

Letters to the Editor

Scientific Vs. Anecdotal Near-Death Studies

To the Editor:

I read with great interest Melodie Olson's article "The Incidence of Out-of-Body Experiences in Hospitalized Patients" in this journal (Olson, 1988). This study represents one of the first attempts to study systematically near-death experiences (NDEs) and out-of-body states in controlled populations. It represents a major advance over what previous work exists in the literature.

Prior research on NDEs in adults has been primarily anecdotal. Although that work is fascinating, and important in that all clinical research must first begin with anecdotes, data obtained by controlled clinical studies is essential to begin to analyze NDEs scientifically. My group's Seattle study of NDEs in children (Morse, Connor, & Tyler, 1985; Morse, Castillo, Venecia, Milstein, & Tyler, 1986) is to my knowledge the first scientific analysis of NDEs in a prospectively identified population of seriously and critically ill patients.

Karlis Osis and Erlendur Haraldsson described their landmark study (1977) as a "broad survey" and readily acknowledged "bias in reporting and sampling." Kenneth Ring, in his book *Life at Death*, subtitled *A Scientific Investigation of the Near-Death Experience* (1980), candidly admitted that he relied on word-of-mouth referrals and used newspaper advertisements in collecting data. He stated that "hospital referrals were not likely to lead to a sufficient number of cases . . . to permit meaningful statistical comparisons" (p. 27). Michael Sabom acknowledged the same problem with obtaining unbiased data in his book, *Recollections of Death*, subtitled *A Medical Investigation* (1982). These authors have given their books titles that imply a scientific method, but that implication is not backed up by their own descriptions of their research methods.

Even those authors who have published articles in mainstream peer-reviewed scientific journals have had to rely on newspaper advertisements and word-of-mouth referrals for their data. Ian Stevenson and

Bruce Greyson (Stevenson & Greyson, 1979; Greyson & Stevenson, 1980) and Russell Noyes (1979) have analyzed collections of anecdotal reports. Furthermore, there are considerable differences between the study populations of Noyes and of Greyson and Stevenson, and yet their research is frequently discussed together as if they were talking about the same phenomenon. For example, Noyes excluded patients from his study who lost consciousness (Noyes & Kletti, 1976), whereas Greyson and Stevenson analyzed patients who survived serious illnesses without such exclusions.

This lack of clinical studies in the literature severely limits the conclusions of such authors as Michael Grosso (1981) and Robert Kastenbaum (1984) who attempt to take these same studies and build shaky speculations based on tainted data. It was for this reason that in our Seattle study we prospectively identified our study populations in a blind fashion, made no assumptions of what a near-death experience should be like based on previous descriptions in the literature, and had a third party, unfamiliar with the sources of our data, review blindly all collected data. Instead of asking people who had had NDEs to tell us about their experiences, we sought out survivors of cardiac arrests and asked them to tell us what such an event was like. We carefully age-matched these patients with a seriously ill control group who were treated with identical medications and had similar degrees of hypoxia and other laboratory abnormalities. Both groups were hospitalized in an intensive care unit setting.

Research on near-death experiences remains in its infancy. I do not mean any disparagement of the excellent work the aforementioned authors have done, but review their work in an effort to highlight the importance of Olson's study. My own study, although surviving the cleansing fires of the Human Subject Review Committee, similarly has methodological flaws, as all clinical research will have. Instead, I am writing to prod the scientific community to realize that there is much work to be done.

Olson's paper actually whetted my appetite for more analysis of the excellent data she has obtained. Her study is unique in the adult literature, which she acknowledged by describing the aforementioned studies as "case-study information." However, I have the following questions for her:

1. Why weren't surgical patients separated from medical patients? Surgical procedures and anesthetic agents introduce a confounding variable that medical patients would not have.

2. Of the patients who had NDEs, could anything be gained by analyzing the types of illnesses, degree of consciousness, severity of illness, or types of medication they were on?
3. Why did she describe NDEs as a subset of out-of-body experiences? In her results, she did not give a breakdown of the types of experiences her patients had, but then in the discussion described some classic NDEs. She frequently referenced Glen Gabbard and Stuart Twemlow's book (1984), but then did not separate out-of-body experiences from NDEs, as Gabbard and Twemlow did.
4. No data were given on the number of patients who refused entry into the study. This is of vital importance since no systematic procedure was used in collecting data.
5. No data were presented on which medications the patients were taking, and any relationship between medications and various experiences reported.

Near-death experiences have profound implications for the living, as we all will die. Unanswered questions abound in the field, including: (1) What effect do medications have on causing or suppressing NDEs? (2) Are pre-death experiences at all related to NDEs? (3) Are NDEs a unique phenomenon at the point of death, or are they a subset of out-of-body or mystical experiences? (4) Are there physiological correlates to the NDE? In order for us to do the hard work of transforming the appalling way we mistreat dying patients into a positive final experience, we must have hard data to share with our medical colleagues.

Today, near-death research is nothing more than excellent collections of folk tales and legends. I challenge Olson and other researchers in this field, my own research group included, to begin the hard work of collecting data that will withstand the peer review process. In this way, I predict that we will catalyze a profound change in the way hospitals and physicians (mis)treat dying patients.

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Melodie Olson Responds

To the Editor:

I appreciate Melvin Morse's kind words about my study, and his interest in more data. I can supply answers for some of his questions.

1. Diagnoses on all patients were recorded from patients' charts. As few patients have only one diagnosis, the primary diagnosis as well as up to three secondary diagnoses were recorded. But diagnoses changed from the time of admission to discharge for many patients. Since data collectors were dependent on records currently in use and undergoing constant change, the accuracy of the diagnoses at any given time could not be guaranteed. Statistical correlations between group (i.e., OBEr vs. non OBEr) and type of patient (i.e., medical vs. surgical) were not significant. These correlations were only done on primary diagnoses. It should be remembered that

many persons reported OBEs that occurred prior to the present hospitalization, some of them completely independent of any hospitalization. For those, anesthetic agents and the like are not confounding variables.

2. & 5. All patients interviewed were alert and able to answer standard orientation questions. We listed medications for each interviewee. We then classified those medications that affect the central nervous system, and narcotics, separately. We noted whether any of those drugs had been taken within six hours of the interview. No relationships were found between reports of OBEs and use of these medications. In addition, the six patients who reported OBEs during this hospitalization were not taking any of the same drugs.

We did not attempt to classify severity of illness at this time.

3. This study focused on the OBE, not the NDE. People who report OBEs frequently have them within the context of an NDE. For our purposes, then, it was useful to consider the NDE as one context or set in which OBEs occur. You will notice that the way the question about OBEs is asked ("Have you ever felt your mind, consciousness, or center of awareness to be at a place different from your physical body?") will not elicit a positive response from NDErs who did not have an OBE as part of that experience. The statistics on OBEs are not reported as if they represent all NDEs that might have occurred.

In our sample of 31 reported OBEs, nine were associated with NDEs, 16 were not associated with NDEs, and in six cases that determination could not be made from the interview.

4. No data were kept related to patients who were not a part of the study, although I agree it would be helpful. Few patients actually refused participation, but many were off the care unit for tests or procedures, or had visitors and did not want to be interrupted while the data collectors were available. Because of short hospital stays, many of the patients were never asked to become a part of the study. One suggestion to avoid this problem in future studies is to work from admission lists and make appointments daily, at the time of admission and at discharge. That would require notification of all impending discharges, a luxury we did not have during this study.

I appreciate the opportunity to respond to these questions. Dialogue enriches the research process. I wish Morse good luck in his pursuit of greater empiricism in near-death research.

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OBEs in the Blind

To the Editor:

Harvey J. Irwin (1987) suggested that sensory deprivation may not be the fundamental cause of the out-of-body experience (OBE). The reason he cited is that although sensory bombardment and extreme elation do not entail sensory restriction, they facilitate the OBE. He added that these and other OBE-conducive situations promote "a state of strong absorption in the content of one's experience or mentation" (p. 58) and therefore the process of absorption should be considered central to the occurrence of the OBE.

I would like to point out that I (Krishnan, 1985) have indicated how both sensory bombardment and states of intense positive emotions can lead to a decrease in, and invariance of, sensory (that is, information) input to the brain. The important role that the reticular formation plays in reducing information input in situations of sensory deprivation and sensory overload has been emphasized by Donald B. Lindsley (1961). As far as I can see, the net effect of absorption, which involves withdrawal of attention, deliberately or otherwise, from sensory and proprioceptive stimulation (Irwin, 1980), is a reduction of input of information to the brain.

Secondly, I doubt whether we can say with certainty what is central to the occurrence of the OBE in the present state of our knowledge about the experience. It has been studied in depth only from a psychological perspective; until it is studied as well from other perspectives, such as the neurological or biochemical, I think we should suspend judgment.

I appreciate Irwin's thoughtful comments on my suggestion that study of visual OBEs in congenitally blind persons may help us understand whether or not out-of-body vision has a physical basis. I hope his