

# Continuing Commentary

*Commentary on K. Ramakrishna Rao and John Palmer (1987). The anomaly called psi: Recent research and criticism. BBS 10:539–551.*

**Abstract of the original article:** Over the past hundred years, a number of scientific investigators claim to have adduced experimental evidence for “psi” phenomena – that is, the apparent ability to receive information shielded from the senses (ESP) and to influence systems outside the sphere of motor activity (PK). A report of one series of highly significant psi experiments and the objections of critics are discussed in some depth. It is concluded that the possibility of sensory cues, machine bias, cheating by subjects, and experimenter error or incompetence cannot reasonably account for the significant results. In addition, less detailed reviews of the experimental results in several broad areas of psi research indicate that psi results are statistically replicable and that significant patterns exist across a large body of experimental data. For example, a wide range of research seems to converge on the idea that, because ESP “information” seems to behave like a weak signal that has to compete for the information-processing resources of the organism, a reduction of ongoing sensorimotor activity may facilitate ESP detection. Such a meaningful convergence of results suggests that psi phenomena may represent a unitary, coherent process whose nature and compatibility with current physical theory have yet to be determined. The theoretical implications and potential practical applications of psi could be significant, irrespective of the small magnitude of psi effects in laboratory settings.

*Commentary on James E. Alcock (1987). Parapsychology: Science of the anomalous or search for the soul? BBS 10:553–643.*

**Abstract of the original article:** Although there has been over a century of formal empirical inquiry, parapsychologists have clearly failed to produce a single reliable demonstration of “paranormal,” or “psi,” phenomena. Although many parapsychological research projects have been carried out under what have been described as well-controlled conditions, this does not by itself make a science, for unless and until it can be demonstrated that paranormal phenomena really exist, there is no subject matter around which a science can develop. Indeed, parapsychologists have not even succeeded in developing a reasonable definition of paranormal phenomena that does not involve, or imply, some aspect of mind–body dualism. Moreover, parapsychology has developed several principles (such as the experimenter effect) that can be used to explain away failures, and the use of these principles contributes to making the psi-hypothesis unfalsifiable.

The “anything goes” attitude in parapsychology, which seems to lend credence to virtually any “paranormal” claim, serves to weaken the credibility of parapsychological endeavors in the eyes of critics. This general willingness to suspend doubt is another indication that parapsychology is more than the quest to explain anomalous experiences, as is claimed. It is argued in this paper that parapsychological inquiry reflects the attempt to establish the reality of a nonmaterial aspect of human existence, rather than a search for explanations for anomalous phenomena.

## The anomaly of the anomalous

Joseph Glicksohn

*Department of Criminology, Bar-Ilan University, Ramat Gan, 52100, Israel.  
chanita@bgumail.bgu.ac.il*

**Abstract:** What R&P term the “noise reduction model” implies that the psi-conducive state is related to the induction of an altered state of consciousness (ASC). Yet there is a problem in embedding psi in the ASC, because one anomaly is replacing another. This seems to be a general strategy in the literature of the anomalous.

The two target articles by Rao & Palmer (1987) and Alcock (1987) on parapsychology are important for an understanding both of the anomaly of psi, and the anomaly of the anomalous. Both articles stress the importance of studying anomalous experiences, but the conclusions diverge.

Rao & Palmer (R&P) argue that “it is the business of parapsychology to find explanations of psi anomalies through scientific

inquiry” (sect. 1, p. 539), keeping open the possibility that the anomalies will ultimately remain anomalous (the omegic, or psi hypothesis). Alcock concludes that “finding explanations for ostensible anomalies is not what parapsychology is really about for most parapsychologists. If it were, much more effort would be made to try to find psychological and neuropsychological explanations for such experiences before even contemplating the radical psi hypothesis” (sect. 7, p. 564). This divergence of view is not directly related to the main issues under discussion – the methodological sophistication of the studies conducted in the field and their replicability. Yet, the discrepancy is clearly evident, and it is because of this divergence of view that some critics label parapsychological research as being pseudoscience.

The question I wish to raise is whether “pseudoscience” is a term reserved for the investigation of anomalous phenomena, for those who believe in the anomaly of the anomalous, or for research that is sloppy. These are three different issues. First, there is no reason for sloppy research. Second, the investigation of anomalous

phenomena may be termed “unconventional” but should not be viewed as pseudoscience, given the soundness of the research. Third, those who believe in the anomaly of the anomalous, having explored alternative hypotheses in depth, may in fact be on to something. So, parapsychology should not be viewed as pseudoscience, and should not be conducted as pseudoscience. Having said this, I move on to some substantive issues.

What R&P term the “noise reduction model” (i.e., “the idea that psi may be facilitated by procedures that result in the reduction of meaningful sensory and proprioceptive input to the organism, and the concomitant redirection of attention to internally generated imagery,” sect. 7, p. 548) seems to be producing replicable results, primarily for those studies using a Ganzfeld. Yet, as R&P note, many of the studies using a Ganzfeld “failed to use control groups or other means of assessing whether the induction procedure was actually responsible for the positive scoring” (p. 550). Note that the implication here is that the psi-conducive state is related to the induction of an altered state of consciousness (ASC). This, in turn, suggests the following research hypothesis (Glicksohn 1986): those subjects exposed to the Ganzfeld who experience an ASC should also exhibit psi; those who do not experience an ASC in this condition should not exhibit psi. This is an easily testable hypothesis, though it has as yet to be explored. Satisfactory evidence supporting the hypothesis would place the anomaly called psi firmly within the anomaly of the ASC, which is a good move, though problematic.

The problem here is that in embedding psi in the ASC one is replacing one anomaly with another. This seems to be a general strategy in the literature of the anomalous. For example, afterlife visions may provide evidence for an afterlife, but it is more likely that they indicate dissociative hallucinatory activity triggered by an acute ASC, the near-death experience (Siegel 1980). Similarly, the out-of-the-body experience (OBE) may be an instance of “astral projection,” but it is more likely to be an ASC (Blackmore 1983). Hypnotic behaviour is also anomalous, and may be indicative of an ASC (Glicksohn 1987). But the discussion appearing in *BBS* [see Spanos: “Hypnotic Behavior: A Social-Psychological Interpretation of Amnesia, Analgesia, and ‘Trance Logic.’” *BBS* 9(3) 1986] should make *BBS* readers aware of the problems associated with using the term “ASC” as an *explanatory* construct. Nevertheless, the strategy is wise, given the fact that people who experience ASCs, such as “fantasy addicts” (Wilson & Barber 1983) also tend to report subjective paranormal experiences.

In considering the possibility that a subjective paranormal experience is related to the induction of an ASC and/or to a personality prone to experiencing ASCs, the researcher studying ASCs can also be on the lookout for the anomaly of psi. What this means is that research in anomalistic psychology (Zusne & Jones 1982) should be concerned with commonalities of experience. For example, in extending the notion of eidetic imagery [see Haber: “Twenty Years of Haunting Eidetic Imagery: Where’s the Ghost?” *BBS* 2, 1979] to include any mental imagery projected onto the sensory environment, one notes similarities with crystal-gazing, hallucinations, and OBEs (Marks & McKellar 1982), and possibly also with psi phenomena.

Thus, in partial agreement with Alcock, I also suggest that we “focus on the anomalies while putting the concept of psi aside until, if ever, it is needed” (sect. 8, p. 565); I further suggest, together with R&P, that we leave the psi hypothesis as one alternative, and to get on with the important work of making some progress in investigating anomalous experiences, bringing them within the scope of cognitive psychology (Glicksohn 1986). In such a manner, instead of discussing the cognitive psychology of the parapsychologist, one would be dealing with the cognitive psychology of the anomalous.

## Authors’ Responses

### Parapsychology, anomaly, and altered states of consciousness

John Palmer

*Institute for Parapsychology, Durham, NC 27701. john@rhine.org*

**Abstract:** Pseudoscience is not an appropriate label for parapsychology. Although the noise reduction model of extrasensory perception (ESP) is explanatory only in a limited sense, research does exist addressing the correlation between ESP and altered states of consciousness (ASCs). The term anomaly is not appropriately applied to experiences such as out of body experiences (OBEs) per se, but only to the question of their source. Research on both topics should be encouraged.

**Glicksohn** has written a very thoughtful continuing commentary on Rao and Palmer (1987t) and I am pleased to offer a brief response to his various points. I agree completely with what I understand to be his position on the application of the term pseudoscience to parapsychology. If parapsychologists use proper scientific methods and standards of evidence in their inquiry, their research should not be labeled as pseudoscience because their unconventional interpretations of data may be inconsistent with currently accepted theory.

I also agree with his point about the noise reduction model. I wish to add, however, that there are a number of published experiments in which ESP scores were correlated with prefeedback subject ratings of the degree to which they felt they were in a psi-conducive state of consciousness as the result of an altered state induction procedure. These studies, which are relevant to Glicksohn’s research hypothesis, have consistently shown that, as a group, the subjects who reported the most pronounced ASCs also showed the most evidence of ESP. The studies are discussed in Eysenck and Sargent (1982), and I can supply references to specific experiments to anyone who is interested.

Glicksohn’s next paragraph targets a conceptual confusion that I find frequently in the ASC literature, particularly on topics such as out-of-body and near-death experiences. It is essential to make a sharp distinction between subjective experiences and the sources of these experiences. OBEs, for example, are purely and simply subjective experiences. They are no more anomalous than ordinary dreams and can be dealt with satisfactorily by the standard principles of cognitive psychology. Anomaly enters the picture only when we consider the source of the experience: Is its origin purely internal, or is it evoked by some sort of external stimulus with which the person has no sensory contact? Only then does parapsychology become directly relevant.

I agree with Glicksohn that ASCs should not be used as an explanation of psi, but rather as the description of a mental state under which psi is relatively likely to occur. The noise reduction model may be considered explanatory, but only in the limited sense of proposing cognitive mechanisms that might allow psi information to reach awareness more readily. However, this is far from a full-fledged explanation of psi.

We need not and should not adopt an either/or approach to whether research should be directed to the study of so-

called anomalous experiences per se or to their sources. The same applies to whether the premise of the research should be that the source of the experience is always internal or is sometimes external. All these kinds of research should be undertaken simultaneously, because knowledge in one domain will very likely facilitate our understanding of the others.

Finally, let me make a small clarification. According to the framework outlined by Dr. Rao and myself in our target article (Rao & Palmer 1987t), if a paranormal or omegic explanation of psi were ever to be generally accepted by the scientific community, psi would cease to be anomalous. Psi is anomalous at present because there are as yet no adequate scientific explanations, omegic or conventional, for the better observations.

## Science, pseudoscience, and anomaly

James E. Alcock

Department of Psychology, Glendon College, York University, Toronto, Canada M4N 3M6. jalcock@glendon.yorku.ca  
www.glendon.yorku.ca/jalcock

**Abstract:** My criticisms of parapsychology are neither based on its subject matter per se, nor simply on a charge of sloppy research, but rather on the whole pattern of theory and research in this domain. The lack of a positive definition of psi, the use of ad hoc principles such as psi-missing and the experimenter psi effect to account for failures to confirm hypotheses, and the failure to produce a single phenomenon that can be replicated by neutral investigators are among the major problems that keep parapsychology outside regular science. **Glicksohn** and I agree that anomalous experiences should be investigated.

**Glicksohn** apparently misunderstands my position with regard to parapsychology and pseudoscience. It is not the study of anomalies that makes something a pseudoscience – far from it. Indeed, it is the study of anomalies that drives science forward, otherwise we would not have advanced beyond Newtonian physics and we would still be stuck with Ptolemaic astronomy.

However, parapsychology does not so much try to explain anomalies as to prove that they exist, anomalies that appear ultimately to indicate that “mind” can exist separate from body. In normal science, anomalies arise as scientists become aware of observations that cannot be accounted for by their theories; this eventually leads either to better instrumentation and a disappearance of the anomaly (if it was due to measurement problems) or to modification, and sometimes even the scrapping, of the theory.

What about parapsychological “anomalies?” We do not hear from the world of mainstream physics about how particles behave strangely when in the presence of one or another thinking entity. Nor do the realms of neurology and experimental psychology report strange communicative abilities that appear inexplicable on the basis of physical brain matter. Parapsychologists are not out to explain the anomalies of modern science; rather, their “anomalies” have to be deliberately sought out, and for over a century the debate about parapsychology has been about whether or not parapsychologists have actually managed to demonstrate an anomaly. Of course, the anomaly can only be demonstrated if all “normal” explanations can be ruled out,

and this then produces a ticklish situation in that any flaw in an experiment may be enough to account for the supposed anomaly. Since all possible flaws may not be recognized, at least at first, one needs to be especially careful in the evaluation of such anomalistic claims. This is what makes independent replicability absolutely vital. “Independent” is an important adjective here, because the same researchers, in repeating their experiments, may repeat them with the same flaws that produced the first results.

Now, as for my concerns about parapsychology and my charges of pseudoscience, let me be very clear: there is nothing unscientific per se about investigating claims about ghosts or out-of-body experiences or extrasensory perceptions. It is the way this is done that is of concern.

(1) Parapsychologists have been unable to produce a single “anomalistic” phenomenon which can be reproduced by independent and neutral scientists.

(2) Unlike in the mainstream scientific domain, parapsychological phenomena are defined exclusively in negative terms – their existence can only be claimed when all “normal” explanations have been eliminated. Hence, the importance of first demonstrating a true “anomaly.”

(3) Parapsychologists are given to explaining away negative outcomes in terms of ad hoc explanations such as the “decline effect,” “psi-missing,” and the “experimenter psi effect” (which supposedly accounts for the failure of some researchers to replicate positive results: the psychic force lies in the researcher, it is claimed, and sceptical researchers may through their negative psychic influence prevent the emergence of positive data).

(4) Because of these ad hoc explanatory mechanisms that explain away failure, there is no way that the parapsychologists’ claims can be refuted. Hence there is no way by which parapsychologists could ever be led to the conclusion that psi does not exist.

These four problems, in my opinion, justify withholding the appellation “scientific” from parapsychology as a whole, although this does not mean that each and every research study in parapsychology is necessarily pseudoscientific.

Having said that, I welcome Glicksohn’s agreement that the focus should be on the subjective experiences that people interpret as paranormal, whether or not there really is anything “paranormal” about them. The strange and certainly compelling experiences that people have reported across the ages provide an important and fascinating field of study for psychologists, neurologists, anthropologists, and others. We can only expand our knowledge of the functioning of the human brain and extend our theories of cognition by coming to understand the genesis of such experiences. Glicksohn’s desire to leave the psi explanation as one alternative does not particularly concern me, so long as that explanation does not “get in the way” – that is, so long as researchers are not distracted from the tough job of coming to understand the complexities of the human brain and human experience by falling back on the label “psi,” which offers no real explanation at all.

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**Commentary on David Sloan Wilson and Elliott Sober (1994). Reintroducing group selection to the human behavioral sciences. BBS 17:585–654.**

**Abstract of the original article:** In both biology and the human sciences, social groups are sometimes treated as adaptive units whose organization cannot be reduced to individual interactions. This group-level view is opposed by a more individualistic one that treats social organization as a byproduct of self-interest. According to biologists, group-level adaptations can evolve only by a process of natural selection at the group level. Most biologists rejected group selection as an important evolutionary force during the 1960s and 1970s but a positive literature began to grow during the 1970s and is rapidly expanding today. We review this recent literature and its implications for human evolutionary biology. We show that the rejection of group selection was based on a misplaced emphasis on genes as “replicators” which is in fact irrelevant to the question of whether groups can be like individuals in their functional organization. The fundamental question is whether social groups and other higher-level entities can be “vehicles” of selection. When this elementary fact is recognized, group selection emerges as an important force in nature and what seem to be competing theories, such as kin selection and reciprocity, reappear as special cases of group selection. The result is a unified theory of natural selection that operates on a nested hierarchy of units. The vehicle-based theory makes it clear that group selection is an important force to consider in human evolution. Humans can facultatively span the full range from self-interested individuals to “organs” of group-level “organisms.” Human behavior not only reflects the balance between levels of selection but it can also alter the balance through the construction of social structures that have the effect of reducing fitness differences within groups, concentrating natural selection (and functional organization) at the group level. These social structures and the cognitive abilities that produce them allow group selection to be important even among large groups of unrelated individuals.

**Reintroducing “Reintroducing group selection to the human behavioral sciences” to BBS readers**

Nicholas S. Thompson

*Departments of Biology and Psychology, Clark University, Worcester, MA 01610. nthompson@clarku.edu*

**Abstract:** Wilson and Sober’s (1994) revival of group selection theory may have failed with some readers because its simple arithmetic foundation was obscured under the complexities of its presentation. When that uncontroversial principle is uncovered, it broadens dramatically the fundamental motives that social scientists may impute to human nature and still be consistent with Darwinian evolutionary theory.

Because unnecessary complexity foments confusion, and confusion is the enemy of conceptual change, I am trying to put in the simplest possible form my understanding of the fundamental insight contained in the Wilson and Sober (1994) target article.

One way to think about their insight is that Wilson & Sober (W&S) show how the bad reputation of group selection for the last 30 years was undeserved, being based on a failure of mathematical intuition on the part of many of their colleagues. One way to describe this failure is that we (sadly, I must include myself in this group) supposed that the mathematical operations appropriate to integers are also appropriate to proportions. Let me illustrate this failure in the simplest possible way. Imagine you have two bags with 100 candy-covered chocolates in each. Imagine that in each bag there are two colors of candy coatings, red and green. Imagine further, that I have a supply of loose red and green candies which I

can add to the two bags. Finally, let us imagine that we both prefer the red candies to the green candies.

As a kindness to you (I say), I agree to increase the number of red candies in each of your bags. You rightly accept because you know that if I add some number of red candies to each of your bags, then the number of red candies you have overall must increase.

But now let us say I make a different sort of offer to you: I agree to increase the *proportion* of red candies in each of your bags. Are you assured that I will increase the *proportion* of red candies overall? Well, in fact, no. Imagine that to start with, there are 20% red candies in one bag and 80% red candies in the other. I double the number of candies in the 20%-red bag, giving me 40 red and 160 green, and then substitute two green candies with two red ones, bringing the new proportion of red candies to 21% (42/200). I now halve the number of each kind of candy in the 80%-red bag, giving 40 red and 10 green, and, again, substitute two green candies with two red ones, bringing the new proportion of red candies in this bag to 84% (42/50). Notice that I have, as I promised, increased the proportion of red candies in both bags (20% to 21% and 80% to 84%). But what has happened to the *proportion* of red candies overall? In fact it has *decreased* from 100/200 to 84/250, or approximately 34%.

What does this all have to do with the evolution of sociality? Let there be a population divided into several groups of individuals. Let there also be two kinds of individuals that inhabit these groups, As and Ss: As are group altruists, each of which acts so as to increase the reproduction of its group at its own reproductive expense; Ss are individuals that profit from the activities of As but do not perform any altruistic acts. It follows from these consider-