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DR. COOK'S REPLY TO COMMENTS ON HER REVIEW OF *PARAPSYCHOLOGY AND THANATOLOGY*

BY EMILY COOK

The last issue of the *Journal* contained reactions to my review of the Parapsychology Foundation's conference on Thanatology and Parapsychology (Cook, 1996), by six of the eight contributors to that conference.[1]

Before addressing some of the individual comments, I can summarize what I had hoped to convey in my essay by repeating that, for most people, the primary question posed by the survival problem is whether or not our personal consciousness, or self, survives death in some form that is continuous with—although likely also greatly different from—our present consciousness; and that we will make no further progress on that question by continuing to debate from the same polarized positions, but only by trying to make predictions based on the competing hypotheses and conducting new, empirical research to test those predictions. Physics made enormous progress, and indeed arrived at a wholly new conception of matter, not by abandoning the wave/particle debate over the nature of light, or by polemical arguments, but by testing predictions derived from the opposing positions. Survival research likewise might progress, and indeed help us arrive at a wholly new conception of mind and mind-matter relations, by a similar approach to its present impasses.

In emphasizing the importance of responsive xenoglossy cases for survival research, Robert Almeder seems to be arguing for this kind of approach: He predicts that fluent, responsive xenoglossy will be found only in "survival cases," since "there has never been such a case outside these survival cases." Since the Sharada case is the *only* case that I know of in which truly fluent responsive xenoglossy occurred, this is hardly a sufficient database from which to conclude that such cases will be found only in what John Palmer calls "survival-related contexts." Nevertheless, a good starting point for research on this issue might be to canvass

[1] Dr. Cook's original review appeared in the December, 1996 issue of the *Journal*, pp. 343-355. Subsequent replies by Robert Almeder, Stephen Braude, Michael Grosso, Justine E. Owens, John Palmer, and Eugene Taylor were printed in the March 1997 *JP* issue, pp. 45-59.

psychotherapists treating multiple personality or dissociative disorders (which the Sharada case certainly resembles) for possible leads to other xenoglossy cases (responsive or otherwise). On the other hand, such an approach may leave us mired in the "ad hoc" arguments that Almeder deplores if he does not specify more precisely what he means by "survival cases." If, say, we find a multiple personality patient in Idaho who speaks fluent Swahili, will someone then suggest that the patient must be the reincarnation of a native speaker of Swahili?

A different approach to xenoglossy cases is suggested by recent studies using functional magnetic resonance imaging to investigate patterns of brain activity associated with the use of language in bilinguals, and especially differences in those patterns as a function of when the second language was learned (Kim, Relkin, Lee, & Hirsch, 1977). Among subjects who acquired the two languages simultaneously, in early childhood, the activity patterns associated with the use of the two languages were essentially co-localized in Broca's area, the brain region associated with speech production. Among subjects who learned their second language in early adulthood, however, the patterns of activity associated with the two languages were spatially separated in Broca's area. In light of these observations, the hypothesis that Sharada's use of Bengali derived from Uttara's exposure to Bengali as an adult leads to the prediction that Sharada's use of Bengali and Uttara's use of Marathi will produce separate foci of activity in Broca's area.

Almeder (and others, including me; I am not trying to place the burden entirely on Almeder's shoulders!) must also specify more precisely what sorts of evidence would persuade us that an OBE experient has truly been "out of the body." Almeder is correct that, if I have an OBE and can correctly describe a picture down the hall, then "either my mind did leave my body or it did not"; in *that* sense I agree that the two hypotheses of OBEs are antithetical. But, as I argued in my original essay, in another sense they are *not* antithetical: They *both* involve psi, or a direct, nonsensory perception of the picture. Thus, the presence of paranormal processes alone cannot distinguish a "true" detachment from an illusory one; neither does the subjective sense of being out of the body. So what falsifiable criteria *can* discriminate the rival hypotheses of OBEs? Osis and McCormick (1980) made an excellent start, as Almeder correctly observes. But, as with the Sharada case, that experiment serves primarily to point to the complexities of the questions addressed; an *n* of 1 might, in William James's terminology, be a "white crow," but it is hardly a sufficient basis from which to start drawing conclusions about the nature of the white crow.

Stephen Braude is mistaken if he thinks that I reject the motivated-psi hypothesis as not "worthy of serious consideration." As I thought my original essay had made clear, I agree that better efforts to understand the psychological conditions in which survival-related cases occur can only improve our understanding of such cases. What I reject is the continued polarized defense of one position or the other, in the absence of efforts to devise and test new predictions derived from the competing hypotheses. It is also absurd to suggest that I doubt we can learn something about people's behavior by studying their psychological needs. We may well learn much about the underlying conditions fostering survival-related cases by studying the subject's psychological motivations. What I question is whether or not we will learn anything more about how to *interpret* the cases in relation to the survival issue. I can only repeat: Did Uttara's needs and motivations drive her to summon up her latent skills and psi capacities? Or to be drawn to, and "connect with," a kindred but discarnate personality named Sharada who had similar needs or inclinations? Or to recover memories of a previous life in which similar needs were (or were not) met? It is precisely because I take the motivated-psi hypothesis seriously that I wish Braude would spell out more precisely why he thinks that identification of Uttara's needs and motivations "may tip the scales" toward a superpsi hypothesis rather than a survival hypothesis. The point that I have tried to make, both here and in my original essay, is that falsifying the motivated-psi hypothesis is not only "difficult," as Braude acknowledges—it is *impossible*, for two reasons: first, because we can never define precisely what a "reasonable amount of digging" for hidden needs and agendas is; second, and more importantly, because motivated psi is surely at work, *regardless* of whether a living-agent hypothesis or a deceased-agent hypothesis is the correct one. Hence, identifying a subject's needs and motives provides no means of "tipping the scales" toward either hypothesis. Understanding the psychological conditions in which cases occur may help us locate and study new cases, but if we are going to arrive at a better understanding of how to *interpret* the cases, we are going to have to find some empirical way to discriminate between the rival hypotheses.

Braude has also attributed to me statements that I did not make. I did not say that empirical observations "demonstrate that mind is wholly dependent on the brain"; I said that they "*seem* to demonstrate that mind is wholly dependent on the brain" (Cook, 1996, p. 353; italics added)—and indeed they do seem to demonstrate such a dependence, which is why the vast majority of scientists and philosophers today assume some form of materialist or identity theory of mind/brain relations. However, I myself do *not* think that empirical research has established this view of mind/brain relations, which is why I still believe that the survival hypothesis is worth examining. As I said in my earlier essay, we now have basically two models of mind-brain relations—what James (1898) called the production model and the transmission model—both of which are equally well supported by the observed correlation between mental and physical functioning. In spite of the longstanding preference among scientists and philosophers for the production model, there are at present no empirical grounds for favoring one model over the other.[2] I repeat, however, that we might arrive at some such grounds if we are successful at making and testing predictions that can discriminate between these two models.

I also never "conten[ded] that further empirical research will settle long-standing issues in the philosophy of mind." What I said, and believe, is more modest and focused: that the mind/brain problem can be translated into empirical questions that allow us to make predictions based on the competing models of mind-brain relations, that survival research should be guided by such questions, and that progress on both the mind/brain problem and the survival problem can be made with such an approach. We will never, however, "settle" the mind-brain problem or "prove" survival with such an approach. The scientific method can never "prove" anything in *any* area of science; it can only make one hypothesis or theory more probable than another.

I am sorry that it was not clear in my original essay that I fully agree with Braude that survival research must be much broader than certain limited kinds of field investigations. However, as I said in my original remarks about Michael Grosso's conference paper (Cook, 1996, p. 349), when we talk about expanding the database, we also need to state explicitly why and how particular kinds of data or phenomena are relevant to the problem we are addressing. Like Braude, Frederic Myers thought that the study of latent and unusual human capacities, including those of prodigies, savants, and persons showing unusual dissociative phenomena, provided a necessary framework for studying the question of survival. As Myers put it, survival research "will not be difficult only, but impossible,—it will lead to mere confusion and bewilderment—if it be undertaken without adequate preliminary conception of what our own personalities, our own intelligences, are in reality and can actually do" (Myers, 1891, p. 121). Unlike Braude, however, Myers was interested in such phenomena not only because they often provide an alternative explanation to the hypothesis of survival, but also, more broadly, because they suggest that our ordinary conceptions of mind and mind-matter relations may be much too narrow and that consciousness may not therefore be confined to the psychophysiological framework in which

[2]James argued that the findings of psychical research seemed to break the deadlock between the production and the transmission models to favor the latter. Some parapsychologists, such as Beloff (1989), agree; others, such as Palmer (1995, pp. 5-6), disagree.

we ordinarily see it functioning. For Braude and anyone else who believes that survival research is "conceptually naive" and "empirically narrow," I can only urge them to go back to Myers for a view of what survival research was in its beginnings, and what Myers and some of the other 19th-century psychical researchers intended it to be.

Justine Owens has misunderstood a major point of my essay if she thinks that I am satisfied to continue to operate within the present "either/or explanatory frame" in which survival research now finds itself. It is true that either our personal consciousness survives in some recognizable form, continuous with our present consciousness, or it doesn't; in that sense, the question remains an "either/or" one. But, as I tried to argue here and elsewhere (Cook, 1987), we will break through our present impasses, and make conceptual progress, only by devising and testing predictions derived from the two competing positions. Abandoning "presently immovable questions" may be one way, as Owens suggests, "to get [them] out of the way"; but we also run the risk that, unaddressed, they will thus remain *permanently* immovable questions. Large, fundamental questions are not questions to be put aside either as unanswerable or as premature, but they are the questions that should be guiding, directly or indirectly, all research. If physicists had abandoned fundamental questions about the nature of light as "immovable" because of the longstanding theoretical impasse on the issue, we would surely have never seen the revolutionary developments in physics that have occurred in this century.

As I indicated earlier, I share John Palmer's view that science will never prove survival; but I share this view because *all* science is probabilistic, not because I think, as Palmer does, that the question of survival is primarily an existential one akin to the question, "Does God exist?" The question of whether or not disembodied minds exist seems to me more akin (although obviously not identical) to the question of whether or not other *embodied* minds exist. We might ultimately infer that disembodied minds exist in much the way that we infer that other embodied minds exist—by observing and making predictions about their behavior. We can never prove that other minds—disembodied *or* embodied—exist; but, by addressing the question as an observational one, we might make some well-grounded inferences.

I also do not wish to argue that Palmer's model is not a conceptually valid model for making empirical predictions, although none of the predictions that Palmer mentioned in his paper seem to me to be unique to his theory. More importantly, perhaps, I suspect that few people would find Palmer's theory either conceptually or empirically compelling enough to override James's (1890) classic arguments against atomism in psychological theorizing. However, I prefer not to get dragged into a discussion of the strengths or weaknesses of Palmer's theory because, I repeat, the theory simply has nothing to say, one way or the other, about the survival question as most people would define it. Palmer's theory does *not*, as Palmer claims, "directly" address the question of whether or not "we" survive death; it only addresses the question of whether or not certain psychological elements that Palmer calls psiads survive death. Even if Palmer can demonstrate somehow that psiads exist and survive death, this fact would tell us nothing about whether or not an individual consciousness or personality survives death—unless Palmer can also take the additional, enormous step of demonstrating that consciousness is simply an aggregate of these atomistic psiads. Again, I refer readers to James (1890) for a discussion of the likelihood that anyone can do this. My objection to Palmer's theory, therefore, is not because I do not find it "very appealing"; it is because I think any atomistic theory in psychology is fundamentally flawed and because I find Palmer's theory irrelevant to the question at issue.

Palmer also objected to my statement that parapsychology has become increasingly isolated from the rest of science because of "the failure of many contemporary parapsychologists to examine the relationship between the phenomena they study and other normal and abnormal phenomena" (Cook, 1996, p. 349). I should perhaps not have made such a strong statement without elaborating on it further; and I cannot elaborate on it much further here. But what I meant was that parapsychology remains outside the mainstream of modern science, ignored (or worse) by most scientists, because its phenomena seem to violate the basic model of the universe, and particularly of the relationship of consciousness to the material world, assumed by most scientists. The huge disjunction between parapsychology and the rest of science is probably not going to be bridged by the relatively minor pursuits, such as demonstrating psychological correlates of psi test scores, that Palmer mentions. What will be required is a theoretical model, supported by adequate empirical evidence, of the continuity and relationship of a broad spectrum of psychological phenomena—normal, abnormal, and paranormal. And, once again, I point to Myers as the primary example of someone who has made an attempt to develop such a model, however imperfect or preliminary it may have been.

Eugene Taylor says that I misunderstood the intent of the PF conference; I may well have, but since I was not at the conference, I could only assume the intent from the opening statements of the organizers. Therefore, the intent of my essay was to assess the contribution of each paper to the science of survival research. If, as a result, I failed to convey the main point of Taylor's paper, for that I apologize. But Taylor has seriously misrepresented my position by suggesting that I am seeking to uphold a "reductionistic" brand of science, a psychology rooted in 19th-century experimental psychophysics, and a "metaphysics of physicalism." (It is difficult for me to understand how Taylor can attribute such views to anyone who believes, as I do, that the methods of science can be directed at such questions as the survival of human personality after death!) Perhaps it would help clarify my position to refer Taylor to an earlier essay that I wrote (Cook, 1991), in which I argued that 20th-century psychologists and parapsychologists have for the most part failed to produce significant new knowledge because they have equated science with methods, adopted directly from the physical sciences, that are too narrow for the subject matter of psychology and psychical research. But the questions posed by psychology (including, and especially, the psychology of inner experience or consciousness), parapsychology, psychical research, and survival research call for an *expansion* of the methods and scope of science, not an abandonment of science in the face of such questions. In my view, the essence of the scientific method is to systematize phenomena, not to reduce or quantify them, and therefore I would argue that science, understood this way, provides not the only or even the best kind of knowledge, but certainly the most reliable and the only shared form of knowledge. Since Taylor contrasted what he presumed to be my view of science with that of Myers, James, Murphy, and others, perhaps I can correct his misunderstanding by again quoting Myers, whose view on this point I in fact share completely: Science, Myers said, may not provide a person's "only or his deepest insight into the meaning of the Universe"; and science

> rests on assumptions which we cannot fully prove; or which even indicate, by their apparent inconsistency, that they can be at best but narrow aspects of some underlying law imperfectly discerned. . . . [But] Science forms a language common to all mankind; she can explain herself when she is misunderstood and right herself when she goes wrong; nor has humanity yet found...that the methods of Science, intelligently and honestly followed, have led us in the end astray. (Myers, 1900, p. 114)

Michael Grosso's remarks made clear to me how many views he and I share, and I think many of his views are, like mine, Myers-inspired. We do, as he points out, have evidence suggestive of survival that is good enough to have created and maintained the theoretical impasse at which we find ourselves. What we do not have is a theoretical model that can make sense, not just of survival phenomena or even of parapsychological phenomena in general, but of all these phenomena in the broader context of an understanding of consciousness and psychophysiological functioning in general. I have alluded to Myers several times in this reply, and I will do so again, because I think what is needed in survival research, in parapsychology, and in psychology in general is a return to research that is guided by the larger questions about the nature of consciousness that guided Myers (Cook, 1993). Like Grosso, Myers believed that the demonstration of latent capacities such as psi *supported*, and were not an alternative to, the hypothesis of survival, and he developed a model of consciousness based on the idea that consciousness is narrowed in response to the demands of the present physical environment and that its latent capacities can emerge under different, perhaps even post-mortem, environments. He recognized, however, that his (and our) theorizing is limited by our present conceptions of mind and matter, and he fully expected that new conceptions of mind and mind-matter relations would emerge in response to psychical and psychological research as he saw them. Unfortunately, neither psychical research nor psychology has proceeded as Myers hoped, guided by larger questions about the nature of mind-matter relations. Survival research, focused as it is on the mind-body problem in its most extreme as well as humanly relevant form, has the potential to redirect the attention of psychologists and parapsychologists to the fundamental questions that in fact underlie both psychology and parapsychology.

Survival research needs new ideas. It needs people who are not content to dismiss, as being beyond the limits of science, large questions about the nature of consciousness and the possibility of the survival of our personal self. But it also needs people who are not content to carry on with business as usual, arguing from the same polarized positions. If part of the intent of the organizers of the PF conference was to stimulate new ideas about new directions for survival research, I hope that this exchange between me and some of the contributors to the conference will help fulfill that intent.

References

Beloff, J. (1989). Dualism: A parapsychological perspective. In J. R. Smythies & J. Beloff (Eds.), *The case for dualism* (pp. 167–185). Charlottesville: University Press of Virginia.

Cook, E. W. (1987). The survival question: Impasse or crux? *Journal of the American Society for Psychical Research*, **81**, 125–139.

Cook, E. W. (1991). Psychology and parapsychology: Some comments on Paul Kline's *Psychology exposed, or the emperor's new clothes*. *Journal of the American Society for Psychical Research*, **85**, 183–192.

Cook, E. W. (1993). "The intellectual background and potential significance of F. W. H. Myer's work in psychology and parapsychology. Unpublished doctoral dissertation, University of Edinburgh, Scotland.

COOK, E. W. (1996). Survival research today: An essay review of of *Parapsychology and thanatology*. *Journal of Parapsychology*, **60**, 343–355.
JAMES, W. (1890). *The principles of psychology*. 2 vols. New York: Henry Holt.
KIM, K. H. S., RELKIN, N. R., LEE, K. M., & HIRSCH, J. (1997). Distinct cortical areas associated with native and second languages. *Nature*, **388**, 171–174.
MYERS, F. W. H. (1891). [Review of] The principles of psychology [by William James]. *Proceedings of the Society for Psychical Research*, **7**, 111–133.
MYERS, F. W. H. (1900). Presidential address. *Proceedings of the Society for Psychical Research*, **15**, 110–127.
OSIS, K., & MCCORMICK, D. (1980). Kinetic effects at the ostensible location of an out-of-body projection during perceptual testing. *Journal of the American Society for Psychical Research*, **74**, 319–329.
PALMER, J. (1995). Toward a general theory of survival. In L. Coly & J. D. S. McMahon (Eds.), *Parapsychology and thanatology* (pp. 1–25). New York: Parapsychology Foundation.

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