



RESEARCH NOTE: SCEPTICAL OF SURVIVAL—AN APPRAISAL OF THE COUNTER-ADVOCATE POSITION

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INTRODUCTION

A broad range of scholars and scientists have presented the case for post-mortem survival, drawing on evidence from an array of sources and evaluating it in the light of counter-explanations that seek to explain the phenomena in terms that are consistent with a materialistic worldview. From this standpoint, consciousness is solely a product of brain activity, and ceases when the brain perishes. When addressing these counter-explanations, proponents usually take a narrow view that focuses on only those arguments that bear on the specific phenomenon under consideration. In this essay review, I intend to consider the sceptical response more broadly; a perspective that allows me to identify any general patterns or strategies that have been adopted to counter the survival hypothesis across all its various forms, and to test the extent to which those objections represent reasonable interpretations of data rather than more shallow rhetorical tactics. Criticism and disagreement are healthy characteristics of scientific practice—they provide important checks and balances against confirmatory bias. Competing theories or interpretations help clarify claims, reveal assumptions, and test evidence. My purpose is therefore to determine whether there are sceptical arguments that merit serious attention and might help in future to direct the search for and evaluation of evidence that bears on the question of survival. It is not my intention to evaluate the case for survival per se, or comment on proposed mechanisms by which post-mortem survival might be effected (e.g., see Moreira-Almeida et al., 2022; Rivas et al., 2023).

When prominent sceptic Ray Hyman was tasked with providing a critical assessment of the field of parapsychology, he recognised that it would be practically impossible to review the entire literature. He decided instead to focus on one exemplar that he could interrogate thoroughly, on the grounds that “it would be fairer to try to assess the case for parapsychology at its best. I suspected that the typical contribution to any research enterprise is mediocre and that the viability of a research program is best judged by its strongest representatives” (Hyman, 1985, p. 4). With this in mind, I will take as my focus the collection of essays published in *The Myth of an Afterlife* (Martin &

Augustine, 2015). Comprising thirty chapters and covering six hundred and seventy-five pages, this substantial collection is described in the publisher's cover comments as "a strong, comprehensive, and up-to-date casebook of the chief arguments against an afterlife", so we might be confident that it offers a fair reflection of contemporary sceptics' responses to the types of evidence that have been interpreted as suggesting post-mortem survival.

In identifying common arguments raised there, I will take my lead from a similar analysis I undertook when appraising sceptics' objections to the case for experimental parapsychological phenomena (Roe, 2017). On that occasion, I compared sceptical contributions to Krippner and Friedman's (2010) book, *Debating Psychic Experience*, with critical commentaries that had been published twenty-five years earlier (for a summary and critique, see Honorton, 1993). This allowed me to explore how the sceptical position had responded to the significant advances in methods and enormous accumulation of data that had occurred in that time. Disappointingly, I found that there had been no appreciable progress in the nature or quality of sceptical arguments; indeed, sceptics' appraisals of the evidence showed very little familiarity with the actual activities and findings of contemporary parapsychology and relied instead on unsubstantiated assertions that were often too vague to be testable. On the few occasions when sceptics referred to particulars, I discovered that they tended to cherry-pick examples that supported their cause rather than those that were representative of the research area (a form of straw man argument) or would misrepresent the research in ways that turned it into a parody of itself. It will be interesting to see if the standard of scepticism is any higher when it comes to rejoinders to claims for survival that appear in *The Myth of an Afterlife*.

ATTEMPTS TO EXPLAIN AWAY BELIEF IN SURVIVAL

A number of chapters contend that survival is impossible on various a priori grounds that allow them to explain away claims for its occurrence without having to consider any empirical evidence at all. A whole section is devoted to philosophical or theoretical objections of this sort. Some chapters are effectively a challenge to dualism, and rehearse objections based on the difficulties in explaining how an immaterial thinking substance could interact with a material unthinking substance (e.g., Horder, Chapter 9; Kim, Chapter 13)¹ and whether such interaction would violate energy conservation (Wilson, Chapter 14). These concerns do not relate specifically to the issue of post-mortem survival, so thankfully I do not have to devote space to them here. Other contributors are troubled by the inconceivable nature of any supposed afterlife.² For example,

1. When referring to chapters from *The Myth of an Afterlife*, I have given the author and chapter number rather than the full citation, so that the reference list does not become unwieldy. All other sources are cited in the conventional fashion.

2. Lester (Chapter 30) criticises near-death experiences for telling us little about the nature of post-mortem existence, but that seems an unwarranted expectation, rather like expecting that someone who paddles at the beach should be able to describe what life is like under the sea.

some (e.g., Drange, Chapter 12; Lester, Chapter 30) see personal identity as inextricably tied to corporeal characteristics, so that discarnate personalities would have no basis on which to recognise each other. This seems untenable to me on a number of grounds. Firstly, we have no trouble making attributions to another person without necessarily having access to particular physical properties of that person—we can confidently communicate with others via body language, a telephone call, an email or a postcard, despite those media having no common substance. Indeed, we have no trouble making attributions to people who are physically absent even when they have not intended to communicate with us (e.g., when they have forgotten, again, to clear away their breakfast things, or have series-linked yet another television programme). These recognitions rely on our shared history and a capacity to see features of the ‘interaction’ as characteristic of them. We are also quite able to recognise the identity of a person even when they have physically changed (e.g., because they have grown up, have gained or lost weight, have hair that has gone grey). It seems clear, then, that people are not slavishly tied to specific unchanging physical attributes of a person as the only (or even primary) means by which they can be recognised. In a similar vein, contemporary mediumistic communications do not typically rely on materialisation or personation through the medium to establish identity but rather do so by capturing some quality of the discarnate’s personality and interests and by conveying information that is known only to the deceased and the sitter (e.g., Braude, 2013).

The identity objection seems to reflect a more general presumption that conditions that pertain in spacetime dimensions that are the context for our current incorporated experience would necessarily apply to any post-mortem existence, so that it is impossible to comprehend how we might, for example, perceive without sense organs, or remember things without a hippocampus. Thus, Blackmore (Chapter 17) assumes that any astral body would need to consist of all the structures found in the physical body because it must perform the same functions (perception, movement) in the same way as that physical body. This creates the paradox that if the astral body is less substantial than the physical body, then it would be incapable of these functions; but if it were sufficiently substantial to be capable of these functions, then it would be incapable of feats that seem characteristic of astral bodies, such as passing through physical barriers or moving rapidly over large distances. But this line of thinking seems to me rather like the fallacy of affirming the consequent: just because perception *can* be achieved via a particular physical system does not imply that perception can *only* be achieved by that method. This logical error influences the kind of evidence that is presented, with the focus being wholly on confirming the already established link between our sensory systems and subjective perceptions as they are experienced in embodied persons. But this does not demonstrate that sensory systems are the *only* pathway to perception, any more than the claim that all four-legged animals are dogs can

be demonstrated by showing that each dog we see does, indeed, have four legs. The art to testing a claim is to look for refutations of it, so that closely inspecting just a single cat can allow us to reject the claim that all four-legged animals are dogs. Is there a corollary when it comes to sensory systems and perception? Well, experiments designed to test for extrasensory perception seem to show that people are capable of accurate perceptions under circumstances that do not involve the peripheral visual system (cf. Baptista et al., 2015; Cardena, 2018), and psychokinesis experiments indicate that physical effects can be achieved without being mediated by the psychomotor system (Roe et al., 2015; Varvoglis & Bancel, 2015). Alas, the philosophical chapters show little appetite for seeking out and engaging with empirical evidence that might challenge their assumptions.

Another way in which belief in survival is explained away a priori is by identifying supposed psychosocial benefits in holding the belief irrespective of its truth. If these can be shown to account for belief, then there may be no need to scrutinise the believed-in phenomena. For example, Stewart-Williams (Foreword) rather grandly refers to ‘wishful thinking theory’ as:

belief in life after death may eliminate—or at least weaken—the fear of personal extinction, the overwhelming sadness we experience when a loved one dies, and the sense that a life of finite duration would be meaningless ... In short, people invent, retain and refuse to relinquish religious beliefs because they provide a balm and salve for the pains of life—our own and those of the people they care about. (p. xv)

Similarly, Gennaro and Fishman (Chapter 5) claim that “historically, belief in dualism has often been theologically motivated by the understandable desire for immortality” (p. 107). Of course, it’s not clear that all flavours of afterlife are so simple or comforting (one only need think of a Hieronymus Bosch painting), and for my part the prospect of simple annihilation seems a much less trepidatious prospect than the unknown of an afterlife.

Augustine (p. 11) identifies religious indoctrination as a chief reason for belief in survival after death, and Stewart-Williams goes on to suggest that paranormal beliefs provide a means of controlling society by offering additional rewards for ‘good’ behaviour and punishments for ‘bad’ behaviour. This sounds rather like the social marginality hypothesis, which has been described in detail by Irwin (e.g., 2009)—while that theory is quite elegant in positioning afterlife beliefs as a form of psychosocial compensation for current injustices or hardships, sadly numerous attempts to support it empirically, as documented by Irwin, have been unsuccessful.

Taking a different tack, Lange and Houran (Chapter 23) avoid having to engage with empirical claims concerning hauntings and poltergeists by arguing that since ambiguous psychological and physical phenomena can be interpreted as paranormal under certain circumstances, then this warrants the development of a theory of hauntings and poltergeists that focuses on

the affective and cognitive dynamics of people's interpretation of ambiguous stimuli. While not without value (for a more detailed review, see Roe, 2024), this approach adopted in isolation seems akin to acknowledging that some people who claim to be ill are prone to hypochondria and then proceeding to adopt a model of human illness that focuses only on factors that affect hypochondria. The UK's National Health Service would be in a very sorry state indeed. Additionally, the approach relies on the presumption that apparitional experiences are usually ambiguous in form, when that is clearly not the case. Take, for example, the many witnesses of the Enfield phenomena (Playfair, 1980), or the detailed reports of after-death communications (e.g. Elsaesser et al., 2021). In conclusion, these a priori objections seem far from compelling.

NEUROSCIENCE

The primary objection in principle to the possibility of survival comes from the *mind–brain dependence thesis*, which holds that “having a functioning brain (or similar physical structure) is a necessary condition (or prerequisite) for having any sort of conscious experience ... and if human consciousness most likely cannot exist in the absence of brain activity, then it must cease to exist when the brain dies” (p. 3). In support of this thesis, Weisman (Chapter 4) refers to the “near perfect correlation between brain functions and mental function” (p. 102), and Augustine & Fishman (Chapter 10) to “tight correlations between mental states and brain states [that] have been observed time and again using both passive observation and active experimental manipulation under a wide range of conditions” (p. 203). McCormick (Chapter 2) goes further, in claiming that “even the most abstract mental faculties—and the most specific features and contents of our private mental states—can be mapped directly onto brain functions” (p. 55).

But are such claims justified? McCormick refers to some specific clinical disorders and claims they have a recognised neurological cause. “What is important about these disorders”, he asserts, “is that we have mapped their specific locations or functional pathologies in the brain, sometimes down to the millimetre”, though sadly no examples of this interpersonal precision are forthcoming. In contrast, Piccinini and Bahar (Chapter 6) concede that while some mental functions, such as early sensory processing and late motor control, appear to be neatly localised in well-defined regions of the brain,

other mental functions may well be distributed over vast networks of neural systems. Some neural systems also appear to participate in many neural functions. As far as we are concerned, the neural structures that implement a mental function need not have sharp boundaries, need not be contained within a contiguous spatial region, and need not stay the same over time. (p. 138)

So, functions of the mind can be mapped precisely onto specific cortical regions (except for those functions that have no identifiable locus), and cortical regions

are associated with particular functions (except when they seem to be implicated in a range of functions). That would certainly cover all bases.

Even that narrower set of brain–mind associations that are presented as tightly boundaried may not be so in practice. The exemplary case of ‘HM’, who was left with anterograde amnesia after suffering damage to the hippocampus, turns out to be more complex than it first appears (e.g., Annese et al., 2014; MacKay, 2023), and the relatively strong evidence for language dysfunction being associated specifically with Broca’s and Wernicke’s areas of the cortex turns out to be much less consistent than generally stated, since “the locations of these regions did not appear to be as neatly organised as the ‘homunculus’ studied by Penfield; they varied from one patient to the next” (Piccinini & Bahar, Chapter 6, pp. 143–147). Disappointingly, the exactitude claimed for the localisation of brain function is often missing when particular empirical findings are presented in this volume to evidence them, and experimental outcomes are typically couched in vaguer terms such as ‘[psychological function x] *corresponds to* activity [in brain region y]’, ‘[activity of brain region y] *is implicated in* [psychological function x]’, or even ‘[brain region y] *plays a role in* [function x]’ (see pp. 147–150). My concern is not in relation to the problems in inferring causality from correlation (though of course those are formidable), but rather with the weak circumstantial nature of the case being made. (I should add that I do not seek to deny the general claim that there are correspondences between brain states and phenomenology, but rather to question the tight and universal binding that is implied in some chapters, despite acknowledging exceptions that violate this in other chapters.) To illustrate, Piccinini and Bahar (Chapter 6) note that taxi drivers in London, UK, have larger volumes of hippocampal grey matter than bus drivers who simply follow set routes. This is interpreted in terms of the greater spatial learning demands made upon the former (who have to be much more creative and flexible in planning their routes), which stimulates growth of the hippocampus because it is said to be responsible for such spatial memory. However, the same review notes that hippocampally damaged residents of Toronto, Canada, did not experience deficits in navigational ability, and Toronto residents with normally functioning hippocampi did not show increased activity in that region when asked to mentally navigate around the city. Thus, it is now claimed, the hippocampus is only required for spatial navigation when the task is complex (Toronto is a more simply laid out city than London), and when need be, we can manage by accessing long-term memories stored elsewhere (p. 144). When the goalposts can be moved so easily, it is difficult to think of any observation that would qualify as a refutation of the mind–brain dependence thesis.

As I’ve already mentioned, it is not enough when testing a theory to simply look for confirmations of it; rather, we must seek out and interrogate apparent disconfirmations. McCormick (Chapter 2) acknowledges, “if brain damage of various sorts left cognitive functions unscathed, or if consciousness persisted

despite alterations in brain chemistry and structure, then we might have some evidence to doubt the correlation and causal connection ... *But we find no such violations*" (p. 57). One potential violation is the condition hydrocephalus. In such cases, during childhood the circulation of fluid from the ventricles of the brain is blocked, or there is increased pressure in the spinal area, forcing excess fluid into the ventricles. As the volume of fluid within the ventricles increases, it can press the overlying brain tissue against the cranium, restricting its growth as well as stretching and destroying connections. In severe cases, ventricle expansion can fill 95% of the cranium. Paediatrician John Lorber found that 60 of the 600 hydrocephalus cases he investigated were of this severe type. Some of these patients presented with substantial intellectual and physical disabilities, as expected; however, about half of them were found to have IQs above 100 (Lewin, 1980). Lorber referred to one exceptional patient,

who has an IQ of 126, has gained a first-class honors degree in mathematics, and is socially completely normal [yet] ... instead of the normal 4.5-centimeter thickness of brain tissue between the ventricles and the cortical surface, there was just a thin layer of mantle measuring a millimeter or so. His cranium is filled mainly with cerebrospinal fluid (as cited in Lewin, 1980, p. 1232).

Lorber's claim has not been taken seriously by the scientific mainstream, but a more recent case has supported his contention. Feuillet et al. (2007) describe the clinical case of a patient who had suffered a massive enlargement of the ventricles, and consequently had a very thin cortical mantle. Psychological testing revealed that although he scored below average on intelligence (his verbal IQ was 84, and his performance IQ 70), he showed no sign of disability beyond a mild left leg weakness that had led to the consultation in the first place. As a married father of two who worked as a civil servant, he was clearly socially functional. Hydrocephalus has a much greater effect on white matter than on grey matter, and this could go some way to explaining the remarkable retention of many normal functions in severely affected individuals. Additionally, these remarkable competences have only been documented in patients with chronic hydrocephalus, during which ventricular expansion may be gradual, and the patient has an opportunity to adapt or compensate (de Oliveira et al., 2012). In contrast, Majorek (2012) cites the case of a female patient, who at the age of three underwent a hemispherectomy (i.e., her dominant hemisphere was surgically removed in order to control intractable epilepsy that had led to right-sided hemiplegia and severe regression of language). By the age of seven, despite no obvious regeneration of brain tissue in that hemisphere, she had recovered to such an extent that she was fully bilingual in Turkish and Dutch, and her hemiplegia was only noticeable as a slight spasticity of her left arm and leg. She otherwise led a normal life. This is not an isolated case; Majorek cites a follow-up study of thirty-three children and adolescents who underwent hemispherectomy at

Great Ormond Street Hospital, London, among whom no significant cognitive deterioration or loss of language occurred despite the excision of a whole hemisphere. Nahm et al. (2017) draw attention to further cases of this type, concluding “findings that pertain to brain dysplasia and brain lesions indicate that in some individuals there is a considerable discrepancy between the cerebral structures and cognitive functioning”, posing a formidable challenge to the mind-brain dependence thesis (for an updated review that draws a similar conclusion, see Kofman & Levin, 2025).

A second potential disconfirmation involves dementia conditions such as Alzheimer’s disease. Augustine and Fishman (Chapter 10) observe:

perhaps no disorder demonstrates this [association between brain damage and loss of cognitive function] as clearly as the progressive brain degeneration of Alzheimer’s disease: the greater the deterioration of synaptic connections, the greater the loss of memory, recognition of others, and characteristic personality traits. (p. 205)

Weismann (Chapter 4), a neurologist at Temple University, Pennsylvania, describes the case history of one of his patients who had a malignant mass deep in his left temporal lobe that caused his condition to worsen over time, first losing the ability to understand language, then the ability to attend to people in his environment, and ultimately to lose consciousness for a time and then die. This and similar cases powerfully demonstrate the linkage between degradations in brain functioning and diminution of subjective experience. As Gennaro and Fishman (Chapter 5) summarise:

brain damage has negative effects on one’s mental states, altering or even eliminating one’s ability to have certain conscious experiences ... it thus stands to reason that when all of one’s brain activity ceases upon death, consciousness is no longer possible, and so neither is an afterlife. (p. 105)

However, there are occasions when someone who is in the later stages of a progressive decline that has left them uncommunicative and unresponsive, spontaneously and with no obvious cause becomes lucid and able to interact with those around them. Mashour et al. (2019) provide a review and speculate on possible mechanisms to account for this phenomenon, which they term *paradoxical lucidity*. More commonly, it is referred to as terminal lucidity, to reflect the tendency for it to occur in the days or even hours before death (Nahm et al., 2012), often with spontaneous recovery of physical and cognitive function (Nahm & Greyson, 2009). Mendoza (2019) cites a representative case:

My grandfather was in palliative care for dementia. He had severe cognitive decline over the months preceding his death. There was no recognition of family or friends, paranoia, hallucinations, confusion, social withdrawal, refusal of food and drink, mumbling incoherent speech, and a lack of ability to toilet or shower himself. He awoke and began talking in a clear voice with obvious recognition of family and surroundings. He was able to inquire about family and friends that he had not been able to recognize previously. He asked that the books he had borrowed months ago be

returned to their owner. He said he wished his death would come quicker. After 20 minutes, he became tired, fell asleep, and died shortly afterward.

There are few quantitative estimates of the prevalence or degree of terminal lucidity, though recent surveys among people who care for the terminally ill suggest the phenomenon may be quite common (Brayne et al., 2008; Fenwick et al., 2010), despite this kind of neurological degeneration being regarded as irreversible.

Despite hydrocephalus and terminal lucidity being commonly cited as remarkable counterexamples to the mind–brain dependence thesis, only two chapters in *The Myth of an Afterlife* refer to them explicitly. Augustine and Fishman (Chapter 10) dismiss them as “rare neurological outliers” (p. 251), claiming “such few and far between cases are better explained as exaggerations based on erroneous data [and] the products of neuroplasticity” (pp. 248–249). Weismann (Chapter 4) similarly tries to equate these periods of lucidity with occasions when a dementia patient “will occasionally make a great deal of sense. From time to time the cobwebs seem to clear, and the patient makes an uncharacteristic and appropriate comment or two” (p. 100), but that does not seem a fair description of the longer-lived and more coherent phenomena described above. Attempts to trivialise these experiences seem doomed to failure—such troublesome data are unlikely to go away simply because they are labelled as anomalous; indeed, they are likely to accumulate.³ Augustine and Fishman (Chapter 10) allude to “neurological improvements” (p. 250), but this too seems inadequate given that several forms of dementia, including Alzheimer’s dementia, are largely associated with *irreversible* degeneration of the cerebral cortex and the hippocampus, so that it is not clear how such recovery could ever take place, and their sudden onset argues against the kind of recovery that might be possible via neural plasticity or regeneration.

ACCOUNTING FOR SURVIVAL PHENOMENA

Just one of the four major sections of *The Myth of an Afterlife* is concerned with considering empirical evidence for survival with the rest, as we have seen, taking care to skirt around such troubling data. This section is particularly important, then, in demonstrating how familiar sceptics are with the observations and research data that suggest consciousness can survive bodily death and in drawing attention to the shortcomings they have identified that allow them to set aside this evidence as suspect. The strongest sceptical arguments will be those that explain the best evidence for survival rather than some straw man version. Sadly, most of the chapters included in *The Myth of*

3. Forsdyke (2015), for example, notes that individuals with exceptional memory (savant syndrome) tend to have normal-size heads with no need for the enlargements of the type that result in taxi drivers. Similarly, microcephalics, who have exceptionally small heads, while typically intellectually impaired, include a number of cases where intelligence is normal.

an Afterlife show little awareness of that extensive literature, and betray an unwillingness to grapple with the most challenging evidence.

Thus, Augustine closes the introduction (Chapter 1) with the glib dismissal, “direct experimental tests of the dualistic survival hypothesis are not only possible, but have been carried out—thus far without success” (p. 32), without indicating what those tests might have been, or in what way they were unsuccessful. Augustine and Fishman (Chapter 10) similarly swiftly dismiss the empirical case for survival, since “the quality of the data for such paranormal phenomena pales in comparison to that available for the neuroscientific and other phenomena we have considered throughout this paper” (p. 248). One is left to wonder how we can assess the quality of that work when no space is devoted to considering it—I term this the ‘Lord Nelson defence’, since it is reminiscent of Horatio Nelson’s (perhaps apocryphal) act at the Battle of Copenhagen of holding his telescope to his blind eye and announcing that he could see no signal commanding him to end the action and retreat. Of course, one sees no evidence that favours the survival hypothesis when one takes no steps to look for it.

Other contributions suffer from an embarrassing lack of currency. A chapter by Blackmore on astral bodies (Chapter 17) was originally published in her 1982 book *Beyond the Body*, and her other chapter on out-of-body experiences (Chapter 24) was excerpted from an article she published in the journal *Anabiosis* in 1983, fully thirty-two years before the publication of *The Myth of an Afterlife!* Neither article is amended or revised in any way. Are we to believe that no research of note has been conducted on these topics in all that time? Tellingly, Blackmore offers a blueprint for future, more persuasive near-death experience cases that is a reasonable fit for the Pam Reynolds case (Sabom, 1998) but misses the opportunity to consider the merits (and weaknesses) of that case because it was not available during the 1980s when her article was originally written.

Example cases chosen by other authors show a similar disregard for contemporary research. Larsen (Chapter 27), for example, analyses the case for reincarnation by focusing on a single investigation by Ian Stevenson that was published over forty years earlier (Stevenson, 1974). Have there been no advances in research methods since then that need to be taken into consideration (for an overview, see Mills & Tucker, 2015)? One might also ask why this case has been selected as representative, when Mills and Tucker estimate that approximately 2,500 cases of children who report past lives have been investigated. If we must take a single case, then why not analyse one that has been more positively appraised, such as that of James Leininger, discussed by Tucker (2013), or that of Purnima Ekanayake, reported by Haraldsson (2000). Similarly, Augustine and Fishman (Chapter 10) focus selectively on attempts to detect out-of-body experiences (OBEs) and the outcomes of cipher tests (i.e., the creation by a living person of a code with the intention of providing the key post-mortem) as the main approaches to falsify the brain dependence theory,

but these forms of research are quite antiquated and are hardly representative of contemporary efforts to test claims for survival. Unsurprisingly, they only make reference to work published in the 1980s and earlier, fully thirty years out of date.

Descriptions of key studies often include serious errors that enable the sceptic to speculate that the reported success of the experiment could be due to participant cheating. For example, Augustine and Fishman focus on an experimental test of OBEs that involved number detection (Tart, 1968). The sole participant, Miss Z, was an acquaintance of Charles Tart's, and she had described to him how she regularly had OBEs as part of her normal sleep experience. He invited her to take part in a formal test that involved her sleeping at the laboratory over a number of nights. While she slept, her brain activity was monitored by EEG apparatus. If she discovered during the night that she was having an OBE, she agreed to try to read a sequence of random numbers that had been written on a sheet of paper and placed on a shelf high up on the wall (the numbers were different each night, and were only generated after she had gone to sleep). On the first three nights, she was unable to view the numbers, but on the final night when she woke up she called out that the number was 25132—the correct sequence, with odds against chance of 1 in 100,000. However, Augustine and Fishman claim:

unfortunately Miss Z was not monitored through the night on any of these occasions, adequate controls were clearly not in place. Tart himself conceded that Miss Z could have concealed a hidden mirror, or otherwise seen the number reflected off the surface of a clock above the shelf holding the target number. (p. 220)

In fact, Tart (2009) clearly explains (in the book Augustine and Fishman cite as their reference) that Miss Z was wired up to EEG apparatus that continuously monitored her, which severely constrained her movements. Tart also describes attempts that he and Arthur Hastings (a skilled amateur magician) undertook to discern the number by normal means, but found that nothing at all could be seen reflected by the casing of the clock, and indeed the room was so dark that they needed to shine a bright flashlight directly onto the numbers in order to see them. This demonstrates the value of providing citations so that the interested reader can check for themselves the accuracy (or otherwise) of a second-hand account.

Another example of this Procrustean zeal in shaping facts to fit a narrative is provided by Larsen (Chapter 28), who evaluates the case for mediumship by analysing a single sitter experiment by Schwartz (2002). Larsen claims that statements Schwartz described as hits “had to be dragged out of the sitter using a string of standard cold reading techniques” (p. 603), but does not elaborate on what these techniques were nor how they led to these correct statements—he could not do so because neither the original journal paper nor Schwartz's subsequent book includes a transcript of the actual reading. Larsen admits as much when he goes on to say:

The whole experiment took almost two hours ... yet Schwartz has said very little about what Campbell [the medium] actually said during it. He tells us about the fantastic hits ... but given the many hooks that mediums usually throw out, we hear next to nothing of Campbell's actual words. (p. 605)

Thus, it is clear that his attribution of cold reading is based on wishful thinking rather than any close analysis of data. He also alleges collusion between participant and medium—even though Schwartz asserts that the medium was blind to the selection and identities of specific sitters—and suggests that the successful outcome was due to some combination of collusion and cold reading. One wonders why, if the participants are colluding (based on Larsen's wild speculation), they would still need to resort to the use of cold reading techniques that, for Larsen, are clearly evident in the non-existent transcripts. Finally, Schwartz's experiment has been described by Sarraf et al. (2020) as "highly controversial", and these authors note "Schwartz has conducted [subsequent] experiments to which many criticisms of his earlier work do not apply" (p. 2), so it seems like another instance of the straw man strategy in which a weak or flawed example is presented as if it were typical research practice, or even represented best evidence.

My final example is Battista et al.'s (Chapter 29) analysis of Julie Beischel's research on mediumship. They speculate about biases (choice bias, confirmatory bias, distinctive bias) that can have no effect on the statistical outcomes reported in the experiment but only serve to muddy the water. They concede that "the fact that each sitter had to make a judgement between the intended and control reading resolves some of this issue, *as bias alone cannot guide their hand to the intended reading*" (p. 618, my emphasis), but this is misleading, since such biases cannot *in any way* affect the statistical analysis. They raise concerns about the mediums being provided with the first names of deceased persons they are asked to contact on behalf of an absent sitter, since generational trends in name popularity could give clues as to the discarnate age or background. They speculate that matching names such as 'Dorothy' and 'Brittany' would allow the medium to infer that 'Brittany', being a younger name, belongs to someone who died prematurely, perhaps by suicide or in a vehicle accident, while 'Dorothy', being older, could be a candidate for cancer or heart disease. First names can also indicate ethnicity, so they could be used to identify hair, skin, and eye colour. These objections seem reasonable until one finds that Beischel (2007) has gone into great detail to describe how names were matched in order to avoid exactly this kind of potential cue, and she is very clear that

in cases in which the names provide overt evidence about the discarnates' ethnicities and in turn their probable physical descriptions ... or provide other identifying information (e.g., religion), either a pair is chosen to include two discarnates of the same ethnicity or religion or the discarnates are chosen only for studies in which blinding is not necessary. (pp. 41–42)

This is another example of a general strategy to offer speculative counter-explanations that sound plausible so long as they are not be tested against actual data.

CONCLUSION

My aim in this note was not to evaluate the case for survival but rather to uncover and evaluate the objections that have been raised by sceptics which might provide a source of constructive criticism for future research and theory development. I began with the hope that my review of *The Myth of an Afterlife* would highlight better standards of scholarship and argument than I found when reviewing sceptical objections to experimental parapsychology in contributions to Krippner and Friedman's (2010) book, *Debating Psychic Experience*. Sadly, the standard seems to be even worse when it comes to survival research. Again, sceptics' analyses showed little familiarity with the actual activities and findings of contemporary researchers, sidestepping thorny phenomena such as hydrocephalus and terminal lucidity cases and preferring vague allusions and broad, untestable pronouncements. When critiquing survival research, their preferred examples were often decades out of date, or focused on weak examples that were not very typical of methods or findings, or they included misrepresentations that suggested design weaknesses that did not actually pertain.

This is disappointing, since the scientific method depends on close scrutiny of research findings and the claims based on them. Open (but informed) criticism can help correct woolly thinking and improve research designs. I began this review by quoting prominent sceptic Ray Hyman, and it seems fitting to close with his advice to sceptics as part of a subsequent joint communiqué with parapsychologist Charles Honorton (Hyman & Honorton, 1986, p. 64, emphasis added):

As in any other area of scientific enquiry, research in parapsychology requires continual scrutiny and criticism. Both critics and parapsychologists want parapsychological research to be conducted according to the best possible standards. Critics can contribute to this need only if their criticisms are informed, relevant and responsible.

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