

chology for explanations. Here they find some dualist accounts and plenty of use of "psi" as an explanatory concept. But neither of these explanatory ploys works. The experiences remain mysterious, and parapsychology meets the mystery with only a negatively defined, nonexplanatory concept. This it cannot do forever.

How then might parapsychology change? First, the long-hoped-for replicable psi experiment might be found. Then psi could cease to be negatively defined, doubters like myself could be appropriately overruled, and parapsychology could progress with "omegic" theory and hypothesis testing (to use Rao & Palmer's [R & P's] neologism). This is certainly possible, but no one can say how likely it is.

While we await this breakthrough, the alternatives depend entirely on how parapsychologists choose to define their subject area, whether it is confined only to psi or whether it includes those "experiences in the 'real world'" out of which R & P say it grew.

We can certainly hope for scientific accounts of such experiences. Already, progress is being made with near-death experiences (Greyson & Flynn 1984), lucid dreams (Gackenbach & LaBerge, in press), and cognitive approaches to out-of-body experiences (Blackmore 1984; Irwin 1985c) – and all with little or no reference to psi. One could speculate that a deepening and extension of an information-processing account of human experience will lead to something close to views expressed in some religions, especially in Buddhism; or, as Alcock puts it, to finding a secularized soul. For example, we may build on the assumptions that the self is a mental construct, that external reality is nothing more than models of the world, and that consciousness is a natural aspect of information processing. This "cognitive mysticism" could force us to accept our fundamental aloneness, our dependence on the physical body, and the illusory nature of the self and the experienced world – while potentially making sense of higher states and transformations of consciousness in terms of models of reality (Blackmore 1986b). Within this approach, the search for the elusive psi can only be seen as a red herring.

The founders of psychic research were deeply concerned with issues of man's place in the world, the government of the universe, and the nature of human experience and suffering (Gauld 1968). This new approach would not, I imagine, have gone down too well with them, but at least it addresses the issues – which a "science of psi" does not and cannot do. It also provides hope of a truly progressive research program. The stagnant program based on psi (which can never be overthrown by blank skeptical denials) could at last be superseded.

But would this be parapsychology? That is up to parapsychologists to decide. So far, the tendency is to be interested only in the evidence for psi; experiences are studied only if they seem to involve psi or are psi-conducive. In the past, this caused parapsychology to shrink, losing such topics as hypnosis, multiple personality, or animal homing, and retaining only the "still mysterious" ones. If it continues clinging to psi, parapsychology could soon lose all those human experiences that originally motivated the subject. Though parapsychology might never die, its claims of relevance to human experience would be seen as false.

Alternatively, parapsychology could opt to study the experiences, even though they may prove to require no paranormal explanation. This way (and perhaps only this way) the subject could take up a valid and valuable place within science, but it would have to accept that, after all, its subject matter might fit within the "basic limiting principles" (see R & P's Introduction, para. 4) and with the rest of science (unless or until the breakthrough ever came).

So parapsychology has a choice: Give up exclusive dependence on psi and settle for real progress in studying human experience; or stick to psi – hope for the jackpot, but risk eternal stagnation.

How to dismiss evidence without really trying

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In most respects, parapsychologists and skeptics differ in their assessment of the evidence for psi. But they generally agree that if any parapsychological evidence is worthy of serious discussion, it is the quantitative experimental work begun in the United States around 1930. Parapsychology of course had a history before that time, a great deal of which concerned the phenomena of mediumship during the period 1850–1930. But the accepted wisdom, both within and outside parapsychology, is that this earlier body of data – *particularly* the mediumistic material – is inherently less "clean" methodologically and evidentially than laboratory work, and that it can easily be undermined by appeals to the unreliability of human testimony or the possibility of fraud.

This view is completely wrong, however, and it is noteworthy that the majority of those who promulgate it have apparently never studied the mediumistic evidence with any care. Skeptics traditionally either fail to cite or else misrepresent the strongest cases. When it comes to the evidence from mediumship, skeptics tend to generalize from the worst cases. Yet within parapsychology, the strategies are not much different. Most parapsychologists either ignore the mediumistic material altogether or else dismiss it for the weakest reasons. For those parapsychologists who cling (quite naively) to the utility of conventional experimental methods in their own work, this is a convenient gambit; it allows them to appear appropriately tough-minded to their critics.

My view, which I have recently defended at length (Braude 1986), is that the most convincing and important parapsychological evidence *by far* is the evidence from physical mediumship. The best cases of physical mediumship are every bit as clean as, and far more convincing than, the best laboratory experiments. Moreover, they promise to tell us more about the nature of psi than quantitative experiments ever could. These cases easily resist the traditional skeptical charges of error and fraud, and they cannot be undermined by references to other cases that are transparently weak in just the respects in which the best cases are strong.

Alcock's target article (and earlier book, 1981) are disappointingly typical of the skeptic's approach to this material. Both works avoid any mention (much less discussion) of the good cases of mediumship. Yet Alcock quickly rejects the mediumistic evidence, frequently invoking one of the least impressive and most generally irrelevant cases of all – that of Uri Geller. It is an egregious error to attack the mediumistic evidence by citing phenomena that (even if genuine) can be easily reproduced by sleight-of-hand. In fact, no dismissal of mediumship should be taken seriously until it deals, *in detail*, with the cases of D. D. Home and Eusapia Palladino, at the very least (see Braude 1986 for references). In his article, Alcock claims that "[gifted] psychics have as yet been unable to perform their feats under controlled conditions for neutral or skeptical investigators." Yet that is precisely the feature of the Palladino case by virtue of which many consider it to be the strongest in the history of mediumship. Alcock must *show* – not merely allege – that his claim holds for the Palladino case, and especially for the 1908 Naples sittings, which were conducted by three skeptical investigators, two of whom were skilled magicians (Feilding et al. 1909). In fact, I ask Alcock whether he has actually read the primary material (rather than merely accepting the accounts of other poorly informed or confused critics). I challenge Alcock to discredit the evidence from the Palladino and Home cases (as described fully in Braude 1986). Alcock also fails to discuss the careful work over the past few years conducted at the Princeton

University School of Engineering/Applied Science (e.g., Dunne et al. 1983; Nelson et al. 1983; 1984).

I will leave it to experimentalists in parapsychology to reply to Alcock's criticism of their work. I prefer, instead, to focus briefly on problems of other kinds. To begin with, Alcock uncritically accepts the view that psi appears incompatible with received science (particularly the laws of physics). That view is actually quite easy to challenge, however, and Alcock seems unaware of the relevant issues. For example, no evidence available now or in the foreseeable future could possibly establish, say, that telepathy violates Maxwell's equations, even if it appears insensitive to distance (see Braude 1986, p. 283; Braude, forthcoming). Moreover, Alcock fails to mention the mediumistic and poltergeist evidence suggesting that PK (psychokinesis) actually obeys conservation laws in physics (e.g., reports of cold breezes preceding physical phenomena, and the measured increase in the weight of certain mediums by the amount of force needed to raise a levitated table; see Braude 1986, Chap. 2). Alcock is also wrong in claiming that it is a "logical" principle that a cause cannot precede its effect. Not only is that principle not a formal truth, but it is also far from obvious (even if true). Indeed, an imposing array of philosophers and scientists consider that the concept of a cause is atemporal, or that the equations of physics are time-reversal invariant.

Alcock's confusions here may be continuous with his apparent failure to grasp important issues about reductionism and dualism. First, Alcock never acknowledges that cognitive or intentional phenomena generally – normal *and* paranormal – might simply lie outside the domain of the physical sciences. Many would argue (quite reasonably) that the methods and theories of physics, for instance, are (respectively) inappropriate and irrelevant to both orthodox psychology and parapsychology. Alcock sees parapsychology as an attempt to defend the existence of a nonmaterial *soul*. Like many other scientists, he apparently does not appreciate the distinction between Cartesian- (or substance) dualism and event- (or level-of-description) dualism. But the nonreducibility of the mental to the physical need not be taken to support the existence of a nonphysical substance, the *mind*. If that were the case, every social or behavioral scientist would have to be as much a closet Cartesian as Alcock supposes every parapsychologist to be. In fact, when it comes to cognitive or intentional phenomena, one can easily be a substance-monist and a level-of-description-dualist. Indeed, there are many different ways to take the position that the only stuff of nature is physical, but that psychological phenomena (normal or paranormal) cannot be countenanced by the physical sciences. The only parapsychological evidence that *could* count against a substance-monism would be the evidence for survival after death, and not even that would specifically support a strong dualism; it could equally support a form of idealism, or even a pluralistic view with an inventory of substance-kinds of at least three (see Braude, forthcoming).

Turning briefly to Rao & Palmer's (R & P's) target article, I was sorry to see them claim that "the first major experimental investigation of psi" was Coover's 1917 study at Stanford, and that "sustained research" began when J. B. Rhine arrived at Duke University in 1927. Apparently, R & P use the terms "experiment" and "research" to apply honorifically only to a certain kind of formal quantitative study. I submit, however, that these terms can be applied without strain to many of the best mediumistic studies. Many of Crookes's studies of D. D. Home (see Braude 1986) deserve to be considered experiments; and sustained research was conducted on Home, Palladino, and other mediums.

More important, conventional experimental methods in parapsychology are powerless to reveal anything interesting about the phenomena, except perhaps that the phenomena exist. There is no way to guarantee that only officially designated subjects use psi, or that subjects use only the psi ability being tested for, or that they use it only at the appointed time. Hence,

once we take psi seriously enough to test for it, we give up the ideal of a truly "blind" or "double blind" experimental protocol; there is no way to render an experiment blind for ESP. Neither can we control for sneaky or naughty psi, on the part of the official subject, the experimenter, or even onlookers. No experimental controls can prevent persons even remotely connected with the experiment from using psi to serve their own needs and interests, which (of course) may differ from those of which the subjects are consciously aware, and which are almost certain to be more deeply motivating than the artificial tasks contrived for the subject. Indeed, in the case of PK, the only time we can be reasonably certain who the agent is, is when we have a "superstar" subject in whose presence phenomena occur repeatedly under different conditions and with different experimenters. But, of course, we cannot hope to understand the psychology of psi until we can identify psi agents. That is one conspicuous advantage of the best mediumistic evidence over recent laboratory data, and a reason for resuming the study of mediums. Hence, although I applaud Rao & Palmer's sensible defense of the integrity of experimental work, I can only be pessimistic about the utility of that body of evidence.

Struggle for reason

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The counterpoint to the Rao & Palmer (R & P) target article has already been provided by the excellent target article of Alcock. The objective of the following commentary (after mentioning some incidental points and complementary material), is to attract attention to a problem whose consequences should be very serious for education, science, and culture.

Concerning the "recognition" of parapsychology, the problem in Europe is a bit different, and in France especially. For example, some parapsychologists (cf. Broch 1985a) claim that there exists an official laboratory directed by Y. Lignon, professor of mathematics at Toulouse University: In fact, Lignon is not a professor, and there is no parapsychology laboratory. Nor are courses in parapsychology taught in French universities: Not a single connection exists between the educational system and parapsychology. To my knowledge, the only official involvement of a French university in the "paranormal" is the telematic service I created for the University of Nice, and this involvement (see below) hardly receives the smile of consent from European parapsychologists!

Concerning Uri Geller, *BBS* readers may be interested to learn that this debunked medium reappeared in Europe in a television broadcast of more than 2 hours ("Droit de Réponse" on March 13, 1987) that was entirely devoted to Geller and his new book. In this broadcast, Eldon Byrd (presented as a major scientist interested in Geller's psi powers) "confirmed" that Geller was tested with nitinol in scientific experiments conducted in U.S. Navy laboratories (*BBS* readers should consult the hilarious Isis center story in Gardner 1981). The behavior of some parapsychologists and mediums seems to depend on which country they are in.

In slight contrast with Alcock, I hold that acupuncture should be considered a superstition because, even in its capacity to produce limited pain relief, its basis seems to be correlation and not causality; Numerous controlled trials have shown that the claims for acupuncture have no scientific validity (Skrabaneck 1984). More generally, medicine is currently confronted with a revival of belief in magic, a trend whose implications may be harmful (Broch, in press).

To complement Alcock's point about the failure to attempt to meet LeBon's stringent test, we can add that the situation is currently unchanged. For example, although the \$85,000 re-