Susan Blackmore Replies

To the Editor:

V. Krishnan makes several interesting points. First of all I think he fails to understand my comments on Michael Sabom's (1982) control group. Yes, the purpose of a control group is to compare, but usually it is most valuable to compare two groups in which everything is held constant except for the variable being studied. In this case there is no obviously ideal control. Sabom's is certainly a good start, but his control patients did not actually go through the cardiac resuscitation and therefore did not have access to all sorts of important information about it.

That this might be important is clear from some of the errors they made. Sabom wrote, "The most common error was the belief that mouth-to-mouth breathing would be the routine method of artificial ventilation in the cardiac-arrested, hospitalized patient. In truth, mouth-to-mouth breathing is a rarely used means . . ." (1982, p. 120). My point is that the control group might well be expected not to know this, but the actual NDE patients had gone through the resuscitation. If one assumes that they were behaviorally unconscious but still able to feel something, then they might well have felt the electric shocks and other manipulations. If they had been resuscitated by mouth-to-mouth breathing they might have felt this too. The same applies to many of the other errors. If a patient could feel or hear even a little, he would gain quite a lot of information about the procedure and order of events that the control group did not have. I therefore suggested that Sabom's control group was not ideal, though I must repeat it was a very useful start. We now need other kinds of control groups as well. For example subjects might be put through a fake resuscitation procedure, perhaps under deep relaxation, and then asked to reconstruct a view from above. Different amounts of information could be made available to see how this affected the accounts. But this is a long task. I only suggest it as a possibility for the future.

Crucial to Sabom's argument is that there are some features that cannot be sensed in this way, such as visual details of the behavior of needles on a defibrillator, which was reported by one NDE patient. This still stands and I totally agree with Krishnan; it is important evidence. The point is that we need to be clear first about how much information a person could gain normally during the resuscitation and secondly whether NDE patients really do acquire information that goes beyond this limit. This is not easy to do but will be necessary if we are to draw strong conclusions about the paranormality of the NDE or its implications for survival.

Krishnan's second point concerns the difference between single, involuntary and voluntary OBEs. I agree that there are suggestions in the literature that the two are different. And in my own experience they are qualitatively different, but I do not agree that this is a very useful categorization with respect to the accuracy of perception. Krishnan confuses two separate issues, twice. First he confuses the number of OBEs a person has had and whether the OBEs were voluntary or not. And secondly he confuses clarity with accuracy of perception. I shall try to extricate these.

First of all, it is true that single OBEs are rarely voluntary ones, but this does occasionally occur and in any case the rarity of voluntary, single cases tells us very little since both single OBEs and voluntary OBEs are rare. I have actually been interested in these differences and therefore asked relevant questions in some of my surveys. In my random survey of Bristol residents (Blackmore, 1984a) I found 39 people who had had OBEs. Of these only 4 had had voluntary OBEs and these were, as Krishnan would expect, multiple OBErs. However, only 6 of the 39 had had single OBEs, so this is not surprising and tells us very little. I also asked about clarity of perception—whether it was as usual, brighter and clearer than usual, or vaguer and dimmer than usual. There are no differences in the reported clarity between those having multiple and single OBEs or those having voluntary or only involuntary OBEs. In this survey respondents only gave one answer to the question on clarity of vision. I did not ask separately about each of their OBEs, so it is possible that a difference was masked. Certainly I would like to follow up this difference.

However, what should we conclude if such a difference were found? It would be very interesting if voluntary OBEs were less clear than involuntary ones but that would be expected for all sorts of reasons even if all OBEs were based on imagination. This is quite separate from any difference in the accuracy of perception. Contrary to Celia Green's opinion (1968), I have no reason to suppose that involuntary OBEs provide more accurate perception. Indeed, the kinds of inaccuracies I pointed out on page 151 are precisely the kind that are reported in involuntary OBEs (and remember that involuntary OBEs are by far the majority). One of the most interesting cases I have investigated was a classical OBE, occurring quite involuntarily in a Canadian architect, who seemed to travel across the Atlantic to England. He described the streets and shops and people he saw in great detail, but his descriptions were hopelessly inaccurate and could not have derived from the actual scene at the place he "visited" (Blackmore, 1982). I agree that we need better categorization of OBEs, but I do not think that the one Krishnan points out is especially helpful.

I am interested in his next point, about viewpoints in memory. Certainly some people, like Krishnan, do not imagine or remember scenes as though from above. However, others, like myself, do so almost all the time. There is recent work (Nigro and Neisser, 1983) showing that people differ in this respect. I would predict that this variable affects the chance of a person having an OBE and have begun research on this (Blackmore, 1983) but it is too early to say whether I am right or not. However, it is an example of the ways in which my approach leads to testable predictions (for others see

Blackmore, 1984b). Clearly Krishnan and I differ in the results we would expect. I hope we shall soon be able to find out who is right.

Finally, I am grateful to him for pointing out the relevance of the fascinating work by Georg von Bekesy (1967). Certainly I totally agree. The work provides an analogy in support of a naturalistic explanation of the shift in viewpoint in OBEs. What I don't understand is that he contrasts naturalistic or physical explanations with psychological ones. The study of the visual system (in natural, physical, and all other aspects) is a fundamental part of psychology. Like Krishnan I hope that we shall be able to understand both visual perception, imagination and the OBE better in relation to each other.

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