Response to “Critique of ‘A Prospectively Studied Near-Death Experience with Corroborated Out-of-Body Perceptions and Unexplained Healing’”

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ABSTRACT: In this article, I respond to a critique by Michael Rush of a 2006 article from this Journal in which I and my co-authors described a case of a near-death experience with veridical components and an inexplicable healing. I address each point from the critique in the order in which it was raised. Overall, I found most of the criticism to have been of points I had already addressed in previous publications, and the critique also provided me an opportunity to clarify a few points I had not previously detailed. For me, this professional exchange has served to underscore the difficulty of conducting methodologically sound prospective research on near-death experiences.

KEYWORDS: near-death experience; out-of-body experience; veridical; healing; critique

I would like to thank Janice Holden, editor of this Journal, for giving me the opportunity to respond to the article written by Michael Rush. I read with interest his many criticisms of my work in general (Sartori, 2008) and the article that I co-authored with Paul Badham and Peter Fenwick that was published seven years ago (Sartori, Badham, & Fenwick, 2006). It is always helpful to get another perspective, and I welcome constructive criticism, as I believe it is essential to take into consideration for future research.

In view of the issues raised, I think I am best placed to reply in full as opposed to the other authors of the article (Sartori et al., 2006),

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as it was I who conducted the research and it is also my PhD thesis (Sartori, 2008) that has been the target of Rush’s criticisms. In the following text, I have responded to each criticism in the order in which Rush presented it in his article. But first, I would like to make some clarifications.

On page [2], Rush stated that “the assumption usually made is that the [near-death experiencer] is clinically dead, or at least unconscious, at this time.” I would like to clarify that no such assumption was made; rather, we (Sartori et al., 2006) stated clearly in the article that the patient concerned was deeply unconscious, his eyes were closed, and he was not responding to verbal command or deep painful stimuli.

Also, in his introduction, Rush stated that we (Sartori et al., 2006) concluded that “[the patient’s] corroborated observations could be taken as validating the objective reality of his NDE.” In actual fact, this is Rush’s inference but is not the conclusion we stated in the original article. Part of the actual conclusion from the original article was,

There are many aspects of this case for which our current brain/mind models cannot provide an adequate explanation . . . Although this is only one case, it strengthens the cumulative experience derived from many other individual cases (Cook, Greyson, and Stevenson, 1998; Sabom, 1998; van Lommel, van Wees, Meyers, and Elfferich, 2001) that suggest that our current models of consciousness must expand in order to provide an adequate explanation of NDEs. (Sartori et al., 2006, p. 83)

Rush stated that the article focused on my research question, Is the OBE veridical? However, that question actually was not the sole purpose of the article. My co-authors and I actually wrote the article as a case history that emerged during a prospective study in which I set out to investigate many aspects of near-death experiences (NDEs), including whether the out-of-body aspect of the NDE in which the experiencer purportedly perceived the material world (ND OBE) involved veridical perceptions. Both the veridical aspect and healing aspect occurred in the case of Patient 10, so we discussed both aspects in the article.

Reliability of Accounts Based on Fallibility of Memory and Expectation

By “cognitive mechanisms” Rush was referring to memory fallibility, expectation, and cryptomnesia.

Regarding memory fallibility, the research I undertook was planned a year in advance, and we had taken into consideration the possibility of expectation and memory errors influencing any experiences that may have been elicited during the course of the study. One reason to interview the patients as soon as possible and take notes during the interviews was to reduce the possibility of errors of memory.

With regards to expectation, if Patient 10 had generated his NDE based on expectation, I would have anticipated that nothing in his experience would have been surprising to him; however, he did, in fact, express surprise at some aspects of his experience. For example, he saw his deceased mother-in-law, but he had never met her while she was alive, so he was puzzled as to why she had appeared. Conversely, he was perturbed that he had not seen his own mother, as he had been emotionally close to her when she was alive. Other patients in this prospective study who reported NDEs also reported unexpected features such as seeing relatives they did not expect to see, whereas some patients didn’t see people they had expected and wanted to see (Sartori, 2008).

**Set and Setting**

I acknowledge Rush’s point that set and setting are also important to consider with regards to NDEs, and I agree that the possibility of suggestion is incredibly important to a study like this. I find it puzzling that Rush has gone to such lengths to point out the concept of set and setting considering that I addressed this matter on pages 57–58 of my Ph.D. thesis (Sartori, 2008). Also, the research that I undertook was very different than the drug research that Rush highlighted. Participants in drug research make an informed choice prior to entering into the research and taking the drug, implying that they have knowledge that something will occur and, therefore, have some form of expectation that may influence their experience.

By contrast, prospective NDE research is totally different due to the unpredictable nature of the NDE. Specifically, Patient 10’s NDE occurred in totally unexpected circumstances and was not at all in the context of drug research. The NDE occurred while he was making a good recovery from the acute phase of a life threatening illness; thus, his expectation was a continuation of further recovery rather than a sudden deterioration in his medical condition that led to a period of unconsciousness.
Patients Being Susceptible to Suggestion

On page [5] Rush pointed out that external factors can influence an individual’s memory of an event, again something I was well aware of before undertaking the study. I would like to emphasise that nothing was suggested to Patient 10 by anyone. At the time of his experience, the data collection procedure had been modified, whereby only patients who had undergone cardiac arrest were routinely interviewed. Patient 10 had not undergone cardiac arrest, so there was no intention to interview him at all. He communicated his experience voluntarily when he regained full consciousness. As the doctors, nurses, and physiotherapists who were on ward rounds approached his bed area, and before any of them had spoken to him, the patient volunteered the information. He pointed to the consultant and identified him as being the doctor who had examined him and had looked in his eye. He pointed to the physiotherapist and described her as having looked nervous and poked her head around the curtains. He also described me having cleaned his mouth with something long and pink. Personnel on the ward rounds had no interest in NDEs, and the consultant told the patient to talk to me about his experience. In fact, I was not present when it was first reported. I returned to the bedside as the ward rounds group was moving on to the next patient and the team told me what he had reported. Therefore, while taking notes, I asked Patient 10 to repeat what he had communicated to the team, and then I explained my research and invited him to participate.

Influence of the researcher and possible contamination of the experience is crucial to a study like this, which is why the protocol had been planned in ways to minimize these possibilities. Patients were first allowed free narrative of their experiences, and then they were asked to complete the NDE Scale (Greyson, 1983). If the Scale score indicated that an NDE had occurred, then a further open-ended questionnaire was administered. Any questions the patient may have had about the study were answered after the interview had been completed. The NDE Scale was implemented before the open-ended questionnaire to avoid an unnecessary lengthy interview if the patient failed to meet the criteria for an NDE. The open-ended questionnaire was based on one used by Kenneth Ring (1980) in his rigorous retrospective study.
Possibility of Patients’ Cryptomnesia

The possibility of cryptomnesia is also of great importance to such a study. In this case, cryptomnesia would have taken the form of the patient thinking his memory of events during his unconsciousness came from ND OBE observations at the time of those events whereas, without the patient realizing it, they actually came from memories the patient had formed through normal means such as conversing with others about the events. However, in this case, when the patient reported his NDE upon regaining full consciousness, he had not spoken to anyone else yet communicated his experience of ‘viewing’ events that I know had occurred because I was present while they were occurring. What he reported was original; there was no mistake in this. Interestingly, on follow-up interviews, the patient reported the experience in the same way as when he had first reported it and did not embellish it.

I also understand other factors that Rush pointed out, referring specifically to nine limitations and risks that Pekala and Cardena (2000) had identified. These factors included “forgetting, reconstruction and confabulation, verbal description difficulties, distortion and substitution in observation, censorship, lack of independent verification, dissembling and social desirability, demand characteristics, and ‘state-specific’ memory” (p. [6]). These are all extremely important factors, I agree. Unfortunately, it was not possible to control for all of these factors for the reasons stated below. Patient 10 reported his experience spontaneously, voluntarily, and unsolicited. Even if I had a team of researchers on standby to evaluate his testimony, by the time they could have got to the patient, he would have finished describing his experience. Rush’s criticisms are all valid when considered retrospectively but virtually impossible to implement in the reality of the clinical area. This research was undertaken in a busy ITU, and the unpredictable nature of NDEs further complicates their investigation. As is often found when research is undertaken, the ideals that were planned for are often not possible in reality.

Data Contamination Due to Giving Patient a Full Description of My Research

Rush has suggested that the description I gave to the patient about the research influenced his response. I would like to emphasize that I
described my research to Patient 10 only after it was established that he had reported components of an NDE. He had clearly described to the doctors, nurses, and physiotherapists on the ward rounds what he had experienced prior to any mention of my research. Rush further asserted that I “did not provide the specific content of this description, so it is impossible to judge if the description may have subsequently influenced the patient’s answers” (p. 6). This is simply incorrect. As evidenced by my Ph.D. thesis (Sartori, 2008, p. 342), I routinely said to patients, “I am interested in the reactions of patients who have survived critical illness. Some patients have indicated that they have experienced certain events while unconscious and very ill. I am sincerely interested in any such experiences no matter what they might be.” I further explained that I would record what patients recalled by allowing them to speak freely about their experience, and then I would ask them to complete a questionnaire (the NDE Scale) followed by asking them a set of questions (from the open-ended interview format). I followed this protocol with Patient 10, and he was then given the opportunity to participate in the research.

Research Study Not Kept Secret

Rush has pointed out that as the research was conducted over a period of five years, it seems unlikely that my research interests would have been a secret. This is correct; it could not have been kept a total secret. The research was deliberately kept as low key as possible in order to avoid such criticisms, but, I agree, it cannot be ruled out that some patients may have been aware of it. However, in the five years of data collection not one patient enquired about the research because they had inadvertently heard about it.

It must also be borne in mind that this study was one of the first to be undertaken in the UK, so the general public were not familiar with this kind of research at the time. The case of Patient 10 occurred prior to the publication of the other smaller prospective study that commenced at the same time as this study (Parnia, Waller, Yeates, & Fenwick, 2001) and the larger study that was published in The Lancet (van Lommel et al., 2001).

Rush suggested that knowledge of the research could have influenced future patients. I totally agree; this is a most important point and should always be taken into consideration when planning to undertake future research. The open-ended questionnaire addressed the issue of prior knowledge and asked such questions, as page 347
of my Ph.D. thesis shows (Sartori, 2008). Interestingly, as my thesis described, most patients did not attach significance to their NDEs, nor did they understand them. If they had been influenced by prior knowledge of the research, then I would have expected them to place more emphasis on their experiences and to better contextualize them.

**Possible Leakage of Information to Hospital Staff During Pre-Study**

Rush also suggested a leak of information due to the pre-study activity of constructing cards for use with the veridical aspect of the study. He stated: “They asked fifty staff and visitors what was on a set of test cards and then excluded use of cards that were guessed correctly.” This description is slightly incorrect. I want to clarify that there were no possible correct or incorrect guesses, as there were no test cards at this point in the research process. In preparation for the study, I asked the 50 people to guess what images they would expect to see if they were out of their bodies looking down on the situation from above. The images that they said they would expect to see were then listed and avoided when I constructed the cards.

Rush appeared most concerned that the staff and visitors asked to guess what images they would expect to see could have influenced what patients who reported an ND OBE would report. The reality of the matter was that no one was remotely interested as to why I had asked these questions. In fact, most of the 50 people asked to guess what images I would use were puzzled as to why I would ask such a question. It must also be taken into account that these people were questioned approximately three months before the pilot study and, therefore, approximately eight months before the formal data collection began, enough of a time lapse for the research not to be foremost in people’s minds. At no time did I inform these people what images I would be using.

However, let’s now take into consideration that the pre-study activity actually had influenced some staff with regard to the impending research. None of the 50 staff and visitors were informed of where the images would be located. Even if staff were aware of the location, no one could possibly know what the images were and which images were on which monitor, as they were rotated weekly.

Patients are admitted to ITU due to emergency situations; they don’t plan to have a cardiac arrest or to be admitted to ITU. Patients
are usually admitted to ITU from outside hospital and are usually unconscious on admission, and the staff has far greater concerns of undertaking life-saving measures than informing patients of NDE research.

The images used in the pilot study were simple images such as letters, numbers, and line drawings. The images used in the study were of random images and advertisements cut out of magazines and in no way resembled any of the images that were suggested approximately eight months earlier by the 50 people who were asked, when planning the study, to guess which images would be hidden.

**Curious Work Colleagues Peeking at the Hidden Images**

Rush's point that curious work colleagues could have peeked at the hidden images is very valid but has already been addressed in my Ph.D. thesis (Sartori, 2008) and in a prior publication (Sartori, 2004). The pilot study was undertaken in the summer of 1997 as a trial run to assess for potential oversights in the planning of the study. It is clearly stated on page 136 of my thesis (Sartori, 2008) that one of the things the pilot study elicited was the curiosity of my colleagues. In my absence some of them had reportedly climbed up on ladders to view the hidden images. All of these initial images were then removed, but the ridges that concealed the images were kept in place. The problem of staff curiosity was rectified by speaking to each staff member individually and showing each member the images that had been removed. This situation illustrates yet another difficulty and the sheer hard work involved in conducting such a study!

The formal data collection commenced in January, 1998, when new images were put in place during a night shift while fewer people around. By this time, all staff had already seen the images used in the pilot study, and their initial curiosity had been satisfied, so they did not show the least bit of interest in the images. In fact, I witnessed no attention paid to the images at all after the initial few weeks of the pilot study.

Rush has implied that patients could have been aware of my research due to leakage of information. If this was so and the factors he mentions had such a great influence on the patients, then I would have expected some patients to have reported viewing the hidden image—but none ever did.
Rush has suggested that it cannot be ruled out that other staff and patients may have overheard the interviews, thereby leading to further influence on patients’ experiences and reports. First, I want to respond with an explanation of why we conducted patient interviews at their bedsides. During the first year of data collection, two patients who had reported NDEs died before in-depth interviews could be conducted. It was therefore decided to conduct the in-depth interviews as soon as possible at the bedside in ITU with the screens drawn around. This modification developed according to how the research progressed due to unforeseen factors that emerged and that could not have been predicted in the planning stages.

The circumstances of interviewing the patients in ITU with the screens drawn around is another good point to address, as my response may help to clarify things for people not familiar with the clinical setting. In all of the interviews that I conducted, not once was I aware of any members of staff or other patients overhearing any of the interviews. First of all, the distance between the patients’ beds is too great to overhear conversation from the next bed. Each bedside has to be large enough to accommodate a multitude of electrical equipment such as the ventilator, the pump stands to which several intravenous drug pumps are secured, the renal dialysis machine, and traction in the case of patients with multiple fractures. In addition, each bed space is divided by a large double-sided work station that houses necessary equipment such as syringes, needles, intravenous infusion lines, catheterization packs, and bed linens.

Secondly, if a member of staff had been within close proximity, they would have been too busy attending to their own workload and would not have time or impetus to listen to our interview. ITU is a busy, intense environment, and each nurse has one critically ill patient to attend to. The nurse’s attention is constantly focused on the patient being cared for; an attending nurse who must, for some reason, leave the bedside may do so only when another nurse steps in for the attending nurse. There is little time to pay attention to other patients due to the demands of caring for one’s own critically ill patient. It is not possible for an attending nurse to redirect one’s attention to a patient in another bed area in order to listen to an interview, as there is simply no time for any staff members to be sitting around listening to conversations about things that do not concern them.
Furthermore, ITU is a constantly busy and noisy environment; many studies have highlighted elevated noise levels in ITU due to ‘white noise’ from the electrical equipment, staff conversation, alarms sounding, telephones ringing, and certain procedures carried out routinely such as endotracheal suctioning (Christensen, 2002; McInroy & Edwards, 2002; Sartori, 2008). Such noise levels are not conducive to a staff member or another patient overhearing an interview taking place at another bedside. Thus, although it cannot be entirely ruled out that one or more colleagues overheard a research interview, I consider the likelihood next to impossible.

Exceptional Claims Demand Exceptional Evidence

Rush stated that “exceptional claims demand exceptional evidence” (p. [10]). He appears to have misunderstood the purpose of the article: It was never intended to make ‘exceptional claims;’ it is merely a case report of a very interesting experience captured during a prospective study—an experience that both I and the reviewers and editors who reviewed the article considered to be not easily explained or explained away.

Interview Technique

With regards to Rush’s comment that on page 76 (Sartori et al., 2006) I said, “Did you hear me say I was going to clean your mouth?” rather than asking more open-endedly, “Did you hear me say anything?”, I would like to point out that on page 74 of the original article (Sartori et al., 2006) I documented that I had already asked the more open-ended question, “Did you recall hearing anything while in this state?” This point is also documented in my thesis (Sartori, 2008) on page 392 and 395, which clearly shows the order in which I asked the questions.

In interviewing, it is appropriate to first ask a more open-ended question and then, when the interviewee has freely given specific information, to follow up with more closed-ended questions to clarify details. I asked the follow-up question, “Did you hear me say I was going to clean your mouth?” after the patient had already replied to my initial, more open-ended question by describing me cleaning his mouth; thus, his memory of that specific process had been established. On page 395 of my thesis (Sartori, 2008), I specifically stated the rationale of the question in brackets next to the question: “May have
caused him to make a mind model of it.” I was referring specifically to that incident and probing further; I wanted to know if he had picked up auditory cues that could have contributed to a cryptomnesic mind model rather than his report having arisen from veridical perceptions. Of course the patient proceeded to say something about his mouth; it was my intention to specifically explore that aspect.

With regards to my reply about the consultant, when I responded, I was actually confirming to the patient actual events because he did not understand them. I provided this confirmation after the patient had given me the necessary information and had already identified the consultant as being the person who examined him. In fairness to Rush, when interviews are written down they are slightly taken out of context; in this case, the transcript did not highlight the puzzled look on the patient’s face or the time gap between portions of my dialogue with him.

The NDE Scale Being Used to Prime Responses

Rush has suggested that administration of the NDE Scale prior to conducting an in-depth interview may have primed the patient’s responses. Having the benefit of undertaking this research in the clinical area, I disagree with Rush’s suggestion that the NDE Scale (Greyson, 1983) be implemented after the open-ended questionnaire. The NDE Scale is a measuring tool. It is a 16-item multiple-choice questionnaire, and its purpose is not verification of events, as Rush has implied, but to establish whether an NDE has occurred and distinguish between an NDE and hallucination. At the time of completing the Scale, the patient had already given a free narrative of his experience. The NDE Scale was not designed to prime responses in anyway. Administering it following the free narrative ensured that a lengthy interview was not performed only to find that when the NDE Scale was administered the patient’s experience didn’t actually fulfill the criteria for being an NDE.

Rush commented that it is unclear if the patient was aware of my research. In the original article (Sartori et al., 2006), under the subheading Did the Patient Construct the NDE to Help the Nurse Who Was Looking After Him?, I specifically addressed this aspect: “The research had not previously been discussed with the patient, and he had no knowledge that such research was being conducted” (p. 82).
The Patient’s Eagerness to Report His Experience Was Because He Knew About the Research

With regards to Rush’s suggestion that the patient’s eagerness to report the experience could have been because he knew about my research, I’d like to reiterate a point I’ve already addressed regarding material on page 82 of the original article (Sartori et al., 2006). This was a suggestion I had considered and made a conscious choice to include when writing the article so that readers could be as informed as possible, yet he has tried to turn this point into a criticism. Interestingly, I was not present when he initially reported his NDE to the ward rounds personnel; if he was trying to please me, then surely he would have reported this experience in my presence. One of my research findings was that the NDE was under reported. It is unusual for a patient to report an NDE due to fear of ridicule or disbelief. In fact, only two of the 15 NDErs voluntarily reported their experience. Both of these experiences had such impact on the patients that they were motivated to share their experiences with others. To report such an experience, as Patient 10 did, in an intimidating situation of being examined by the ward rounds personnel, consisting of several doctors, nurses, and physiotherapists, is highly unusual and not characteristic of someone trying to gain attention because he had prior knowledge of research being undertaken.

I thought Rush made a valid point regarding how accuracy of a verbal report can be measured. I was present during the time the patient was unconscious, so it was apparent to me that what the patient reported correlated with actual events and, thus, was accurate. The loss of consciousness and treatment given at the time were also documented by the consultant. The physiotherapist also agreed that the patient had accurately reported the events.

Rush suggested that the ND OBE patient’s perceptions of hospital events during his unconsciousness should have been judged by a panel not associated with me. To control for this factor is virtually impossible, as it would have entailed a panel of people; not associated with the research to assess the validity of a report of something to which they were not witness. It must also be taken into consideration that there was no funding for this study, so even if such stipulations made sense, there was no means to finance them. It is one thing to judge something retrospectively and quite another to be present during the emergency situation. To be able to judge the actual emergency situation itself would entail a panel being on-call 24 hours a day, seven
days a week for the duration of the five years of data collection in the hope of an ND OBE occurring during which time they could all pay attention and make notes while the patient was being resuscitated. Again, this issue highlights the infrequency and unpredictability of NDEs that underlies one of the many difficulties in researching them in the clinical area—as well as the perseverance required to sustain such research.

Accurate reporting of events is an essential part of an ITU nurse’s role due to the life-and-death situations that patients may be in. Handover to the nurse taking over on the next shift is an extremely important aspect of such a role. It is essential that details of the previous shift are accurately described and verbally reported in order for the appropriate care and treatment to be continued. Constant attention is given to detail through thorough documentation in the nursing notes and observation charts of the patients. In my opinion, ITU nurses’ training and daily practice in careful observation should increase rather than decrease speculation that their perceptions and reports are valid.

**What is a ‘Hit’ Testing?**

Rush stated that “any ‘hits’ by patients in describing the hidden cards could potentially be taken as evidence, not for a veridical OBE, but for precognition or telepathy” (p. [9]). By ‘hit’ he is referring to correct identification of the hidden target.

Whether a ‘hit’ would be indicative of telepathy, veridicality, or precognition would have to be investigated further. Incidentally, the only person who knew the possible identity of each target was I, but not even I was aware of which target was situated at which location. The purpose of testing the veridical aspect of the ND OBE was to investigate if it is possible that what people have anecdotally reported of leaving their body and viewing the situation from above is replicable or if it is a mind model.

**No Formal Assessment of the Hand Prior to the NDE**

Rush made a valid point that Patient 10’s hand was not assessed while he was in ITU, but, again, this is a point I had already stipulated in the original article (Sartori et al., 2006, p. 82) but one that he has
reinforced and turned into a criticism. It is correct that the hand had not been formerly assessed immediately prior to the NDE. However, the hand had been assessed throughout the patient’s life, as the medical diagnosis was documented in the medical notes: a right spastic hemic paresis of his right hand. This is a congenital abnormality that the patient had had for the 60 years of his life leading up to the time of his NDE. He also had a splint in his belongings that had been made several years earlier but the patient felt was ineffective. Such a splint would not have been constructed if such a contracture were not present.

The healed hand remains inexplicable, as there is no known mechanism for how a hand that has permanently shortened tendons due to a spastic hemic paresis from birth has since been able to open fully. This development is something that should not be physiologically possible and something the patient stated he has not been able to do before. This entire matter was also supported by a signed statement from his sister.

In no place in the article did I claim this resolution of the contracture as evidence for anything. I have merely highlighted, “The fact that he was able to open his previously contracted hand defies explanation” (Sartori et al., 2006, p. 83). I am still truly fascinated by this development; if a mechanism was found as to how it occurred, the knowledge could potentially lead to new ways of treating such ailments in other people.

The Study Was a Failure

I am mystified by Rush’s conclusion that, “Regarding the results of the study, the main observation is that, strictly speaking, it failed” (p. [10]). I can’t understand why he would draw this conclusion. One of the research questions was, “Is the OBE veridical?” I didn’t specify that the research would be considered successful only if a patient correctly identified the hidden images. As I’ve mentioned previously:

If a multicentre study is undertaken there is the capacity to understand the NDE and also a greater potential for verifying the OBE. If hundreds of patients report an OBE there is a greater potential for the symbols to be viewed. Equally, if hundreds of patients report an OBE but none correctly identify the symbols then it could lead to the conclusion that the OBE is a mind model. Either way, the research would be of great importance. (Sartori, 2004, p. 39)
It seems to me that Rush’s conclusion that the research was a failure rests on his misunderstanding its original purpose. In a future publication, I will be discussing the complexities of veridicality research specifically relating to the results of this study, which may add further clarity to this matter.

A very clear and precise reflection of an event that occurred while the patient was deeply unconscious was reported. This report was in stark contrast to the thousands of other patients I have nursed while they regained consciousness—a process during which they usually are confused or ‘spaced out’, are vague for some time before becoming fully reoriented, and have poor recall of events surrounding their unconscious episode. This patient was forthright about an experience that was so significant to him that it motivated him to report it to the intimidating ward rounds personnel immediately upon regaining consciousness.

Consolidating Three Interviews

I understand Rush’s criticism of my article containing a consolidation of three different interviews. (There were also the initial notes that I made when the patient first described his experience to the ward rounds personnel that, in retrospect, I should also have included in the original article.) Three lengthy interviews were conducted, and it was not logistically possible for these to be included in full in this article. This point was specifically noted on page 73 of the original article (Sartori et al., 2006), and the interviews are available in full in my published thesis (Sartori, 2008) so that they can be scrutinized and fully assessed. In retrospect, I would have done better to identify which excerpt was from which interview, and I will certainly bear this in mind for future presentations and publications.

What Was Used to Clean the Patient’s Mouth

With regards to the patient identifying what was used to clean his mouth, Rush implied that I, rather than Patient 10, first specified this datum. However, his implication is incorrect: The patient came to this point independently. I used a long suction catheter to suction away the mucous secretions from his mouth—which were blood stained and, therefore, pink in color. I then used a pink sponge on a stick that was dipped in water to clean his mouth. Both were used in succession: first the catheter, then the sponge. I still do not know which one it was that
he viewed—it could have been both; I have only reported the patient’s testimony. The fact is that I cleaned his mouth with something that was both long and pink. With regards to the lollipop, the fact that the patient had been in ITU and was familiar with procedure was, again, specifically highlighted in the original article (Sartori et al., 2006); yet again in his critique, Rush has reinforced the very point I had made and turned into a criticism.

Again, as I have pointed out in the article, the patient did not accurately report the audible cues; that is, the patient reported the words of the doctor’s question incorrectly but reported a good comprehension of the meaning of what the doctor had asked. However, what is interesting and accurate is that the patient correctly identified the consultant as examining him and not the other doctor whom he had seen prior to losing consciousness. The consultant entered ITU after the patient had lost consciousness, examined him while he was unconscious, and then—while the patient was still unconscious—returned to his office until he began the ward rounds.

It is apparent to me that some of the points that Rush is using as criticisms were all addressed in the original article so that readers were aware of these issues. I tried to report this incident as thoroughly as possible so that readers could be as informed as possible when drawing their own conclusions. I have not tried to convince readers of anything; rather, I reported as accurately as I could what I had witnessed. Readers can make up their own minds; all I did was report on a situation in which I was in a unique position of being both witness and researcher at a time when the patient was unconscious and reportedly experiencing an ND OBE. In fact, having studied these experiences in depth for the past 19 years and having undertaken prospective research in the clinical area, I am still unable to formulate firm conclusions about how to explain NDEs.

The Timing of Unconsciousness

I thought Rush made a fair point regarding the period of unconsciousness. On page 72, we (Sartori et al., 2006) stated that the patient regained full consciousness approximately 3 hours after the event the patient subsequently reported. However, the actual time period was four hours, as we stated in the same article on page 78. Our reference to three hours was an approximation, whereas our reference to four hours was to stipulate the actual time it took for the patient to regain full consciousness.
However, the crucial time period was the initial 30 minutes when the patient was deeply unconscious. After this time he began to slowly show signs of regaining consciousness, and he became fully responsive four hours later. As it states in the article, after the 30-minute period of deep unconsciousness, the patient began to flicker his eyelids and move all four limbs—signs of neurological improvement and regaining consciousness. An oversight on my behalf is that I thought readers would be able to infer that the patient at this point was slowly regaining consciousness, so I didn’t specifically stipulate this information. His blood pressure and heart rate had also stabilized, and the screens were drawn back.

The patient described viewing the physiotherapist poking her head around the curtains. The physiotherapist was not there for the whole duration as she had other patients to attend to but she kept intermittently returning to check on the patient’s condition during the first thirty minutes by poking her head around the curtains. The curtains were closed for the duration that the patient was deeply unconscious—this was when the physiotherapist was concerned and checking on his condition.

The curtains were open when he began to flicker his eyelids; therefore, the viewing of the physiotherapist could have occurred only while the patient was deeply unconscious. Rush has suggested that the observation of the physiotherapist’s behavior was due to drifting in and out of consciousness. At the time the patient was drifting in and out of consciousness, there were no screens around, so the patient could not have viewed the physiotherapist poking her head around the curtains.

Rush commented that unconscious patients can have ‘implicit perception’ and ‘blindsight’ during anaesthesia and REM associated with the dreaming state. Psychological and neurological processes were considered in the original article, although reference was not made to the specific processes of implicit perception and blindsight; these processes were generalised as mental reconstructions / mind models. The subheadings Was His OBE a Mental Reconstruction?, Did the NDE Happen as the Patient was Regaining Consciousness?, and Did the Patient Construct the NDE to Help the Nurse Who Was Looking After Him? in particular address these points made by Rush.

The purpose of the research was to investigate NDEs in general, so I’m not sure why Rush has assumed that “the research project was attempting to investigate whether or not consciousness can exist independently of the physical body” (p.13). The possibility of consciousness existing apart from the body is only one aspect of the study. As heal-
ings have been anecdotally reported in other NDE cases, and this was
an individual case report, the apparent spontaneous healing of the
patient’s hand was also of great relevance to this study.

Many patients who are acutely ill develop renal failure and require
renal therapy, as Patient 10 did. It is very common for patients in ITU
to recover their renal function after having the appropriate treatment.
Staff working in this area would be well aware of this phenomenon, so
there is nothing remarkable about it. To the patient, this matter was
obviously significant, and he retrospectively reported in a follow-up in-
terview that his kidney function had returned after the NDE whereas
in fact it had begun to improve before the NDE.

However, it is highly unusual for a congenital spastic hemi paresis
to spontaneously resolve. This aspect would have been overlooked had
the patient not misunderstood my question during the interview. The
question was:

PENNY: When you were in this state and not in your body, were
there things you could do that you can’t in your physical
body?
PATIENT: Well, yeah, that’s what I mean; when I came back down
I could open my hand. [He misinterpreted the question.] This hand has always been strong [left] but this hand
[right] used to be like this [fist clenched and contracted
under]. All my life, for 60 years, my arm has always been
like this; I could never open it. (Sartori, 2008, p. 394,
Sartori et al., 2006, p. 77)

At one point, Rush stated, “it is easy to overlook another impor-
tant aspect in studies such as these” (p. [14]) and then went on to
quote excerpts from my thesis where I emphasized this very aspect.
Considering that I mentioned it, I did not overlook it, so I’m unsure
of his point. This aspect was of no particular relevance to the article.
In my overall study, I attempted to conduct as thorough an explora-
tion of NDEs from as many perspectives as possible in order to have
a greater understanding of the phenomenon. Again, Rush appears to
be under the misconception that it was specifically investigating only
veridical aspects of ND OBEs and the possibility of consciousness ex-
sting apart from the body and that I was expecting many ‘hits’ re-
garding the hidden images.
Veridical Aspects Were Inaccurate

I disagree with Rush’s comments that the veridical aspects were not accurate. As I pointed out in the article, the patient was familiar with ITU procedures, so he might have inferred that I used a pink lollipop to clean his mouth. The fact is that I did clean his mouth with something pink while he was unconscious, and he reported viewing this procedure from above. However, I find it hard to reduce the following to circumstantial evidence:

1) Correctly identifying the consultant as examining him and not the other doctor he had seen earlier that morning.
2) Correctly describing the physiotherapist poking her head around the screens. These actions occurred only while the patient was deeply unconscious. By the time he began flickering his eyes, the screens had been opened.

The events that he reported were actual events that I was witness to as well as participated in, and these occurred at the time the patient was deeply unconscious (during the first 30 minutes).

Also another important aspect of my research is of relevance here. Documented in my thesis (Sartori, 2008, pp. 212–215), I described further investigation of the ND OBE component. To ascertain if the ND OBE could have been inferred from residual sight, hearing, or tactile stimulation, I asked the patients who had been successfully resuscitated but did not report an ND OBE to guess what had been done to revive them. Most of the patients couldn’t guess. Of those who did, none of the guesses of this control group were accurate: In every case, there were misconceptions of equipment used and procedures carried out. If ND OBEs are due to mental reconstructions, then I would have expected a few of the control group to have reported fairly accurate resuscitation procedures, but this was not the case.

There is no question that NDEs are a highly complex phenomenon, and I absolutely agree with Rush’s point that it is unlikely that there will be a single explanatory mechanism. I have never claimed to have come up with an explanation for NDEs.

Having 17 years experience of nursing unconscious, critically ill patients, I still consider Patient 10’s report to be exceptional. Of the thousands of patients I have nursed during recovery from unconsciousness, never before or since has a patient reported events that I was witness to and that occurred while the patient was deeply unconscious. Most patients have no recall of a period of unconsciousness, and if they do
it is usually vague and confusional. In contrast Patient 10 reported
events that I had actually participated in while attending to him as he
was deeply unconscious. It is highly unusual for a patient to have such
clarity of thought and recall following a period of unconsciousness.
To infer that such clarity could be produced by a person who is deeply
unconscious and whose brain is severely physiologically insulted does
not correspond to the current prevailing view of consciousness being
a mere by-product of the brain. However, I acknowledge that some
people who do not have the experience of working in such an environ-
ment may find this a difficult concept to grasp.

It is interesting to note Bruce Greyson’s (2007) comment:

Without exception, every report of a large study of NDEs published in
a mainstream medical journal has concluded that these phenomena
cannot be explained as hallucinations. Such unanimity among scient-
fic researchers is unusual and should tell us something. Why is it
that scientists who have done the most near-death research believe
the mind is not exclusively housed in the brain, whereas those who
regard NDEs as hallucinations by and large have not conducted any
studies of the phenomena at all? (p. 140)

As was concluded in the original article (Sartori et al., 2006), “This
interesting case history was elicited from one small prospective study,
conducted in one hospital. Further prospective research on a much
larger scale is warranted in order to provide a wider understanding
of the NDE and, indeed, consciousness” (p. 83). I did not claim that
my study alone would warrant a new understanding of consciousness;
all it can do is point future researchers in the direction of further
understanding.

Closing Comments

In my view, the most valid criticisms Rush made concerned how I pre-
sented my study in the article rather than what my actual methodol-
ogy was. The majority of the methodological concerns he raised had
already been addressed in my thesis. As I see it, Rush took the points
that I had made so that readers could be as informed as possible when
drawing their own conclusions, he affirmed their validity, and then he
turned them into criticisms.

My research was one of the first of its kind, and as with many new
research studies, it is not flawless, and I have never claimed it to be
so. It has provided valuable information and is a platform for future
research. I have never claimed exceptional evidence for anything; that
is an interpretation that Rush seems to have taken on board himself. All I have done is reported on how I conducted the study and what my findings were. I have not been trying to prove or disprove anything. All I have attempted to do is get a greater understanding of a highly complex phenomenon by researching it prospectively.

Undertaking this kind of research in the clinical area is laden with obstacles and confounding variables that are easy to spot in retrospect but have to be experienced before they can be learned from. It is hoped that future researchers will build on this study and also take into consideration the few valid points that Rush has raised. Perhaps the greatest strength of Rush’s article is that it has highlighted just how difficult it is to undertake and sustain such a study in the clinical area.

References


