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PROGRESSIVE SKEPTICISM: A CRITICAL APPROACH TO THE PSI CONTROVERSY

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ABSTRACT: Many skeptics of parapsychology adopt an uncritical attitude toward conventional explanations for ostensible psychic events (OPEs). This negates the use of the term *skeptic*, which should imply an attitude of critical doubt for *all* unproven explanations of OPEs. A nonpejorative label for this group of critics would be the term *conventional theorists* (CTs), which does not assume an unbiased attitude. The author gives examples from the literature of the uncritical bias of this group of critics, explains the rationale of applying the term conventional theorist to them, and points out why their approach cannot resolve the psi controversy. The author therefore calls for a new direction among investigators in this area. This new direction would be toward criticizing *all* the current explanations of OPEs, recognizing that none are satisfactory, and maintaining faith in the ability of the scientific method to eventually provide the correct ones. This would require research from both paranormal and conventional perspectives. He proposes that this new approach be called *progressive skepticism*.

Whereas bias on the part of many "believers" in psychic phenomena has long been recognized, such biases also exist among many "skeptics." I would like to begin this talk with two examples of a particular aspect of this bias, namely, an uncritical attitude toward conventional explanations of ostensible psychic events (or OPEs, for short). My first example comes from the book *Anomalistic Psychology* by Leonard Zusne and Warren Jones (1982). It concerns their critique of an experiment designed to test precognition in dreams by determining whether a subject could incorporate into his dreams events that would occur to him the following morning. However, because a secondary objective was to study incorporation of such events into *subsequent* dreams, dream reports were also collected the nights *after* the events. Thus odd-numbered nights were to test for precognitive (psychic) dreams and even-numbered nights for retrocognitive (normal) dreams.

This is not the way the study was rendered by Zusne and Jones. They reported the study as if all nights used the retrocognition pro-

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cedure but were intended to test for precognition; that is, the subject was asked to dream precognitively about targets he already knew! Obviously, no investigator in his right mind would test for precognition by showing the subject the target first. Yet that is exactly what Zusne and Jones claimed.

I think one can make a reasonable inference as to what happened here. Zusne and Jones's source for the experiment was a brief chapter by the experimenters in a popular paperback book (Ullman & Krippner, 1978). The chapter was equivalent to a short abstract and lacked descriptive detail. Apparently, Zusne and Jones simply misinterpreted the brief description of procedure in a manner consistent with their own prejudices and the general theme of their book—namely, that psychic experiences are due to biased interpretations of natural events.

What is remarkable about this example is that Zusne and Jones were willing to go into print attributing this positively idiotic procedure to the researchers based solely on a slim abstract. There are no indications that they ever questioned the accuracy of their own interpretation. True, the experimenters (or is it the editor?) should be faulted slightly for not citing the full experimental report when describing the procedure (although it did appear in the reference list), but this in no way exonerates Zusne and Jones. What we have here is a classic example of an uncritical attitude toward a conventional interpretation of an OPE.

My second example is Persi Diaconis's critique (1978, 1979, 1980) of an ESP card-guessing series that had as the subject a person who claimed to be psychic. Diaconis, a Stanford statistician and accomplished performing magician, could find nothing wrong with the experimental procedures as described in the detailed research reports, nor did he offer any suggestion as to how the significant results could have been achieved fraudulently. Nonetheless, he concluded without any apparent shadow of doubt that the subject achieved his significant scores by sleight-of-hand.[1]

Diaconis based his judgment primarily on an informal demonstration of ostensible psychic powers by the subject, which he witnessed and which he attributed to sleight-of-hand. I happen to think that his verdict in *this* instance was reasonable. But to draw a positive conclusion about the controlled experimental work on the basis of this kind of circumstantial evidence, especially when the kinds of

[1]Although Diaconis did not say in so many words that the subject used sleight-of-hand, it is a fair inference from his remarks that this is what he meant.

"tricks" ostensibly used in the demonstration were controlled against in the experiments, is unwarranted. Moreover, the degree of psychic power ostensibly demonstrated in the experiments, while impressive by parapsychological research standards, would not have been at all impressive in a theatrical setting. Thus, even if the subject had psychic powers, it is understandable why he might not choose to rely on them in that type of setting. If this consideration ever occurred to Diaconis, there is no evidence of it in his critique.

One of course cannot condone the subject's use of legerdemain, if it in fact took place, but that is not the issue here. Neither is it my purpose to show that Diaconis was necessarily wrong; he could still be proven right, and he has raised a legitimate *concern*. My purpose is, rather, to point out that Diaconis's apparently supreme confidence in the correctness of his conclusion about the experimental outcomes, which he based on circumstantial evidence from another setting (supplemented by unfavorable impressions he had acquired of some *other* researchers), betrays the same uncritical attitude toward conventional explanations of OPEs that Zusne and Jones exhibited.

From Skeptic to Conventional Theorist

I chose the two preceding examples partly because Zusne, Jones, and Diaconis are widely regarded as being among the more moderate critics of parapsychology. The uncritical attitude exhibited in these examples is extremely widespread among those who call themselves skeptics in this area. Thus, it seems to me that *skeptic* is an inappropriate label for this group. I agree with Paul Kurtz (1984) that the term *skeptic* should not be restricted to those who, like Paul Feyerabend, contend that the scientific method cannot lead to even approximate truth. But I hope he would agree with me that to go to the other extreme and suggest that a skeptical person need only be skeptical about hypotheses he or she dislikes for some a priori reason is equally indefensible. Though I cannot stop these people from applying this label to themselves, I do not have to join them in what I consider to be a misleading use of language. Marcello Truzzi (in press) has also criticized the inappropriate use of the term skeptic on grounds similar to my own.

As a nonpejorative substitute for *skeptic*, I propose the term *conventional theorist* (or CT, for short). I choose this term because the one positive thing erstwhile skeptics seem to share is a commitment to the view that conventional scientific theory provides adequate explanations for OPEs. I hope that others will join me in using it, or at least be more circumspect in their use of the term skeptic.

I do not wish to leave the impression that all erstwhile skeptics are CTs. Some critics of parapsychology, especially many of those who identify themselves with the field to some degree, are demonstrably capable of consistently adopting a critical attitude toward conventional explanations of OPEs while denying that the existence of psi has been established. For this group, the label *skeptic* remains appropriate. Unfortunately, they constitute a small minority.

I fear that some of you may find all this concern with labels a bit sophomoric, and I sympathize. The problem is that, regrettably, the psi controversy has a considerable rhetorical element to it that infects even its more intellectual levels. An important component of this rhetorical battle is the use of labels that have a positive valence for one's own group and labels that have a negative valence for the opposition. The term skeptic has positive valence in a scientific context because of its appeal to the important scientific principle of critical doubt. Although the appropriate use of labels can have a beneficial effect on scientific discourse, their misuse can give an unfair advantage to the guilty side, particularly in communicating with outsiders. Thus I feel I have good reason to be concerned about CTs who call themselves skeptics and thereby mask their lack of a uniformly critical attitude.

The CT's lack of skepticism does not necessarily imply incompetence or generalized loose thinking. Ironically, this uncritical attitude has a basis in critical philosophy, and it is to these more abstract issues that I now wish to turn.

The Burden of Proof

The root of the problem, it seems to me, lies in the premise—ironically shared (at least implicitly) by both the CTs and many parapsychologists—that the burden of proof falls exclusively on the claimant and that the only claimants are the parapsychologists. The parapsychologist must prove that psi exists; the CT does not need to prove that psi does not exist.

On the surface this position seems plausible, for several reasons. It is difficult if not impossible to prove a negative proposition, especially in the case of OPEs where the potential domain of their occurrence is so vast that the poor CT would have to debunk every case that comes down the pike to prove the nonexistence of psi. Moreover, why should CTs be asked to prove the nonexistence of something just because someone suggests it as a possibility? We don't ask them to prove that fairies do not exist, so why psi?

There is another way to deal with the burden-of-proof question, however, and that is to propose that the question "Does psi exist?" is the wrong question, or at least a wrong phrasing of the question. It is certainly odd, if not downright inappropriate, to apply the verb *to exist* to a hypothetical construct such as psi, and this reification is implicit in the line of argument summarized in the preceding paragraph. But I think I can attack the traditional question most effectively by proposing and defending what I consider to be a better question.

Before I do that, however, I would like to digress a moment and direct a little fire at my own field of parapsychology. It turns out that many of the formal definitions of our field are patently invalid. For instance, in Michael Thalbourne's glossary (1984), parapsychology is defined as "the scientific study of paranormal phenomena" (p. 51). Taken literally, this definition implies that we have concluded that a given phenomenon is paranormal *before* studying it. In fact, in most cases the investigation is undertaken to determine if the phenomenon is in fact paranormal. Thus, the definition should read, "Parapsychology is the scientific study of certain *ostensible* or *potential* paranormal phenomena."

I doubt that many parapsychologists would defend the traditional definition if the above point were brought to their attention. The problem is that this definitional inaccuracy has gone so long unnoticed. I think this unawareness is symptomatic of a deeper and more serious problem, the ramifications of which I can only touch upon in this talk. The problem is that we make no clear distinction in parapsychology between the phenomena under investigation and the principles proposed to account for them. We use the same terms (e.g., *psi*, *ESP*, *PK*) for both.

We may also have confused some CTs owing to this use of language. Some of them seem to feel that it follows from the conclusion that psi (i.e., paranormality) has not been established that there is no subject matter in parapsychology. But a moment's reflection can reveal that such an inference is incorrect. Surveys have shown that *ostensible* psychic events have been reported by over half the American population (e.g., Palmer, 1979). Parapsychologists have carefully documented thousands of these spontaneous cases. Moreover, hundreds of documented reports testify that such events occur in the controlled setting of the laboratory. Even if one exercises the behaviorist option and equates the phenomena themselves with the reports of the phenomena, those reports still constitute a subject matter, a database in need of explanation.

By appreciating this point, one can see that the better alternative to the question "Does psi exist?" is "How can OPEs be best explained?" Although I cannot offer a neat deductive argument, I think it is our failure to appreciate the distinction between psi as an anomaly (or a report of an anomaly) and psi as a paranormal process that has prevented us from formulating our fundamental research question in this more constructive and scientifically typical manner.

The new question invites us to seek a real *understanding* of the subject matter in a way that the old question—"Does psi exist?"—simply does not. However, its most important implication for present purposes is that it places the burden of proof on *anyone* who proposes to explain the anomalous reports. Strictly speaking, it is improper to speak of just one explanation, as inevitably no single explanation will account for the entire database. The range of possible explanations varies from pure fabrication at one extreme to some paranormally mediated process at the other. The point is that any of these explanations must be backed up by sound empirical evidence if it is to be considered acceptable. The situation is no different than it is for any other topic in psychology, and all the problems we have in psychology about how to define good evidence, how far we can generalize research findings, and so forth, tag along for the ride.

The Coherence Principle

But we are still left with too many CTs repudiating their share of the burden of proof. How can their recalcitrance be interpreted in light of the new question? Their recalcitrance seems to imply that, given the present state of the evidence, we are entitled to conclude that all the anomalous reports can be adequately explained by some conventional process, even if it cannot be specified in every case what that process is. But how do the CTs justify *this* conclusion?

The answer, it seems to me, is that they rely excessively on a priori conventions acting as adjudicators of scientific knowledge claims. The particular convention at issue is that explanations of

OPEs be consistent with the currently accepted scientific explanations of other natural events. This is usually claimed to be the parsimony principle, but I think a more representative and appropriate candidate is the coherence or unity principle. To the extent that it applies, it insures that paranormal explanations are automatically excluded.

Everything else being equal, we all would prefer a unified science to a disjointed one. It is a goal worth striving for. But premature acceptance of a unity may actually postpone the emergence of a more satisfactory unity later. At one time scientists were convinced for what seemed perfectly legitimate reasons that Newtonian mechanics was universal, but they were wrong. Is it therefore really so outrageous to be suspicious that perhaps C. D. Broad's (1969) "basic limiting principles," which negate paranormal processes, might also not be as universal as scientists now assume, especially when there are so many events that, when taken at face value, seem to contradict them?

It seems to me that in a proper empiricist science, a priori conventions should not be invoked until the empirical research process has been exhausted and a winner still has not emerged. Since CTs are not willing to hold off this long, they effectively demote empirical evidence to a secondary status in their philosophy. Coherence becomes the tail that wags the dog, and in practice it functions as all but a final arbiter of scientific claims.

Aura of Empiricism

The CT's position, however, does project a certain aura of empiricism, and no sane philosophy of science, even a hyper-rationalist one, can completely exclude empirical evidence as being irrelevant. But I think a careful examination of the remnants of empiricism that do exist in the CTs' program will serve primarily to further illustrate the dominant status that the coherence principle has achieved in their thinking.

CTs do claim that they would accept "the existence of psi" if conclusive empirical evidence were forthcoming. However, for such evidence to qualify, all conventional explanations must be completely eliminated, even if they are unlikely or implausible. This is the famous position taken by C. E. M. Hansel (1980). I have never known any CT to repudiate it, although Ray Hyman (1981) has taken a few bites around the edges. The explanations proposed by CTs often are quite implausible and are not even defended as being plausible. Most assume dishonesty or gross incompetence on the part of the experimenter in carrying out elementary research procedures. If all else fails, the CT stalls for time, suggesting that a flaw in the procedure may be revealed at some later date. This of course is true, and dishonesty and incompetence could indeed account for all the experimental evidence; there are precedents for both. However, none of this takes away from the extreme elasticity evidenced in the CT approach; one never really gets a clear sense of what CTs would minimally accept as evidence for paranormality. Exactly how elastic the CT approach is in practice will only be known as the empirical evidence becomes harder and harder to explain away, but it clearly can be carried much further than it presently has been. Sooner or later it can be overwhelmed by data, but the CTs have made this as difficult as possible to achieve. The problem, in a nutshell, is that the coherence principle is used to arbitrate what counts as acceptable empirical evidence.

Not only does the existence of an alternative explanation render a psi experiment worthless as evidence for paranormality, but many CTs take it to mean that there is no reason to even *question* that whatever happened has an adequate conventional explanation. Even though CTs will often deny that any one of their specific alternative explanations is necessarily what happened, at the same time they will insist that there is nothing to explain. This conclusion can be shown to imply that the correct interpretation, whatever it is, is conventional. The conventional explanations are not necessarily accepted individually, but they are accepted as a class. What transforms a set of possible explanations into a set of acceptable explanations is—you guessed it—the coherence principle.

To see how this follows, consider a statement made by Ray Hyman (1981) in his critique of the ESP and PK experiments by Helmut Schmidt using automated methodology. This research is widely regarded by both parapsychologists and CTs as among the best the parapsychologists have to offer. The quote is as follows:

> Only when the parapsychologists settle upon a standardized paradigm, tidy up the procedures, demonstrate that the results follow certain laws under specified conditions, and that these results can be duplicated in independent laboratories, will we have something that needs "explaining." (p. 39)

Although I take exception to some of the premises of this statement, in particular the one about the lack of independent replication, I wish to focus on the conclusion of the statement. The logical equivalent of this concluding phrase is that Schmidt's results constitute nothing that needs to be explained.

Now up to the time that Hyman's paper was published, Schmidt had published 22 separate experimental series. Of these, 20 yielded statistically significant deviations, and 18 were in the predicted direction. Obviously, *something* is happening. Now either we have an adequate explanation of this something or we don't. If we don't, then it follows that we *do* have something to explain. Thus the only remaining way to interpret Hyman's statement literally is that we already have an adequate explanation of Schmidt's results. Even if we take the statement in the looser sense to mean that Schmidt's results provide nothing that scientists should bother themselves about (which I suspect is what Hyman really meant), the implication remains that the population of potential explanations includes nothing very interesting. Since a paranormal explanation would be quite interesting, even to a CT, I think the reader is entitled to infer that Hyman does not take the possibility of such an explanation seriously.

It is true that CTs occasionally attempt to provide empirical support for conventional explanations of OPEs. Most often this is in the form of what they call "debunkings," exposés of particular psychic claims or psi experiments. Occasionally, this approach provides scientifically useful information. The most successful example I can think of is Scott and Haskell's (1974) and Markwick's (1978) detection of irregularities in the data of the S. G. Soal ESP experiments. (Dr. Scott is one hard-line critic whose writings do reflect an empiricist value system, but I consider him an anomaly in this regard.)

Taken as a whole, however, the CTs' empirical research program is not impressive by normal scientific standards. Its most damning feature is that debunking need not confirm that a conventional process occurred, but only that it *could* have occurred. Only a small percentage of debunkings actually provide empirical evidence for conventional hypotheses. Because of the absence of any systematic sampling procedures, it is even more unclear than usual how far one can generalize the "real" debunkings that are achieved. Debunkings are highly concentrated on publicity-seeking self-proclaimed "psychics," generally one of the last places I as a parapsychologist look for "real" psi.

The preceding critique says more about the inadequacies of debunking as a research strategy than it does about how specific debunkings have been carried out. The greatest deficiency of the CT research program is its almost total lack of systematic laboratory research and model-building for assessing relevant conventional hypotheses on a broad scale. (One exception is a handful of studies exploring the cognitive styles of "believers" and "skeptics" [e.g., Alcock & Otis, 1980; Troscianko & Blackmore, 1983], but this approach is very much in its infancy.) CTs often like to cite the research literature in general psychology concerning human capacities to misperceive or misinterpret events in line with preconceived biases, but such research is far from consistent in its implications and is not directly relevant enough to the anomalies at issue to be satisfactory. The premise that people's perceptions and cognitions are sometimes biased does not lead to the conclusion that such biases are responsible for the critical mass of OPEs; this is an *empirical* question. What is needed is research that is both (a) nomothetic and (b) targeted as closely as possible to potentially evidential psychic events.

This of course is a very difficult research area, and CTs, were they to launch such a program in earnest, would run into many of the same difficulties (e.g., the elusiveness of the phenomena) that parapsychologists have had to put up with for decades. But no one has said that science is easy, and these difficulties cannot be used as excuses to justify basing conclusions on insufficient evidence. If CTs were to apply the same ingenuity to serious research as they have to some of their more celebrated debunkings, they might be able to come up with some sound, broadly based evidence for their hypotheses that is worthy of being taken seriously by the scientific community at large.

Progressive Skepticism

No one should conclude from the preceding discussion that I am seeking an easy path for verifying paranormality. It is my position that this conclusion requires confirmation of a theory incorporating some paranormal principle, which in turn requires that predictions based on the theory be confirmed more successfully than those from competing conventional theories. Moreover, such confirmations must meet the same standards of evidence required of any scientific hypothesis, including an adequate degree of replicability. This might be construed alternatively from a Lakatosian framework (Lakatos, 1970) as being a progressive research program based on a core assumption involving paranormality. Although I do not insist that each confirming experiment be completely immune from any ad hoc hypothesis that some CT might concoct, I nonetheless think my approach is conservative, reasonable, and responsible. I have no objection whatever to demands for rigorous methodology. All I am really asking is that some rigor be exhibited at the other end of the spectrum; in particular, CTs must recognize that until such time as they can make a credible scientific case for *their* explanations, the OPEs documented by parapsychologists over the past century represent a genuine challenge to conventional theory that cannot be swept under the rug by appeals to a priori conventions or ad hoc interpretations.

I label my position *progressive skepticism.* It is skepticism because it requires a critical attitude toward *all* hypothesized explanations of OPEs, and it concludes that at present there are *no* scientifically adequate explanations for the critical mass of these events. It is progressive because it has faith that scientific method, broadly interpreted, can ultimately provide or at least contribute to satisfactory explanations for these events, and it encourages research toward that end. Progressive skepticism is similar, although not necessarily identical, to *zeteticism* as defined and promoted by Marcello Truzzi (in press).

An important corollary of progressive skepticism is that research from both conventional and paranormal perspectives should be encouraged, with resources, status, and so on, being allocated as independently as possible of the theoretical orientations of the investigators. In some respects I was encouraged by our friends Zusne and Jones's proposal (1982) for a specific branch of psychology called anomalistic psychology to study OPEs. The proposal is welcome because it acknowledges that there are outstanding questions requiring empirical solutions. But a negative feature of their proposal is that for the foreseeable future it excludes research from a paranormal perspective. That those of us who have a paranormal perspective are not even allowed to compete again shows the lengths to which the coherence principle will be taken, and it undercuts any claims that they, or those who share their position, can make of open-mindedness and commitment to free inquiry in this area.

This closed-mindedness also seems to be alive and well in many university psychology departments. I am particularly distressed at how frequently I hear of students being told that they will wreck their careers if they pursue research in parapsychology. I am not necessarily criticizing the professors who give this advice but rather the existing climate that obliges them to give it. I am not referring here to the local TV astrologers and self-proclaimed psychics who often end up teaching in the Extension Division, but rather to the bright, competent, and level-headed students who usually are given this advice precisely because they are bright, competent, and level-headed. Not only does this state of affairs prevent the average quality of personnel in parapsychology from improving (something that CTs constantly complain about), but more importantly it is a blatant violation of the principle of academic freedom that psychology as a profession espouses. I hope that those psychologists who really do cherish academic freedom will take steps to correct this situation. Several prominent scientists from other (conventional) sciences have told me that similar tactics are used to enforce conformity to the current conventional viewpoint in their own disciplines and thus stifle new ideas and approaches. One wonders if science is not the ultimate loser as a result of these practices.

I would like to end this paper on a conciliatory note. I feel that progressive skepticism is a position that has the potential of uniting large segments of the more moderate constituencies on both sides of the psi controversy. In particular, I see a consensus beginning to emerge that there is only one defensible conclusion about the current status of the evidence regarding OPEs. That conclusion is that we can draw no conclusion, that we must suspend judgment. In the scenario I envision, people on the two sides would continue to have different opinions about how the issue will finally be resolved, and they would continue to do research from their respective orientations. We would lose the false dichotomy between proponents and critics, for each side would conduct research, propose interpretations of that research, and criticize the research and proposals of the other side.[2] This is the path along which I think we may eventually resolve the psi controversy, to the benefit of everyone.

One final word to my CT friends. I think it is not just a coincidence that occult ideas thrive in just those areas where conventional scientific knowledge is least complete—to put it metaphorically, inner space and outer space. What this suggests to me is that if the objective is to finally stamp out occultism, the best approach is not to expend your resources debunking stage psychics and ESP experiments, which most real occultists take even less seriously than you do, but rather to promote solid scientific research on these problems from a conventional perspective. But to do so with maximum effectiveness you will need the stimulation and, yes, the criticism that can

[2]Criticisms, of course, would also occur *within* each perspective.

only be provided by a vigorous competing paranormal research program. Of course you must run the risk that the paranormal research program may ultimately prevail, but if you really have confidence in the correctness of your position that risk should be tolerable. In short, I would like to invite you to become progressive skeptics.

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