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IS SCIENTIFIC PARAPSYCHOLOGY POSSIBLE?

Some Thoughts on James E. Alcock's *Parapsychology: Science or Magic?*[1]

By Rex G. Stanford

Unlike some other famous critics of parapsychology, James Alcock—to the best of this reviewer's knowledge—is not trained in stage magic or sleight of hand. Nonetheless, he has succeeded in this volume at making strangely invisible the evidence for the reality of psi phenomena. The secret of this rhetorical legerdemain is, essentially, to dismiss the evidence by arguing that trying to adduce evidence for psi is like trying to lift oneself by one's own bootstraps—that it is a logical absurdity. Alcock feels "that 'scientific validation of psi' would be a contradiction of terms; if psi exists, science as we know it cannot" (p. 191). He feels this way, apparently, because "if psi exists, practically *everything* [Alcock's italics] is possible" (p. 191), and, if everything is possible, this undermines the epistemology underlying science. As Alcock sees it, it is the statements of parapsychologists themselves, their treatment of negative evidence, their framing of the question of the existence of psi, and their willingness to invoke experimenter-psi explanations for the alleged lack of replicability, which undermine, more or less categorically, the credibility of whatever "evidence" exists and which lead him to the conclusion that parapsychology as a science is a logical absurdity.

While these are not the sole grounds upon which Alcock dismisses our field, they are among his more fundamental reasons for doing so. His discussion of specific research reports is extremely scant, though he devotes one chapter (Chapter 7) and part of another (Chapter 6) to some discussion of our methods, usually with examples which seem selected for weakness rather than representativeness.

In any event, for Alcock, "the evidence is not there" (p. ix). Furthermore, he says, "thousands of research reports and monographs and books attest to the strength of the evidence, but thousands of books and documents have attested to the reality of Satan, and I am not persuaded by either" (p. ix). This clouding of the issues by linking

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[1]New York: Pergamon Press, 1981. Pp. xi + 224.

psi research with various dubious or intrinsically extrascientific claims is characteristic of remarks spread throughout this book. Given this inauspicious start, it is little wonder that this book does not provide what many parapsychologists might have hoped for, a thoroughgoing critique of our evidence by a reasonable, if skeptical, outsider.

Alcock's stated objectives here are to expose the central problems with the theorization, methods, and data of parapsychology, problems which, he alleges, block parapsychology's acceptance by the scientific community, and to explain how belief in the paranormal can continue even in the absence of rationally convincing evidence. The second of these two general objectives is the concern of Chapters 2 through 5; Chapters 6 and 7 address the first objective. Chapter 1 is an introduction which sets the general tone by, among other matters, dragging into the discussion sundry topics that are not part of scientific parapsychology (e.g., the Bermuda Triangle, commercial "past-life seminars," creationism, and so-called psychic surgery), and by assuring the reader that supposed paranormal phenomena would violate one or more of the basic assumptions of the contemporary scientific worldview. It includes approximately one page of historical overview followed by equal space extolling the supposed virtues of C. E. M. Hansel's work.

Origins and Maintenance of Paranormal Beliefs

Chapter 2, "Magic, Religion and Science," gives little space to magic, but is concerned primarily with several theories of the development of religious beliefs, with the decline of many such beliefs in the face of scientific advance, with the supposed effort to salvage the basis of some such beliefs through parapsychology, and with various data and arguments bearing upon possible explanations for the increased interest in the paranormal. This otherwise thoughtful and informative chapter fails to consider at least the possibility that genuinely paranormal events may be important in the development and maintenance of religious beliefs. It is possible that many religious precepts have as their basis implicit, but experience-based, knowledge of the circumstances under which psi events can play a constructive role in life experience. As a means of explaining the development of religious beliefs and practices, Alcock prefers the Skinnerian concept of operant conditioning without consideration of any paranormal reinforcers, and he correctly points out that resistance to extinction

produced by a partial-reinforcement schedule may play an important role here.

The chapter is marred by a strong tendency toward evangelization for the humanist version of "rationality": "North American society may be entering one of those dangerous phases in history where the forces of irrationality and anti-rationality threaten science itself" and ". . . if we are wise, we shall be careful to guard our rational heritage jealously" (p. 38). Among the dreaded attacks upon rationality are "more and more universities . . . teaching courses in parapsychology" (p. 38). It would appear that Alcock's "rationality" is implicitly defined in terms of adhering to a materialistic worldview! It would seem that the horror and threat of these various forms of "irrationality" lie primarily in their going contrary to the scientistic religion of the humanists. It is entirely conceivable that insofar as threats to society are concerned, the effort by Alcock and others to expunge "irrationalism" from the human mind may itself be psychologically and socially dangerous. Perhaps the human mind has needs for "irrationalism" such as magical thinking and the religious impulse. If so, suppression of such impulses might lead to some undesirable consequences. Perhaps the road to the happy human may lie in some reasonable compromise between "rationality" and "irrationality." The danger would seem to be in seduction into belief systems which are resistant to change in the face of evidence, whatever those beliefs may be, including the belief that the world is just as the humanist philosophers imagine it to be. It may be difficult for humankind to live by science alone, and to try to do so may be both fatuous and dangerous for society. To try to do so may indeed be impossible—a circumstance that may account, in part, for the evangelical fervor of the humanists on behalf of a belief system which cannot in principle be justified by science itself.

Chapter 3 is entitled "The Psychology of Belief." Early in this chapter Alcock notes that merely to give a name to behavior or events is not to explain them, and to act as though such naming provides an explanation (the "nominal fallacy") can effectively discourage further inquiry. This reviewer would add the caveat that nonexplanatory labels can sometimes also carry connotations which introduce implicit conceptual biases into the area of investigation. The term *extrasensory perception* is an example of this, and the biases it engenders have not always been examined in the deliberate way that would have been desirable.

Alcock then discusses the sources and nature of beliefs; in the latter

case he relies heavily upon the theoretical ideas of M. Rokeach. In Alcock's discussion of personality and belief in the paranormal, the propagandistic thrust of his writing is transparent. If you can persuade your readers that belief in the paranormal might indicate something bad about their personality, they are unlikely to adopt such beliefs or, if they are already believers, they might abandon their beliefs out of fright. After summarizing some relevant research, he concludes that believers in the paranormal, at least in the student population, tend to be somewhat more dogmatic and less skilled in critical thinking than are skeptics. Whether or not this finding is reliable—and it is not at present on very firm ground—an appeal to rationality would seem to dictate that readers be encouraged to evaluate parapsychology on the basis of the evidence, whatever may be their fears about their personality. But while Alcock seemingly gives his reader every basis that he can for such fears regarding their self-image if they believe in the paranormal, he studiously avoids reviewing in any meaningful way—anywhere in his book—the major, substantial lines of research in parapsychology.

Instead, here and elsewhere in this volume, Alcock skillfully builds the picture of a frightfully dangerous inclination on the part of all of us to become seduced into irrational, magical thinking. He continues by warning his readers that important or central beliefs are usually highly resistant to change. While many of the psychological points he makes are valid or, at least, credible, Alcock's tendency to paint a picture of a jungle of irrationality may discourage his reader from examining the evidence for himself.

Alcock puts the finishing touches on this chapter by pointing out the not surprising fact that not all scientists are paradigms of rationality. The net effect of this is simply to provide a basis for attacking the credibility of parapsychologists' claims. If he can undermine the credibility of scientific testimony through attacks upon scientists themselves, then his reader will be less inclined to take seriously, at the very least, those scientists who make unorthodox claims. To be sure his reader gets the point, he ends this discussion with the statement that near the close of the last century those scientists who studied the alleged powers of mediums and who drew positive conclusions about their paranormality "assumed that they could not be fooled" (p. 62). Such a blanket statement is startling because of its gross inaccuracy and unfairness—especially so when it is followed by the revelation that it took Houdini to expose their credulity, even as the "Amazing Randi" is doing in today's analogous situation. There is not one word here about the genuine skepticism of

a number of those who investigated such claims but who came, eventually, to feel that the claims might have some credibility, nor is there one word about the psychical researchers who did considerable "exposing" and who in their own clever investigations demonstrated how easily persons can be deceived by trickery (e.g., Hodgson, 1892; Hodgson and Davey, 1887).

Chapter 4, "The Psychology of Experience," covers a variety of topics, but the central theme is that we often do not know—are genuinely unaware of—the factors that influence our thinking and perception and that lead us to a certain conclusion or experience of a set of circumstances. The intended moral of this chapter would seem to be that if we naïvely draw conclusions from what we experience, we are likely to find ourselves misled.

Much of our information processing, Alcock correctly notes, occurs outside our consciousness. However, some of his examples and arguments are weak. For instance, he states that "the difference between tacit and explicit knowledge, between unconscious and conscious mind, was startlingly demonstrated in a study of patients who had undergone 'split-brain' operations" (p. 66). The examples he gives from such work seem to this reviewer to have little or no direct or conceptually clear bearing upon the "conscious–unconscious" distinction in normal individuals. The major conclusion which can realistically be drawn from his examples here is that the left hemisphere is unaware of what the right hemisphere is doing if its communication channel is severed. In another weak example, he states, "When people are led to perform some action, such as opening a window, as a result of post-hypnotic suggestion, they typically rationalize their action for which they do not 'know' the cause by saying something like 'It's hot in here'" (p. 67). Both the content and context of Alcock's statement about posthypnotic behavior make it clear that he is suggesting that the hypnotic subjects are not consciously aware of the source of their action during enactment of the posthypnotic suggestion. This is a very controversial statement (e.g., Barber, 1969), but there is nothing in Alcock's treatment to suggest how controversial it is. Another Alcock statement which is at best debatable is: "It is the verbal language system which seems to be the essence of consciousness" (p. 68). The context of this statement makes it clear that this indicates the essence of consciousness as being in the left hemisphere. Ironically, the leading figure in "split-brain" research, Roger Sperry, would seemingly disagree quite strongly with such a statement as is made so categorically by Alcock. Consider, for example, the recently published text of Sperry's lecture (Sperry, 1982) on the occasion of his receiving

the Nobel Prize in Medicine in 1981 (shared by David Hubel and Torsten N. Wiesel). In that lecture he describes the downfall of the notion that the right hemisphere is not conscious, a downfall which began by the middle seventies.

Alcock then turns to perception and makes the point that many of the very automatic features of perception are learned—as are the ways in which we categorize and label experience. Despite this, he says, we cannot ordinarily verbalize the rules by which an object is assigned to a category. The category and related label used for an experience will later influence recall of that experience, and this may be an especially important factor in the case of anomalous experience.

He also notes that much of the information entering our sensory systems does not receive conscious attention—though he uses the less theoretically neutral terminology of its not reaching "consciousness." As applied to the ostensibly (but not actually) paranormal, he states that we are capable of responding to information which we do not notice, and we may infer a paranormal origin for it. And finally, because our expectancies or psychological sets influence the way in which information is organized into percepts, past experience and beliefs can greatly influence what we come to believe we have experienced, especially when the stimulus being perceived is ambiguous.

Alcock discusses evidence that, just as our percepts depend upon active, constructional processes, so does the process of remembering, for in remembering, the "memory" is actively constructed—e.g., on the basis of the labels we earlier used for the events. When we try to recall a puzzling, possibly paranormal event, our memories may lack the details necessary to demonstrate its proper, normal explanation, especially if our labels at the time of the experience now cause us to fail to recall information relevant to the normal explanation. Regrettably, Alcock fails to point out that in investigating reports of spontaneous psi events researchers have recognized such problems since the very beginnings of our field and have developed specific methods of minimizing the probability that the conclusions of research are biased by such difficulties. His general tone could lead his readers to believe that psi researchers have never thought of many of the difficulties which he raises.

When discussion turns to transcendental experience, Alcock's biases are very evident and his analysis is particularly superficial. While making the valid point that mystical experience will be interpreted, to some degree (though he does not qualify it in this way), in accord with the experiencing person's beliefs and, possibly, needs, he fails to acknowledge that some persons have radically changed their

beliefs because of such experiences. Nor does he discuss the important point that many belief systems may be derived at least in part from persons' earnest attempts to understand the experiences they are having. Alcock may not fully appreciate the genuinely novel character of such an experience for many persons and the creative influences such an experience may have upon their beliefs.

Alcock outlines some familiar concepts concerning such experiences, concepts set forth by persons such as A. M. Ludwig, A. J. Deikman, R. Ornstein, and H. Benson. Alcock's blatantly humanistic bias is nowhere more evident, however, than in his discussion of cognitive labeling (attribution) concepts as applied to such experiences. "It is the spontaneous experiences, often triggered by experiences of pleasure or beauty . . . that are most likely to be *misinterpreted* [italics added] since they seem to arrive without cause" (p. 79). If he were being objective here, he would say something like ". . . that are most likely to result in efforts by the experiencer to find an explanation since they seem to arise without cause." How can he assert so cockily that such experiences have been *misinterpreted*? Such a statement surely arises from a trenchant metaphysical bias, one which is entirely too evident throughout this entire volume. The explanation adopted by the experiencing individual may be correct or incorrect, but it is difficult to know why Alcock can feel so sure that, if it is a religious or transcendental explanation, then it must be incorrect.

In his analysis of transcendental experience, Alcock opines—following C. L. Kleinke—that what causes a person to have an affectively positive experience in meditation is not the state of mind itself, but the positive label the individual places upon the outcomes which result from the factors involved in meditation. As an example, Alcock cites some very relevant observations in work by J. J. Lynch in which individuals who erroneously believed themselves to be in the "alpha state" and who believed that state to be ecstatic reported very moving and "meaningful" experiences.

This reviewer agrees that such an analysis may be very accurate in many situations, especially during biofeedback when the subject's attention is focused on an external feedback signal rather than exclusively on what is being experienced internally. But it would be gratuitous to assume that the internally generated experiences of at least some meditators are not intrinsically pleasant or otherwise special—without the need for secondary reinforcement via a label! Some meditators may actually be learning to stimulate the portions of the brain needed for just such experiences and to do so in a relatively direct fashion. The existence of brain areas which, when stimulated,

can produce intense pleasure has been known for some time, and some commentators have suggested particular neuronal circuits as being directly involved in ecstasy or lesser, positive experience during altered states and meditation (e.g., Mandell, 1980).

Though the attributional analysis may be important in explaining reports of "highs" associated with simple biofeedback procedures (and in some other situations), something else is probably needed to explain the internal states sought by yogis and other mystics. (This is not to say that it plays absolutely no role there.) Why should the training of a yogi for such states be as time consuming as it is if the explanation is simply an attributional one? It is only fair to note, however, that Alcock is not saying that the attributional explanation taken alone is sufficient in all instances to account for the affective report; he cites, for instance, biochemical changes produced by various means. This reviewer does feel, however, that Alcock relies too heavily upon that particular analysis.

Alcock then discusses Herbert Benson's "relaxation response" and, like Benson, uses it to try to explain mystical experience. Again, it seems to this reviewer, Alcock leans far too heavily upon this rather simplistic explanation, for careful study of both the mystical literature and the scientific studies of transcendent experience do not support a simple relaxation explanation as underlying the basic kinds of experiences. Relaxation may play an important role in eliminating some of the distractors which would otherwise impede progress toward the mystical goals, but surely relaxation per se is not the essential ingredient here.

Alcock feels that most mystical experiences can be accounted for by a common physiological state combined with whatever labeling is suitable for the situation. He quotes William Sargant's opinion that the same physiological processes underlie most forms of religious and mystical experience, as well as hypnosis, drug states, and sexual excitement. This simplistic psychophysiological thinking makes Alcock, along with Sargant, use a single, poorly articulated, explanation for possession phenomena, unitive mystical experience, speaking with tongues (glossolalia), various phenomena of "enthusiastic" religious experience, utterances of mediums and prophets, faith healing, witch doctoring, hypnotic behavior, behavior under certain drugs, and sexual arousal! Such an "explanation" is surely so vague as to be next to scientifically useless and, possibly, is harmful because of its misleading character as applied to so many different areas. It is extremely misleading, for example, as applied to the glossolalia investigated recently by Spanos and Hewitt (1979), which was accompanied by no

evidence of "hyperaroused trance." Efforts to "explain" a wide range of phenomena through a very vague and ill-defined concept such as Alcock's "common physiological state" can properly be termed pseudoexplanation. (In fairness to Sargant, his main explanatory construct is somewhat less vague than Alcock's, though it is certainly used without sufficient consideration of the specifics of the many behaviors and internal states which he would explain.)

The final segment of Chapter 4 is concerned with experience of the "paranormal." Alcock says, in effect, that we should not be bothered with thinking about paranormal explanations for odd experiences we may have, for when the human information-processing system is properly understood, it is only to be expected that we will find some sense of strangeness in some of our experiences. Here, as elsewhere, he misleadingly insists upon labeling as "transcendental belief" any belief that something has occurred which is inexplicable by current scientific thinking.

Emotion, according to Alcock, temporarily weakens our thinking ability and may thereby encourage us to believe that we have had a paranormal experience. He urges us, therefore, not to be impressed by anecdotal evidence. He fails to note that in the annals of psi research there are excellently documented spontaneous cases of sometimes impressive character. This is doubly ironic because the warning never to take seriously any anecdotal evidence is followed by a statement that even though early parapsychologists devoted much effort to the investigation of anecdotes, they found repeatedly that checks of story details against objective records revealed such glaring discrepancies as to discredit all parts of the report! While this sometimes happened, some such cases were amply confirmed by careful investigation and cross-checking. But Alcock gives no hint of this, nor of the fact that the method is still proving successful (Stevenson, 1970). (Of course, what one can make of even the best documented spontaneous case is still debated among researchers.)

Next comes a series of exercises in demolishing straw men. The section, for instance, on "ghostly phenomena" simply does not address itself to the characteristics of the traditional, parapsychologically interesting, apparition cases. It is flippantly irrelevant to almost everything of importance which has been discussed historically in this connection. Similarly, Alcock's treatment here of "near-death experiences" ignores everything of parapsychological interest, namely, the few instances of seemingly paranormal information obtained during them. Turning to déjà vu, he claims that this has been explained (apparently by parapsychologists) as due to precognition and reincar-

nation, though he cites no evidence that we have used those ideas in that connection. Alcock's own attempt to explain déjà vu is disappointing; it does not explain a persistent feature of such cases, namely, that their frequency declines with age (McCready & Greeley, 1976; Palmer, 1979). His discussion of spontaneous "precognition"—his own term, since parapsychologists would not use this term for the kinds of cases discussed here—cites cases that give an erroneous impression of precognition having occurred. This discussion is useful, as is the subsequent consideration of similar cases of "telepathy" (pseudotelepathy), because it could alert inexperienced readers to some of the pitfalls of leaping to conclusions from personal experiences. It might even allow them to spot sources of normal causation for pseudopsi in their own experiences. Some of Alcock's examples are excellent in this regard. Under "psychokinesis" (pseudopsychokinesis), he appropriately discusses table tilting, which he terms "table-levitation," since nonpsi events in such a setting are often misconstrued as psychokinesis by novices. His inclusion here of the use of the ouija board and of the dowsing rod is puzzling since such events are usually not construed as psychokinesis, even by novices, whatever may be the explanation put forward for them. One can only agree with Alcock that ". . . until such possible normal explanations can be eliminated, there is little reason to fall back on paranormal ones" (p. 88).

The thrust of Chapter 4 is that we may be influenced by sensory information which we were not aware of receiving and which we cannot recall having had access to, and that, partly for that reason, we are inclined to draw false or misleading inferences about the causes of our experiences, attributing paranormal or transcendental causation to them.

Chapter 5, "The Fallibility of Human Judgement" continues in a somewhat similar vein. Here is found a discussion of how our reasoning processes are subject to error and bias and how these factors can contribute to interpreting normal events as paranormal. This chapter gets off to a fine beginning by making some points which any person considering the possibility of paranormal events—and especially anyone who thinks of doing research in this field—should take to heart. For example, most people encounter the following problems in statistical thinking: A tendency to underestimate the probability of even fairly commonplace events (like meeting someone with the same birthday as oneself); belief that the probability of an event changes in an upward direction if it has not happened recently (the "gambler's fallacy"); a failure to recognize the problem of regression to the mean

(for which Alcock gives an easily understandable example); and the difficulty of inferring the effects of a treatment (of an illness, for instance) when the factor intended to be influenced follows cyclical variations in magnitude.

Alcock then discusses how inferences about causality are based on assumptions that are influenced by past experience and how, according to Piaget, the adult sense of causality is developed in stages. Alcock maintains that during periods of stress we tend to regress to earlier, magical stages of thinking which are irrational and which involve assumptions of paranormal causation (e.g., "prayer helps"). The tendency, found even in some adults, to infer causality for two events which are temporally contiguous receives much discussion.

It is at this point that Alcock begins to cast unjustified and undocumented slurs against the parapsychological community. For example, in discussing the logical fallacy of *post hoc, ergo propter hoc* ("after the fact, therefore because of the fact"), he cites E. G. Romm as having referred to a "prominent parapsychologist" (unnamed) who argues that when people step into the shower, they unconsciously use psychic energy to cause others to phone them! The use of the term "prominent" makes it appear that if the best among parapsychologists do this, the others must do much worse! The failure to name the alleged parapsychologist insures that the alleged incident cannot be refuted and makes the slur more likely to generalize to other parapsychologists.

Alcock discusses the well-known difficulty in extinguishing the learning produced by coincidences and how this is related to the tendency to remember concordant events of this kind but to forget about nonconcordant ones (e.g., to remember the time a war followed the appearance of a comet but to forget the times when this did not happen). He cites experimental evidence that persons tend to perceive correlations between events when none exist, a circumstance which may be related to the tendency to notice and/or to remember only nonconcordant cases. He also notes how a priori biases concerning what kinds of things go together can bias the perception of such correlations, and he discusses various possible explanations for the general difficulty in estimating correlations.

He concludes this chapter with a discussion of "the illusion of control" (i.e., factors which enhance a person's feelings that something other than chance is happening or will happen in a PK-like setting or in an ESP task) and the "illusion of validity," factors that illegitimately increase the feeling that one's predictions are accurate. In the latter case, Alcock's example and discussion are somewhat confusing and

unclear. A much better example than the one he used would have been the drawing of a conclusion about the occurrence of ESP from a probability value calculated by using multiple judges for a single set of data and treating those judges' judgments as though they were independent, for the purpose of deriving the probability.

Science or Pseudoscience

It is only with Chapter 6, "Science or Pseudo-science: The Case of Parapsychology," that Alcock turns his discussion squarely on parapsychology as a science and its empirical and conceptual status. He concludes that parapsychology, at least in practice, is functioning as a pseudoscience.

He sets the stage for his arguments by making blanket and undocumented statements about parapsychologists and their motives and beliefs. For instance, he flatly asserts that "most parapsychologists express the feeling that materialistic views of the universe are too limited, that there is another metaphysical reality which has been too long held separate from science, and that the scientific pursuit of evidence of this spiritual reality, through parapsychological research, will ultimately lead to an integration of materialistic and spiritual beliefs" (p. 106). Oddly, he cites no poll of parapsychologists to support this claim. He further states that such feelings make us ambivalent about science—ambivalent because of what we allegedly hope science will do for the worldview even while we recognize that scientists distrust the tendency to bring metaphysics into science. Furthermore, we are alleged to defend ourselves against the criticism which thus emerges by arguing that our ideas are "premature," that they await a further scientific revolution to have their real value recognized. (This reviewer is not denying that such statements may have some validity as applied to particular parapsychologists. He is simply questioning the fairness and legitimacy of characterizing all parapsychologists, or most of them, in the same way in the absence of clear evidence that such characterization is correct. Such blanket characterization of parapsychologists too often typifies the writing in this book and gives it a highly propagandistic tone.)

Alcock first tries to demolish the claim that parapsychology merits the label "premature science," that is, a legitimate science whose time of recognition has not yet come. First, he notes, science comes eventually to accept genuinely premature ideas because considerable, undeniable evidence is finally amassed which demands acceptance of

claims which were once premature because they could not be connected to "generally acceptable knowledge by a series of simple logical steps" (p. 108). Furthermore, science has repeatedly come to accept theories which were initially viewed as ridiculous. This was because of sound evidence and because the theories proved useful for explaining anomalies which emerged in an accepted body of knowledge. He states that physicists are not now observing anomalies in their research data which make them uncomfortable in such a way that they are going in search of parapsychological concepts and empirical generalizations in order to explain what they are finding.

There are several problems, however, with this line of argument. How can Alcock be so sure that because physicists are not beating down the doors of the parapsychology laboratories, parapsychology is not premature? Certainly, if they were now approaching parapsychologists in this way, it might legitimately be seen as constituting evidence that in the past parapsychology has been legitimately premature but is soon to emerge from that status. It does not, however, follow logically from the fact that physicists are not beating on parapsychologists' doors that parapsychology is not presently premature. Such a conclusion would require some additional—and, probably, unrealistic or arbitrary—premises. Alcock seems to forget that (a) the kinds of events studied by physicists and/or the ways in which they are studied may help to prevent or to make unlikely psi influences on the outcomes of those studies; (b) if psi events occur in physical laboratories, they may emerge, very often, as failures to replicate which appear across laboratories and which may be "explained" as instrument or measurement errors; (c) the direction one looks for an explanation of an anomaly depends on one's past training and predilections; and (d) anomalies may be best noticed when they are systematic (e.g., are a function of a specifiable set of physical circumstances), but psi events may not be of this character and, thus, may tend to remain unnoticed or not immediately thought of as any researchable, systematic, source of error. Also, some bona fide physicists (e.g., Helmut Schmidt) are reporting anomalies in data gathered from physical experiments which employ technological devices; they are reporting these at meetings of physical societies; and they are asking whether we do not need some new models to encompass such events (and Schmidt himself is proposing one). Alcock's case for the nonprematurity of psi research is at best flimsy.

Next, Alcock, who is a psychologist, examines the claimed relationships between parapsychology and modern physics. He engages in a form of verbal legerdemain in which everyone who tries to link psi

phenomena and physics is termed either a "parapsychologist" or a "paraphysicist" despite training in a nonpsi research area such as physics. A physicist, for example, is no longer labeled as such because of work in the psi area. Indeed, frequently throughout the book scientists who have received their major training in an area other than parapsychology are not granted the title representing their area of training if they have become tainted by addressing questions in the psi area. They are labeled "parapsychologist" or "paraphysicist" in the same sentence in which a person with similar credentials receives proper labeling. Scientists who are opposed to the reality of psi events receive their proper professional titles and are not called "antiparapsychologists" or "goats," even while, ironically, the term "parapsychologist" is used for some persons who surely do not qualify for that designation.

Alcock gets off to an exceedingly bad start in this section by making several misleading statements: (a) He states that some writers who are trying to link psi phenomena to quantum mechanics are trying to understand psi phenomena "in terms of *current* [his emphasis] physical theory" (p. 111). This is misleading because all major theorists in this area have clearly indicated that they are not saying that standard, traditional interpretations of quantum mechanics can explain psi events; they are, instead, saying (e.g., Walker, 1975) that an extension of some particular interpretation of quantum mechanics might be thus used. (For example, Walker extends the Copenhagen interpretation of quantum mechanics by making assumptions about consciousness as a hidden variable); (b) Alcock ignores the distinction between statements (e.g., by Ehrenwald or LeShan) which suggest merely a greater compatibility of psi phenomena with the general worldview associated with modern physics (than with earlier physical worldviews) and statements by physicists who are making specific efforts to explain psi phenomena in terms of some extension of modern physics. And (c) he ridicules serious efforts at theorization in this area by saying that such efforts "may appear convincing to the individual totally unfamiliar with relativity and quantum mechanics" (p. 112)—a highly pompous statement from a nonphysicist, especially considering the fact that the individuals proposing such ideas are themselves physicists! Though Alcock, who is a psychologist, assures us that specific developments in modern physics, such as the Einstein–Podolsky–Rosen paradox, can have nothing to do with psi phenomena, a Nobel laureate in physics, Brian Josephson, has discussed such developments—specifically mentioning the EPR paradox —as having possible linkages with the paranormal (see interview with Josephson by Gliedman, 1982).

Alcock's discussion of the psi–physics issue is further muddied by his efforts to link it with the work of those physicists and nonphysicists who see physics as revealing the truth of some form of metaphysics found in Eastern mysticism. Actually, those physicists who have been most concerned with psi phenomena seem to this reviewer to have stayed away from trying to make these broad connections with Eastern religion.

Fortunately, when discussing the belief that tachyons can provide a basis for precognition, he attributes it to "some proponents of the paranormal" (p. 115), not to parapsychologists. Also, he notes that there are persons in parapsychology who are critical of the attempts to reinterpret psi phenomena in terms of quantum mechanics.

Perhaps Alcock's basic message in this physics section is that the concepts of contemporary physics cannot legitimately be seen as strengthening the case for the reality of psi phenomena. In his view, arguments about physics should not be allowed to remove the focus of the psi controversy from the methodological and statistical issues. Unfortunately, Alcock never carries such thinking to its logical end by reviewing the methodology of the actual productive lines of research in parapsychology. Instead, he cries, "Where is the evidence that there is any paranormal process which needs to be explained?" (p. 116) and steadfastly refuses to examine the actual evidence.

A failure of the physics section is that Alcock does not discuss or even cite a single one of the modern theories of the physicists interested in psi phenomena. Nor does he point out that such theories—which physicists debate like many other issues in theoretical physics—are testable and are leading to some specific, meaningful research. He simply has failed to treat such theories explicitly, thoroughly, or fairly.

Alcock's next task is to show that parapsychology is not a science. He begins by comparing parapsychologists' use of statistics to infer the reality of psi with the fact that some persons interpret various "mundane events . . . as evidence of the vampire's existence" (p. 116). He further states that parapsychologists—not just *some* parapsychologists—"turned to laboratory studies in the hope of *demonstrating* [Alcock's italics] phenomena that they *a priori* believed to exist" (p. 116). Notice the implication that parapsychologists obviously were not being scientific because we started out in the hope of "*demonstrating*" (not *testing*) beliefs which, he alleges, were held in advance. He does not tell his reader how he knows these things or even mention the possibility that some (most?) investigators turned to laboratory work in order to examine in the most adequate way possible whether or not there was any reality to such claims. Of course, the effect of such

statements is to discredit the research by discrediting psi researchers as individuals who are genuinely interested in what the scientific method can reveal about reality.

Alcock next turns to examining the status of theory in parapsychology. He begins by confusingly mixing together two separate issues, the fact that ESP and PK are negatively defined and the claim, following C. D. Broad, that paranormal events, by definition, conflict with the "basic limiting principles" found in science concerning the world.

What seems to bother Alcock most about the negative definitions is the problem of showing that all forms of sensory communication, both known and unknown, can be ruled out and thus the occurrence of ESP firmly established. Certainly, we cannot prove that no such unknown sensory channels are operating, but that fact alone does not make the scientific study of ESP unmanageable or uninteresting. We can say that in a given study we have ruled out the known kinds of sensory communication and still have evidence, based on conventional rules of statistical inference, that something other than "chance" is operating. This suggests that, with a specified risk of being wrong, we need to pursue further the nature of the anomaly thus observed.

The fact is that scientists regularly make inferences based on the assumption of negative propositions. They do it every time they make an empirical generalization or use the process of induction. In order to make any empirical generalization, we assume that there are not exceptions to it. In the case of empirical generalizations which imply causation, we infer that there are no unknown factors responsible for the observations in question. True, we use massed evidence for such inferences, but, nonetheless, we have, in the end, to make the assumption that there are limitations to the possible alternative explanations (Pap, 1949/1953). Making such an assumption is not uncongenial to the spirit of scientific inquiry, for all conclusions in science are regarded as tentative and thus subject to later revision in light of new evidence. Earlier generalizations may be qualified, modified, or replaced because of such evidence.

In this reviewer's opinion (Stanford, 1974, pp. 141–142) there is a much more troubling aspect to parapsychologists' negative definitions of "psi events" than not being able to rule out all conceivable alternatives to the conclusion they wish to draw:

> The thought of trying to scientifically validate a proposition like, for example, "There exists extrasensory communication of thoughts" or "There exists extrasensory communication of the faces of cards" boggles

> the mind. This kind of proposition is essentially an existential proposition—"There exists . . . (such and such)." It says nothing of the functional characteristics of the phenomenon supposed to exist. It only tells us we should observe something which could be recognized or construed as something which is itself negatively defined. . . . How can you empirically test a proposition which itself gives you no clues as to when the crucial observation will or will not occur? Such a proposition is not capable of experimental demonstration because it is incapable of experimental falsification. . . . One can test the validity only of constructs regarding the observations we label psi phenomena. In short, we should treat "ESP" as a specific, delineated construct and thus deliberately develop it and study it as such—and change the construct as necessary. (pp. 141–142)

Parapsychologists have sometimes failed to recognize that in parapsychology the raw data are *anomalies* and that the question is how to interpret those anomalies. Any interpretation which indicates under what circumstances they should and should not be observed is capable of scientific test. Any interpretation which does not do this is incapable of scientific test. (The latter, incidentally, includes the claim that the anomalies are due to "unknown sensory communication" unless that interpretation can be more clearly specified.)

Alcock, citing Flew, claims as a "fact" (p. 118) that subjects who come up with ostensible extrasensory information cannot recognize that they are at that moment receiving information via ESP and cannot distinguish those moments from nonpsi responses. Alcock should have taken a look at the actual, rather extensive literature on "confidence calls." Despite some mixed results and problems of interpretation, such calls do show promise of utility for isolating responses likely to have been influenced by psi (Palmer, 1978). In a related vein, Alcock states, "The laboratory evidence of ESP is limited to extra-chance scoring; it is impossible to tell *which* [his emphasis] of the various hits are 'extra-chance' ones (i.e., due to ESP) and which are just 'chance'" (p. 118). Such remarks ignore the quite consistent extrasensory response-bias literature (reviewed or discussed by Carpenter, 1977, and Sargent, 1981) which suggests that counterbias responses are more likely to be psi mediated. Signal detection theory (Swets, 1973), as applied to extrasensory tasks, would lead us to expect, whenever any sensitivity is present, a greater rate of hits on counterbias responses because of a reduction in false alarm rate for such responses; that is precisely what is found (first discussed in Stanford, 1967). Some progress seems to have been made in pinpointing trials likely to be psi mediated. These issues might be further clarified if the confidence-call phenomenon were examined in light of

the response-bias findings and the concepts of signal detection theory (Stanford, 1982).

Alcock follows such remarks by citing statements by Flew, Rogo, and Beloff which essentially ignore the vast literature of experimental parapsychology. He should, instead, have addressed himself to the kinds of evidence actually found in the parapsychological literature, especially the process-oriented research. Such studies can now more easily be located because of recent review articles in books and journals. But characteristically, Alcock had rather ignore the evidence —and damn it by indirect means—than to confront the issues raised by it and the methods involved in particular studies. For him ". . . the 'body of knowledge' in parapsychology is all but non-existent" (p. 119). Despite such a disclaimer, Alcock's failure to review the bulk of process-oriented research in the field makes this volume inadequate as a parapsychology text, regardless of how the publisher advertised it.

One of the most outrageous sections of this volume is the one entitled "The Lack of 'Competition'" in Chapter 6. Completely ignoring the several lines of specific, testable theoretical development in contemporary parapsychology, Alcock asserts that "unfortunately, parapsychology lacks anything at all that resembles a serious theory and thus lacks *competing* [Alcock's italics] theories" (p. 120). The reader never learns from all this about the systematic lines of research which have developed from the several contemporary models and theories in parapsychology; nor does the reader learn about the discussion from within this field of the adequacy of these various theories and specific assumptions within them. Instead, Alcock continues with another undocumented, false, and highly insulting blanket statement:

> There is little or no "competition" among parapsychologists, and thus there is little sorting out of wilder speculation from more conservative notions. In consequence, there is little motivation towards criticism within the field, little effort to demonstrate that a given experiment must have been poorly controlled because its results go against someone's pet theory. (p. 120)

The first statement of the above quotation suggests that Alcock might be ignorant concerning what actually transpires within professional parapsychology. There may be no other field in which there is more internal criticism than in parapsychology. Some of it may derive from conceptual differences among its workers, and some may derive simply from a desire to insure rigor. (Alcock does not seem to acknowledge that strident and effective criticism often results from

other than theoretical differences, in parapsychology as elsewhere. Also, because critical commentators on an experiment may be reluctant to admit any theoretical biases which engender their criticism—and, indeed, may be unaware of them because of the degree to which they constrain their thinking—it is often the case that one can infer the genesis of such criticism only on uncertain and, sometimes, rather subjective grounds, such as the particular "alternative interpretations" suggested for a study or the past history of the critic's own work. This is often the case in any science.)

A relatively recent example which thoroughly refutes the Alcock claim concerning criticism is the detailed examination (Stanford, 1977a) of the conceptualization, the empirical findings, and the conclusions of an attempt to apply learning theory to parapsychology (Tart, 1975), the response to such criticism (Tart, 1977), the counter-response (Stanford, 1977b), more criticism of those claims from other workers (Gatlin, 1979, Kennedy, 1980b), replies (Tart, 1979, 1980) and a counterreply (Kennedy, 1980a).

The informed reader of this volume must wonder what Alcock really knows about parapsychology. His familiarity with the field appears to be largely secondhand, and his criticisms are often borrowed from others. In his large bibliography, which consists of approximately 490 entries, only 22 or 23 articles are listed from the empirical or experimental papers which have appeared in the refereed journals affiliated with the Parapsychological Association. An additional 20 articles, mainly of nonempirical character, are listed from a nonrefereed parapsychological journal which is not P. A. affiliated. Among the journals which he lists under "Suggested Readings" (p. 211) as "devoted to experimental parapsychology" are *The Journal of Research in Psi Phenomena*, a Canadian journal which, it appears, ceased publication some time ago and *Parapsychology Review*, a foundation-sponsored journal which can hardly be described as concerned mainly with experimental research.

While Alcock admits that there is some "critical commentary" (p. 120) in parapsychology, he implies that it is centered around what constitute the "real" psychic phenomena. Although there is some such commentary, the bulk of the extended critical writing in recent years has centered around process-oriented work (i.e., work intended to address particular questions or hypotheses about psi function) and theoretical developments aimed at fostering such work. In advancing his claim that parapsychologists debate, primarily, existential questions concerning particular phenomena, he alleges that Charles Tart believes in "astral projection." Ironically, Tart's coining of the term

"out-of-body experience" shows his eagerness to avoid strongly loaded terms such as "astral projection," a metaphysical, rather than a scientific, term.

Alcock next discusses what he says are our responses to critical views and negative evidence. His continual willingness to saddle parapsychologists with negative stereotypes is again in evidence: "Discussions of a particular aspect of parapsychology by its proponents (apart from those of a few 'critical' parapsychologists) rarely even mention the existence of negative evidence or give any serious consideration to critical views" (p. 121). (Note his tendency, once more, to compartmentalize some leading parapsychologists so that they do not mar his stereotype of the field in general.) He continues, ". . . it seems that in parapsychology critical viewpoints are almost never given serious treatment. . . . Parapsychologists seem critical only when discussing some putative paranormal phenomenon that *they* [Alcock's italics] do not happen to believe in" (p. 123). Alcock seems to suffer either from a bad case of selective attention or from great ignorance. His stereotype is simply and grossly wrong. While there are certainly occasional lapses among us in these regards and while we should not object to being pinned with those actual lapses, it is Alcock's seeming effort to create a strong and negative stereotype which marks his words as highly propagandistic and unfair.

His words near the very end of this section seem even more calculated to hurt the reputations of parapsychologists at any cost: "In summary, parapsychologists usually fail to report negative outcomes and skeptical criticism in their discussions of the paranormal" (p. 123). This—especially the remark about negative outcomes— amounts to a charge of rampant intellectual dishonesty. More will be said later about the reporting of "negative outcomes," for this charge is easily refuted.

Parapsychological claims, Alcock feels, are basically untestable because there are so many ways of rationalizing failures—especially failures among skeptics. First, he considers the "experimenter effect." He alleges that instead of considering whether investigators who get positive results are doing so because of experimental flaws, parapsychologists simply allege that differences in outcomes across experimenters are due to psi-mediated experimenter effects. But in fact, careful examination of the contents of refereed journals in the field shows that parapsychologists give consideration to a multiplicity of possible explanations for divergent outcomes, including the possibility of flaws.

Alcock alleges that parapsychologists invoke experimenter-psi expla-

nations in an effort to wring psi evidence out of studies in which there is no legitimate psi evidence, and even studies with "null" results are supposedly regarded as evidence of experimenter psi and, thus, of psi in general. This is surely a misunderstanding on Alcock's part. While it is true that certain writers do discuss experimenter psi vis-à-vis failures to replicate, this reviewer is not aware of any evidence that such individuals regard this kind of circumstance as providing any evidence of psi reality. Rather, they already assume psi reality and are simply discussing one possible interpretation of differences in outcomes. Once again, Alcock flippantly throws out an unreasonable and undocumented allegation.

On the other hand, discussion of interexperimenter differences in outcomes probably has scientific value only when it points toward ways of potentially controlling the occurrence of such differences or, at the very least, ways of pinpointing their causes. Alcock makes an important and valid point in this section, and it is hoped that it will sink in deeply among those from the ranks of parapsychology who believe in what might be called the "omnipotence of the experimenter effect" or, perhaps, the "inevitability of the experimenter effect"—this referring to psi-mediated experimenter effects. There are those parapsychologists (hopefully, a relative few) who earnestly believe that the attempt to exercise experimental controls in psi studies—including controls for "experimenter effects"—is in principle fruitless because there is no way of controlling for the inevitable experimenter-psi effects, which will ultimately contaminate the results, and thus the conclusions, of the psi studies. This reviewer's position, which apparently accords with Alcock's, is that if this is parapsychologists' belief, they should close up shop today as scientists, as parapsychologists, for their belief systems logically undermine the application of scientific methodology for the empirical solution of process-oriented questions. Science, as normally defined, would be impossible, even laughable. To justify what is being done in parapsychology, it is necessary to adopt as a working hypothesis that the omnipotence or inevitability position is wrong.

This is not to deny that experimenter-psi effects can and do occur. Rather, this is to say that they occur to differing degrees under differing, identifiable, circumstances so that it is possible, at least in principle, to reduce or eliminate their occurrence. Even the belief that cross-laboratory replication attempts will weed out the genuine effects from the spurious experimenter-psi effects is based on assumptions concerning some limits on psi function.

It is easy to understand why Alcock, like some other writers,

regards parapsychology as a scientific program which is intrinsically absurd. Such writers are assuming that if we admit psi into our worldview, we are living in a totally magical world, one where order and lawfulness are, at best, artifactual and tentative. This would seem to undermine the entire scientific process. There is, however, a more cautious and reasonable basis from which we can legitimately give scientific consideration to psi events, the position outlined in the paragraph above. That position cannot presently be demonstrated as either correct or incorrect, but, for scientists interested in studying psi, this is the only ground upon which to stand.

Alcock's final evidence for the "non-falsifiability" of "parapsychological beliefs" (p. 124) is that parapsychologists believe that "strict scientific procedures inhibit psi, that controls must be somewhat loose in order to allow its manifestation" (p. 124–125). As his primary support for this false charge he cites a remark by "physicist-parapsychologist" Fritjof Capra from an undated mimeographed paper. Insofar as this reviewer is aware, however, Capra is not a parapsychologist and has not published in the refereed journals of this field. Nonetheless, Alcock is willing to charge the entire field with the scientifically unthinkable on the basis of this kind of material! Though he quotes the Capra statement, he does not quote any material at all to support his charge that J. B. Rhine had made a "similar argument" (p. 125). Rather, he quotes a vicious and bitter remark by B. F. Skinner which is supposedly a reaction to Rhine's alleged belief that psi phenomena are suppressed through the application of scientific methodology. If psi phenomena were thus suppressed and Rhine were aware of it, he would have been the scientific con-man of the century to have continued to raise funds to support a field which could not exist because adequate scientific controls prevent the occurrence of the phenomena. In fact, the Rhine position was that there are no "intrinsic" effects of scientific controls in suppressing psi phenomena; rather, there are psychological ones which occur when an individual who is unaccustomed to scientific testing or who has been working under more lax conditions is now, suddenly, subjected to rigorous controls. Rhine recommended that the shift toward more adequate controls be done gradually so as to allow the subject to adjust to it. The point that sudden changes in procedure or even changes in the number of observers present can *temporarily* suppress performance until the subject has a chance to adapt to it are evident in Rhine's first monograph (1934/1964, p. 178). There is absolutely nothing in such statements to support the Alcock charge that Rhine viewed psi events in a way which made their existence untestable.

In this section on testability Alcock laments that the sheep–goat effect, as he construes it, would, if taken seriously, close off the skeptic "from experiencing the paranormal directly" (p. 125). His discussion of the sheep–goat effect here and elsewhere in this volume is very brief and superficial. It is also misleading because he gives his reader the impression that the effect is defined in terms of accepting the reality of ESP or not, rather than noting that various kinds of questions have been used to make "sheep–goat" comparisons, and with varied success (Palmer, 1971); he does not refer his reader to the excellent review of this topic which was just cited. He does suggest here and in Chapter 7 that the sheep–goat effect might be explained by a greater readiness on the part of sheep to make conscious or unconscious use of nonpsi cues which are (presumed to be) available. This cavalier effort to dismiss the sheep–goat effect on sensory-cue grounds is not documented by any discussion of the experimental conditions which would have allowed this hypothetical cueing in particular studies; and Alcock certainly provides no evidence that sheep would, in fact, be more likely to use such cues even if they existed. His discussion is entirely and conveniently suppositional.

Alcock next notes that the skeptic might consider decline effects as evidence either of regression to the mean or that the subjects were less likely to cheat or use sensory cues as, over time, conditions were tightened up. However, what has generally been claimed as the important scientific evidence on the issue has concerned within-testing-unit declines, not declines over the larger blocks of time which would be relevant to the second of Alcock's proposed explanations. An example is the quarter-decline effect in the dice PK work. Nor is the regression-to-the-mean explanation applicable to the systematic studies of declines across replicated blocks of data, such as in the PK quarter-decline work. Such an analysis would, of course, be applicable if workers selected out of larger masses of data only those where subjects "started good" and then exclaimed about their declines over time. But who does this?

A single paragraph is given to the combined topics of psi-missing and displacement. These Alcock treats by analogy with conjurers' tricks; by their use we can produce "psi" where there really is none. This reviewer does not defend the use of displacement analyses unless there is a compelling a priori reason for anticipating such an effect. Generally, they are much abused, though this transgression has been at the hands of only a few researchers. On the other hand, Alcock's easy disposal of psi-missing is ill-advised and unfair; he fails to acknowledge that examination of psi-missing is perfectly legitimate

when handled with statistical propriety and that it has proven to have much scientific value, especially when its possible causes are studied systematically, as in the work on consistent missing (or consistent confusions between particular calls and particular targets).

As further evidence of the untestability of psi hypotheses, Alcock finally mentions John Taylor's observation of children being unable to do their ostensible macro-PK when watched, the "shyness effect." The implication of this would seem to be that we spend our time making "laws" of our inability to find psi under proper test conditions. But Alcock provides no evidence for generalizing such attitudes beyond the investigator whom he actually cites, though the tenor of his subsequent remarks suggests such generalization.

It would appear from Alcock's remarks near the end of his "testability" section that no amount of data would convince him of the reality of psi. Regarding the "psi hypothesis": "There are always other possible hypotheses that would lead to the same prediction" (p. 127); he makes it clear that these alternative hypotheses include scoring "above chance" due to chance fluctuation and the possibility of sensory cueing. Once again, the last resort of the professional skeptic is to say that there *must* be some other explanation than psi regardless of the conditions in a particular study—note the statement that there are "*always*" alternative hypotheses available (p. 127). Alcock, however, tries to make his unwillingness to accept any evidence look like a noble invocation of a cardinal scientific principle, namely, that the prediction from a hypothesis must be such that it is "unlikely that anyone should correctly arrive at such a prediction unless they used the hypothesis in question" (p. 127).

Alcock then addresses our supposed "relationship with other areas of research and theory" (p. 127). Once again, he attempts to undermine our credibility as scientists: "Moreover, the extent to which the vast majority of parapsychologists are ignorant of or disinterested in 'normal' science is striking" (p. 127). Note the words "vast majority." Has he conducted a survey? Alcock should document this insult or publish an apology for it. Both the canons of science and ethics demand it, for he has here published an undocumented and potentially very hurtful statement.

He continues by alleging that parapsychologists have no interest in studying basic issues in perception in parapsychology. He ignores a considerable body of research on possible perceptual factors in psi research, work which has gone on sporadically over several decades and continues into the present. He attempts to deny any "overlap" of psychology and parapsychology, as well as physics and paraphysics. As

concerns the psychology–parapsychology connection, he again ignores the thrust of considerable process-oriented psi research.

In this same vein, Alcock alleges that psychologists and physicists are not turning to parapsychologists to gain insights into the problems they are studying. This may well be true, but while most of them are not inclined to do so, they probably should. The history of psychology is rife with examples of the failure to borrow methods and concepts (and even findings) from other areas which might have been helpful. The failure to do so has come from a failure to notice or to take an interest in those ideas or methods simply because they were unfamiliar, were regarded as cast in a "different language," or were not in the theoretical or methodological mold to which the outside investigator was accustomed. This did not mean that they had no potential use or interest for that investigator. For example, the methods and ideas of signal detection theory (see Swets, 1973), which were first employed within psychology in sensory psychophysics, took much longer to find use in various other areas of psychology, even though the generality of their potential usefulness is great. Today they are still not used in many areas of behavioral science which might profit by them, or they are used too little. Their use, or lack of use, in social psychology is an example of this (Martin & Rovira, 1981). "Alien" ideas and methodologies tend to be ignored, even when they might prove useful. Sometimes, too, methods, findings, and concepts are systematically ignored —however valid or useful they might be—because they do not fit easily into the worldviews or the conceptual biases of those who could potentially utilize them.

Alcock's metaphysical bias is clearly revealed by his quotation from D. O. Hebb wherein parapsychologists are chastised for not asking how the brain accomplishes this supposed psi and for the lack of evidence that one brain "physically" influences another. Again, Alcock criticizes parapsychologists for answering the "criticism" that psi seems unaffected by physical variables by asserting that perhaps psi does not involve transmission of information across space. Alcock also feels that the claim that psi is goal oriented is magical thinking; he apparently feels that such a role for psi would make life much more complicated and scientific thinking less simplistic. Perhaps he is right. (But that is not to say it makes science impossible, for advocates of the goal-oriented psi concept have delineated the implications of their thinking for boundary conditions for psi events.)

In his final, major section of Chapter 6, Alcock discusses the methodology of parapsychology. He begins by considering experimental controls. He feels obligated, it would seem, to generalize his

remarks such that they could harm all parapsychologists (not just the offending parties): "For the present, we are concerned with the way in which poorly controlled experimentation is often presented as having been very carefully controlled. . . . In many cases, such reports belie the extent to which controls were either inadequate or lacking" (p. 129). The idea here is that when parapsychologists write for scientists they may reveal the full extent of the weaknesses in their studies, but that they are white-washed when the same studies are presented in a more popular context. Such charges quite clearly have some serious implications, but they are bandied about very carelessly by Alcock. He provides but one example of such behavior, one, ironically, which was discussed in detail and criticized years ago in this very journal by this reviewer—though Alcock says nothing about that. (Parapsychologists presumably both engage in such behavior and condone it.) Careful, conscientious writers take care that negative remarks are not over-generalized. What are the vague statements, "often" and "in many cases," supposed to mean in the quotation above from Alcock?

To make things worse, Alcock, after discussing the single example just mentioned, states: "That particular miracle is used as evidence time and again by parapsychological writers who wish to convince their audience of the reality of out-of-body experiences" (p. 131). Are Alcock's "parapsychological writers" parapsychologists? It seems doubtful that most of his readers will recognize that this statement could not be true of professional parapsychologists in general.

Alcock next considers how, on closer analysis, published parapsychological studies and their conclusions are seen to be faulty. For example, he makes remarks obviously intended to cast doubts on all ESP–ganzfeld studies, but he cites only the first such study to be published, that of Honorton and Harper. He vaguely mentions "sloppiness" and "crude" randomization in that study, but he fails to mention the dozens of later such studies—many of them improved methodologically—or the striking replication rate of significance in such studies. It is, of course, easy to single out for criticism the earliest study in a particular paradigm, for such studies are likely to have flaws which are eliminated in later work.

He concludes this brief "experimental controls" section by quoting Persi Diaconis's statement, published in *Science*, to the effect that every psi test he had ever witnessed involved inadequate experimental conditions. How useful is such a statement unless we know how many studies— especially published studies by established investigators—he has witnessed? Quoting such a statement is worse than useless.

Alcock then launches into a discussion of replication, but with

hardly so much as a mention of the specific data in the several areas where there would appear to be a reasonable replicability rate. This section is essentially worthless for the reader who wishes to assess the replicability rate in any of the problem areas studied by parapsychologists. Alcock apparently feels that he can simply dismiss the several areas of appreciable replicability because the work has supposedly been done by "researchers who *believe*" [Alcock's italics] (p. 136). He quotes a scurrilous remark by Paul Kurtz to the effect that listening to parapsychologists discuss their replicated studies is like listening to the American Tobacco Institute claim that smoking does not cause cancer. Skeptical and supposedly neutral scientists should be able to get positive results before psi can be taken seriously. (Ironically, nowhere, to this reviewer's recollection, does Alcock encourage others to undertake replication attempts.) The problem may, in part, be how such scientists are to be defined. One gains the clear impression that whenever anyone publishes a positive study in the psi area he or she becomes, in the eyes of persons like Alcock and Kurtz, a "believer," and thus their data can be discredited.

Nonetheless, cross-experimenter replicability in the field leaves something to be desired. While there is some cross-laboratory replicability which cannot, apparently, be accounted for by sloppy methodology, certain investigators—even some who believe in psi reality—have repeatedly failed to find any evidence of psi in their data, even with a technique such as ganzfeld. Until it is possible either to isolate the factors responsible for such divergence or to eliminate it by some means, this "science" will remain, to some degree at least, an art. Alcock. who is knowledgeable about social psychology, does not even suggest the importance of conducting studies designed to examine the parameters of social interaction which might underlie such replicability problems.

Alcock states: "Since replication by independent, impartial researchers seems not to be possible in parapsychology, the possibility of fraud poses a particularly serious danger" (p. 138). One wonders what evidence he has that none of the replication in this area comes from impartial investigators or, given that he next discusses "the spectre of fraud," what evidence he has that impartial investigators turn in a lower replicability rate than experimenters who are less impartial. The scientific approach here would seem to be for "skeptics" to encourage investigation of parapsychological claims by impartial persons so that such central questions can be answered. Alcock's book could have the effect of discouraging such investigation.

One source of possible fraud is the experimental subjects, and

Alcock clearly believes that if a "psychic" has once cheated, this tends to discredit his or her performance under whatever more stringent conditions of testing may be used. Alcock's dismissal of the psi evidence for E. Kelly's star subject, Bill Delmore, is a prime example of his willingness to ignore evidence on the flimsiest of grounds. Instead of discussing specific weaknesses in the published experimental work with Delmore, he simply supplies innuendo on the grounds that P. Diaconis, a statistician and "skeptic," claims he detected fraud in Delmore's performance under what were never alleged to have been experimental or controlled conditions.

In discussing the question of fraud by S. G. Soal, Alcock is apparently unaware of the final, most telling episode in the gathering of evidence of fraud, the report by B. Markwick (1978).

The tenor of the discussion in this fraud section, as elsewhere, raises the question of whether the "skeptics'" position is falsifiable. Under what conditions would they admit they were wrong? Let them state those conditions clearly and forthrightly and without sleight-of-tongue, if that is possible among the magician-obsessed humanists.

Alcock concludes this chapter with the charge that parapsychology lacks systematic research. It is evident that he is referring at least in part to what has been termed "process-oriented" research. The credibility of this section will be low for persons familiar with the published experimental literature, for there has been considerable such research. Nevertheless, in this reviewer's opinion, it has not been sufficient. For example, the sheep–goat effect and various personality–ESP correlations have been around a long time, with precious little effort to provide an experimental basis for understanding them. Similarly, research has only recently begun on trying to understand why the ganzfeld appears to favor the occurrence of ESP. A partial explanation for such lapses may be that, because of the controversy within and surrounding the field, parapsychologists have adopted very high criteria for being "sure" that an "effect" exists. Until there is this certainty, it may seem unprofitable to investigate alternative hypotheses intended to explain a merely alleged effect. Such conservatism may, however, be self-defeating, for efforts to investigate the nature of an effect can result in clearer evidence of the reality of the effect and, by delineating some of its boundary conditions, can lead to an enhanced magnitude and, presumably, greater replicability for the effect. Despite these remarks about a need for more systematic research, it would be misleading to conclude with Alcock that this field lacks such research. Research of process-oriented character has

occurred throughout the history of experimental psi research. Why there has not been more of it is a question too complex to analyze here.

Contrary to the apparent implications of remarks by Alcock near the end of Chapter 6, the vast majority of parapsychologists, in their studies of psi phenomena, do not start from the kinds of assumptions regarding those phenomena which are prevalent in occultism—though that is not to say that something useful might not be gained from studying those beliefs to see if they give direct or indirect clues which could be scientifically investigated. The first consideration above is why terms like "out-of-body-experience (OBE)" are used rather than "astral projection"; and the second is why parapsychologists empirically investigate whether the information-processing mode(s) associated with OBEs favor or do not favor presently unexplained information acquisition.

Alcock concludes this chapter with the highly debatable suggestion that parapsychologists plead their case by asking for a special relaxation of the usual rules of scientific evidence.

Of Control Groups, Experimental Controls, and Other Considerations

Chapter 7, "Parapsychology and Statistics," begins by considering the basic nature of statistical inference. Alcock's tone here might cause his reader to think that psi researchers are statistical ignoramuses. They use circular logic, he alleges, when they use statistics to try to prove psi reality. They both draw the inference of ESP from nonchance results, so-called, and explain those results by "ESP." Psi researchers, Alcock says, must demonstrate the existence of psi independently of the nonchance results. Curiously, this kind of argumentation is not, per se, statistical. It has, instead, to do with broader issues related to explanation. Unfortunately, many readers of this volume may come away with the feeling that Alcock has exposed something fundamental about how parapsychologists abuse statistics.

Contrary to what a naïve reader might assume from reading the Alcock volume, all parapsychologists of this reviewer's acquaintance regard "extrachance" results as simply an anomaly to be explained. Though most conveniently use the term "ESP" as a summary term for such statistical observations when they are made under conditions controlled for sensory leakage, parapsychologists certainly do not

believe that by having used that term they have "explained" anything. Indeed, many—this reviewer included—have objected to the theory-laden nature of the term *extrasensory perception.*

Extrachance deviations do not explain themselves, so merely attaching a summary term to them does not help in this regard. This is why some parapsychologists have pointed out that theoretical constructs should be developed to attempt to explain such purely statistical observations. Those constructs should have definite implications about conditions under which "psi" observations will and will not occur. They should, in short, be readily falsifiable. Rather than being considered as tests of "psi reality"—whatever that may mean, since "psi" is negatively defined in most contexts—the statistically evaluated studies should be aimed at providing an opportunity for supporting or refuting a particular idea intended to explain such extrachance deviations. This approach should encourage both "parapsychologists" and "skeptics" to make explicit their assumptions about how such anomalous observations are to be explained and to submit them to empirical tests.

Alcock next discusses the importance of "control groups" in research in general and parapsychology in particular. "Parapsychological researchers," he says, "rarely use control groups, and instead usually compare the outcomes of a psi experiment with what one would expect if chance alone were operating" (p. 149). He neglects to mention the substantial proportion of studies in this field which involve more than one group, for the purposes of statistical comparison; the sizeable number of studies contrasting hypnotized and nonhypnotized subjects is only one example. A vast number of studies involve significant a priori contrasts in the performance of groups differentiated either by treatment or individual differences, and various commentators have noted that such results—which have often been replicated—constitute some of the best evidence for ESP.

It must not be conceded to Alcock, however, that comparisons, for example, of group means against mean chance expectation are dubious statistically speaking. Such comparisons are legitimate when certain precautions are taken, such as using a target source tested for randomicity and with adequate sensory shielding for the targets. ESP work with feedback to subjects about individual trials is a somewhat trickier matter, though this is not the place to discuss its ramifications.

A possible cause of Alcock's claim that parapsychologists have rarely used any form of empirical control might be a lack of awareness on his part of the vast amount of empirical control work done in the early days of the field and summarized in an important volume which

Alcock does list in his references (Pratt, Rhine, Smith, Stuart, & Greenwood, 1940/1966). Large amounts of empirical control data are considered in that volume, including the important method of cross-checks in which subjects' calls are checked against ESP targets other than those for which they were intended. Such work, incidentally, generally confirmed, rather than brought into question, the assumptions used in testing ESP results against mean chance expectation. Given that many parapsychologists nowadays have computers at their disposal, they might, in the case of at least some studies, be in a position easily to provide, through the cross-check method, empirical checks upon conclusions based on the use of the "chance" model. In this way it might be possible to usefully examine the allegation that the statistical results depend on improper assumptions regarding the null hypothesis. It certainly does not hurt to have some reassurance on such matters, especially when many investigators are using targets generated de novo from random event generators. (Of course, it is recognized that those who use such machines generally subject their outputs to various statistical checks of their randomness.)

In connection with these same kinds of statistical issues, it is worth pointing out that in contrasting group means with theoretical mean chance expectation, whenever it is appropriate, most parapsychologists nowadays use the *t* test for an empirical against a hypothetical mean; they thus base the standard error of the mean upon an *empirically* based estimate of the standard deviation (the unbiased estimate of the parameter in question). They thus make no assumptions which depend upon the "theoretical" standard deviation. Alcock's readers are not, however, apprised of this development.

Alcock makes another sweeping and hasty generalization by claiming that parapsychologists misunderstand even the meaning of the *p* value; he alleges that they erroneously take it to be a measure of the strength or size of the effect one is measuring, not recognizing how intimately it is tied to sample size. Once again he tries to make parapsychologists look like statistical ignoramuses. He provides not one reference supporting such a serious charge of statistical idiocy, and, of course, even if he had supplied several instances of such a fallacy, it would be unfair to generalize the charge in the absence of examples from the overwhelming majority of parapsychologists.

Further, he charges, parapsychologists "violate the statistical inference model by setting . . . criteria for rejection of the null hypothesis *after* [his emphasis] examining the data" (p. 151, footnote). This charge is utterly unsupported by his alleged documenting evidence that studies "habitually" report varying *p* values rather than simply

stating whether or not the computed statistic is significant at a prespecified value of alpha. Here is a classical non sequitur argument; reporting the probability of an observed outcome under the null hypothesis does not imply that one sets one's rejection criterion after the fact. Indeed, it has nothing to say about that. Nor does it imply that parapsychologists believe that the p value is simply a function of strength of effect. While they often do report the actual p value under the null hypothesis, this per se implies nothing about adhering to the kinds of statistical fallacies alleged by Alcock. Perhaps the reasons for doing so derive from having been attacked, repeatedly, by some critics for not using a stringent enough alpha level for psi studies (so such values are reported in order that the individual, critic or not, can satisfy himself or herself according to an individual criterion) or in order that anyone who wishes to do so can later easily combine the outcomes of a series of studies within the same paradigm, if that is desired.

"Probability models" are next discussed, and one only wishes that Alcock—given the general context of criticizing parapsychologists' treatment of statistics—had pointed out that such remarks are aimed (one hopes) at the general reader and are not intended to reflect upon the statistical sophistication of parapsychologists. There is nothing new here for parapsychologists. Some of his discussion in this section caused this reviewer to wonder if Alcock had forgotten that even small effects, if real, may have immense theoretical significance. Alcock writes as though the random target sequences of parapsychological studies usually come from shuffling cards, though this is not the case in contemporary research. This greatly reduces the importance of some of his discussion. From Alcock's discussion readers may also gain the erroneous impression that discussion in the literature of the field has ignored the possibility of subjects' learning to anticipate targets from information contained in target sequences from which they have had feedback. It sounds as though he is making a revelation, though this has, even recently, been a focus of extensive discussion in parapsychology journals.

Next, Alcock discusses studies in which individuals have endeavored to show that they could simulate ESP by matching two randomized sequences. These studies represent either unreplicated effects or outrageously post hoc selections of data to try to prove a point. Thus, the very kinds of things for which parapsychologists have sometimes been excoriated are now extolled as the great accomplishments of critics. To compare such outcomes with those in psi research is to ignore both the replicated status of many of the parapsychological

claims and specific differences in the statistical circumstances. For example, Alcock's argument, following Spencer Brown, that Oram's study with random numbers produced a facsimile of the PK quarter decline ignores many and vast differences in the two procedures, as well as the fact that this post hoc finding in the single study by Oram has never been replicated—whereas the quarter decline in the PK data was replicated many times over (Rhine & Humphrey, 1944). To use this post hoc finding from an isolated, unreplicated data set with disparate methodology to explain away the PK quarter-decline effect is an exercise in scientific and statistical sophistry.

Alcock is certainly correct in emphasizing the central importance of randomization procedures in ESP studies. This reviewer would personally concur with his feeling that the randomization procedures are too often inadequately described (or not described at all). (That is not to say that in this reviewer's opinion this occurs often, but even occasionally is too often in such important matters.)

"The problem of control in psi research" is Alcock's next concern. Once more, the discussion is usually very general and is not applied to specific studies, possibly because it is rarely applicable. There is an extended discussion of the handling of ESP cards by subjects, which occurred, says Alcock, "in the earlier days when the most successful demonstrations took place" (p. 161); however, there is no listing of either the studies in which this allegedly occurred or of the "successful demonstrations" in which this was the case. (Successful demonstrations are, of course, still going on today.)

Though Alcock argues that the idea of "chance" may be wrong in particular instances, he nowhere provides a rationale to show that this is even feasible when the source of targets is tested for randomness and when, additionally, there is no feedback to the subject about targets.

Alcock's concept of experimental control is at times puzzling. An example is his criticism of the ESP dream work at the Maimonides Medical Center in Brooklyn, N.Y. "No control group of subjects was used, although a control group, for which no sender or no target was used, would appear essential" (p. 163). However, he does not reveal what is the problem with the excellent controls which were actually used by the Maimonides researchers, namely, control pictures for comparison judging, with the pictures that are targets and the pictures that are controls being randomly determined, and with judges blind as to which are which. Alcock's comments on some of the Maimonides work give the false impression that no control data were gathered.

From Alcock's remarks on PK research near the end of his section on "control," his reader could easily gain the impression that PK results have simply disappeared as adequate controls have been instituted. Recent work with random event generators, however, has produced excellent success and involves a much higher level of safety against alternative interpretations than is found in almost any of the die-face work. (Alcock's remarks actually apply to the die-face work, but his failure to mention the good success with more rigorous methods is potentially very misleading.)

In a brief section on "response bias" Alcock discusses what is essentially the stacking-effect problem, but he does not let his reader know that parapsychologists have long been aware of this problem and have developed methods which can be applied in the unusual circumstances in which it arises. On the contrary, his words could easily be misread by the poorly informed as indicating that this problem has been ignored.

Next follows an extremely brief but very nasty section on "selective publishing" in which Alcock alleges that psi researchers fail to publish nonsignificant outcomes. He quotes a statement of 1973 vintage by John Beloff which suggests that they are failing en masse to publish nonsignificant results (though, apparently, with no documentation supplied). Alcock does not tell his reader that for some years now the Parapsychological Association has had policies which strongly encourage the publication of nonsignificant results. The Association may well be unique among scientific societies in its stance on such matters.

Alcock's remarks on recording errors are appropriate, and he notes that precautions against these became part of the standard practice at the Duke University Parapsychology Laboratory.

Near the end of Chapter 7 Alcock singles out for criticism the work of Helmut Schmidt. His discussion generally follows earlier criticism by Hansel (1980) and really adds nothing new that is useful. It totally ignores the large number of independent replications of the PK results using Schmidt-type machines. Other reviewers have discussed the weaknesses in Hansel's arguments in this connection.

Alcock criticizes Schmidt's conclusion from his work that psi is goal oriented. (This concept rejects the idea that PK can be understood in terms of some clever way in which the mind, through information processing, interferes with the machine in order to accomplish its objective.) Alcock states: "This apparent lack of concern with the intermediate steps is another example of how magical thinking creeps into parapsychological explanation" (p. 170). Whether or not Alcock elects to throw around bad names ("magical thinking"), any scientific

explanation must fit the observations. Schmidt, like this reviewer, whose "magical thinking" in this very connection Alcock criticized earlier (p. 129), was driven by empirical evidence to the hypothesis that psi is goal oriented. Some parapsychologists are continuing to investigate that hypothesis, however startling or unsavory it may seem, for hypotheses must fit the available data and be subjected to testing in new studies. (In this reviewer's own case, the hypothesis that psi is goal oriented was directly contrary to his earlier published theorization; it was not his preferred hypothesis.) It is inappropriate to ignore empirical outcomes merely because they do not fit preconceived biases or because they give one bad dreams concerning the possibilities of "magical thinking."

The concluding remarks in this "statistical" chapter suggest that, in a very general sense, the statistical outcomes in parapsychology can be ignored as evidence for psi because there are always conceivable ways in which error might have crept into the studies! Conceivable error is, unfortunately, always conceivable in any area of research, and conceivable error is not always the same thing as plausible error or error which has a realistic probability of occurrence. Plausible or realistically probable error can be ascertained only by examination of the methodology of studies in a particular line of research.

Near the end of Chapter 7 Alcock, following P. W. Bridgman, makes an important and valid observation: Parapsychologists interpret their nonchance events, which are demonstrated statistically, to imply some sort of regularity. However, the only justification for such a conclusion would be the demonstration of such regularity. If parapsychologists cannot make statements about how to elicit psi, that is, the requisite conditions for its appearance, including statements about when it will not be observed, there are no grounds for claiming the existence of a regularity. In this reviewer's judgment, these constitute the basic conditions under which parapsychologists can say that they actually have a science of parapsychology as distinct from an area to which the scientific method is being applied in the hope of someday creating a science.

Ad Hominem Attacks and Guilt by Association

Chapter 8, "The Public Debate Continues," adds nothing substantial to the criticisms presented earlier. It does show once more that Alcock is more than willing to cast general, undocumented slurs against the parapsychological community and to make statements

which could link it, in the minds of the uninformed, with movements such as biorhythms and UFOs which most parapsychologists regard as other areas of inquiry altogether. Alcock's calling the Parapsychological Association the "Parapsychology Association" (p. 174 and p. 187) raises further questions about his personal knowledge of this field.

This chapter hits a new low through ad hominem attacks against particular researchers. Without benefit of any documentation, Alcock states that Helmut Schmidt "was much impressed by the psychic ability of Uri Geller" (p. 177) in a context which clearly shows it is intended to undermine his credibility as a researcher. He includes in that undocumented charge Thelma Moss, William Cox, and "other leading parapsychologists" (p. 177). Considerable space is taken up with trying to justify ad hominem attacks as a basis of ignoring persons' research contributions! Presumably the SRI parapsychological work can be ignored because of John Wilhelm's cited claim that there are in that laboratory a number of "practicing Scientologists." Alcock justifies this character assassination by claiming that "paranormal experiments are not replicable, either by skeptics, or even by many believers" (p. 178). (Note Alcock's persistent use of the "believer–skeptic" dichotomy. This makes parapsychology sound more like a religious than a scientific endeavor, and parapsychologists more like religious fanatics.) Claims by biased observers, Alcock alleges, cannot be checked by others. We are to believe either that errors motivated by too-strong belief or actual fraud could very easily explain the work of certain individuals or groups. But why does he not encourage others to try to replicate those results and thus to learn for themselves the truth of the matter rather than engaging in implicit character assassination and hurling about strong, but undocumented, suggestions of fraud and/or incompetence? One possible interpretation is that he is more interested in discrediting parapsychology than in encouraging others to examine its claims with scientific methodology.

This chapter seems to this reviewer like a last, desperate attempt to drag in everything mysterious, phony, and spooky, to mix it in close temporal and spatial contiguity with parapsychology, and to hope that the bad odor is associated with this field. Alcock does not even intimate that parapsychologists are highly critical of the sensationalized, popular, "mysterious" claims which he sprinkles throughout this chapter—everything from Carlos Castaneda and the Bermuda Triangle to UFOs and biorhythms and, of course, fairies. On the contrary, he states: "The parapsychologists themselves seem disinterested in trying to separate the wheat from the chaff, although many play lip-service to the need for skepticism and critical thinking"

(p. 186). John Beloff, he alleges, is an exception, a parapsychologist who is worried about the "wave of pseudo-scientific and occult belief that is sweeping society" (p. 186). But he adds: "Beloff's view is not representative of parapsychologists in general" (p. 186). He cites no evidence to support such a damning claim. He further alleges that parapsychologists in general have made essentially no effort to help defend the public against "the wildest of claims and promises" (p. 187). He seems unaware of Robert Morris's role in exposing the "Amityville Horror" or his efforts to investigate scientifically some of these popular claims. Perhaps Alcock is unaware of the Parapsychological Association's sponsorship at the 1975 convention of the American Association for the Advancement of Science of a strongly consumer-oriented symposium which included a paper in which this reviewer attacked, on explicit, scientific grounds, the claims of commercial "psi-training" courses. That paper was given wide national and international attention, and the author went on National Public Radio to discuss its content. Another parapsychologist, Rhea White, published the paper in a volume which she edited (Stanford, 1976). One parapsychologist lectures before civic clubs on parapsychology, but he begins each lecture with a bogus spoon-bending act which he later discloses as fraudulent to let his audience know how easily one can be deceived. Alcock does not mention the exposé, after intensive investigation, by Rolf Ejvegaard and Martin Johnson (1981), of an apparently bogus apparition case published in a Swedish magazine. The list could go on and on. It could include some pseudopsychics whose public fraudulent activities were apparently halted as a result of the exposures coming out of parapsychologists' investigations. A number of parapsychologists devote considerable time to communicating with a wider audience about the pitfalls in popular claims concerning the "paranormal."

Alcock brings this chapter to an exciting finale. Into the picture of a frightful wave of public irrationalism and occult threats rushes, at last, the hero of this drama, CSICOP (pronounced *psi cop*), blowing his whistle on the thugs of unorthodoxy and irrationalism. (CSICOP is the Committee for the Scientific Investigation of Claims of the Paranormal.) One can almost hear the triumphal music and see the final credit lines slip by. Out are trotted the names of the scientific–humanistic culture heroes who have helped put darkness to flight: George Abell, Isaac Asimov, Bart Bok, Milbourne Christopher, Martin Gardner, C. E. M. Hansel, Ray Hyman, Paul Kurtz, James ("The Amazing") Randi, and B. F. Skinner, among a number of others. And a final warning is posted concerning the nasty culture criminals, the

parapsychologists, who have condoned and encouraged this terrible outrage: "The problem is that much of parapsychology . . . teaches people to abandon critical thought, to consider the scientific method as too restrictive, passé, incapable of reaching ultimate truths, and to view the individual and the world in magical terms" (pp. 188–189). There is a warning, too, that the tactics used might not be so "fair" (?) next time, in the form of a quotation from Girden which warns about the horrible costs to science and society unless proper corrective steps are taken.

In Chapter 9, "Conclusions," the key arguments presented earlier are summarized, along with a stern warning that if psi exists, science cannot, for psi would undermine the scientific method. This volume does not, however, lay down any clear, rational basis for such an assertion. It seems to be a statement built upon scientifically untestable metaphysical assumptions.

For many parapsychologists there will be a chilling note in the final words of this volume. There Alcock urges, it would seem, that the schools teach to children the precise message which he has presented in this book. Here is paradigmatic humanistic thinking: Let us correct all the ills of society by instituting training programs to get people to think in ways which will benefit society. Of course, the humanists are sure they know exactly what is good for individuals and for society because they are sure they know exactly what is real and unreal, possible and impossible.

Evaluation and Conclusion

Though this book is highly touted by its publisher as a text, it is, in this reviewer's judgment, unsuitable for that purpose because it fails to provide anything approximating a review of parapsychology. It comes across in his mind more like rhetoric which will discourage genuine inquiry into the validity of parapsychological claims. It appears to be an effort to rid humanity of the scourge of "irrationalism"—those things which the humanists disdain. As this reviewer sees it, it represents, not science, but an effort to justify a particular brand of faith by claiming to be the voice of science.

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Experimental Psychology Laboratory
SB-15 Marillac Hall
St. John's University
Jamaica, N. Y. 11439