on p. 916 to a speculation by Moss.) Would he like to have his views attributed to *all* psychologists, or those of all other psychologists attributed to him? Siegel seems to be vexed that societies for psychical research continue to flourish in the United States and England; I am vexed at Siegel's implication that the societies for psychical research hold any collective opinions except that concerning the value of further research in this field. (See his attribution to the societies, also on p. 916, of views expressed by Gauld.)

Perhaps it is captious to draw attention to such blemishes; we should pardon them as thoughtless excesses of Siegel's enthusiasm for his disbelief. But I cannot overlook another. Siegel (p. 923) states that Osis and Haraldsson (1977) "call . . . 'otherworldly messengers' " the representations that some dying persons experience of dead relatives or religious figures. I censure this as quite unfair. Siegel's use of quotation marks around "otherworldly messengers" misleadingly conceals the fact that Osis and Haraldsson use quotation marks themselves, as for example, on p. 186 of their book, where they refer to the "otherworldly visitors" experienced by some dying persons; they do not refer to otherworldly visitors. In the places where they do not use quotation marks around "otherworldly," a reader can easily tell that they are considering the merits of various interpretations for these experiences. A heading in one of their chapters is: "Apparitions Worldly or Otherworldly: A Critical Evaluation" (Osis & Haraldsson, 1977, p. 101). Their book strains to consider alternative explanations that may accommodate all the facts related to deathbed visions better than that of survival after death.

Toward the end of his review, Siegel slides into a predicatory tone. He sagely warns his readers that perhaps, after all, there may be a life after death. He acknowledges that this question concerns "our basic cosmology of life and death." And he admits rather wistfully that his review will not put a stop to inquiries about it. I agree with all that.

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Reply to Stevenson

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I am sorry Stevenson (this issue) feels that some scholars and scientists may have been wronged by my review (Siegel, October 1980). I may have made some mistakes, either of fact or of emphasis, and I would be happy to put things right. I will address each of his points.

First, I do not consider myself an expert on survival of the human personality after death. I have only aspired to be a good student and observer of human and animal behavior while it is still alive. I agree that the results of a century of survival research thus far have been meager, but an increasing volume of publications in lay and medical periodicals has not strengthened the evidence—only made it more visible, not veridical. The study of animal behavior, rather than being a digression, is an important avenue through which we can explore parallel human behaviors. If we can explain such behavior as elephants' burial of their dead with food and flowers in terms of ethological stimulus control, we may not need to invoke the belief or disbelief in survival after death to explain similar human behaviors.

Certainly the phenomenology of the afterlife or near-death experience is similar to hallucinations. The differences remain meager and unproven in controlled scientific tests. The Stevenson articles I reviewed are brief accounts of his extensive case studies, but they remain reflective of his data and interpretations regarding reincarnation. They also appear to avoid the difficult question of why ethology is a digression when there are so many reincarnated sacred cows around.

Stevenson finds a strange division in my reference list: popular or "trash" books and scholarly or scientific ones. I confess I can't always tell the difference here. The place of purchase is also no help in deciphering this mystery. For example, I recently purchased a copy of Ring's (1980) fine study *Life at Death* in a California airport and got a copy of Reyes' (1970) fanciful *Scientific Evidence of the Existence of the Soul* at the UCLA medical bookstore. I join with Stevenson in inviting interested readers to judge for themselves, and I also admit that the differences between ghosts and apparitions are confusing to me. I am hopeful that my deceased cousin will call in to explain it.

I do know that telepathy is a type of ESP, but the literature I was describing used these terms separately and I chose to retain the same phrasing. I also know that communication with the dead via mediums, spiritualists, and ghosts is like apple growers, apples, and apple baskets all bunched together. I hope they can sort themselves out from all that fruit. As for tricks of punctuation and sprinkling on the Queen's English in public, "I" "am" "guilty." I missed the two other commentaries on Stevenson's article published by the *Journal of Nervous* *and Mental Disease*, but I am pleased that he has more than one impartially partial friend and admirer. I am sorry he was vexed, more sorry to learn I had caught it as well. I will hasten to clear up any remaining blemishes. But I will not remove the quotation marks from "otherworldly visitors" until they request it themselves.

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On the Mitosis of JCPP

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Although I am among the majority of Division 6 members who favor the division of the *Journal of Comparative and Physiological Psychology* (JCPP) into separate comparative and physiological publications (Demarest, November 1980), this prospect does raise a number of concerns. Given the poor marketplace for comparative psychologists (Demarest, 1980), one may ask if there are enough psychologists conducting genuinely comparative research to justify such a publication? Although comparative/ethological approaches to behavior are increasingly important in psychology and have stimulated considerable research (e.g., Hinde & Stevenson-Hinde, 1973; Seligman & Hager, 1972), much of this effort has rapidly become focused on very few species. Learned taste aversion is a case in point. Although the far-reaching ecological/evolutionary implications of this phenomenon continue to be investigated in a variety of species by a small body of workers (see, e.g., Barker, Best, & Domjan, 1977), the bulk of contemporary psychological research is conducted on the rat. I attempted to get some minimal estimate of comparative psychological research activity among psychologists by surveying the 1977–1979 contents of two journals (*Animal Behaviour; Behavioral and Neural Biology*) that regularly feature comparative animal research. I identified all articles that were comparative in nature and that had been authored or co-authored by individuals affiliated with psychology departments or laboratories.[1] Although the criteria employed were conservative and almost certainly underestimated comparative research (Gottlieb, for example, was excluded) they may provide a "bare bones" estimate of potential contributors to such a journal. The survey yielded 29 (1979) to 34 (1978) publications per year that would be appropriate for a journal of comparative psychology. If one adds to these numbers the comparative articles that already appear in JCPP and assumes that other disciplines would, as they do now, contribute to such a journal, it seems clear that there is ample research to justify at least a modest journal of comparative psychology.

A second concern relates to numbers. Although there is a lively body of comparative research, the predominance of the rat in psychology has remained largely unabated from the 1930s (Beach, 1950) to the present,[2] and the number of publications of research conducted on this species simply dwarfs all others. Without sustained editorial commitment to a broadly comparative psychology, such a publication would be in great danger of becoming yet another outlet for rat data. I believe, however, that the growing theoretical importance of ethological, ecological, and sociobiological approaches to behavior demand the institution and nurturance of a genuinely comparative publication within APA. It would be unfortunate if APA did not participate in this exciting area.

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Sociobiology: Some General Considerations

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When an economic system is foundering, its apologists in science rush to dust off the old answers or, better, to dress them up anew. Always, however, the centerpiece is human nature as a kind of predetermined destiny, sanctifying possessive individualism and counteracting the idea of social progress. Social science, especially psychology, is a tool used, consciously or otherwise, by saint or sinner.

Sociobiology is the latest resurgence of this phenomenon. Professedly neu-

[1] Excluded from the tally were (a) all brief reports, (b) articles in which physiological manipulations were the primary variable of interest, (c) articles that employed rats as the sole species (unless strain comparisons were carried out), and (d) articles in which one or more of the authors could not be unambiguously identified with a psychology department or laboratory. Publications from "hybrid" programs (e.g., psychobiology) or with ambiguous institutional affiliations (mental health institutes) were rejected.

[2] My comparative psychology class recently surveyed the species used in articles appearing in *Animal Learning and Behavior*, JCPP, and the *Journal of Experimental Psychology: Animal Behavior Processes* from 1977 through 1979 using the categories described by Beach (1950). This survey revealed that the rat was used in 55%–59% of all articles. Although the "All Vertebrates Except Mammals" category had increased to 17%–19%, this increase is largely attributable to the increased use of pigeons.