

### **Kenneth Ring Responds**

To the Editor:

I appreciate Ken Vincent's thoughtful critique of the Omega Project and the reasons for some of his reservations about the study. Indeed, in my reply I think I can show that we are, on most points, in essential accord and not nearly so much at odds as his letter may have suggested. Let me, then, take up each of his concerns in the order in which he presents them.

His first animadversion relates to the instruments that comprised our Omega Project battery about which he correctly states that there is essentially no information concerning their psychometric properties. This lack is certainly a deficiency of the study, but it is hardly one of which I am unaware. For example, in another, more complete published version of the Omega Project (Ring and Rosing, 1990), I conclude with the following cautionary statement:

Finally, we must, needless to say, recognize the many limitations of this study, especially its total reliance on self-report measures and in some cases previously untested measures. (p. 95)

Moreover, there are slight, but important, inaccuracies in Vincent's critique on this point. For example, it is not true that all the questionnaires in our study are in unpublished sources. Two of them, for

example, are either identical to or slightly adapted forms of the instruments that I used for the near-death research I reported in *Heading Toward Omega* (Ring, 1984). And an additional study on psychological dissociation, carried out by Barbara Sanders and Marina Giolas (1991), also used one of our Omega project questionnaires—the one pertaining to child abuse and trauma—and found a Guttman split-half reliability of 86.

The reason that Christopher Rosing and I did not report reliability coefficients in our study was simple, if regrettable: it turned out that the statistical program we used to analyze our data did not, to our chagrin, provide any indices of reliability. But, in fact, our research was not undertaken with the aim of assessing specific psychometric issues. It was, rather, an effort to explore entirely new ground concerning predispositional factors and aftereffects of NDEs, and for this the general approach that I followed in *Heading Toward Omega* seemed warranted, especially in view of the fact that the findings reported there concerning aftereffects have generally been supported by the research of others (Atwater, 1988; Flynn, 1986; Grey, 1985; Sutherland, 1989). *Of course*, the validation of these instruments is a necessary task for the future, but, because, as I have indicated in a future article in this *Journal* (Ring, in press), I have now completed my own work in near-death studies, I must hope the direction of research that Rosing and I initiated with the Omega Project will be carried forward by others.

In answer to Vincent's second point—why not some generally recognized personality tests to measure mental health (or psychopathological) factors?—I have a similar reply: that was not an objective of this study. I have elsewhere (Ring and Rosing, 1990) argued that such assessments have not proved especially illuminating in investigations of persons reporting other kinds of anomalous experiences, and I had no special reasons for thinking they would be particularly helpful here in clarifying the questions that the Omega Project was designed to address. After all, in any research project, one can only do so much, especially when one is already asking respondents to fill out a batch of questionnaires, and the instruments we employed were precisely the ones we thought would give us the most return for our investment. Needless to say, I have no objection to someone conducting the kind of study that Vincent suggests, but the evaluation of the mental health status of NDErs and those interested in NDEs was never at issue in the Omega Project.

This last comment relates in turn to Vincent's final major objection,

which has to do with his contention that the NDE is independent of psychopathology. Of course, that is, as Vincent notes only in passing, exactly what Rosing and I say in our article (see p. 236) and indeed say emphatically. I will not take the space here to comment on each of the various points Vincent makes to buttress his own position, other than to state that, by and large, I agree with him and to say that I'm surprised that one could draw any other inference from our article. Again, however, as much as I generally concur with Vincent's views here, I must note that there are again some inaccuracies and misleading implications in his commentary on this matter. For instance, it is simply not true that a difference between OBEs and NDEs is that the former are voluntary whereas the latter are not; it is well known that many OBEs occur inadvertently and unintentionally. Furthermore, in consistently using the term dissociative *disorder* in his critique, an important distinction is lost: there are dissociative tendencies, which are not in themselves pathological; and dissociative disorders, which are. Someone who suffers from multiple personality disorder certainly does, by definition, have a dissociative disorder. NDErs, as a rule, do not suffer from such disorders, and nothing in our paper was meant to imply that. Some NDErs may have dissociative *tendencies*, however, and that would be the term that I would prefer to use in this context.

Still, these quibbles aside, I want to conclude by re-emphasizing what I said in the beginning: Vincent and I are largely in agreement, and I found the spirit and substance of his critique helpful in suggesting the kinds of theoretical, methodological, and empirical issues with which further research along the lines of those explored in the Omega Project needs to concern itself.

## References

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