TAXONOMY AND THEORY IN PSYCHOKINESIS

STEPHEN E. BRAUDE

The term "PK" covers various apparently anomalous phenomena which, due to our ignorance of their nature and possible underlying causes, seem for the present to form a natural group. "PK" may be defined, roughly, as "the causal influence of an organism on a region of the physical world r without any known sort of physical interaction between the organism's body and r." Clearly, this definition applies to many superficially different phenomena studied by parapsychologists—from the apparent influence of dice and RNGs to levitations, apports and materializations. And for all we know at this stage, it is possible that the variety of phenomena considered psychokinetic are fundamentally alike. They might all be manifestations of a single and as yet mysterious, process. Our classification of different sorts of PK phenomena might thus locate different points on a single continuum, perhaps in the way tennis as played by a beginner represents a terminal point on a continuum whose other terminal point is represented by the tennis of Bjorn Borg, or (if level of proficiency does not determine points on the continuum) perhaps in the way the different visible colors are superficially distinct manifestations of the same underlying sort of process. On the other hand, the superficial differences between the various PK phenomena might be manifestations of deeper differences. Our classification of RNG or thermistor fluctuations with table levitations and materializations may thus obscure deep differences in their underlying causal processes. Parapsychologists might have hunches as to which of these two general pictures of PK is closest to the truth. But research in the field is nowhere near the point where we can confidently choose one over the other. One would think, then, that theorizing about PK would reflect or acknowledge our ignorance concerning the possible unity of PK phenomena.

Curiously, however, a great deal of recent PK research and theory seems oblivious to this issue. For example, many parapsychologists study apparent PK effects on RNGs and develop elaborate theories to account for them, without considering whether the theories have anything at all to say about, e.g., the more bizarre phenomena reported in poltergeist cases and in the best-attested cases of physical mediumship. To be fair,

some parapsychologists *do* attempt to extend their theories about laboratory PK to spontaneous phenomena. But my impression is that, with few exceptions, they feel the need only to account for small-scale and relatively non-dramatic effects, such as object movements or apparent spontaneous combustion. No recent theorizing I have seen about PK makes any effort to explain, e.g., the apparent materializations of D. D. Home and Eusapia Palladino, or Home's apparent ability to elicit melodies from an accordian held in one hand (or sometimes not held at all). But since we simply do not know whether the different forms of PK represent deeply different phenomena, this rather widespread neglect of the most interesting physical phenomena strikes me as an inexcusable bit of scientific myopia. It is especially unfortunate that the fashionable ways of talking about PK have tended to promote the view that PK phenomena are not continuous. We thus distinguish *micro*-PK from *macro*-PK and *experimental* from *spontaneous* PK, and often proceed without further ado as though these are distinctions between genuinely different and apparently independent kinds of phenomena. I suspect that these distinctions were not originally intended to be used in this way. Nevertheless, their apparent utility seems to have led to some unwarranted assumptions and muddy thinking; and I believe the time has come for some conceptual housecleaning.

Consider, first, the distinction between micro- and macro-PK. Micro-PK is usually understood to be PK on systems too small to be observed by the naked eye, but which, nevertheless, has observable causal consequences (e.g., feedback of RNG outputs). It is not clear what, exactly, macro-PK is supposed to be. The term "macro-PK" is usually defined only by ostension or by example. Nevertheless, one might think that if micro- and macro-PK were distinct phenomena, then the latter would be the direct PK influence on macroscopic systems, bypassing the sorts of microscopic causal interactions ordinarily thought to produce the macroscopic effects in question. But, in fact, parapsychologists tend to regard macro-PK phenomena (like object levitations and spoon bending) the same way they regard feedback of RNG outputs—i.e., as observable causal consequences of PK effects occurring at the micro level. This makes the aforementioned neglect of the more unusual poltergeist and mediumistic phenomena seem even more peculiar. The received wisdom in parapsychology is not merely that we don't *know* whether micro- and macro-PK are deeply different (so that we should be careful to avoid begging the question one way or the other). Rather, the general assumption is that the fundamental sort of PK is micro-PK, and that observable PK effects are *analyzable* in terms of two main lines of attack. (a) We might argue that while PK on observable systems is mediated by PK on unobservable

systems, it may not be reducible to any set of underlying interactions. That is, we might claim that an observable PK phenomenon *P* is a by-product of micro-level causal processes, but that no set of subsidiary processes is either identical with or necessary and sufficient for the production of *P*. We could put this briefly by saying that these cases of observable PK are non-mechanistic. (b) On the other hand, we might take a more radical line and claim that (at least some) observable PK results from no *process* at all—that is, that it may be entirely non-mediated. We might maintain that observable PK (e.g., an object levitation) need not even be a by-product of unobservable PK and that even micro-PK experiments need not provide evidence of PK effects on non-observable systems. For example, we might entertain the possibility that PK on RNGs is a phenomenon in which nonrandom RNG outputs are explained *independently* of reference to underlying processes, such as PK on radioactive decay or electronic noise.

Now, I realize that it is more controversial to suggest that some instances of micro-PK might be subsumable under macro-PK than to suggest the reverse. And I realize it is likewise controversial to suggest that PK phenomena might have no analysis or underlying structure. But that is because these proposals conflict with a pair of assumptions that dominate scientific thinking as a whole, but which seem to me to have little merit. The first is (a) that physics is the fundamental science of nature, in the sense that all natural phenomena are analyzable in terms of processes studied in physics—specifically, those occurring at the atomic or quantum level. The second assumption is simply the general mechanistic assumption (b) that observable phenomena generally have underlying structures and that it is, in principle, possible to analyze every observable phenomenon in terms of its underlying processes and mechanisms. In other words, the second assumption is that there are no unanalyzable facts or phenomena on the macroscopic level. This assumption may be held without holding the first, since one need not regard the analysis of observable facts as a task falling ultimately on the shoulders of the physicist. The first assumption is merely a very popular version of assumption (b).

Now in my opinion, the scientific community has been slow to realize that assumption (b)—and hence (a)—is false, particularly when it comes to explaining human cognitive functioning and other organic phenomena. For example, instead of seeing the wholesale abandonment of the theoretical abomination called *memory trace* theory, we see a proliferation of versions of trace theories, including the now popular holographic version. This is not the place to launch an assault on mechanistic theories of organic phenomena generally or trace theories in particular (but see Braude, 1979; Bursen, 1978; Heil, 1978; and Malcolm, 1977 for criticisms

of trace theories). It should suffice for now to observe that it is no more than a well-entrenched article of faith—not an established fact—that observable and, especially, intentional phenomena can be analyzed in terms of more fundamental processes and, in particular, processes of a non-intentional or non-organic nature, as assumption (a) would have it.

In fact, as I have pointed out elsewhere (Braude, 1979), the assumption (b) that observable phenomena are identical with or causally specifiable in terms of underlying processes or mechanisms, turns on an even deeper—and hopefully even more obviously arbitrary—assumption. Most people recognize that explanation by analysis cannot continue indefinitely. Phenomenon P might be identified with or causally specified with respect to some underlying set of phenomena $R_1 \cdots R_n$, and each R_i might itself be identified with or causally specified with respect to a set of subsidiary phenomena $S_1 \cdots S_n$. But this process of analysis cannot continue forever. At some point, vertical explanation, explanation by analysis, must cease. That is not to say that *all* explanation grinds to a halt. It merely means that *if* further explanation is possible, vertical explanation will be replaced by some form of horizontal explanation—for example, explanation by analogy rather than analysis. Now, the very deep assumption underlying mechanistic assumption (b) is (c) that *wherever* explanation by analysis stops, it will always be at the level of the very small—e.g., the neurological, biochemical or atomic level and never closer to the surface, at the observable level. And this clearly *is* an assumption, vulnerable (it so happens) to a battery of powerful objections.

But once we entertain seriously the possibility that some observable phenomena are ultimate in the sense of having no analysis or explanation in terms of underlying processes or mechanisms, then we should be prepared to entertain the two major suggestions mentioned above, concerning the way in which observable PK phenomena might be fundamental and unanalyzable. The first is that while PK is *mediated* by subsidiary processes and mechanisms, nevertheless it does not *reduce* to some such set of underlying phenomena. It might turn out that certain small-scale physical or physiological conditions are PK-conducive or even necessary for the exercise of PK, without there being a set of physical or physiological conditions necessary and sufficient for the production of a given observable PK effect. Analogously, I might be unable to remember unless I have a functioning brain (contrary to what spiritualists and Cartesian dualists maintain), without there being a set of brain processes identical with or necessary and sufficient for the occurrence of a given act of remembering.

Cognitive phenomena generally are unanalyzable in this way. To the extent such phenomena involve relations of representation and meaning, they can only be characterized rather loosely and functionally with respect

to the position of various events within a sequence of events and also within a larger local and global (societal and cultural) context (see Braude, 1979). These *positional* aspects of cognitive phenomena reduce to no antecedently specifiable, much less purely structural or topological, features of nature. Yet the manifestation of the phenomena might presuppose some purely physiological goings-on, processes which may be said to partly constitute the cognitive phenomena in question.

On the surface, however, there would seem to be an interesting difference between PK and cognitive phenomena—namely, that only in the latter case do positional features play an essential role. Remembering, believing, etc., can only be characterized with respect to just those positional characteristics not reducible to any underlying mechanisms or processes. But in the case of PK, we are interested (so it seems) precisely in the bringing about of a certain physical phenomenon, no matter what positional attributes the phenomenon has. If I can paranormally produce raps in a séance table, it is irrelevant to PK theory whether my pattern of raps represents "yes," "no" or "maybe" (although just these sorts of positional features are essential to an explanation of the process of communication). In PK theory, we want to know how the sounds are produced, no matter what they mean. Perhaps, then, observable PK phenomena might not be irreducible to subsidiary processes in the way cognitive phenomena are. If so, then perhaps the theoretician is on the right track after all in looking for the underlying processes necessary and sufficient for the production of the observable physical phenomena.

I think, however, that this difference between PK and cognitive phenomena may only be apparent, or at least not pervasive, and that we might think otherwise only by failing to take seriously some of the more interesting poltergeist and mediumistic phenomena. In many of these, irreducibly positional elements *do* seem essential. For example, some poltergeist phenomena seem menacing or hostile, and such nuances of the phenomena might be no more accidental or arbitrarily imposed on the phenomena by an observer than would the hostility of an intentionally antagonistic remark or a punch in the mouth. Yet hostility cannot be analyzed except with respect to a set of contextual conditions that reduce to no formal description or set of specifiable states of affairs. The same would be true of the gracefulness of a D. D. Home accordion rendition, the affection in the touch of a materialized hand or the playfulness of some poltergeist antics.

If this is the respect in which some PK phenomena are fundamental and unanalyzable, then scientists will presumably be able to analyze only those aspects of the PK process that *can* be described mechanistically. This may not take us very far, just as an analysis of the processes un-

derlying the production of vocal sounds or hand movements in writing ultimately tells us very little about communication. But at least there would be processes to study.

But observable PK may be unanalyzable in a more interesting and radical way. As I suggested earlier, there may literally be no process to PK, no set of phenomena intervening between the PK effect and the state of mind causing it, almost as if a phenomenon could be instantaneously produced by waving a magic wand. The reasons for taking this option seriously are not simply the abstract matter discussed above, concerning the assumption about where explanation by analysis *must* stop, nor the equally abstract matters I discuss below. This approach to PK might also seem reasonable when we take seriously some cases of apparent materialization and apports, those in which complex and well-formed objects appear (and sometimes disappear and reappear) apparently instantly. If this more radical point of view has any merit, we might therefore want seriously to consider the possibility that phenomena classified as examples of micro-PK are really instances of direct, unmediated interactions between organisms and observable objects or states of affairs. We should, therefore, be more open to the suggestion that spoon bending, for example, might be a phenomenon whereby macroscopic deformation of the spoon produces a corresponding microstructural change, rather than a process in which a change in the spoon's microstructure produces a corresponding macroscopic change. By the same token, we should be prepared to consider the possibility that changes on the quantum level are by-products of PK effects on the observable level, rather than as the reverse.

Some may still protest that causal interactions between an organism's state of mind and ordinary physical objects like tables, spoons, or accordions cannot be occurrences unmediated by lower-level processes. They might feel that such phenomena cry out for analysis in terms of less mysterious subsidiary phenomena—specifically, interactions between mental states and phenomena at the quantum level. But I must point out that insofar as both sorts of interaction posit a causal link running from the mental to the physical, neither is less mysterious than the other. Perhaps the most popular approach to PK these days, promulgated by physicists and embraced enthusiastically in many quarters, is to appeal to consciousness collapsing the state vector (see, e.g., Mattuck and Walker, 1979 and Walker, 1975). But in my opinion, it is rather philosophically naive to think this renders less enigmatic the causal efficacy of mental states on physical states. In fact, as a number of authors have recently observed (see, e.g., Beloff, 1980 and Thakur, 1979), partisans of this approach seem to be impaled on the horns of a dilemma. Either they must

reduce states of consciousness to physical states and then construe the causal efficacy of mental states as merely a form of purely physical causation, or they must admit that the collapse of the state vector by consciousness is a form of mental → physical causation. The first horn of the dilemma, outright reduction of the mental to the physical, is implausible for too many reasons to be summarized here. Besides, Mattuck and Walker make a point of rejecting the reductionist platform and claim explicitly that consciousness is non-physical. This leaves the second horn of the dilemma. But this option retains the principal mystery which the retreat to quantum physics was intended to avoid. The causal link between the mental and physical remains, and presumably the physicist is here forced to concede that this link is not one capable of further analysis. *How* it is that consciousness directly brings about the collapse of the state vector is a question without an answer. It is simply the way the universe works. But this form of mental → physical causation is every bit as mysterious as any other direct causal link from a mental state to a physical state of affairs. So the popular quantum physical approach offers no further insight into the causal link between consciousness and the physical world. It merely restricts attention to one of the mind's possible stages of operation, the quantum level. So once we grant (i) that it is possible for primitive or unanalyzable facts about organisms to be facts about observable phenomena and (ii) that there may be unanalyzable causal interactions between states of mind and the physical world, we are simply no longer constrained to locate those fundamental interactions on the quantum level.

Moreover, we should bear in mind that there is no reason to assume that PK phenomena must respect our distinction between observable and unobservable. After all, the distinctions between macroscopic and microscopic, and observable and unobservable, are not sharp. They are merely matters of degree; they do little more than allow us to systematize phenomena in terms of the limitations of our sense organs. But, to put it somewhat colloquially, these limitations are something we human beings are stuck with. They are not a problem for Nature. It is completely implausible to suppose that Nature's laws must make a sharp distinction where mere humans are forced to make an unsharp distinction—that is, that Nature inherently divides herself into ontologically distinct or nomologically independent domains of phenomena corresponding to the domains marked off by our perceptual limitations. To suppose otherwise would appear to be an act of extreme hubris; it would place a totally unwarranted importance on man's place in nature. So perhaps fundamental PK interactions can occur *anywhere* on the observable/unobserv-

able continuum; perhaps PK is a phenomenon that involves primitive interactions between organisms and physical systems generally, whether observable *or* unobservable.

Perhaps, then, we should shelve the distinction between micro- and macro-PK. As we have seen, the categories do not clearly discriminate among the various phenomena considered psychokinetic. In fact, they seem to serve no useful function at all. Moreover, they tend to foster the dubious assumptions about the primacy of the microscopic which I have been at pains to criticize, and in the process encourage lines of theorizing which may be profoundly mistaken. The micro/macro distinction, however, is only one of the currently popular ways of dividing up PK phenomena. Parapsychologists also like to distinguish experimental from spontaneous PK. So we might wonder whether this succeeds any better in drawing a clear line between kinds of PK phenomena.

Consider: What justifies the familiar classification of PK on dice or RNGs as experimental or non-spontaneous and a table-levitation at a séance as spontaneous or non-experimental? Is the latter considered non-experimental because it is conducted in a private home, hotel or other location outside a lab? If so, then apparently neither the results of Schmidt's take-home tests with RNGs (Schmidt, 1978) nor those of William Crookes' elaborate spring-balance tests with D. D. Home (Medhurst, 1972) would count as experimental PK. So perhaps experimental PK phenomena are those conducted in a more or less formal setting (whether in a lab or not) under controls of some sort, and spontaneous PK phenomena are those that are non-experimental. But in that case a good many mediumistic phenomena—some usually considered to be paradigm cases of spontaneous PK—must also count as experimental (e.g., the object-levitations and accordion phenomena produced by D. D. Home for Crookes, and those produced by Eusapia Palladino at the 1908 Naples sittings [see Feilding, 1963]). Even more seriously, this way of distinguishing experimental from spontaneous PK has the following awkward—but revealing—consequence. A phenomenon would count as experimental only when being observed carefully and with good controls, but spontaneous otherwise. Thus, PK on dice would sometimes count as experimental and sometimes not (e.g., when done for fun in a person's home). This shows that the distinction between experimental and spontaneous PK is not a distinction between kinds of PK. Rather, it is a distinction between different *conditions of observation*. As such, it seems to be perfectly reasonable. We should simply recognize the distinction for what it is and realize that it contributes virtually nothing to the taxonomy of paranormal phenomena. And, of course, it certainly affords no basis

for studying and theorizing about PK on RNGs (for example) to the exclusion of mediumistic and poltergeist phenomena.

Another way of drawing the distinction between experimental and spontaneous PK may, however, have a bit more utility (I am grateful to Carol Irwin for pointing this out to me). Some phenomena achieve the status of *prima facie* PK effects only in virtue of quantitative statistical analysis, whereas others would count as ostensible PK effects independently of such analysis. RNGs will, if left to themselves, produce nonrandom sequences; and dice will land with a face up independently of any PK. What inclines us to regard certain such sequences or events as ostensible PK effects is their statistical improbability. But no such quantitative analysis is needed to conclude that an apparent levitation, materialization or elongation is an ostensible PK effect. So perhaps the distinction between experimental and spontaneous PK is at bottom the distinction between PK phenomena anomalous on statistical grounds and those anomalous independently of any such quantitative considerations. For convenience, let us call this the distinction between quantitatively and qualitatively anomalous PK.

Now, I have no objection to this distinction. To be sure, some phenomena seem to fall somewhere between the two categories—for example, those in Vasiliev's (1976) studies in suggestibility at a distance. And, of course, if we try to apply the distinction to ESP phenomena, then all the results of free-response studies seem to fall into this twilight zone. It is unclear whether correspondences between subjects' responses and targets are *prima facie* anomalous on quantitative or qualitative grounds. Granted, we evaluate the results of free-response tests by means of quantitative procedures. But that may be little more than a way of attempting to demonstrate—in a manner acceptable to the scientific community—the soundness of our perception that such correspondences are profoundly out of the ordinary.

In any case, however useful the distinction between quantitatively and qualitatively anomalous phenomena may be, we must again be careful to observe that these classes may correspond to no deep differences in the nature of the two sorts of phenomena—for example, with respect to their underlying causal mechanisms (if there be any). That is, the process by which RNGs are made to behave nonrandomly may differ in no deep way from that which produces levitations or materializations. The quantitative/qualitative distinction may thus prove to be nothing more than a distinction between methods of determining ostensible paranormality.

So far, then, it appears as if we have no grounds for making anything but extremely rudimentary methodological distinctions among the various

PK phenomena. And in some of the cases it would seem that the clarity secured by the distinction is overshadowed by the theoretical obfuscation it produces.

Reproachful Postscript

I earlier decried the myopia of many parapsychologists who claim to produce theories of PK, but who deal exclusively (or almost exclusively) with the small-scale statistical effects produced in laboratory experiments, ignoring much or all of the mind-boggling physical phenomena reported in some poltergeist cases and studies of the great physical mediums. In some cases, this narrowness of focus results merely from a failure to acknowledge the possible continuity or connectedness of all PK phenomena. But in many others it is due to an outright ignorance of, or failure to take seriously or think clearly about, the enormous body of evidence for physical phenomena. In the past year I have been astonished to learn how many people who consider themselves serious parapsychologists have never given more than a few minutes' thought to D. D. Home or who have never read—and in some cases even heard—about the remarkable 1908 Naples sittings with Palladino.

This is not the place to defend the evidence for the physical phenomena of mediumship. That project is on my agenda for the future. I will simply close with an admonition. I doubt seriously whether any significant progress in PK theory will be forthcoming until this evidence is confronted squarely. Those parapsychologists who today fancy themselves to be heavyweight PK-theoreticians have offered little more than mere promissory notes, agreeing to extend present theories (e.g., about PK on the quantum level) to large-scale phenomena. But in view of the considerations advanced in this paper, this gesture is hardly reassuring. It may be nothing more than ill-disguised deficit spending. Not until the large-scale phenomena are regarded as data every bit as clean and important as purely statistical PK effects will we be able to deal effectively with the question of whether all PK phenomena fall on a single continuum and, if so, what that continuum is. To theorize about the nature and mechanisms of PK while ignoring (say) the achievements of D. D. Home, might (for all we know) prove to be as foolish and misguided as attempting to study the art of playing tennis while ignoring the game as played by the most gifted professionals.

BIBLIOGRAPHY

Beloff, J. "Could there be a physical explanation for psi?" *Journal of the Society for Physical Research*, 1980, 50, 263–72.

Braude, S. E. *ESP and Psychokinesis: A Philosophical Examination.* Philadelphia: Temple University Press, 1979.

Bursen, H. A. *Dismantling the Memory Machine.* Dordrecht, Boston, London: D. Reidel, 1978.

Feilding, E. *Sittings with Eusapia Palladino and Other Studies.* New Hyde Park, N.Y.: University Books, 1963.

Heil, J. "Traces of things past." *Philosophy of Science*, 1978, 45, 60–72.

Malcolm, N. *Memory and Mind.* Ithaca, N.Y.: Cornell University Press, 1977.

Mattuck, R. D. and Walker, E. H. "The action of consciousness on matter: A quantum mechanical theory of psychokinesis." In A. Puharich (Ed.), *The Iceland Papers.* Amherst, Wisc.: Essentia Research, 1979.

Medhurst, R. G. *et al. Crookes and the Spirit World.* New York: Taplinger, 1972.

Schmidt, H. "A take-home test in PK with pre-recorded targets." In W. G. Roll (Ed.), *Research in Parapsychology 1977.* Metuchen, N.J.: Scarecrow Press, 1978, 31–36.

Thakur, S. C. "Hidden variables, bootstraps and brahman." *Journal of the Society for Psychical Research*, 1979, 50, 135–148.

Vasiliev, L. L. *Experiments in Distant Influence.* New York: Dutton, 1976.

Walker, E. H. "Foundations of paraphysical and parapsychological phenomena." In L. Oteri (Ed.), *Quantum Physics and Parapsychology.* New York: Parapsychology Foundation, 1975, 1–53.

DISCUSSION

RUDOLPH: My impression is that, in practice, people distinguish between micro- and macro-PK by measuring the effect rather than where it's happening. In particular, if you need statistical inference to see the effect then it's micro and if you don't, it's macro. But, mostly, I wanted to comment on your assumption b, the mechanistic assumption, explanation by analysis. It seems to me that the physicists are at the forefront of giving that up and that it's the psychologists who are still wedded to that. The Copenhagen interpretation of quantum mechanics fifty years ago, gave up explanation by analysis. So I think maybe that's a bum rap for the physicist.

BRAUDE: I didn't mean to be laying that entirely or even primarily on the physicist, but I think you may be right that it's primarily psychologists and behavioral scientists generally who are still aspiring to be scientific in the way thought appropriate to the hard sciences. About your other remark, how the distinction between micro- and macro-PK is used, I agree it's often used only as a methodological distinction, but somewhere along the line it seems to me something has gotten muddled up. An assumption gets made that perhaps we're dealing with nomologically independent kinds of phenomena, and that we can theorize about and study one sort without having to seriously consider the aspects of the other. I think that's a serious mistake. For all we know, cranking out theories about PK on RNGs or dice or small objects, while ignoring materiali-

zations and elongations, might be analogous (as I've said) to studying tennis played by beginners and then ignoring tennis as played by the very best professionals.

BRAUD: I question whether those assumptions that you attack so well were ever really made. To me, distinguishing things such as macro-PK and micro-PK, spontaneous and laboratory are just convenient communication aids and ways of dividing up the experimental pie. We don't need to imply anything more than that.

BRAUDE: Well, I think that some people are more careful about this than others. The distinctions may have initially been conceived of as doing little more than that, but I think somewhere along the line it's been assumed that we marked off genuine distinctions between kinds of phenomena and that it's possible to study one sort without the other. If I'm giving the parapsychological community a bum rap on this, I apologize.

STANFORD: I agree with William Braud that you are making some distinctions for parapsychologists that most of us don't make for ourselves. You seem to be concerned about parapsychologists developing theories, models and so forth perhaps derived primarily or initially from the micro-PK situation. I guess you feel that such persons are somewhat oblivious to the larger gross phenomena such as D. D. Home ostensibly produced. Now, I think that most of us would agree that the use of delimited models dealing with specific domains (somewhat arbitrarily delimited even within a given science) has in the past proven useful. Sometimes, those models have a great deal of use within the domain they're originally applied to. But later on it may be possible that they will have generality beyond that, and it takes time to learn whether that is the case or not, so that this type of delimited focus may have a use for understanding the kind of things that D. D. Home did eventually, but we cannot say. Even if there is no underlying unity of PK, the models may still be useful. If so-called macro-PK or the gross manifestations that you talked about are in fact distinct, our models may still have usefulness for the domain from which they were originally generated. I don't think we should be afraid of developing such models on that account.

Another point concerns your attacking trace theories. I think I understand the way that you're applying that. It refers to an attempt to explain mental events in terms of discrete traces, say in brain activity, etc. But, perhaps, what I'm about to refer to is not an example of that—it wouldn't necessarily have to be—but I think some of the most useful, current modeling that we have in parapsychology, specifically with regard to ESP, comes from what might be called, in some sense, trace theory. I particularly admire Harvey Irwin's attempts to refine and develop Bill Roll's

old memory trace release model in his recent book and writings. It's particularly useful because it just so happens that this is one of the few constructs under which we can subsume a lot of things that seem to be regularities in our research. In some sense, you might call it a reductionistic effort.

You notice I use the word reductionistic. It seemed to me that was what you were saying in your second point essentially. I don't consider myself necessarily a reductionist; I'm just open about the question. But, in terms of the direction that this takes you in, I get the feeling that I'm hearing a philosopher talking here, not someone who feels charged with doing actual research. Because it worries me when I hear someone from that perspective criticize the direction that research is taking. I would like to know what direction that leads you in terms of falsifiable ideas that you could bring into the lab and then test. If we're going to do science with parapsychology we've got to have something that projects some new knowledge that can be falsified. If you can do that with your approach, then more power to you.

The approaches we have taken have been the ones that have seemed obvious to most of us, so that we could make come clear-cut predictions and follow them. If they're reductionistic, maybe that's incidental, maybe not, I don't know.

BRAUDE: First of all, I don't want to discourage the development of models that deal initially, for example, with some of the micro-PK work. All I would caution is that at least one eye be kept on the more exotic phenomena. It may, in fact, turn out that some of the models dealing with the smaller-scale effects can be later applied in some way or another to the larger-scale ones. It's also not so much reductionism that I'm concerned about as a certain kind of mechanism. It doesn't matter whether the mechanisms proposed to explain various phenomena are purely physical or something more occult. What I'm concerned about is the idea that observable phenomena must have some underlying structure, whether it be purely physical or some hybrid of the physical and mental or purely mental, as certain idealists historically would have had it. What I think we have to get away from is the notion that every observable phenomenon or fact must have some analysis in terms of underlying processes.

With regard to the role trace theories have played in Harvey Irwin's work or in other work, I would say that I consider that to be not much of a gain at all. It seems to me that the situation is analogous to the situation we had in work on artificial intelligence, where it seems that impressive gains are made by means of various kinds of models and then, somehow or another, those models never get extended to the really in-

teresting phenomena. I think that Hugh Dreyfus once said that analogously, climbing a tree would bring you closer to the moon, but ultimately it won't get you very far.

What bothers me finally about trace theories is that I don't think any such theory could possibly make sense, unless it was assumed that there was an intrinsic structural isomorphism between physical states, such as states of the brain and states of the world. And any idea that there could be *any* sort of intrinsic structural isomorphism between *any* two objects is absurd.

With respect to the role of the philosopher in all of this, let me wax philosophical for a moment. I often feel that scientists lose sight of the fact that their activity is not purely empirical. That is, there is no way of investigating nature that's devoid of a number of extremely deep assumptions about what observation is, what the world is like, and a variety of methodological observations or assumptions about which investigative procedures are appropriate to which domains. These are exactly the things that I think it's a philosopher's duty to challenge, including, for example, the assumption in parapsychology that we should even be able to have a theory of PK. Now, I'd like to see that assumption challenged more openly, just as I would be willing to challenge the assumption that there can be theories about memory. It's my view that there can't be a theory of memory—at least, not a theory in the sense in which psychologists are trying to find one. And it may just be that organic phenomena generally can't be accounted for in terms of the sorts of things that we would call theories in the hard sciences.

JAHN: Many of the inadequacies that you point out in the modeling of psychic phenomena have their analogies in the modeling of hard physical science. I agree with you that it would be presumptive to search in the first instance for the unified theory of psychic phenomena. The physical scientists, for three or four centuries, have been searching for that without success, and in its place, as you know, there exists an arsenal of theoretical strategies for dealing with observable, physical phenomena—classical theories, quantum theories, particle theories, field theories, statistical theories, relativistic theories and so on.

I also agree that one ought to be concerned about the interfaces between these domains of modeling and the interfaces between the micro and the macro, whatever that means in the given context. Here again, in physical modeling, this has been, perhaps, some of the most fertile area for theoretical and experimental work. The most consequential physical models are those which help in relating adjacent domains to each other. From my view, the overriding importance of, say, the Einstein $E = mc^2$ equation is not that it permits you to design nuclear weapons, but rather that it

related for the first time what had previously been regarded as totally distinct concepts of mass and energy. The importance of statistical thermodynamics is that it is the overlap domain between the microscopic world of kinetic theory and the macroscopic world of thermodynamics. In being able to relate random atomic scale processes and probabilities to thermodynamic functions, physical theory made a major step forward in the ability to represent a range of observed phenomena, but that representation did not come easily.

It is surely presumptive to think that the course of modeling of paranormal phenomena is going to be any easier. The physicists had the advantage of replicable experiments and scientific paradigms. To think that one is going to be able to proceed in as difficult and empirical a field as psychic phenomena with a single model and that one will not have problems defining the difference between micro and macro or in relating adjacent domains, is very optimistic.

BRAUDE: I didn't mean to suggest that we're dealing with a very clear cut or simple matter. One of the things I mean to emphasize is, first of all, that certain sorts of theories may not even be possible. And, whatever our theories have to embrace and whatever our models have to cover, the domain of phenomena to be considered is very likely to be much wider than that which, for example, theoreticians about PK have traditionally considered.

ROSEN: There is one area of clarification I think I need. It bears on the distinction you made, between mechanistic or physicalistic concepts which I take as vertical forms of explanation and forms of knowing or understanding that have some degree of structure but are analogical or horizontal. Could you say a little bit more about that distinction?

BRAUDE: Go back to the old days of the Rutherford-Bohr atom when the electrons and elements of the nucleus were considered to be indivisible. There was a time that, say, certain behavior of electrons demanded some sort of explanation, but it wasn't thought that these might be explained in terms of even deeper underlying processes. Nevertheless, it was believed that we could shed some light on the behavior of atoms and electrons by comparing the two to the behavior of the solar system. So by comparing the structure of the atom to that of the solar system, there was a certain amount of conceptual benefit, even though it wasn't obtained by further breaking down the processes of the atoms into subsidiary processes.

ROSEN: Would you say that where a vertical explanation might fail, instead of being faced with a total inability to explain, we might make some progress, clear new ground, by seeking horizontal explanations? I'm seeing a trichotomy here. One is the vertical explanation which would be purely analytical or reductive. The second would be no explanation

at all, but a sort of nihilistic acceptance of absolute limitation. The third possibility would be a horizontal kind of explanation.

BRAUDE: I agree that all of those options are possible, but not with respect to the very same phenomenon. Let me give you an example about memory. I don't expect most of you to agree with this, but suppose I had the ability to remember phone numbers, but not names. And suppose I remember your phone number, but not your name. Now, we might ask why was it that Braude was able to remember your phone number but not your name. We might explain that with reference to some regularity of mine, that is, that Braude has the ability to remember numbers, or phone numbers, but not names. But at that point, we may get to a place where no further illuminating explanation of any sort is possible. It may just be that my ability to remember phone numbers is something about me which is a completely unanalyzable fact and about which we should be every bit as satisfied as with what we take to be the fundamental behavior of sub-atomic particles. It's an attitude that takes a little working on, but it's one which I want to push for at the moment.

MCCARTHY: This last point which you just raised, Steve, calls to mind something about the model that William Braud was talking about earlier, in terms of "What kind of a model is this"? In a sense, this is an attempt to take things at a certain level without seeking further mechanistic explanation. It's an attempt to say, let's not try to describe a mechanism whereby PK and ESP actually operate in detail, but, instead, let's try and look for some underlying principle that we can describe and let the explanation stop right there.

BRAUDE: That's your description of William's model, right?

MCCARTHY: Yes.

BRAUDE: Yes, to some extent I think that's true. There are some questions that I have about William's model that I'll probably get around to later. I'm not entirely sure that it's intended to stop quite where I would like to see it stop, since I think it makes some assumptions about similarity between labile systems and, at that point, I'd raise a number of questions. But, to the extent that the approach that William and Rex Stanford seemed to favor doesn't really try to describe actual mechanisms for psi interaction, to that extent I'm quite sympathetic to it.

TAETZSCH: Is it really practical for the scientist at any point to say, "This is a question without an answer? We shouldn't have a theory here and we should stop?"

BRAUDE: Well, scientists do say that all the time. It's just that they usually assume that the point at which it's OK to say that concerns phenomena in very small domains. What I'm suggesting is that the attitude which is so readily taken by scientists with respect to atomic or microscopic

phenomena ought to be every bit as easily taken with respect to at least some macroscopic phenomena. I mean, once we allow fundamental facts into the world, we just have to be very careful to decide what they're going to be. And, I would suggest that in some cases, they concern observable phenomena.

TAETZSCH: But suppose you're wrong? You stopped too soon.

BRAUDE: Well, there is always that possibility, except that when we come to the mechanistic analysis of cognitive phenomena, there's an enormous literature which shows that any attempt to analyze this further will rest on either false or absurd presuppositions.

DUNNE: I guess I have a problem with the purpose of the concept of modeling or theorizing that you're referring to. Maybe a question to be asked is, are we looking for models which will explain the phenomena or are we looking for models which will help us to make predictions or to design replicable experiments? One's theorizing, I think, should depend on what one expects to do with one's model and why. Perhaps you could speak on that for a moment.

BRAUDE: A model, of course, isn't a theory and models can be used in a variety of ways. I would argue, for example, that ordinary human communication is not a phenomenon that can be analyzed in terms of specifiable micro-structures. Nevertheless, certain aspects of human communication can be modeled mechanistically and we can consider certain mechanistic aspects of the processes of communication. We can even describe various regularities of human communication. What I would caution against is being blinded by the models into thinking that we're actually doing something more than we are doing. Suppose the model contains within it a germ of a theory, so that we come to believe that we're doing more than simply describing certain of a phenomenon's aspects and abstracting from the overall phenomenon. This is what concerns me, since although certain parts of human communication might be described mechanistically, it would be hasty to conclude that the entire process may be.

DUNNE: Perhaps I should clarify my question. What I was trying to say was, we should have a specific goal or purpose in mind as we develop these models. I think that unless we define ahead of time what we're trying to achieve, we're liable to go off in a lot of different directions and very likely end up where we started, or pretty close to it.

BRAUDE: If our models lead only to theories that rest on false or absurd presuppositions, then those are the ones I think we have to guard against. Other than that, it seems to me that anything goes in modeling.

MORRIS: To a certain extent we're dealing with a social component of science and scientists and how they go about doing science. Most scientists tend to lead toward reducing the phenomena they study to un-

derlying structures quite frequently and many people, as they go about the business of being scientists, set limits on themselves, such as we were advised in graduate school—if you're not making any headway after five years, clear out. Either there's no more headway to be made or you're not the person for the job. And I suspect that at any given moment, as we're trying to reduce any given phenomena, most of the folks who have done business in parapsychology have cleared out. And whether they will be back in business tomorrow perhaps, reducing things yet further into more basic facts we don't know. All we can say is, at the moment, it may seem quite sensible to say, as you have, that x looks like a basic fact and is not likely to be reduced further in the future. But you don't know what's going to happen tomorrow.

BRAUDE: I don't want to legislate a priori against the possibility of a phenomenon, but against the possibility of certain theories working out. The only way to get clear on those matters is just, I think, to examine very closely what the underlying assumptions or presuppositions of a particular theory are. If it turns out, for example, that mechanistic theories of cognitive functioning require intrinsic relations of representation between states of the brain and states of the world, no such theory can possibly work if it turns out that that form of intrinsic resemblance is nonsense.

MORRIS: At a place where there doesn't seem to be much headway being made, one piece of advice you would have would be to abandon the assumptions that people who were doing business in that area seem to be making. Maybe they're just not making some very good assumptions and that's what's standing in the way of doing business in that area.

BRAUDE: Right. But it may be that the very research models that they're following rest inevitably on those particular assumptions and, if so, then I would suggest abandoning the entire research project.

MORRIS: Yes, just as long as we understand the basis of inferring inevitability.

ULLMAN: Are you saying something analogous to what the theoretical physicist David Bohm seems to be trying to get at in his belief that physicists are not going to get much further in the analytic approach to smaller and smaller interactions and his movement towards what seems to be a more holistic approach in his concept of the implicate order which, in itself, is unknowable, but out of which observables arise?

BRAUDE: I can't really claim to understand his approach well enough to give you a firm answer on that. To the extent that I understood what you just said, I would say, yes, I'm suggesting something similar.

ULLMAN: It sounds very much like that.