CRITICISMS OF PARAPSYCHOLOGY: SOME COMMON ELEMENTS

JOHN PALMER

Although criticisms of parapsychology vary widely in their levels of sophistication, fair-mindedness and recourse to emotional rhetoric, certain common themes and strategies which cut across these dimensions appear frequently in the writings of external (and some internal) critics. The purpose of this paper is first to define and critically discuss these common strategies and second to suggest means by which they might be combated.

Before beginning, a few qualifications are in order. First, by "common" I do not mean "universal." Like parapsychologists, critics are individuals who conform in varying degrees to the pattern I will outline below. Second, I do not pretend to speak for the critics or to describe their views as they would describe them. For one thing, many of the strategies I claim to have discerned are based on premises that are only implicit in their writings and are not necessarily employed deliberately. Also, my orientation to the critics is generally a critical one. As this attitude no doubt will become evident as the paper proceeds, perhaps this is a good time to acknowledge the fact that, however exasperating the critics can be, they have rendered a valuable service by exposing legitimate weaknesses in particular experiments and stimulating advances in methodology and tightening of controls. Nonetheless, I operate from the premise, which I consider quite justified, that the primary objective of most critics is not constructive, but destructive, to discredit the case for psi to the maximum extent possible.

The Strategies

The following seven premises and strategies are highly interrelated and collectively define what might be called a meta-strategy for "debunking" parapsychology.

1. Parapsychologists as Claimants. At the end of *Psychology Today*'s recent tribute to the Committee for the Scientific Investigation of Claims of the Paranormal (Cornell, 1984, p. 34), Paul Kurtz is quoted as echoing the highly regarded statement that ". . . the burden of proof always rests with the claimant." This statement is indeed hard to dispute; in fact, it approaches a tautology. The much more interesting question is who is, or are, the claimants? The critics' answer, which is clearly implied by the context in which the above statement is customarily introduced, is "the parapsychologists." It is certainly true that many parapsychologists claim to have demonstrated something called "psi," by which they seem to mean some new principle of nature that transcends known physical laws. Thus, most parapsychologists would agree that they are claimants, but are they the *only* claimants? Critics constantly propose alternative explanations for ostensible psi effects (OPEs), but these are customarily not construed as claims requiring proof. Although it is true that critics often decline to be pinned down to any *particular* conventional explanation of OPEs, neither do parapsychologists customarily commit themselves to any *particular* paranormal explanation of them. Thus, the critic's assertion that *some* conventional explanation accounts for OPEs is isomorphic with the parapsychologist's assertion that *some* paranormal explanation accounts for them. Yet only the latter is construed by critics as a "knowledge claim" requiring proof. This implicit strategy strikes me as a simple misuse of language which cannot withstand close scrutiny.

2. *The Priority of the A Priori.* Although by avoiding acknowledgement of the role of claimant the critic diverts attention from the positive nature of his assertions, his position is not compromised if we insist that he accept this label. However, such acceptance does force the critic to explicitly confront the more important question of how he justifies his knowledge claims. Here it seems to me that the primary appeal has been to certain a priori conventions which have been widely adopted in orthodox science. This strategy is most explicit in the writings of Hansel (1980), but I find it implicit in the writings of all major critics with the possible exception of Blackmore (1983). In a nutshell, the position is that, in the absence of overwhelming evidence to the contrary, a priori conventions provide sufficient grounds for accepting a conventional explanation of OPEs. Indeed, it is primarily with respect to such a priori principles that paranormal explanations are construed as "extraordinary."

Exactly which a priori principle or principles are being appealed to? It is often assumed that the appeal is to the parsimony principle, the principle of simplicity. However, this is not quite accurate. Mario Bunge (1963), perhaps the most outspoken critic of parapsychology among major philosophers of science, has written a major treatise attacking the parsimony principle as a necessary attribute of good scientific theories, noting both the ambiguity of the concept and the fact that many of our most successful theories in science are quite complex. Indeed, the models we have in parapsychology are quite simple by comparison and this simplicity is symptomatic of their relative lack of explanatory power.

The principle at issue is rather what might be called the coherence principle, the principle that the laws of nature must be consistent with each other (the famed "unity of science") and with "well grounded metaphysical beliefs" (Newton-Smith, 1981, p. 229). Psi seems to violate this principle because it seems to be incompatible with these known laws and particularly with the meta-principles enunciated by C. D. Broad (1969).

The coherence principle, it should be emphasized, is a *convention* and it is usually defended on the grounds that it has held up in the past, i.e., that it has proven sufficient to account for the known phenomena of nature. However, as we have seen in the case of parapsychology, it is also used to define which phenomena of nature are allowable, or at least what hurdles they must overcome to achieve reality status. This can be seen to imply circular logic: conventional theory is appealed to in order to deny legitimacy to inconsistent data and the absence of legitimate inconsistent data is then used as an argument to support the validity and/or generality of conventional theory. I do not mean to suggest that the circularity is complete; conventional science does allow for the possibility that psi could be empirically established. Nonetheless, the circularity applies to the degree that higher standards are required for the acceptance of data inconsistent with conventional theory than for consistent data.

However, the main thing that distinguishes critics of parapsychology is not the appeal to the coherence principle per se, but the priority it is given over empirical confirmation of conventional hypotheses. With all due respect to rationalism, science should be first and foremost an empirical enterprise. A priori conventions should not enter the picture until the research process has been exhausted and the data still prove incapable of deciding the winner from among the competing theories. Bunge (1963, p. 97) makes the point even more strongly: "The wish to secure hypotheses by a priori and conventional criteria . . . is both antiscientific and at variance with any sound empiricism." Yet critics of parapsychology often write as if empirical confirmation of their proposed explanations of OPEs is a dispensable luxury.

This strategy has a subtle, but important implication, namely that we already have a scientifically acceptable explanation or set of explanations for OPEs. Again, the strategy does not rule out the possibility that a "better" paranormal explanation may someday be found; its power derives rather from the fact that is absolves science of the responsibility for addressing OPEs. Why should we pay attention to something we can explain when there are so many other things out there that we cannot? Perhaps more important, this implication serves as the basis for the increasingly popular "time is up" argument stated explicitly by Flew (1982). Parapsychologists have had ample time to prove their case, so the argument goes, and since they have failed to do so the case should be closed. But it only makes sense to close the case if OPEs *have* been satisfactorily explained by *someone*. Flew's argument only makes sense if one assumes that conventional science has already accomplished this and this assumption, in turn, only makes sense if one accepts the priority of a priori conventions over empirical confirmation.

3. Ideographic Methodology. Critics do sometimes adopt an empirical approach, but this approach is limited almost exclusively to retrospective attempts to "debunk" the claims of "psychics" or to verify conventional explanations of the results of specific psi experiments. These endeavors are clearly steps in the right direction and they sometimes have made important contributions. Probably the best and most successful example of this approach is the reanalysis of the Soal-Shackleton data by Scott (Scott and Haskell, 1974) and Markwick (1978). However, this ideographic or "case-study" approach has the important limitation that it does not allow legitimate generalization beyond the particular case exposed. By its very nature it cannot demolish the case for psi generally.

The latter objective requires a nomothetic or dimensional strategy which, through proper sampling methods and research design, allows conclusions to be generalized beyond the immediate sample tested. Such research is conspicuously lacking in the critics' program. The only modern examples I am familiar with are experiments by Alcock and Otis (1980) and Troscianko and Blackmore (1983) which tested predictions that "sheep" are more prone than "goats" to make cognitive errors that would lead to misattribution of psi to chance events. While potentially providing useful and relevant information, this approach is severely limited by the fact that it does not address OPEs directly. A better approach in this regard is that described by Schouten (1984) in his paper for this conference, which provides a good illustration of how nomothetic methods can be applied to testing conventional hypotheses of OPEs in the context of spontaneous case reports.

But can such a nomothetic approach be applied to laboratory psi? I think it can. Laboratory experiments are no different from spontaneous cases in that they represent reports by individuals who claim to have observed OPEs. Although the conditions of observation are generally much better in experiments than in spontaneous cases and the capacity to precisely assess the likelihood of the events occurring by chance much greater, advantages which I in no way wish to minimize, the fact remains that in the final analysis we are still dealing with the same basic kind of event. This means that a nomothetic approach is equally applicable. For example, a nomothetic approach could be used to increase our understanding of the factors that lead to faulty observation and technique in laboratory settings or even fraud. The methodology employed by Rosenthal (1966) to study experimenter bias effects could serve as a useful model. Once such factors have been uncovered, an analysis of the type employed by Schouten could be used to see to what extent such factors are applicable to psi research. Other approaches are also possible, such as testing the viability of various sensory cue hypotheses relevant to ESP experiments (e.g., Palmer, 1983c). Most important, conventional hypotheses can serve as the basis for testable predictions instead of merely being applied retrospectively.

4. "Black-White" Conclusions. Despite occasional flirtations with research, the critics' strategy, as noted previously, is primarily non-empiricist. To review, a conventional explanation of a given OPE is devised and declared preferable to any paranormal alternative on the basis of appeal to the a priori principle of coherence. However, many critics who employ this strategy customarily go farther than this. Not only is the alternative explanation considered preferable, but the fact of its existence is taken as a basis for declaring the experiment *worthless* as evidence for psi. Thus inconclusive experiments are equated with non-evidential experiments; e.g., one would be hard pressed to ever find a critic judge an experiment as "suggestive" of psi. This leap of logic is difficult for the parapsychologist to deal with because it is never made explicit, even by Hansel who has generally been more conscientious than his successors in putting his premises on the table. Yet it is an extremely important component of the meta-strategy of a great many critics; it is what allows them to dismiss the evidence for psi entirely. (One critic who stands out in his avoidance of this strategy is Hoebens.)

One symptom of this "black-white" approach to evidence is critics' almost total lack of concern with the plausibility of the conventional explanations they propose. All conventional explanations are treated as being equally plausible and thus equally fatal. Since it is always possible to dream up some alternative explanation for any psi experiment and since worthless experiments cannot reinforce each other (i.e., the "bundle of sticks" principle does not apply), the critics' judgment that there is no credible evidence for psi is virtually a foregone conclusion.

Finally, it should be stressed that I am not criticizing critics for proposing implausible counterhypotheses, but for treating them as fatal.

5. Allusions to Incompetence. Many critics devote a great deal of attention to commenting upon the competence of psi research and thus indirectly on the competence of the researchers who conduct it. Even the writings of Ray Hyman (e.g., 1984), one of the more sober and fair-minded of the critics, show this tendency. Sometimes the evaluation is sugar-coated by observing that the researcher "knows better" or does good research outside of parapsychology, but even this "praise" serves the function of making the point, at least implicitly, that the researcher's objectivity is being compromised by his "belief in the occult." Occasionally the competence of a parapsychologist is extolled—usually someone who gets negative results, attacks collegues who get positive results, or does something else the critic likes—but even here the objective seems to be to draw a contrast with the incompetence of some other parapsychologist. I do not mean to imply that most critiques dwell on the researcher's competence as an explicitly central theme (one exception here is Diaconis' (1978) critique of Kelly's research with Delmore); instead statements are usually added to an otherwise standard methodological critique which encourage the reader to view the specific flaws alleged or uncovered as indications of incompetence, which in turn suggests that the research should not be taken seriously even if the criticisms specified are implausible in themselves. In fact, this argument can serve as a convenient rebuttal if a parapsychologist protests that a given conventional explanation lacks plausibility.

It is possible that the use of this strategy is conditioned in part by the excessively ad hominem nature of the psi controversy on both sides. But whatever its impetus, it functions as an extremely effective weapon for undermining research conducted from a "pro-psi" perspective. I feel that parapsychologists have been somewhat slower than our critics in appreciating the importance of a researcher's overall scientific image and "respectability" in determining how his research will be received by other scientists. Funding agencies, for example, often give preference to a mediocre proposal from someone who has established a good reputation over an excellent proposal from someone less well known or with a less sound reputation. Thus, if a researcher can be labeled as incompetent, whether the label is justified or not, the potential reviewer is unlikely to take the trouble to examine a research report from that person carefully to determine its true merit; or he will simply assume that whatever merit exists in the report is not representative of the research in back of it.

The effect is even more devastating if a whole class of scientists (e.g., parapsychologists) can be stereotyped as incompetent. Such a stereotype of our field unfortunately exists, at least among psychologists, and I find it difficult to avoid the impression that many of the writings of our critics are designed intentionally to reinforce this stereotype.

The point I want to stress is that what makes this strategy effective is its capacity to "level," to encourage the audience to respond to the stereotype rather than to examine in depth the research at issue, including the research on which the stereotype is presumably based. If research or the researcher who conducts it is indeed incompetent, this should be evident either from the research reports themselves or from critiques that stick to the normal rules of scientific objectivity. Editorializing does nothing but increase the risk that the label will be applied unfairly.

6. The Magician's "Paradigm." A remarkably large number of external critics of parapsychology are either amateur or professional stage magicians and this background understandably has a major impact on the critical strategies they employ and the kinds of psi research to which they direct most of their attention. Magicians are very accustomed to seeing things that most people cannot explain (and perhaps that they cannot explain) and yet knowing that these things have conventional explanations, that they are "tricks." This experience helps to define a worldview, a paradigm if you will, into which OPEs can readily be fitted and through which they can be understood. It is clear that many critics view parapsychology through the spectacles of the "magician's paradigm." As usual, the award for transparency goes to Hansel (1980, p. 22) for his quote: "An ESP experiment can be analyzed in much the same way as one tries to discover how a conjurer performs his trick."

Of course, the magician's paradigm is by no means irrelevant to parapsychology. The history of our field up to the present day is strewn with the names of "psychics" who tried to fool and sometimes succeeded in fooling scientific investigators by means of magician's tricks. Thus, at least in the case of "gifted subjects," concern with subject fraud is understandable. Likewise, unscrupulous "psychics" have often been promoted by equally unscrupulous managers, which perhaps has contributed to the present-day suspicion of experimenters who seem to be promoting their star subjects. In any event, the issue of fraud has assumed a place of prominence in parapsychology that far exceeds its role in conventional science as well as what can be justified by the amount of fraud that actually has been uncovered in experimental psi research. It is this *obsession* with fraud and its tendency to subvert more traditional scientific approaches that can be seen as problematic. It is doubtful that the scientific study of OPEs is really advanced in the long run by continually asking did so-and-so cheat and by trying to work in the paranoid atmosphere thereby created. Yet such considerations customarily leave critics cold. At times their writings seem to suggest that psi is not an appropriate topic for scientific research at all, that the field would better be left to magicians than to scientists. On the other hand, major critics such as Alcock and Hyman have downplayed the fraud issue in their critiques and stressed more traditionally scientific forms of criticism. While their writings suggest that a more balanced perspective on the fraud issue may be in the offing, the impact of the magician's paradigm on the general attitudes of critics should not be underestimated.

7. *Ignoring Process-Oriented Research.* Particularly since the 1970s, a great deal of psi research has been "process-oriented" (Palmer, 1983b). Attempts to relate psi to "normal" psychological and physical variables have become commonplace and are often guided by low-level or even high-level theories of varying degrees of explicitness and sophistication. Hypothesis testing and followups of effects found by the same or other investigators are also common. Yet this research is almost completely ignored by the external critics, who focus their attention almost exclusively on the admittedly more colorful proof-oriented work. When such studies contain process-oriented elements, those elements are ignored.

Several plausible explanations can be proposed for this narrowness of focus. Since proof-oriented experiments with special subjects are more amenable to interpretation as magic tricks than are process-oriented experiments with unselected subjects, the focus might be partly explainable as a subtle influence of the magician's paradigm discussed above. A more fundamental reason, however, may simply be that the existence of psi is the only question of interest to external critics, who seem to feel that any attempt to understand the nature of psi and thus process-oriented research, is premature and useless until the existence of psi is established. Perhaps they even feel that to show interest in process-oriented research might be perceived by others as a tacit admission of the reality of psi. I have argued elsewhere (e.g., Palmer, 1983b) that process-oriented research need not presuppose the existence of psi and that whatever lawfulness it uncovers provides as good evidence for psi as that produced by proof-oriented investigations, but this view has not seemed to strike home with the critics. A final reason for critics' focusing on proof-oriented research might be that it is the only research which has been covered by the media.

Although there is a certain irony in the fact that a group of scientists are selectively uninterested in the very psi research that conforms most closely to conventional scientific practice, there is nothing particularly objectionable about their focus, except insofar as it provides their readers with a distorted perception of the character of the bulk of modern psi research and the kind of thinking that guides it. It does become objectionable, however, when the existence of process-oriented psi research is not only ignored, but denied and the denial is then cited to support the claim that psi research is pre-scientific or pseudo-scientific. The recent book by Alcock (1981) is a particularly flagrant example of this strategy. However, most critics simply ignore the process-oriented research and content themselves with vague references to a "lack" of theory or hypothesis testing in parapsychology.

Changing the Ground Rules

By using the above strategies, critics of parapsychology have succeeded in discrediting psi research in the eyes of the scientific community with a minimum of intellectual effort. In particular, they have been able to assign fatal status to criticisms of psi without needing to concern themselves with either the plausibility or the empirical grounding of the conventional alternative explanations in back of them. Indeed, the more bizarre and ungrounded the criticism is, the more "rigorous" and "scientific" the critic can claim to be.

How could this happen? It has happened because both the critics and many of the parapsychologists they criticize have implicitly agreed upon a set of ground rules for the psi controversy that allows it to happen. Parapsychologists must share a good deal of the responsibility and perhaps the primary responsibility, for creating these ground rules. It is parapsychologists who first insisted that psi can only be claimed when all conventional possibilities have been eliminated, thus insisting, in effect, that a perfect experiment is necessary for its demonstration however vehemently we may try to deny it. Then when we fail to achieve this lofty ideal in practice (and our most skilled and conscientious methodologists consistently fail to achieve it), we are trapped. If we literally follow our own standards we should throw the experiment in the trash can. If we argue that the flaw is trivial or unlikely to account for the result, *however plausible that argument may be,* it inevitably sounds like an apology for sloppy methodology, a vindication of the charges of incompetence and bias leveled against us. We really cannot blame the critics for exploiting our dilemma. We lie in a bed of our own making.

It goes without saying that we should constantly strive to improve the rigor of our methods and to adopt the recommendations of our critics that are sensible. However, I do not think that the way out of our dilemma is to continue trying to match wits with our critics in pursuit of the elusive goal of perfection. The fundamental problem is with the ground rules themselves, and we must insist that they be changed. However, changing them will require nothing less than a radical transformation of how we conceptualize our subject matter and how we define our research objectives. I will conclude this paper with a brief outline of the form I think this transformation should take. I should add that these ideas are still in the process of evolution and that I plan to publish more extensive and mature versions of them at a later time. There are also numerous individuals, both parapsychologists and critics, who have expressed similar concerns and have stimulated my own thinking. I promise to acknowledge these persons individually in the later versions.

The Concept of "Psi"

The main difficulty as I see it centers around the way we define our principal concept, psi. I have discussed the difficulties with this term elsewhere (Palmer, 1983a), but I would like to make the point here in a slightly different way. When we claim to have demonstrated psi in an experiment, we usually mean to say that we have demonstrated some new principle of nature transcending known physical laws. Yet if we are pinned down we are at a loss to describe what that principle is. We are in fact reduced to saying that we cannot explain what it is that we have observed. We do not call something "psi" when we understand it, but precisely when we do *not* understand it. The claim "this is psi" is not a claim of knowledge at all, but a claim of ignorance.

Neither can we claim that our observations require *some* kind of "paranormal" principle to explain them. The reason is that we have no way of knowing whether all potential conventional explanations have been considered. Frequently we have thought at a given point in time that all such explanations have been eliminated, only to learn later that they have not. Some conventional explanations may not even be known to normal science at a given point in time, as was the case with the explanation of navigation in bats.

In short, when we say that we have demonstrated psi, all that we can legitimately mean is that we have demonstrated an *anomaly*, nothing more and nothing less. We have explained nothing; we have demonstrated no new principle of nature—and this is true regardless of how "well controlled" the demonstration may be.

It follows from these considerations that psi should be redefined in such a way that it no longer implies a new principle of nature and is stripped entirely of its theoretical and/or metaphysical connotations. Specifically, I propose that an observation be labeled "psi" if, and only if, the following conditions are met:

(1) The observation when taken at face value suggests an interaction between a living organism and its environment that transcends the basic limiting principles of Broad (1969) or meets the revised definition ("D6") of "paranormal" proposed by Braude (1979, p. 260).

(2) No conventional or paranormal explanation of the observation, or the class of observations to which it belongs, has achieved adequate empirical validation by normal scientific standards.

Note that according to this definition it is just as meaningful to speak of psi as ultimately having a conventional explanation as a paranormal one. Moreover, it makes no difference how "evidential" the observation is in terms of the old framework. The only difference between psi observed under poorly controlled conditions and psi observed under well controlled conditions is that the latter is apt to be more scientifically *interesting* to a thoughtful scientist. For example, whereas such a scientist might pass off marvels at a spoon-bending party as misinterpretation of physically produced effects in a climate of heightened suggestibility, in the case of biased outputs of a Schmidt REG in a laboratory setting he might not be so sure. In other words, the one legitimate function which so-called proof-oriented experiments in parapsychology can serve is to make psi anomalies more interesting to other scientists (and perhaps to the investigator himself). But they tell us nothing about the nature of the anomalies, including whether or not they are "paranormal."

At this point an important qualification about the use of the term "interesting" must be introduced. Events in themselves are neither interesting or uninteresting. The term "interesting" implies a relationship between an event and the cognitive activity of the person addressing it. The judgment of an event as "interesting," even "scientifically interesting," is both subjective and individual, conditioned by the personal a priori probability one attaches to the inviolability of our currently accepted natural laws, one's degree of involvement with a research problem for which psi is irrelevant and many other things. Thus psi anomalies observed under well controlled conditions can only be considered more interesting than psi anomalies observed under poorly controlled conditions in a sociological or actuarial sense; i.e., a greater percentage of thoughtful scientists are likely to find the former anomalies scientifically interesting, however high or low that percentage may be. This semantic principle implies that both parapsychologists and their critics should refrain from making objective-sounding pronouncements about how "interested" other scientists should or should not be in given psi anomalies and stick to more truly objective discussions of the various possible explanations of these anomalies insofar as they can be identified and articulated.

The Concept of "Omega"

Of course, the ambitions of parapsychologists extend beyond making an anomaly interesting to other scientists. Most of us, I think, would like to show that there really does exist some revolutionary new principle of nature that accounts for them. To achieve this objective, however, it is necessary to scientifically verify a theory or model (such as, for example, the observational theories) which contains this new principle as a premise. I propose the Greek letter omega (Ω) as a generic term to symbolize any such new principle proposed by any such theory or model. It is important to recognize, that unlike psi in either the old or the new framework, omega is inextricably theory bound; the term makes no sense outside the context of some theory or model.

Thus, according to the new framework the ultimate objective of parapsychology is not to demonstrate psi, but to verify omega, not to document an anomaly, but to confirm a theory. For this reason, the new framework gives priority to so-called process-oriented research which is either guided by theory or seeks to uncover correlates of psi from which theory can emerge.

The implications for research in parapsychology of this new framework are far-reaching and can be discussed here only briefly. Perhaps the most important implication is the following: whereas in the old framework psi is demonstrated negatively by attempting to eliminate all conventional alternatives, in the new framework omega is demonstrated positively by confirming a theory or model in which it is embedded. If the theory is then to be overthrown, the critic must demonstrate empirically that some conventional theory has the same predictive or explanatory power in the relevant domain. Only at this point, if he succeeds, is he entitled to invoke the a priori coherence principle, since the empirical work has now been done. The parapsychologist must then go back to the drawing board and try to either develop a new theory or modify the old one so that its power again exceeds that of the best conventional alternative. It is perhaps evident by this time that although my proposal is radical from the standpoint of parapsychology, it is conservative from the standpoint of science generally.

Implications for the Psi Controversy

Finally, I would like to briefly discuss some of the broader implications of the new framework for the psi controversy and for the relationship between parapsychologists and critics. Perhaps the most obvious implication is that stripping psi of its theoretical and metaphysical connotations takes a great deal of the steam out of the controversy. I think many critics would be able to live with the existence of psi as I have defined it. Indeed, some whose interest in the field was ignited by a dread of the metaphysical connotations of psi might decide to retire to the sidelines, at least until such time as parapsychologists are prepared to claim verification of omega. In any event, the new framework promises to remove an obstacle to more constructive interaction between the two sides and promote acceptance of the field by conventional science.

This "demystification" of psi is really not so great a concession as might appear on the surface. In reality, it is nothing but an explicit affirmation of what most of us say implicitly all the time, at least in our more sober moments when we confess that we have no idea what it is we are dealing with. Moreover, since the new framework continues to stoutly defend the status of psi as a genuine anomaly, the quest for the real prize, omega, remains as viable and legitimate as ever, perhaps even more so.

What I see as a major benefit of the new framework is that it forces critics to assume their fair share of the responsibility for providing clearly articulated, empirically validated conventional explanations of psi anomalies, rather than being content with merely explaining them away. For instance, if a critic operating within the new framework wished to "win" the psi controversy by proving that "psi does not exist," the burden would be on *him* to demonstrate that a conventional explanation is available which meets the necessary requirements of scientific validation, something I submit that critics are nowhere near to achieving insofar as the critical mass of the anomalies is concerned.

The importance of getting critics out of their armchairs and into the laboratory cannot be overstated. We have allowed ourselves to be put into an intolerable situation in which the representatives of one theoretical orientation (ours) are labeled as "proponents" and do all the research, while the representatives of the other theoretical orientation are labeled "critics" and do nothing but criticize that research. This situation literally guarantees that we are constantly on the defensive and constitutes a major reason why the critics have been successful at tagging even our better research as incompetent. The new framework requires a balanced system in which each side does research and criticizes the research of the other. This means, for example, that we would finally have the opportunity to criticize the research of the "critics" using the same kinds of standards which they apply to our research. In practice, I believe that this would quickly cause them to abandon their "anything goes" approach in favor of more realistic standards that allow for empirical progress while maintaining the necessary level of rigor. Moreover, the quality of our research would be judged in the light of the quality of their research, rather than exclusively with reference to platonic ideals, as is presently the case. Finally, nothing in the new framework either prevents or discourages parapsychologists from "crossing over" and testing conventional hypotheses ourselves, as suggested by Schouten (1984). Likewise, we should not be willing to unequivocally accept evidence for conventional hypotheses provided by "critics" until we have replicated such effects ourselves in experiments agreed by both sides to have been adequately designed and executed.

If the conventional viewpoint ultimately prevails, we will at least have the satisfaction of knowing that we provided the stimulus for normal science to finally address psi anomalies. If, on the other hand it fails, and the progress we have made so far in parapsychology gives us good reason to believe that it will fail, the legitimacy of the quest for omega will be enhanced and the resources put as its disposal will increase. A great opportunity lies ahead of us. To place that opportunity in jeopardy by attempting prematurely to assert the confirmation of a principle that we cannot even define is futile, suicidal and an abdication of the responsibility passed on to us by those who founded our field—which includes the responsibility to change course when the times demand it.

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DISCUSSION

BENOR: I applaud your suggestions to take a more positive approach, because certainly we have been in a very self-defeating position, as you pointed out. Trying to establish a research project on healing in a hospital has given me some practical experience in having people who are very uncomfortable with an aspect of psi come to accept it, or tolerate it at least, within a hospital setting. Yet, although it was tolerated there were enough people who were uncomfortable with it, that the project could not be completed because of all the resistance. Even if we view psi more positively, people who are not comfortable with it are going to find all the reasons not to consider it. Certainly they will not accept it. Even though we could live with just a conceptual model that we are testing, setting aside all implications until psi is properly demonstrated, the average person makes the extrapolation that if psi exists then something is wrong with my view of the world and either I am crazy or you are crazy. And I would rather believe that you are crazy. My expectation is that no maneuvers on our part are going to sway people away from a discomfort with it, other than practical applications where the average person sees a way to use psi and to benefit from it.

PALMER: I certainly see the problems and I know there is no ironclad guarantee that taking this approach is going to have any practical effect. Perhaps I am being too optimistic. If my approach is going to have an effect, it would be most likely to do so with other scientists. People with an interest in more theoretical matters, might have the training to appreciate these kinds of distinctions. I think it would be least likely to have an effect on the lay public, since they get most of their information from the popular media.

BENOR: My experience with physicians who are well grounded in double-blind studies is that they will not begin to consider this kind of material. They will assume that there has to be something wrong in it somewhere. I was faced with physicians who, when I presented the evidence, said in effect: "We can not flaw the studies, we can not flaw your presentation, but something has to be wrong somewhere, therefore we will not touch it, because it contradicts logic as we know it."

PALMER: Even if you allow there might be a logical explanation?

BENOR: Yes.

PALMER: Well, there's not much you can do with that, I agree.

MORRIS: What distinctions are you drawing between talking about the critics versus criticism per se? On the one hand, I feel that focusing on the critics certainly validly reflects at least a partial social reality. Yet on the other hand, it reinforces the notion of a strong, sharp dichotomy here, us versus them, which I think is partially true, but also artificially contributes to the problem.

PALMER: I don't recall how many times I used "criticism" versus "critics." I used "criticism" in the title; I may have used "critics" in other places. I intended the focus of the paper to be on the criticisms, although I felt at the time it was necessary to identify them with certain kinds of people. I would like to see a field in which we did not have separate camps, but it seems to me that those camps exist. Most people who are involved in this field have a pretty clear bias about the way they would like to see things turn out. Some control that bias better than others. I would much rather not see this conducted in an advocacy framework, but rather one in which people could just go in without preconceptions and let the chips fall where they may. I know that you are one of the people who epitomizes that ideal, perhaps as well as, if not better than anybody in the field. But my overall impression of what is happening in parapsychology leads me to conclude that the reality we are faced with is in general different, and that reality must be reflected in our thinking, like it or not.

ROLL: Thanks for a very interesting paper, John. You mentioned Broad and his basic limiting principles. I always had difficulty with those. I never saw that our findings really were incompatible with the known laws of nature. Broad was such a distinguished philosopher and such a fine thinker that he talked us into believing that there is a basic incompatability. In fact, the basic philosophy of many of the members of the Society for Psychical Research really was an insistence upon our defining ourselves in terms that just did not fit with the contemporary scientific picture of the world. Certainly Broad was consistent with that view, but it is a view that just turns out to be mistaken. Psi phenomena fit in quite well with our knowledge from the other sciences. We need to see these things as normal, as natural. There is no need to define our field negatively and to remain in scientific limbo.

PALMER: This relates to another problem which I am really struggling with: how do we define an anomaly? That is something that I hope the anomalistic societies will start dealing with directly. As for Broad, perhaps the social reality of the basic limiting principles is more important than their reality in terms of, say, quantum mechanics. The important thing is that our phenomena are *perceived* as anomalous by the majority of other scientists, including most physicists.

MISHLOVE: I would like to commend you on your paper also, John. I think it is extremely important that we seriously address the dialogue with the critics and devise strategies by which we can turn it from something which has been negative and destructive for our field to something which can really be a positive impetus for us. I think that you have taken some important steps in that direction. I especially like your proposal of the term "omega." I think that it is a very good contribution that can really help to clarify our thinking. But if we are going to look at the kind of semantic changes that are necessary to make the dialogue more constructive, I would propose some revisions to what you have suggested. I do not think we can just arbitrarily strip psi of all of the theoretical baggage which is attached to it and redefine it as you have proposed to mean a pure anomaly. I would like to suggest that we find another word, maybe "alpha" or some other neutral term that would represent a pure anomaly devoid of any theoretical baggage whatsoever. Psi has an etymological meaning. It is derived from the word "psyche." Psi has a specific meaning in physics which has been related to parapsychology by physicists referring to the probability waves that underlie the physical universe. Many of the quantum physicists have pointed out the striking synchronicity there. When I use psi

I am always thinking of those dual meanings. I am a little confused about your use of the term "paranormal" in the context of a paranormal explanation. I do not know what it means for an explanation to be paranormal. I think Charley Tart once suggested that we think of explanations as being paraconceptual rather than paranormal. Maybe if we dropped that word it would help us. As Daniel Benor pointed out, when you use a term like paranormal it is so self-contradictory that people cannot get beyond that to see the logic.

PALMER: A lot of our explanations in parapsychology, the ones we might want to use to embody paranormal are non-mechanistic. They are not the billiard ball type of things, but rather teleological. But what, exactly, does a teleological explanation mean? I have been trying to get a little bit into this in philosophy of science and find it rather tricky. I agree it is very important to define what we mean by a paranormal explanation and what exactly are the limits of that, but the issue is complicated and I am not prepared to go into it in detail here.

MISHLOVE: I would also like to add that teleological explanations are considered very normal within systems theory.

PALMER: Regarding your first point, one of the reasons why I want to retain the term "psi" is ironically that I want to get rid of the term the way we are using it now. I think it has been a major source of problems, particularly because it is used simultaneously to refer to what we are trying to explain and how we are trying to explain it. I think it is a very serious problem that has led to a lot of unnecessary confusion and conflict. Thus, part of my reason for wanting to retain "psi" is to redefine "psi" so that it does not linger on with the old definition.

GIESLER: I am a bit confused, John. Let me, just for my own thinking, review the idea of psi with constructs and without constructs. Is that viable? If I were to follow what you said or take that on as my research Weltanschauung or perspective, then I could do research with constructs—I could cross the boundary, test hypotheses and so on. Or I could take a look at anomalies, look at some of these deviations from mean chance expectation in experimental research *as an anomaly,* rather than as "psi" and somehow study that without constructs. Did you mean to say that?

PALMER: If I understand what you are saying, yes, at least in so far as you are talking about "psi" as the construct.

GIESLER: The big advantage of distinguishing these two is that the critics will not have targets for their criticisms which would be really the constructs rather than the actual fact of something you have found.

PALMER: Right. Let me amplify that. I think the term "psi hypothesis" is an absolute disaster. I do not see how you can have a hypothesis with positive content, it just sets up a straw man. I am trying to get rid of that kind of thing.

GIESLER: In one camp, then, you said approach this without constructs and try to do research.

PALMER: What I am suggesting is that we treat psi in a somewhat more operational sense as psi scores. In other words, it is quite appropriate to say that psi correlates with extraversion, while leaving open the possibility that there are many potential reasons why that might be the case, not all of which need assume that "psi" is paranormal.

GIESLER: I guess the difficulty is this. I just foresee someone trying to design a laboratory experiment to look at the extraversion issue. It would necessarily be construct laden. I don't know how you could really get around that. We have to first observe and then go on developing and modifying constructs, rather than the other way around. We have all been pushing for that and I totally agree. Also, I do believe that the constructs are the targets for the skeptics, so I am right there with you. But you know, if you look at these concepts, they have come from the spontaneous cases and the beliefs about spontaneous cases. These are the foundations for the constructs. When J. B. Rhine came up with the cards and the telepathy set-up, the experimental designs were founded on constructs, interpretations of the spontaneous cases. We have gone on from there with those same constructs disguised in the Rhinean methodology for "demonstrating psi." I wonder whether we would get any of this "psi" without the constructs? I totally agree, let us get away from the constructs, look at the anomaly. But I am wondering what role these constructs have played in producing the very parapsychological approach that you essentially suggest we employ "without constructs?"

PALMER: I think what you are addressing in fact is something I really had not thought of adequately—the whole role of the theory-boundness of observational terms. There is a lot of talk about this in the philosophy of science. There are inevitably implicit theories involved in what I would want to call psi. I guess what I am trying to say is we should accept those, but stop there. What I am objecting to is our going beyond that.

HEARNE: It is indeed difficult to deal with the unreasoning attitude of critics. Horn, a sleep researcher in England, recently said on the air: "Well, there has been some research into lucid dreams. It has been done by parapsychologists." This is the sort of thing that happens and no doubt the traditional academics nodded furiously at such a statement. In your paper you say we cannot gain acceptance until we have replicated experiments agreed by both sides to have been adequately designed and executed. In practice, do you really see a critic sitting down with a parapsychologist to design an experiment? It is such an unreasonable bias. In no way do they want to be linked with parapsychology.

PALMER: In a way, this gets back to what Dan Benor was saying. I would like to put the critics in the position of either doing it or refusing to. Then we could further expose that attitude. I think that there are some critics who are more reasonable and indeed would sit down with us. I do not think that they are all as unreasonable as perhaps is coming out of this discussion, but I feel the reasonable ones are a minority. In any case, I am not really worried about them. I am more worried about the type you are talking about.

WEINER: I like your idea of separating the concept of psi from the explanatory principle. In order to put your idea into practice, we somehow have to avoid the reification of the concept of psi into an explanatory principle. That is what brought out the problem in the first place. It seems as though reification is something human beings do very easily. I am wondering if you have any practical advice as to how we can actually keep the psi/omega distinction in our mind as we continue our work?

PALMER: Well, there is a tremendous force of inertia. You know, you get an idea and it dies. I am going to try to push these themes to see if I can do something about that. I think simply having another term, "omega," available that you can use when you are talking about psi in a more theoretical sense is important. And also trying to encourage people, if they say they are going to theorize, to actually theorize. Before you do that, when you are just trying to find correlates, for example, then I think you should use the term "psi."

SCHOUTEN: I would like to go back to Giesler's problem. I think he really touches the heart of the method, to a certain extent, but I would address it in a different way. The field exists due to the fact that throughout the ages spontaneous paranormal experiences have been reported. Now, that in itself does not yield a concept, but it yields a problem. And my feeling is that the moment we say: "Look here, there is a problem. These people report these experiences," there are many ways you can think about it. There are conservative ways, traditional psychological explanations, but there are many examples to cite that there is also the possibility that something else is involved. I think the problem at present exists because something else from the beginning on has a very strong impact. That is, that based on this problem a concept was construed (ESP) which was attributed omnipotence.

GIESLER: You mean the position of constructs?

SCHOUTEN: Yes, it has been construed in such a way, in a very specific way, I think. That is why most critics really are upset. If there is a problem and somebody comes up and says: "Well, you know, it can be explained so and so," by introducing an unknown process with unlikely properties and then tries to convince us that the problem no longer exists, we would resent that. That is exactly the same as critics feel about a concept such as we applied and so I agree with that. You can leave the possibility open that you do not know, that there is a problem and there are many explanations and various concepts possible and among them perhaps there is an omnipotent factor like psi. But nevertheless as long as we stress that we are entitled to do research into psi and we are allowed to test out several possibilities and concepts, then I think there is not so much of a problem. Of course, there are psychological factors. You know how it is in science. A few people are looking into an area of research and all the colleagues accept more or less what these few people are saying, as long as you consider them as colleagues. Now, from a social psychological point of view we are not colleagues from experimental psychology as a group. That is what I think caused the problem and that is the thing which you will not overcome by introducing any new words whatsoever. That, I think, will probably slowly disappear at the moment that the critics begin to understand that they also have misconceptions about what we think psi is. Because those critics have very specific ideas about psi themselves which do not fit at all with our ideas. Take a critic such as the late Piet Hoebens. He found out that we are not idiots with funny ideas. Even for critics it is very difficult to maintain this attitude the moment they are confronted with the fact that their stereotype is not true.

PALMER: I think basically you are saying what I am saying. I am willing to say that psi is a generic label for what you referred to as the problem. All this omnipotence business would be shoved onto "omega." When "psi" is what we are claiming, we are simply saying that a problem exists that should be studied, so I think we are really saying the same thing there.

BENOR: As I am listening here, I am hearing a right and a left brain problem. I think we are addressing in your suggestions a left brain approach to a right brain problem. In doing healing I find that I am in conflict because my left brain's logical linear thinking is that I want to understand this, I want to follow these steps so that I can achieve thus and thus results. I have some ideas when I approach a person with a physical or an emotional problem what direction I am headed in. When I do the healing, I find that I have to let go of that left brain approach and just be; I cannot really push what has to happen. The way I do the healing, it happens. Sometimes the results are in a totally different area from what I would expect, not even addressing the problem that was brought to me. I think that in approaching healing I find myself in conflict, though, because my right brain, my global gestalt of how things ought to be, gets goosebumps when I encounter something which goes counter to everything that I experience in my everyday life. A physical change that occurs within a matter of minutes or days, that all my linear, left brained medical training says ought not to happen, goes against my expectations and there is a part of me that gets indigestion with it. I have been studying this for a long time and I still have this gut reaction. I cannot fault others who have very little exposure to it and are very uncomfortable with it. They choose to deal with the discomfort by pushing it away, denying it, defending against it in other ways. I do not think a logical approach is going to work in most cases, although I have had the same kind of reaction. There are a few honest people who are not defensive and who are willing to change their beliefs. In general though, people are very lazy. They do not want to examine their beliefs, much less to change them. I think if we are going to spend a lot of effort in trying to convince other people, we are going to be wasting our time.

PALMER: I am not trying to suggest that my semantic approach is any kind of panacea. I think a lot depends on the way in which we approach other scientists, as Sybo suggested. Perhaps they need to be approached in a somewhat more right brain kind of way, but would that even be successful? I am not sure whether you are saying that it is just hopeless or whether there are perhaps other kinds of approaches that might be better. I do not want to put all this on a pedestal, but I do think that if we can become clearer in our use of our own concepts, we can avoid some of the unnecessary exacerbation and maybe help things a bit.

BENOR: That is like saying the fault is not in our concepts but in ourselves, that we, too, suffer from this discomfort. I think we find it hard to be positive in doing our work as you point out. Your suggestions for ourselves, I think, are much more important than for others.

PALMER: The only point I would make is that in addition to whatever effect my suggestions might have on the critic—that is only one of the reasons I am proposing this—I think they also can help in the way we conduct our research and conceptualize our subject matter.