to follow the connection or relevance between your introductory lecture on traditional epistemology in European philosophy and your main thesis concerning the problem of repeatability. When we talk about the problem of repeatability what we have in mind is that in some sciences repeatability seems to be attainable to a reasonable degree whereas in parapsychology it seems to be peculiarly intractable. That is the problem that we are confronting here at this conference. Now, I can well understand that a very naive kind of objectivity, such as might have been implied in some of these early philosophies, would make the problem of repeatability of observing and understanding nature much simpler than it really is, but the point is that even in such highly sophisticated sciences as modern physics there is no real problem of repeatability such as we face. So I can't agree with you that it is our conception of the self or the role of the mind in the observation of nature—or whatever way you would like to put it—that is really at the root of this repeatability problem. It is a problem that peculiarly concerns us as parapsychologists. Although, of course, we all agree with you, I think that repeatability is a means to an end and that end is the greater understanding of the position of the phenomena in the scheme of things. Nevertheless, it seems to me that this was a diversion that really befuddled the issue.

EDGE: It is not a diversion because to a great extent we live out of the past. We are historical beings and we have perceptions of what things should be, based upon the development of some of these ideas. It is not as if ideas come pure and naked into the world and we use them as such. I think to a great degree you are right in saying that the connection between the parts of the paper is not as clear as it could be. Culturally we still are probably living in the 17th and 18th centuries in our typical understanding of science. I certainly think that this is true traditionally in the philosophy of science. It was really not until the last couple of decades that we even began to understand that science has gotten into the 20th century. So I would simply disagree with you and say that I think we carry a lot more of this baggage into our understanding than we think. Now the transition is that it is the notion of self that is important. When we get to the natural and the social sciences, the transition becomes even more obviously an erroneous notion of self. That brings us into the second part of the paper. If our discussions earlier were correct and if Harris

Walker was correct, replication is to some degree a problem in the natural sciences. Certainly it is a problem in social sciences if what I have said is correct and it is not just a problem for parapsychology. We may have peculiar problems, but the point is that replication does not take place so obviously in other social sciences. Why is this? Why is there this magic about the notion of replication?

SCHECHTER: I want to go back to something that you implied but did not actually say. In going over the history you said that if one has seen and reasoned clearly, replication is a simple matter. One implication is that replication is a test of the clearness and the accuracy of the observation and the reasoning. If a finding cannot be replicated then perhaps there was something wrong with the initial observation. I think that the call for replicability both by those outside the field and within the field is often used in this sense, as a reality test.

Now, intelligibility and replicability seem to me to be inter-related, here. If a finding doesn't make sense to us, we may deal with it by asking "Is there really anything here to worry about?" The question becomes "Are these observations at all accurate?" Then we use replicability to assess accuracy. Rather than the linear chain you were suggesting I think intelligibility and replicability are very interactive ways of interpreting things. There are many examples of ideas that were initially accepted because the theories behind them made a lot of sense, only to disappear later because the phenomena turned out to be disputable, nonreplicable. I think we have a circle here rather than a line towards intelligibility.

EDGE: I was to some degree rather indiscriminate in my uses of "replication" and "repetition." In an earlier version of the paper the title was "Replication is not Repetition." What I am objecting to is the notion of repetition being important. We can build in a lot of other notions when we talk about replication, but notice that when we talk about the number of tests that are statistically significant, when we do the pattern analysis we are talking about repetition. Now, if one wants to make the distinction between repetition and replication, it is possible to build into the notion of replication a whole host of ideas including intelligibility. In our concern for replication we are focusing on repetition and repetition is not a virtue, because in itself it does not bring in intelligibility. This is not to say that we shouldn't be concerned about repetition. Surely, we should. The hope is that some degree of repetition will help us in intelligibility. On the other hand, I think that it is possible to have intelligibility without a very high degree of repetition. I am trying to

see the various concepts that are involved and I take it that the central notion is one of intelligibility. The other notions are derivative. Important, but still derivative. I am just concerned that if we talk about the problem of replication, we really think we are talking about the problem of repetition and that is not the problem that we should be concerned about mainly.

HONORTON: First, I was a little surprised to learn that you don't consider voodoology to be a science. I thought it was part of the science of economics practiced in Washington for the last several years. Certainly, if there was a practitioner of voodoology here who was successful even 25 percent of the time, I don't think any of us would challenge him too much if he wanted to insist that he was a scientist.

The other point that I would like to make is going back to what constitutes a replication. There are different kinds of replication and the debate over what constitutes a competent replication occurs most frequently in the case of intended literal or operational replications. David Lykken has talked about three kinds of replication. There is literal replication, in which the original experiment is repeated in every detail. Of course, as we know, that is virtually impossible in the human sciences. Then there is operational replication where you take the original recipe, the original procedure, and follow it as best you can. And then there is the third kind of replication which is construct or constructive replication, where you take the original hypothesis and deliberately ignore the specific methods used by the original investigator(s). Instead, you develop alternative methods for testing the hypothesis. If the new experiment succeeds, you have greater confidence in the hypothesis because it has been supported by converging evidence from different methodological starting points. None of us would get too excited if we found that we get an ESP effect only if we use subjects who fall into a very narrow classification of some personality inventory and were between 18 and 18½ years of age, left handed, with no ear lobes or whatever. That would be much less interesting than to say that we have a procedure that is robust enough to work on a wide range of subjects. Now, the weakness of the construct type of replication is that if it doesn't work, it doesn't say anything about the original test of the hypothesis. For example, Ganzfeld is one way of assessing the effect of sensory isolation on ESP. Dreaming is another, hypnosis is another and one could say that all of these types of experiments are replications of the sensory isolation hypothesis. You certainly wouldn't call a dream study or a hypnosis study a Ganzfeld replication. But clearly there

would be replication of the central hypothesis of a sensory isolation component. And I think that in psychology most of whatever replication is done is that of the constructive replication variety, because conditions are always changed to a degree that makes direct comparison to the original experiment tenuous.

EDGE: I agree that there are these types of replications, but once again I simply urge us to be careful about what we mean by replication. You have the ability to repeat certain kinds of experiments and you have repetition again. The question is are these at all made intelligible? How do we explain them? Does it make sense to bring all of these together? For what reason? How do you connect it up to other theories? The aim of all of this, it seems to me, is not constructive replication—which I would call it—once again the aim is to make all of this intelligible. And you could have all sorts of constructive repetition without having intelligibility.

STANFORD: I certainly concur with the feeling that we need to try to make our phenomena intelligible. Now, under what circumstances do we try to make phenomena intelligible? I have heard parapsychology criticized on more than one occasion for not having a theory, but the basis of that criticism, as stated by the most intelligent of the critics, is that we have not established any regularities around which to build a theory. The way in which you establish regularities is, of course, in some sense or another by replication. If the replication is conceptual, if the observed regularities are supposed to reflect some conceptual understanding of the phenomena, then we have to practice various types of replication, not just exact, but the constructive replication to which Chuck Honorton alluded.

Let me just ask this question. You said replicability isn't the real problem that keeps parapsychology from being viewed as a science. Don't you think that if we had a demonstrated level of replicability—I am not talking about 100 percent replication—that that might force persons to come to grips with trying to understand the phenomena? Yes, understanding is okay, but our understanding is purely an attempt at understanding until we can find some regularities.

EDGE: The statement that we need regularity to build a theory I just think is factually wrong. I mean there are examples where you don't have a lot of regularities and you have theories accepted. They may not be the kinds of theories that we want to have, but they are theories accepted by the scientific community. And there are just not a lot of regularities. I agree with you that if we have certain levels of replication it would force people to come to terms with psi research. Yes, I think that is a value of repetition, but it wouldn't

force people to try to understand it. The aim of repetition is intelligibility.

BLACKMORE: You and other people earlier this morning have kept making comparisons with other sciences, a couple of which I don't agree with. One simple one is all these tests of looking in books to find out whether they mention repeatability or not. Now, I have done an even worse experiment than you, but I can tell you for example that the *Handbook of Parapsychology* does not have in its index repeatability, replicability or replication. In preparing for this conference I was not able to find more than a couple of books that had references to repeatability or replicability at all. One was one of Rhine's books which had it in the index, but when you actually turned to the page it had two lines and there were a couple of others like that. If you look in some other representative parapsychology books you won't find it either. So I don't think that is a fair comparison.

Now, a more general point is all these statements that repetition doesn't happen in psychology and other behavioral sciences. I agree with Chuck Honorton about different types of repetition; it is clear that conceptual or construct repetition does happen. But other forms of repetition are happening all the time. People don't set out to repeat somebody else's experiment exactly, but if you take a progression through a simple small area, let's say some new visual effect is discovered (which is the sort of thing that we study in our lab), you'll find that the original results raise a lot of questions to be asked. Then people say "Right then, let me try this effect and I'll try and see what happens if I do it X, Y and Z." The aim of those follow-up experiments is quite different and what they do is quite different, but embedded in them is always the assumption that the original effect is there. Indeed it is and there is a sort of hidden repetition. If, of course, they don't find the original effect that they are supposed to be studying, then the whole thing falls apart, but typically it carries on. That kind of hidden repetition is happening all the time in other sciences and I think that is part of what we don't have.

RAO: I don't know what you mean here by science. Are you talking about the methods of science, are you talking about a full blown science with methods and a body of knowledge and the theory to explain the facts? I am not sure what you mean by intelligibility either. I am not sure whether intelligibility, theory, understanding, orderliness all mean the same thing. Even if we do not have a theory, can't we do science when we have brute facts to deal with? You

collect facts and the theory emerges at a later date when you try to understand the interrelationships that you have been able to find between the facts. You can, I am sure, begin with a theory and then attempt to collect facts, but you can also start with observations and from these observations weave a theory. The whole process of science does not come to you as a full-blown finished product in which you have facts and theory all at the same time. And no one claims that parapsychology is anything but a beginning science that is attempting first to validate the basic phenomena to see whether they exist. And this pursuit in itself, to my mind, seems to be a basic element of science. There doesn't seem to be any need to have an overall arching explanation or a theory at this stage to qualify our endeavor as a scientific one. Do we have a theory for our psychology that is acceptable to everybody? And is not psychology a science in that sense? So it seems to me that this point must be kept in mind. As long as we are using methods that are considered by consensus as scientific, as long as we are in the business of collecting facts and attempting to understand them, I think we are doing science.

Another point is a little bit in defense of Collins. I don't know whether Chuck Honorton implied that he is disagreeing with Harry Collins on this. But the point that Collins was making here is one relating to the existential questions of the phenomena. He is talking about distinguishing between the genuine and the spurious and replication considered as something that enables you to distinguish between the genuine and the spurious. When that is the question, the matter of constructive or conceptual replication is irrelevant, because you can always go back and say "Your procedure may not work, but if you follow my procedure it would work." So in the context of what Collins was trying to do, I think his dealing with replication as he did is adequate.

EDGE: Perhaps my use of the word "science" was sloppy. There is a sense of science in which, if one is using certain methods, one is doing science and I fully agree with that. But I do think that there are theories in science and they are intelligible. What I was trying to get at is that there really is something in the charge that something is wrong with parapsychology, because it doesn't have a repeatable experiment. In a sense, simply arguing that we have this kind of repetition really doesn't meet the charge that there is no replication. Something legitimate is going on and is not just a question of repetition. I think we have that—I think there is adequate evidence that we have that—something else is happening and that is what we need to focus on. In this second, more sociological context science is

what people accept as science and, in that kind of sociological context, intelligibility becomes important and psychology, but perhaps not parapsychology, is intelligible.

WALKER: I am delighted with what Hoyt Edge has said about the importance of theory. I am also delighted to see a polarization with regard to this issue. There was a comment made earlier that if we see a lack of replicability in science, then we ask the question "Does it make sense?" If the answer is "Yes," people try new experiments that may fail if there is no replicability. But they don't go to work with the intention of replicating generally. If it doesn't make sense, they replicate for the purpose of discrediting the original work. There is polarization in that those who are theorists will view this from their point of view, that the experiment is secondary, whereas experimentalists see it the other way around. This type of attitude has been extremely powerful and healthy in physics. There are really two classes of people in physics: those who are the theoreticians who see theory as the primary way of looking at scientific facts and the experimentalists who have exactly the opposite view, a view that theory is secondary and that the experimental data are the primary scientific facts.

EDGE: Overarching theory is only one way to achieve intelligibility and it may not be the best way for parapsychology. There are other ways and I think we ought to explore all of them.

BALLARD: There is probably enough in Dr. Edge's paper to keep us busy for the rest of this gathering. There are many ideas here. I think it is important to keep in mind who we are in parapsychology and where we came from. Various sciences go through different stages. We need to consider the fact that we are really at the point where we are engaged in inquiry using the scientific method, whereas we may not be recognized as a science. And I think it is important to recognize what then is a science from that perspective. Most of science is engaged, as Kuhn would say, in puzzle solving. I think that we are engaged not in normal science by any means, but rather extraordinary science. But in doing so we are using the scientific method. What brought all of us here today is that at some point we recognized or the people who came before us recognized that there are phenomena that are occurring out there; phenomena that are affecting people's lives. We as scientists want to bring those phenomena into the laboratory and demonstrate them. Now science has two requirements in terms of evidence: (a) procedural evidence and (b) validating evidence. We have procedural evidence in terms of the experiences that are being reported. We feel that there is an anomaly

of some type out there that is not being understood or investigated. We are trying to investigate it. So we take those ideas based on the procedural evidence and bring them into the laboratory and try to obtain validating evidence. That is what we in our laboratories are trying to do—obtain that validating evidence. Once we have validating evidence, we try to demonstrate that there is indeed an anomaly. Because we don't have evidence that people can look at and say in an objective sense that yes, there it is, we then need cross validation of whatever validating evidence we achieve. Hence the need for repeatability with or without theory. Once we have a demonstrable anomaly then we can work to build theory. It may be that we will need theory to demonstrate that anomaly.

EDGE: What you are talking about is proof oriented research. I think, in fact, we should do more process oriented research.