

## EVIDENCE OR PREJUDICE? A REPLY TO MATLOCK

By Keith Augustine

Before I respond to James G. Matlock's comments on my coedited volume, *The Myth of an After-life: The Case against Life After Death (MoA)*, I would like to thank him for taking the time to review such a large volume—and review it conscientiously—even if we ultimately disagree about its import. I would also like to extend my thanks to *Journal of Parapsychology* editor John Palmer for inviting this response, as it gives me an opportunity to clarify why many secondary issues (and some significant matters) were untouched in the volume. While I find most of Matlock's complaints specious, they nevertheless underscore the need to clear up misconceptions about the collection's purpose, some of which are also found in others' reviews (e.g., Hasker, 2015; McLuhan, 2015).

As you might have gathered from the volume's title, I have a point of view on the survival question. It also happens to be a viewpoint that most readers of this journal probably do not share. For those of you who do not share it, I ask you to bear with me as I offer a different, but no less reasoned, perspective on the issue. You need not agree with my conclusions to see why I think that, regrettably, biological death marks the end of our mental lives, and I think that you might find that we have more in common than you initially expect.

### Poisoning the Well

From the start Matlock explicitly portrays those who share his worldview as data-driven, whereas those who do not share it are said to be driven instead by paradigms. Ignoring the wishful thinkers (whom I will say more about at the end of this section), this depiction neatly divides the world into two camps that, conveniently enough, places Matlock within the scientific camp, while placing his opponents in the unscientific one. This sort of rhetoric has no place in a journal that strives to be substantive and impartial.

On the survival issue I am a mortalist (one who thinks that distinctive conscious personalities cease to exist at biological death), whereas Matlock is a survivalist (one who denies this). Mortalists advocate personal extinction, whereas survivalists advocate personal survival. Mortalism, we are told, is a paradigm—or is said to be a consequence of some larger paradigm—that evidently constrains what sorts of data are acceptable, leading weak-willed mortalists to throw away inconvenient data. Belief in personal survival, by contrast, constitutes a different paradigm—or is perhaps the consequence of one—that also constrains what sorts of data are acceptable, but whose constraints survivalists are gallantly able to resist when making their assessments. Aside from painting opponents with such a broad brush and neatly dividing the world into a zodiac of two kinds of people, on what *grounds* does Matlock base his implication that intellectually hobbled mortalists cannot see past their own paradigms, whereas more gifted survivalists are able to rise above the rest and see the world for what it really is?

This is a rather self-serving and evidently groundless generalization for Matlock to make. He cites no psychological studies of mortalists as a group (“extinctivists” in the psychological literature) to back it up (Bering, 2002; 2011, pp. 117–120; Thalbourne, 1996), to say nothing of the fact that particular individuals need not fall prey to groupthink or other cognitive biases just because they can be divided into different

belief or other categories (as of course we all can be). On what grounds, then, are readers able to assess whether Matlock is just casting his own predilections in favorable light—and to a largely amenable audience—while casting the tendencies of his opponents as pernicious? One does not have to read between the lines to see what is going on here: This is mere prejudice, an instance of a well-worn fallacy known as *ad hominem* poisoning the well. Matlock “poisons the well” by opening his review in a way that predisposes readers to form a negative first impression of the volume and its contributors, rather than simply perceptively addressing the actual content of *MoA*. Let me turn to some additional problems.

Matlock makes the assumption that there is “every indication” that the editors (the late atheist philosopher Michael Martin and I) and each of the over two dozen contributors are “paradigmatic thinkers . . . who have adopted a particular worldview and see everything through its prism” (JM, p. 190), in particular “a materialist worldview and a reductionist account of mind/brain relations that rule out of hand any possibility that consciousness, personality, memory, or anything else about personhood might survive bodily death” (JM, p. 191). Ignoring the mind-reading, while it is true that we provide arguments that conscious individuals do not survive bodily death (an aim that we hardly obscured), the rest of what he says here misses the mark on several levels. Matlock does not get off to a good start.

First of all, the contributors are not—or are not necessarily—either materialists or reductionists. I will say more about this in the section on the metaphysics of mind. For now, just consider what contributor Theodore M. Drange explicitly writes elsewhere about atheists, which is equally applicable to mortalists:

[A]n atheist can believe in the existence of universals (Plato’s “forms”). He can also believe in such abstract entities as numbers or propositions. I know atheists who believe in nonphysical mental states and/or objective moral values. They would readily grant that such things are not reducible to matter or energy. So, they are not materialists, but as long as they deny the existence of God, they are atheists.

I myself am an atheist but not a materialist. I would say there exist things that are not reducible to matter and energy. Consider, for example, propositions, which are abstract entities of a certain sort. They are neither [physically spoken or written] sentences nor thoughts inside anyone’s brain.<sup>4</sup> (Drange, 1999)

In fact, two of the strongest mortalistic arguments come from the prominent philosophers Hume (1755/1987, p. 596) and Russell (1947/1986, p. 90), neither of whom were reductionists or materialists. For now, suffice it to say that this is not merely an issue of semantics (Fales, 2007, pp. 127–128).

Next Matlock simply fires off a list of those philosophers, psychologists, neuroscientists, and others who happen to share his predilections on this issue. But of course anyone advocating any point of view can come up with such a list; the fact that creationists and climate change deniers can also fire off lists of credentialed thinkers who agree with them ought to give psychological researchers pause in resorting to such tactics. (This point stands even if parapsychology is not a pseudoscience comparable to creation science or climate change denialism, a view that I do not intend to suggest.) What matters here is not that you can find some thinker or other who endorses your own point of view, but whether or not your point of view reflects the *consensus* of experts in the relevant fields. I think that it is pretty clear that as far as survival is concerned, David Ray Griffin and Robert Almeder’s views do not reflect those of the majority of philosophers, Imants Barušs and Julia Mossbridge’s views do not reflect those of the majority of psychologists, Jeffrey Schwartz and Mario Beauregard’s views do not reflect those of the majority of neuroscientists, and so on. So what is Matlock’s point?

Matlock continues the theme by firing off a list of survivalist and antimaterialist *books*, such as *The Waning of Materialism* (Koons & Bealer, 2010), *After Physicalism* (Göcke, 2012), and *The Soul Hypothesis* (Baker & Goetz, 2011). Chief among these is, of course, *Irreducible Mind* (E. F. Kelly et al., 2007), which I’ll say more about shortly. Suffice it to say that if one limits one’s genre to books that argue in favor of survival or against materialism, it is hardly surprising that one can paint a picture of mortalists or materialists

<sup>4</sup> Nor even thoughts within one’s nonphysical soul or astral body, I might add, since abstract objects are (on Platonic realism) neither physical *nor* mental.

on the run, whatever the larger scholarly reality.

Matlock goes on to disparage the volume's professed purpose to "explore the *grounds* for thinking that we irrevocably lose consciousness, once and for all, at the end of life" by asking "questions that have often been overlooked, but which are essential to ask" if one wishes to assess the truth of that proposition (p. xxvii). His description of its stated purpose as a "pretense" (JM, p. 192) misconceives the point of the collection while further poisoning the well.

First, the volume's contributions were not assembled in order to persuade convinced survivalists to give up their belief, but to give undecided or skeptical readers food for thought about highly relevant issues that are typically handled superficially or overlooked altogether in the extant survival literature. More specifically, they were intended to present the other side of the case to professors and students who would like to consider *both* sides of the afterlife issue in their coursework. That is why endnote 24 of the Introduction recommends a number of survivalist books that would make "an excellent opposing companion to the present volume" (p. 38n24). (For all of its virtues, I don't recall any parallel recommendation of any "materialist" books in *Irreducible Mind*.) That is why there is a one-paragraph summary of each chapter preceding each of the four parts—an easy guide to which chapters would provide useful reading material depending on what kind of course one was teaching or what topics one wanted to cover. That's why key terms are bolded in the Introduction. The chapters were meant to be read as stand-alone pieces that dive into the details of whichever particular issues are of interest, rather than as parts of a book that would be read cover-to-cover. So, for example, one might pair Drange's "Conceptual Problems Confronting a Totally Disembodied Afterlife" in the volume with H. H. Price's (1953) classic "Survival and the Idea of 'Another World'" to highlight and assess points of disagreement, or pair Christian Battista, Nicolas Gauvrit, and Etienne LeBel's "Madness in the Method: Fatal Flaws in Recent Mediumship Experiments" with Beischel and Gary Schwartz's (2007) triple-blind study.

Second, if you consult *Philosophy of Mind: Contemporary Readings* (O'Connor & Robb, 2003) as a typical example of similar anthologies, you'll find discussions of a variety of topics, ranging from arguments for the soul's mereological simplicity (its absence of parts) to attempts to resolve the grain problem (the apparent mismatch between the relative structural simplicity of our perceptual experience and the enormous physical complexity of our brains), with reprints of material first published as far back as 1959 or as recently as 2001. For such collections the reader should have no expectation that any particular author be well versed in the diverse subject matter of other chapters that fall outside of his or her area of specialization, so why Matlock thinks it is reasonable to expect neuroscientific or philosophical contributors to know the psychological research literature back and forth, or vice versa, is beyond me. Each contributor to an anthology normally limits his discussion to the particular purview where his expertise lies, rather than addressing (or even appreciating) the nuances of every topic that might be covered in it.

It is also unclear to me why Matlock regards *MoA* as particularly "dated" (JM, p. 191) simply because, out of 30 chapters in all, it contains one abbreviated summary of the general features of a never published report written in 1972 by Champe Ransom, two reprints dating back to the early 1980s (both by Susan Blackmore), two from the late 1990s (one by David L. Wilson and one by James Houran and Rense Lange), two from the early 2000s (by Jaegwon Kim and David Papineau), and two abridged and *updated* selections from the mid-2000s (by myself and David Lester). The oldest of these selections warrants further comment here. The original Ransom report detailed 18 methodological problems with the late Ian Stevenson's reincarnation research, 13 of which were noted in the abbreviated summary of the report published in the volume. The remaining 13 items address problems inherent in the testimonial nature of the evidence that Stevenson collected, which means that they are of the sort that cannot be eliminated, or cannot be eliminated very easily. Thus they are just as relevant today as they were in 1972. Since no other contribution explores the inherent weaknesses of the sort of testimonial evidence that survival research relies upon so heavily, the original Ransom report seemed a good fit for the volume. Although some of the items in the *original* report may be dated, they would have been offset by the inclusion of both Stevenson's reply to the report and Ransom's response to it, had Ransom and I been able to secure permission from the Division of Perceptual Studies to publish the entire exchange.

The other two older selections by Blackmore concern, in the first case, theoretical arguments about the nature of astral bodies whose cogency has not diminished with time, and in the second case, summaries of attempts to experimentally document the abilities of out-of-body experience (OBE) adepts during OBEs *that do not accompany near-death experiences* (NDEs). Because no further such research has been conducted since the early 1980s (Alvarado, 2000, pp. 199–201), Blackmore’s second chapter cannot legitimately be said to be out-of-date “from the research point of view” (JM, p. 191).

Returning to the partiality of Matlock’s opening comments, it’s not clear to me why he thinks that it is a mere pretense that “the book’s purpose is to ask questions and explore reasons for thinking that death is the end” (JM, p. 192). Sure enough, the volume explicitly takes the position that in light of our best evidence, “in all probability, biological death permanently ends a person’s experiences” (p. xxvii). Now perhaps in taking a position on this issue—or at least in taking *that* particular position—the volume can be nothing other than “an unabashedly polemical text, with the answers decided at the outset” (JM, p. 192). But Matlock provides no reason to believe that *MoA* is any more “polemical” than his preferred *Irreducible Mind*, which no less explicitly starts from a particular position, namely that “the materialistic consensus . . . is fundamentally flawed” (E. F. Kelly et al., 2007, p. xiii). And since no one who holds an opposing point of view was invited to respond to the largely congruent views expressed in *Irreducible Mind* within *that* volume, either, one cannot help but wonder why such exclusivity is insidious when it occurs in *MoA* but innocuous when it occurs in *Irreducible Mind*. It is also notable that since “the answers” that our contributors “decided” were often decided in whole or in part on *evidential* grounds, there is no reason (apart from prejudice) to characterize our contributors as the antithesis of “data-led thinkers . . . whose worldview is shaped by the facts before them” (JM, p. 191).

It is also worth pointing out that assembling representatives from one side of the issue in *MoA* because they are virtually impossible to collectively find elsewhere (p. xxviii–xxix) in no way constitutes an attempt to dissuade readers from considering the other side, too. (Indeed, were that supposition correct, it would be even more detestable to produce survivalist or antimaterialist books, given how abundant they already are.) Instead, presenting the other side of the case *when there is a paucity of such discussions to begin with* ought to be seen as a praiseworthy call for readers to consider the *total available evidence*, not just the evidence that psychical researchers favor.

In any case, I don’t see how it benefits anyone for volumes like *Irreducible Mind* to monopolize the discussion. *MoA* merely adds a voice to the issue, a voice that only comes into view virtually every 80 years or so (Lamont, 1935/1990). It does not attempt to suppress anyone else’s voice. So why does Matlock find it so objectionable? In any other case the standard by which to judge *MoA* would be how it compares to other mortalist works. By this measure no previous work has assembled such a wide-ranging, interdisciplinary consideration of the case against life after death from several different authors, each of whom bring different areas of expertise to bear on the question.

Since there is nothing underhanded about defending a particular point of view on an issue—especially when you make no bones about the fact that that is exactly what you’re doing—I can only surmise that Matlock finds it morally objectionable to *specifically* defend the position that there is no afterlife, as opposed to defending the view that there is an afterlife, or at least remaining undecided about it. If he is not simply banking on readers sharing his biases, one cannot help but ask: Why is taking a negative position on an issue (or at least *this* issue) so objectionable, whereas taking an affirmative or completely agnostic position is not? After all, the issue can just as easily be reframed so that survivalists such as Matlock are the ones taking the negative stance, polemically “attacking” (JM, p. 191) the view that death ends consciousness, rather than affirming it or remaining agnostic about it. Yet he seems to have no problem with (and indeed seems to be happy about) the fact that many of the books that he cites as support (e.g., E. F. Kelly et al., 2007; Koons & Bealer, 2010) openly take negative positions critical of “materialism.”

All of this naturally raises the question as to why Matlock holds mortalist authors to such a high standard, while simultaneously giving survivalist ones a pass. (I’ll say more about the latter shortly.) Time and again we are told that *MoA* ignores *this*, or fails to take into account *that*.<sup>5</sup> For example, we are told

<sup>5</sup> In an earlier review, the Christian dualist Hasker (2015) made a similar complaint, namely that Fishman and I failed to “discredit entirely any and all evidence for an immaterial mind.” Leaving aside the fact that this conflates the mind’s

that its contributors “are not much interested in [survivalists’] evidence and ignore it, distort it, or dismiss it without what [we] would consider a fair hearing” (JM, p. 191). This is a significant concern if true. But is it true? Matlock certainly thinks so.

Here a few points of clarification are needed. First of all, as previously hinted at, it is pretty unreasonable for Matlock to require most of the contributors to engage the survival evidence directly. The first part of the volume considers primarily neuroscientific evidence for personal extinction. Part II addresses conceptual and empirical obstacles to personal survival. The third part is limited to *moral* objections to widespread theological conceptions of the afterlife. Were it up to him to put together a collection like this, it would be his prerogative to limit its content to critiques of the survival evidence alone—the subject matter of Part IV—or perhaps just any *supposed* evidence for the independence thesis (whose relevance I will call into question later). But since it was not up to him, I was certainly within my rights to ensure that the volume *additionally* address empirical evidence *against* personal survival, conceptual and empirical considerations that render its reality less plausible, and problems with the coherence of widely held conceptions of the afterlife. In other words, I was under no obligation to limit my audience only to those whom Matlock would address.

If there is any substance to Matlock’s complaint at all, then, it would have to (almost) exclusively concern the contributions to the final part of the volume, “Dubious Evidence for Survival.” I say “almost” because my cowritten Part I chapter with Fishman, “The Dualist’s Dilemma,” also assesses the survival evidence—the only genuinely relevant possible evidence for mind-brain independence on offer, I’ll argue below—through its probabilistic assessment of whether, in biological creatures, having a functioning brain is a necessary condition for having a mind (the affirmation of which we call the “dependence thesis”). In order to carry out such an assessment, one has to perform a *comparative* analysis of how the *total* available evidence impacts the likelihood of *each* rival thesis. Since I will respond to the more specific comments that Matlock makes about the Part IV contributions in the final section of this article, let me now turn to the *lower* standard that Matlock holds survivalist works to.

If we are to judge *MoA* against the rest of the survival literature, perhaps the extent to which the volume addresses the contrary evidence *for* survival from psychical research ought to be compared against the precedent that survivalists have set in the extent of *their* discussions of the evidence *against* survival, chiefly that from neuroscience, in their major survivalist works. How deeply (if at all) have survivalist authors addressed what the survivalist Lund (2009) concedes are “empirically-grounded indicators of extinction” (p. 24)?

Gauld (1982, pp. 188–214) limits his discussion of the evidence for personal extinction to a *conceptual* analysis of attempts to establish the physiological basis of memory. Braude (2003, pp. 288–293) makes a few brief comments about the search for memory engrams, but mostly just notes that the neuroscientific evidence will always be logically *compatible* with survival, a point that no sensible mortalist would deny. Lund (2009, pp. 23–25) just notes some of the general lines of evidence for personal extinction, later challenging trace theories of memory, and adding—as Robinson (2011) also notes—that any evidence for extinction can always be reinterpreted in such a way that it no longer counts as evidence for it (Lund, 2009, pp. 83–89), which of course is true of *any* evidence for *any* hypothesis. Robinson (2011, pp. 46–67) also casts doubt on purported facts about a single case of aphasia, that of pioneering neuroanatomist Paul Broca’s “Tan” Leborgne (pp. 57–59), and the classic Phineas Gage brain damage case (pp. 59–61), but the relevance of errors or mere assumptions in the reports of these particular cases is dubious given that the effects of these brain disorders on the mind are not in doubt in countless other such cases.<sup>6</sup> Finally, Carter

nonphysicality with its survival (it could be nonphysical yet fail to survive), notice how ridiculously high Hasker sets the standard. Does he hold fellow survivalists or antimaterialists to the same standard, expecting them to “discredit *entirely any and all* evidence” for personal extinction or materialism? I doubt it! It is no more reasonable to expect mortalists to address (let alone decisively refute) *every last piece of evidence* that could be cited in a case for personal survival than it is to expect survivalists to do the same with respect to the evidence for personal extinction.

<sup>6</sup> A similar point applies to his reference to some contemporary neuroscientists who, in a popular science book, relied on an evolutionary “missing link” that turned out to be nothing of the sort to make the case that the alleged greater cognitive powers of this presumed hominin ancestor were “generated by their larger brains” (Robinson, 2011, p. 61). This one poor choice of case hardly accounts for why evolutionary biologists as a whole accept that hominin mental capacities have increased over evolutionary time as brain complexity has gone up.

(2010) briefly paraphrases a few of Lucretius' and Corliss Lamont's general lines of evidence for mind-brain dependence (pp. 6, 11–12) before waving them away with a stock appeal to the merely logical possibility that the brain “filters” consciousness in some vague sense (pp. 14–23), which does not even predict such evidence (*MoA*, pp. 230–231). Carter (2010) goes on to refer to the unrepresentatively dualistic, mid-20th century conclusions of neuroscientists Wilder Penfield (pp. 24–27) and John Eccles (pp. 27–30) before superficially criticizing trace theories of memory (pp. 84–86, 93–97) and ignoring altogether contemporary evidence for the role of long-term potentiation in memory formation (Clarke, 2015, pp. 57–65; Goldstein, 2011, pp. 190–197).

None of these authors say anything about the undeniable effects of brain damage on the mind garnered from clinical neuropsychology since the days of Phineas Gage over 150 years ago (Ramachandran, Blakeslee, & Sacks, 1999; Sacks, 1987), let alone what impact these findings have on the prospect that we will actually survive bodily death with our minds more or less intact. They pay no mind to what we've learned about the prognosis for developmental delays during childhood, the psychopharmacological treatment of mental disorders in adults, or the progression of degenerative mental disorders as patients approach the end of life, never mind what these chronic conditions might have in store for our postmortem identities given that they constrain or define who we are so profoundly while we are still alive. They have very little to say about the significance of genetic contributions to cognitive and affective traits, much less whether the results of the genetic lottery will follow us into the grave. What bearing might the evolution of the brain on the mental capacities of different species of animals have on the likelihood that human minds perish at death? At least when discussing survival, on these and other vital questions about the biological basis of the mind, survivalists are simply silent. Even the best books defending the prospects for survival do not come close to meeting the standard that Matlock expects of mortalists.

In a number of places Matlock also chastises *MoA* for failing to substantially engage with *Irreducible Mind* in particular, a point to which I will respond in a moment. For now, to get a sense of how prejudicial this expectation is, consider the extent to which *Irreducible Mind* addresses the sorts of topics extensively covered in *MoA*. In addition to the sort of neuroscientific evidence already noted, *Irreducible Mind* says little to nothing about conceptual problems confronting disembodied existence (Hospers, 1967, pp. 417–419; Penelhum, 1970), the scientific implausibility that human beings possess astral bodies (Blackmore, 1982, pp. 226–236; Irwin, 1985, pp. 225–232, 256–259), the pairing problem for interactionist substance dualism (Kim, 2001), whether interaction with a nonphysical mind violates physical laws (Clarke, 2014, pp. 111–115; Wilson, 1999), or why no traces of interactive influences have ever been found in the brain (Melnik, 2003, pp. 87–88; Moore, 1981, p. 40; Papineau, 2000). Now there may be legitimate reasons why *Irreducible Mind* neglects these particular topics, reasons that perhaps parallel why *MoA* does not engage the sorts of topics that *Irreducible Mind* canvasses. The point here is that Matlock's complaint once again employs a double standard.

In any case it's true that *MoA* has little to say about the sorts of issues that captivate the contributors to *Irreducible Mind*. The reason for this is simple: *Irreducible Mind* does not make a case for personal survival per se, whereas *MoA* does make a case against it. Although there may be some overlap in their subject matters, the focus of both is not the survival question.

Setting aside its recommendations for further reading, *Irreducible Mind* in fact has little to say about the survival evidence (and even less about empirical considerations that militate against personal survival). Regarding the primary sources of survival evidence, it includes one chapter on near-death experiences (E. W. Kelly et al., 2007), which is in fact addressed in *MoA* on pp. 553–554, 559–560n1, one brief section on apparitions in that chapter (pp. 405–408) and another on non-near-death OBEs (pp. 394–403)—only a small part of which concerns their paranormality—plus one section of another chapter on cases of the reincarnation type (E. W. Kelly, 2007b, pp. 232–236). These discussions are centered around developing specific theoretical concepts based upon these sources of survival evidence—not on providing a balanced overview of the evidential features of paradigmatic cases from them, as Sudduth (2016, pp. 47–133) does—and the rest of *Irreducible Mind* practically makes only passing reference to these sources (E. F. Kelly et al., 2007, pp. 40n30, 60, 96n30, 110, 112n44, 282–286, 293, 295n51, 296n52, 308, 314, 334, 361, 403, 409, 431n4, 438, 439, 448–450, 483, 490, 523–524, 527, 561, 588n7, 592, 594, 599, 608, 624–625).

Since *Irreducible Mind* does not even aim to show that minds do not extinguish at death, then, its relevance is secondary. At best, the bulk of the evidence that it cites leaves the door open for the *possibility* of personal survival; it does not demonstrate survival, render survival much more probable than extinction, or even show that survival is merely more probable than not. For example, how, *in principle*, could the fact that psychophysiological influence occurs establish that personal survival also occurs, or even just that minds can continue to function in the complete absence of neural activity? (I will say more about the evidential irrelevance of psychophysiological influence in the mind-brain correlations section.) Perhaps through some convoluted machinations the reality of psychophysiological influence could show this; but if so, it's not at all clear how. And if it does not show this, then how is it relevant?

In an alternate universe where brain damage typically would leave our minds unscathed or even enhance them, neuroscience would provide powerful evidence for the mind's independence from the brain. But since we do not live in such a universe, the only evidence on offer that *could* support the independence thesis would be evidence for the human personality's continuance after death, or at least its ability to function separately from the body while we are still alive (ostensibly in OBEs or NDEs). Outside of idiosyncratic parapsychological circles, very few people would be persuaded that the placebo effect, stigmata, or levitating monks—some of the stock of *Irreducible Mind*—have much of anything to do with the reality of life after death or mind-body separation.<sup>7</sup>

Let us now turn to whether *MoA* actually exhibits the biases that Matlock sees in it. Nowhere in the volume are “all those who disagree” (JM, p. 191) with us about personal survival characterized as wishful thinkers. (Indeed, such a categorical characterization paints our diverse group of contributors with a rather broad brush.) True, the Foreword considers “wishful thinking” as one of many possible explanations for the *prevalence* of afterlife beliefs throughout human history (pp. xv–xvi)—but also notes how that kind of explanation falls short (particularly because “wishful thinking does not account for the fact that so many afterlife beliefs are *anything but* comforting” (p. xv). And a few of our contributors *mention* the undeniable fact that wishful thinking motivates at least *some* belief in an afterlife (p. 105, 135), a point that Matlock himself concedes is true of New Age or religiously based belief. But the contributors often acknowledge that some survival proponents engage with empirical evidence (it could not be otherwise given how much of the volume addresses empirical issues), even if “*sometimes* they also appear to engage in wishful thinking” (p. 135, emphasis mine). Here again, it seems to me undeniable that even those whose belief in survival is empirically grounded sometimes engage in wishful thinking, such as when they continue to insist that a physical medium has genuine paranormal abilities despite having been caught red-handed manufacturing “evidence.” None of this entails that *each and every* survival proponent is a victim of wishful thinking, however.

Nor is it accurate to suggest that the volume's contributors set out to portray afterlife beliefs as “religious beliefs, which science has debunked” (JM, p. 192) in order to prejudice the case. It's true that some contributors note that “belief in dualism has often”—not always—“been theologically motivated,” and that they say things such as “if the weight of the empirical evidence points toward the dependence thesis, then so much the worse for substance dualism and the possibility of immortality” (p. 107). But there is nothing particularly *prejudicial* in these comments; even when personal survival or the independence thesis are put forth as scientific hypotheses (as they are treated more often than not in the volume), the weight of the evidence could nevertheless militate against them (as we obviously argue). It goes without saying that the vast majority of afterlife beliefs are probably due to religious indoctrination—a fact that justifies treating them as religious beliefs at least *some* of the time—but to concede this is not to deny that *other* afterlife beliefs are due to “reports of paranormal phenomena taken to be evidence of survival” (p. 11). The real issue here is whether such reports actually constitute good reason to believe that biological death is not the end of one's experiences.

Ultimately, what matters here is whether the survival issue is assessed on primarily *evidential grounds*. Out of 30 total contributions, three chapters (20, 21, and 22) focus on moral objections to particu-

<sup>7</sup>After all, the proposition is not the extraordinary claim that *my* religious devotion can cause stigmata to appear on *your* body; and even if it were, such would not render either survival or mind-brain independence more likely than not since neither survival nor independence would, before looking at any data, lead us to expect/predict phenomena like efficacious placebos, stigmata, or levitation.

lar religious beliefs about an afterlife, whereas four out of nine chapters on obstacles to survival (12, 13, 18, and 19) address mostly conceptual issues. The remaining 23 chapters—three quarters of the total—are either moderately or wholly empirical. *All* of those who contributed to Parts I and IV centered their arguments on empirical data, as did *the majority* of those who contributed to Part II. And since the volume explicitly was aimed to provide a comprehensive introduction to the key contemporary arguments against an afterlife, it was never intended to present *exclusively* empirical considerations.

Nevertheless, much more of the volume deals with empirical issues than with purely conceptual or theoretical ones. This is a feature that is rather atypical for a philosophical work, and it is something that I would expect the “data-driven” readers of this journal to appreciate. Since conceptual and theoretical arguments either for or against particular ways of surviving death are hardly decisive (as they so rarely are anywhere), the issue hinges on a probabilistic assessment of the empirical evidence (Moore, 1981, p. 75). In particular, it hinges on an assessment of the *overall* relevant evidence, which means going beyond an undue focus on the evidence from psychical research alone. Scholarly works defending the reality of an afterlife rarely do this, and within those that at least cursorily address the neuroscientific/biological evidence for extinction, almost all of them only raise the issue in order to wave away (rather than evaluate) the evidence (typically with unreflective slogans like “correlation is not causation” or bad analogies with television sets). One of the larger aims of the volume was to prompt future contributors to the survival literature to at least acknowledge this evidence, and with any luck maybe even *weigh* it against the evidence from psychical research, rather than dismiss or ignore it altogether.

### **Mind-Brain Correlations Are Data That Test Hypotheses**

As Matlock correctly notes, in the Introduction I quickly reframe the mortalist-survivalist debate in terms of comparing the dependence and independence theses. Since the comparisons are not equivalent, the reason for doing so requires some explanation. Technically speaking, *pure* mortalism is the position that (absent technological intervention) individuals’ minds *permanently* cease at biological death, which survivalists deny when they affirm that such minds survive bodily death. One religious conception of personal survival (“Christian materialism” in the philosophical literature) involves the miraculous resurrection of dead human bodies whose minds were extinguished at death and will be restored when their bodies are resurrected by God. This form of personal survival is not undermined by evidence for mind-brain dependence (indeed its proponents should *expect* such evidence), so any objections to bodily resurrection will have to come from elsewhere (primarily personal identity considerations). Consequently, this monistic form of personal survival has to be treated separately (pp. 8–11, 162–164; Chapters 12 and 19). And since it’s unfalsifiable in principle (what evidence could ever show that God *won’t* resurrect us?), the *bare possibility* of surviving death in this way is of little interest from a scientific point of view.

Thus, the focus of *MoA* is dualistic conceptions<sup>8</sup> (such as soul or astral body views) held by both religious and nonreligious survival proponents, which *are* undermined by evidence for the dependence thesis. These conceptions *require* some form of the independence thesis to be true, and *can* conceivably be falsified (so long as independence thesis proponents do not reinterpret away any evidence against it). Thus evidence for the dependence thesis undercuts the idea that we have separable “souls,” whereas evidence for the independence thesis bolsters it. Mind-brain independence does not entail that our consciousness will persist forever, or so much as one minute after brain death, but it does entail that it *need not* perish once the brain dies (although it still *could* perish, I suppose). If our conscious minds do not require brain functioning in order to persist, then personal survival is at least allowed, even if it is not guaranteed. And any potential evidence for the *occurrence* of dualistic personal survival would of course also be evidence for mind-brain independence. Thus in order to resolve empirically whether dualistic personal survival is likely to occur, we have to look to evidence against it from mind-brain correlations on the one hand, and evidence for it from the survival evidence on the other.<sup>9</sup>

<sup>8</sup> We might also survive death under Berkeleyan idealism, which I’ll discuss in the metaphysics of mind section. I here assume that minds stand in some relationship to a mind-independent physical world that includes brains, in order to simplify the discussion.

<sup>9</sup> There could be clear-cut neuroscientific evidence *for* the independence thesis apart from the survival evidence, but



Next, Matlock accuses me of caricaturing the filter theory/transmissive hypothesis as holding that “the mind is mostly independent of the brain, requiring the brain only as a means to control the body” (p. 4) in order to “set up a straw argument, the easier to defeat it” (JM, p. 192). In fact, that was not a description of the filter theory at all, but rather an *appropriate* characterization of the independence thesis *simpliciter*—the independence thesis “in its most basic form, unamended by any auxiliary assumptions” (p. 225)—which is the natural starting point for *any* analysis of the independence thesis. I later go on to consider versions of the independence thesis that are more sophisticated (or “convoluted,” depending on one’s point of view), including the filter theory (pp. 230–232).

The upshot of that analysis is that, while you can endlessly tinker with the independence thesis by tacking on auxiliary assumptions such that it ends up predicting the same observations that would be a matter of course if the *unadjusted* dependence thesis were true—observations that would otherwise *falsify* the independence thesis<sup>10</sup>—this immunizing stratagem removes such amended versions of the independence thesis from scientific testing altogether. So long as the auxiliaries that you’re adding are not *themselves* capable of being tested against observation—and in this case they are not—then the more auxiliaries that you add, the less parsimonious the thesis becomes, rendering it increasingly less likely to be true (since the more that you assume, the greater the likelihood that at least some of your assumptions are false).

The solution to this problem is to refrain from adjusting the independence thesis with untestable assumptions in the first place—in other words, to start from the *most parsimonious* or *least ad hoc* version of the independence thesis. That happens to be the independence thesis *simpliciter*—my supposed “caricature” (JM, p. 192)—whose observational consequences have already been massively falsified by observed mind-brain correlations. Since such massive falsification is something that committed independence thesis proponents cannot tolerate, they often change the subject from this simple point of the logic of confirmation. Since Matlock’s all-too-predictable reinterpretation of the neuroscientific evidence is already addressed in the volume, it is his review, not *MoA*, that sets up an easier-to-defeat straw man.

Similarly, when Matlock opines that critiques of the independence thesis “mostly fall flat” because they “conceive of the ‘soul’ in terms of substance dualism.... [which] is not the basis of the transmission model” (JM, p. 192), it is in fact Matlock’s criticism that falls flat. The independence thesis is the *negation* of the dependence thesis, the view “that having a functioning brain (or similar physical structure) is a *necessary condition* (or prerequisite) for having any sort of conscious experiences—at least for biological creatures like us” (p. 3). The filter theory *affirms this independence thesis* no less than any form of substance dualism that would allow brain-free experiences. Evidence that a functioning brain is necessary for having a human mind is evidence against *any* view that would allow us brainless consciousness, and the filter theory is just such a view.

Matlock is correct that the Part I selections argue that the brain *in some sense* “produces the mind and that when the brain fails, so does the mind” (JM, p. 193). However, it’s not quite right that the conclusion of this argument is that the mind “is *fully* dependent on the brain” (JM, p. 192; emphasis mine). The conclusion is certainly that it is dependent upon the brain *for its existence*, if that’s what Matlock is getting at. However, it need not be *entirely* dependent upon the brain; brainless consciousness would be no less ruled out by the mind’s *partial* existential dependence upon the brain (Sudduth, 2016, pp. 26–27; Swinburne, 1986, pp. 176–177, 298–301, 310; cf. *MoA* pp. 3, 108, 273–276, 281n37). Although I think that there are moderately good reasons to maintain that neural activity *alone* gives rise to our mental states (namely, the absence of interactive traces), there are *exceptionally good* reasons to maintain that brain activity *at least* partially gives rise to them. And even if our mental lives only *partially* depend upon brain functioning for their existence, the absence of brain activity just as strongly entails the end of our mental lives (Broad, 1925, pp. 535–538).

as I’ll argue later, the *actual* neuroscientific evidence as a whole points the other way.

<sup>10</sup> One can *always* contort *any* hypothesis to prevent data that would otherwise falsify it from counting against it. If physicists *wanted* to add all sorts of unparsimonious, untestable auxiliary assumptions to Newtonian physics—aptly called “fudge factors”—they *could* force Newtonian physics to “predict” the same consequences that naturally fall out of relativity theory (see pp. 216, 240, 262). Nevertheless, the consensus view of physicists is that relativity theory is much closer to the truth than Newtonian physics. Similarly, the dependence thesis accounts for the neuroscientific facts much better than any ad hoc “dependence-looking independence thesis,” just as the “old Earth hypothesis” better explains the geological facts than the “Omphalos hypothesis that God created the world to *look like* it had an enormous prehistoric past” (p. 246).

Matlock's point that none of the Part I contributions "prove that the neural activity gave rise to the mental activity" (JM, p. 193; emphasis mine) is certainly true, but none of the Part I contributors claim otherwise. Instead, what the volume makes abundantly clear, in a number of places, is that "though evidence for the dependence thesis is *logically* compatible with the mind's independence from the brain, such evidence renders it *highly unlikely* that the mind can exist without the brain" (pp. 4–5). On the face of it, well-established mind-brain correlations *by themselves* constitute strong *data* that render any sort of personal survival other than by miraculous or technological intervention *highly improbable*, full stop. This point, which seems to me undeniable, is perfectly compatible with Terence Hines' conclusion that such correlations do not "prove the negative that some sort of mind does not exist independently of the brain" (p. 193), which Hines immediately precedes with a distinction between what we can *imagine* about disembodied minds and what the neuroscientific evidence appears to *indicate*. In fact, all but one of the Part I contributors *explicitly* make the same point: Matt McCormick (pp. 61–63), Jean Mercer (pp. 69–70), David Weisman (pp. 102–103), Rocco J. Gennaro and Yonatan I. Fishman (p. 121), Gualtiero Piccinini and Sonya Bahar (p. 137), and Augustine and Fishman (pp. 209–211).

Matlock thus rightly notes the technicality that mind-brain correlations "are as [logically] compatible with the independence thesis as with the dependence thesis" (JM, p. 194), but he does not underscore our concomitant point that this "is only true in a trivial sense—the sense in which any data will always be 'neutral' with respect to any hypothesis that might be proposed to explain them" (p. 210). If any scientific conclusions are warranted in any sphere, different hypotheses can provide better or worse explanations of the data on *probabilistic* grounds, a point that we develop at some length when using both inference to the best explanation (IBE; pp. 211–255) and Bayesian confirmation theory (pp. 256–271) to assess the dependence thesis in light of the primarily neuroscientific evidence. Since Matlock does not dispute the reliability of that evidence, and since in his closing paragraph he *explicitly adopts the same IBE principles* when he evaluates his own reincarnationist theory using the more debatable parapsychological evidence, by parity of reasoning he ought to likewise conclude that "our mental states almost certainly depend for their existence upon a functioning brain" and thus that "our mental lives cannot continue once our brains have died" (p. 272).

When the effects of severe brain damage on our mental lives are brought to bear on the epistemic probability of the dependence and independence theses, we find that the dependence thesis readily explains *why* severe brain damage typically produces mental deficits: we would expect no less if mental activity *requires* brain functioning, full stop. But if in some alternate universe severe brain damage typically had no effect on one's cognitive functioning, or even enhanced it, either finding would be pretty perplexing on the dependence thesis. As a scientific hypothesis, then, the dependence thesis is rendered highly probable by the data that we find in the actual world, but would be quite improbable had we found *different* data of the sort that we just imagined in the alternate universe.

As for the possibility that the dependence thesis has already been falsified, it is important to proceed with caution here, as "The Dualist's Dilemma" does, when different sources of evidence appear to conflict deeply. In such circumstances the appropriate response is to *weigh* the strength of each source of evidence and then tentatively give more weight to evidentially stronger sources. Here I can only reiterate that the dependence thesis "accounts for data that are typically much more reliable than those purportedly explained by its rival" (p. 251), a point that is prodigiously supported by our assessments of the evidence that most strongly supports the dependence thesis (pp. 205, 228–243) and that which would most strongly support the independence thesis were it to be found (pp. 218–223, 240–241, 281n33). This is a point that is also widely granted, both by philosophers and by scientists who study the mind. This is why even agnostic philosophers conclude that representative neuroscientific facts provide "very strong evidence for the position that human consciousness and personality are properties of brains or nervous systems or bodies rather than properties of immaterial substances" such that "nothing mental (and human) happens unless something physical happens" (Draper, 2002, p. 202). Even neutral monists—who are neither dualists nor materialists—have argued that the *neuroscientific grounds* for affirming mind-brain dependence are "as strong as those upon which most scientific conclusions are based" (Russell, 1925/1957, p. 51).

It is also noteworthy here that Matlock's insistence that dependence thesis proponents must always "dismiss" (JM, p. 194) any possible evidence favoring the independence thesis as *insufficient* evidence (as if the adequacy of the evidence were immaterial) would seem to cut both ways. That is, independence thesis proponents would seem no less obliged to explain away any possible evidence for mind-brain dependence as *misinterpreted* evidence (p. 215, 248, 254). So what's a survivalist to do when "Most modern neuroscientists regard memory as totally a function of the brain, a view which if justified ... is fatal to the possibility that memory and related features of personality might survive death" (Gauld, 2007, p. 295)? If survivalists are unwilling to admit that the independence thesis has been falsified by research into the physiology of memory, their only apparent recourse is to *reinterpret* that evidence—and that "they must do and do do" (JM, p. 195), typically using contentious theoretical or conceptual arguments rather than empirical ones (Braude, 2006; Gauld, 1982, pp. 188–214; Gauld, 2007; E. F. Kelly, 2015, p. 33; Lund, 2009, pp. 86–88). But if the neuroscientific consensus about memory *justifiably* counts for so little among survivalists (instead of for reasons that suggest themselves), then how seriously should unamenable nonbiologists take the biological consensus about biological evolution, or unreceptive nonclimatologists take the climatological consensus about anthropogenic climate change?

The salient difference between doubting the parapsychological data and reinterpreting the neuroscientific evidence is that the sort of testimonial evidence that grounds most survival research (the last best hope for the independence thesis) is widely acknowledged to fall short of scientific standards even outside of psychical research (e.g., Loftus, 1979), and so is no more reliable here than elsewhere. By contrast, independence thesis proponents who wave away the straightforward implications of well-established neuroscientific data *do not hesitate* to grant the clear implications of similar data elsewhere.

Thus, they employ a kind of double standard when they fall back on unreflective slogans such as "correlation is not causation." As David Weisman puts it in our volume, they "question only a highly selective correlation. Just one: the near perfect correlation between brain functions and mental functions.... They don't question the correlations that they make all the time, those in which their rigid beliefs don't have a dog in the race" (p. 102). The implications of the neuroscientific data are straightforward in the sense that we don't have to adjust the dependence thesis to entail the mind-brain correlations that we actually find with any sort of fudge factor. On the whole, the sorts of facts that neuropsychologists, psychopharmacologists, behavioral geneticists, and others have discovered are confirmed predictions that naturally fall out of the hypothesis that mental processes cannot take place in the absence of a functioning brain. Substituting the dependence thesis for a duck in the old adage: If it looks like dependence, walks like dependence, and quacks like dependence, it's probably dependence.

One reason why it is eminently reasonable to conclude that biological death permanently ends our experiences is that, among other things, even just the *partial* and *temporary* cessation of brain activity when passing out or while under general anesthesia is enough to remove conscious awareness for a time (p. 412). To extrapolate from this sort of everyday evidence that the *complete* and *permanent* cessation of brain activity at death wipes out conscious awareness for *all* time is hardly a stretch. Even some of the best minds sympathetic to psychical research have acknowledged the point (Broad, 1925, p. 533; Dodds, 1934, pp. 153–154; Murphy, 1945; Stokes, 1997, pp. 201–202). As former President of the Society for Psychical Research C. D. Broad noted, "The inference seems only too obvious" (Broad, 1925, p. 533).

It is odd that Matlock hinges so much of his critique of "The Dualist's Dilemma" on our almost passing reference to temporal precedence, for we mention it only to establish the *direction* of causation (pp. 207–209). It should be uncontroversial that if event A precedes event B, and there is a causal relationship involving A and B, and they have no common cause C, then A is the cause of B. If so, "that frontal lobe injury precedes the mental deficits that accompany it" (p. 209) would seem to indicate that the injury *causes* the deficit (rather than the other way around). Of all of the factors that we have used to determine whether or not mind-brain correlations indicate that brain activity gives rise to mental activity, this one ought to be the least objectionable.

In order to resist the "only too obvious" conclusion that troubled Broad, Matlock appeals to various examples of psychophysiological influence that we are supposed to believe show "the mind's independence

of the body” (JM, p. 191). Apart from unnecessarily exotic examples such as stigmata, it has long been accepted that psychological stress (to take a simple and more commonplace example) can damage one’s own bodily health—and yet no one outside of certain parapsychological circles ever seems to have regarded this as evidence that the mind can exist independently of the brain, let alone that it can *persist intact* once the brain has stopped functioning.

Consider the ability of obsessive-compulsive disorder (OCD) patients to will changes in their brain states, which seems to me no more problematic for the dependence thesis than biofeedback. I can do no better than quote psychologist Clark’s and philosopher Dennett’s response to neuroscientists Beauregard’s and Jeffrey Schwartz’s claims about patients who deliberately alter their own brain states:

But this would lend support to the proposition that minds are non-material—in the strong sense of being beyond the natural order—only if we were to accept the assumption that thoughts, attending and mental activity are not realised in material substance.

For if they are, then all we are seeing is that one set of physical changes can lead to another. Their argument thus assumes that which it sets out to prove.

Nor should we be surprised that the mere impingement of information can itself have an impact on a physical system: for that information, too, is materially encoded and materially transmitted. For instance, there is nothing brutally physical about the overdraft in your bank account, but the representation of that overdraft is a material state that has plenty of well-known effects, all without benefit of immaterial minds.

We do not, of course, claim that there are no interesting problems facing a science of mind and of conscious experience. But the ability of physically encoded information to bring about physical changes in a purely material system is not one of them. (Clark & Dennett, 2008, p. 22)

Thus, dependence thesis proponents don’t question the fact that mental events can have bodily effects; rather, they don’t think that this fact means what Matlock and the contributors to *Irreducible Mind* think it means. The mind’s dependence upon the brain for its existence does *not* entail that the direction of causation cannot go both ways, physical-to-mental or mental-to-physical.<sup>11</sup> As Piccinini puts it, “the issue is not about causation at all; it’s about the synchronic metaphysical relation between mind and brain. Again, we argue that the mind depends on the brain in a way that rules out independence” (G. Piccinini, personal communication, September 15, 2016). What *specific* synchronic metaphysical relation that might be—type identity/reduction (Armstrong, 1968; Smart, 1959), token identity/functional realization (Fodor, 1968; Melnyk, 2003), supervenience (Davidson, 1970), strong emergence/nomological dependence (Chalmers, 1996), constitution (Corcoran, 1999), or what have you—makes no difference (*MoA*, p. 136).<sup>12</sup> For now, simply note that it would be hasty to conclude that because this is a *metaphysical* issue, the empirical evidence must forever be neutral with respect to it. For causation itself is a metaphysical relation, and it is pretty uncontroversial that scientists genuinely discover physical causes of physical events at least some of the time.

The point of citing instances where brain damage brings about mental deficits is *not* to try to establish *that* physical-to-mental causation occurs in these instances (although it does), or that only such causation is possible (which it isn’t). Rather, the point is that in many (not *all*) cases where the direction of causation is from physical-to-mental—in particular when changes to the brain *radically* alter the mind itself—the *profoundness or depth of the effects* of neural changes on the mind is extremely difficult to reconcile with the mind’s supposed ability to function virtually unscathed in the absence of brain activity altogether. We see this in Hume’s classic concomitant variation argument:

<sup>11</sup> In fact, *because* the causation goes in both directions, the stock “correlation is not causation” objection is rather misconceived. For taken to its logical conclusion, it would entail that we cannot know that mental events like willing have physical effects like mitigating one’s OCD, either—we can only know that the two are correlated. But of course no contemporary independence thesis proponent believes this, nor should they. Thus they should stop leaning on this objection simply to avoid contradicting themselves.

<sup>12</sup> To see this point more clearly, consider that one doesn’t need to know *how* one’s hardware enables a computer program to run on one’s computer in order to know *that* it does so.

Where any two objects are so closely connected, that all alterations, which we have ever seen in the one, are attended with proportionable alterations in the other; we ought to conclude, by all rules of analogy, that, when there are still greater alterations produced in the former, and it is totally dissolved, there follows a total dissolution of the latter. (Hume, 1755/1987, p. 596)

As Jamie Horder notes, if “our faculties of judgment and discernment are susceptible to chemical control [by psychoactive substances]... this obviously raises the question of what mental states, if any, are *not* subject to chemical manipulation” (p. 198). If minds can function completely independently of brains, brain states should not be able to fix or determine our mental states so completely (e.g., when PCP alters one’s moral compass). The data that are most inconvenient for the independence thesis are those that show that one’s supposedly independent mind is so *thoroughly* at the mercy of the condition of one’s brain.

That “behavioral changes guided by will can sometimes bring about the neural reorganization” (JM, p. 194) seen in neuroplasticity is similarly hardly surprising; what we have in such rehabilitation is little more than an extension of the fact that learning a new fact (consolidating a long-term memory) produces neural changes, only here to a greater degree. As previously noted, outside of particular parapsychological circles today, no one ever seems to have regarded such an obvious point as indicative of anything anomalous, even when they’ve thought that there *are* relevant anomalies elsewhere.

It may be unnecessarily strong to claim that “the decline of psychological function in a compromised brain *demonstrates* that the mind cannot exist apart from the brain” (JM, p. 194; emphasis added), but it certainly makes it *highly unlikely* that human mental activity can exist in the absence of brain activity. For if mental activity were in fact inseparable from brain activity, we would expect the degeneration of the brain to result in a corresponding degeneration of the mind, the actual occurrence of which everyone acknowledges to be the rule despite the fact that some researchers unduly hone in on any potential exceptions in the hope of avoiding this unwelcome implication.

It is thus notable that Matlock also mentions rare terminal lucidity cases where “although the brain may have been severely impaired by advanced dementia ... [purportedly] patients suddenly become responsive, recognize and even converse with loved ones, usually shortly before dying” (JM, p. 194). Since he acknowledges that we address such cases but does not interact with anything that we say about them, I should note that the reports themselves are questionable (Nahm, 2009, p. 98), that the cases might just be occasional lucid intervals that are selected for presentation because by chance they happen to occur sometime before death (Nahm et al., 2012, pp. 139–140), and that when there are parallel intervals in which motor functions are temporarily restored, no one suggests that *motor processes* might function independently of the body or brain (*MoA*, p. 102).

Most importantly, though, even at face value terminal lucidity cases don’t constitute evidence for the independence thesis. For if we started with the assumption that the independence thesis is true, prior to looking at the data we would *not* anticipate that we would find cases of terminal lucidity. The hypothesis does not predict that data, which is why the *failure to* uncover terminal lucidity cases would not have falsified the independence thesis.<sup>13</sup> And “if a severely damaged brain is what prevents a person from being able to hold lucid conversations in the first place, a brain that *remains* just as damaged in the weeks or days prior to death will *continue* to prevent their occurrence—even on the popular filter theory” (p. 250).

Matlock’s proposition that “a correlation between brain degeneration and loss of awareness is to be expected” (JM, p. 194) given the tightness of observed mind-brain correlations seems to me trivially true. It is simply one of many instances of the tight correlation between mental functions and brain functions. The question is what *explains* the tightness of these correlations. Again, such tight correlations are straightforward observational consequences of the dependence thesis: if *that* hypothesis were true, *those* data are exactly what we would expect to find. As such, they constitute *confirmed predictions* of the dependence thesis.

Matlock goes on to suggest that perhaps hidden subconscious processing continues when the mind’s interaction with the brain is interrupted, which could go some way toward explaining responsive-

<sup>13</sup> The independence thesis may be *compatible with* terminal lucidity, but that no more constitutes evidence in its favor than the compatibility of Pavlovian classical conditioning with such independence constitutes evidence for mind-brain independence.

ness in locked-in syndrome cases or persistent vegetative states. This proposition is rather ironic given that the first example of a confirmed prediction of the dependence thesis that Fishman and I cited in our IBE assessment centers on the difference between brain damage that produces unconsciousness versus that which merely produces paralysis: “temporary brain damage leading to unconsciousness is not phenomenologically like bodily paralysis, as substantial dualism [or the independence thesis simpliciter] would predict, given natural auxiliary assumptions” (Johnston, 2010, p. 131). In any case, if conscious awareness requires interaction with the brain (as Matlock seems to imply), then the death of the brain makes impossible any sort of conscious existence after death—as personal survival requires—as *a discarnate*, though one’s otherwise exclusively subconscious self *might* regain conscious awareness if (and only if) it is conjoined with a new brain after possession, reincarnation, or resurrection.<sup>14</sup> And this would be inconsistent with Matlock’s belief in genuine “memories of the intermission in reincarnation cases” (JM, p. 198), such as memories of “veridical perceptions of the terrestrial world” (JM, p. 198) formed while simultaneously conscious and discarnate.

Similar comments apply to Matlock’s depiction of Horder’s argument that on the independence thesis, “the mind should be consciously aware and in control of everything that befalls the body” (JM, p. 194) (“everything” is obviously a bit of an exaggeration), which does not occur and thus suggests that the independence thesis is false. Here, too, Matlock suggests that “our subconscious could preserve our identities when our conscious awareness is offline or confused” (JM, p. 194), but again, in the absence of brain functioning, we would not then have “*personal* continuance and *conscious* existence after death, where the individuals we knew in life would be recognizable to us because their minds have survived death largely intact” (p. 1). Or as Horder himself puts it: “the brain evidently also produces (or releases or transmits) our consciousness of *having or lacking these functions*. So even if we assume that the brain is more of a transmissive ‘stained-glass window’ than a productive ‘steaming kettle’ for the mind, without a brain, everything must go” (p. 202).

If one must have a functioning brain in order to even be aware of one’s mental functions, then any conceivable disembodied mind that one might posit could have no consciousness.<sup>15</sup> Saying that a mind “survives” under such circumstances is a bit like saying that someone in a permanent coma is “active” because his autonomic functions continue to operate. Since Matlock does not imagine that we *technically* survive death as something less than conscious discarnates—as “souls” locked in a persistent vegetative state from which we only awake when conjoined with a brain—this response is not available to Matlock.

### The Prior Probability Diversion

Matlock wrongly accuses me and Fishman of rigging our Bayesian analysis by stipulating a very high prior probability in favor of the dependence thesis—namely by assuming “that the mind cannot affect the brain and body and that the physical realm is causally closed” (JM, p. 200)—such that we “guarantee that the dependence thesis comes out ahead” (JM, p. 200) when its final (or posterior) probability is calculated. That the assignment of the priors is arbitrary and thus prone to self-serving manipulation is a common parapsychological criticism of Bayesian analyses, but it simply does not apply to our case. It is plain wrong on a number of levels.

<sup>14</sup> Though I concede the *logical* possibility that we might “persist” after death without consciousness and then regain it once we become conjoined to a new brain, I don’t mean to suggest that I think that this possibility is at all *likely* to occur. On the contrary, our best data suggest that *everything* about our mental lives—conscious *and* subconscious—cannot occur in the absence of a functioning brain, and thus cannot persist once our brains have died. More specifically, since the unique biological features of my *particular* brain determine my distinctive personality traits, for instance, it’s not as if some additional part of me—such as Broad’s “psi factor”—could just interlock with a new brain with the result that I merely “wake up” in a new body. For even if a psi factor existed, becoming conjoined to a new brain would not preserve what is *distinctive* about my personality, since that was determined (even if only in part) by the old brain that was lost, not by the persisting psi factor alone.

<sup>15</sup> See Broad (1925, p. 539) on the distinction between genuine “Survival and mere Persistence” and Sudduth (2016, pp. 34–36) for a discussion of limiting one’s consideration to “the strong psychological survival hypothesis.”

First of all, as seen in the last section, we do not assume that psychophysiological influence does not occur, but in fact *grant* that it does. Nor do we assume that causal closure obtains; rather, we find *evidence* that it does.<sup>16</sup> (I will lay out this evidence in the next section.) Moreover, even if the data indicate that closure *is* violated, this would *still* be consistent with the minimalist definition of the dependence thesis that I've been discussing throughout here. That is because it is possible that it is not brain activity *alone* that gives rise to mental activity, but brain activity entangled with the activity of some other independent thing (e.g., Broad's psi factor) that is less than a mind itself, but which nevertheless contributes to giving rise to minds *when it is conjoined with a functioning brain*. In that case our mental lives could not survive death because the "compound" of the two things that give rise to minds would cease to exist with the death of one of its parts, the brain (p. 273). Thus, *had we* assumed closure—which we didn't—this would not have bolstered the dependence thesis in our minimalist sense, since *either* closure *or* its violation is compatible with the thesis in that sense. Potential violations of closure are only relevant because any version of the *independence thesis* (as well as Broad's "compound theory") positively predicts that they will occur. The failure to find any interactive traces thus constitutes a falsification of the independence thesis (and compound theory), provided that proponents do not resort to ad hoc maneuvering to avoid that falsification (such as maintaining that the interactive traces are there but, conveniently enough, forever undetectable). If the independence thesis that dualistic personal survival requires is true, there *have to be* interactive traces. The apparent absence of such traces thus suggests that dualistic personal survival does not occur.

To see how widely Matlock's rigging allegation misses the mark, consider the prior probability considerations that we *do* mention, almost in passing. First, in our related IBE analysis there is the *initial* parsimony of each thesis to consider—that is, how many assumptions each thesis makes about what sorts of things exist or occur *prior to* looking at any data. Second, there is plausibility or fit with background knowledge, "the extent to which a hypothesis is consistent with background knowledge that has been independently established by conventional science and history" (p. 212). Aside from noting that in general it is less parsimonious to postulate spirits and spiritual realms than not to postulate them, all that we have to say about parsimony here is this: "The independence thesis postulates an additional entity and an additional process—either a nonphysical soul or an astral body, and whichever, its interaction with the brain—that the dependence thesis does not require at all" (pp. 211–212). And the upshot of what we have to say about how well each thesis fits within our background knowledge is simply:

On the face of it, the dependence thesis does not clash with any well-established scientific data, theories, or laws. By comparison, the independence thesis predicts that there will be interactive influences on the brain that—unless they come from physical astral bodies—appear to require the violation of well-established physical laws. Moreover, the independence thesis flies in the face of our understanding of the evolutionary origin and development of animal minds. (p. 213)

In the separate Bayesian analysis itself, the prior probability considerations that we mention are that *some* views that postulate discarnate perception, cognition, and emotion suggest an implausible break in our evolutionary continuity with other animals, and that interaction requires *either* the violation of well-estab-

<sup>16</sup> There is a technical point to make here, but it is not a trivial one, as it accounts for why Matlock makes the mistake of assuming that these issues concern the assignment of the priors in the first place. Namely—even if they had been relevant to whether the dependence thesis is true—neither psychophysiological influences nor violations of closure (i.e., interaction) are prior probability considerations to begin with. Rather, they are facts to be explained by the hypothesis in question. They neither concern how the two theses compare in their parsimony, nor how consistent each is with other things that we know "in the background." Had they been relevant, they would have concerned issues of *explanatory power*—the extent to which each thesis leads us to expect/predict the observations that we actually find (and thus how well each thesis *explains* those data). So, for example, they would have been more like the effects of brain damage on the mind—observational consequences of hypotheses that are either confirmed or falsified by the data. A true background consideration here would have been whether, for example, either thesis is consistent with the laws of physics, or with the fact that biological evolution has occurred. These are background considerations because neither thesis *predicts* that particular conservation laws will hold, or that biological evolution has occurred, but either thesis can be *consistent or inconsistent* with that knowledge (p. 213).

lished physical laws *or else* the postulation of astral bodies otherwise unknown to science. Admittedly, we do here say of the independence thesis that “we would need a considerable amount of compelling evidence in its favor—and at the expense of the dependence thesis—to outweigh its initially low prior probability.” But literally *the very next sentence* explains that “we will *charitably assign equal prior probabilities* of 0.5 to the dependence and independence theses” in our Bayesian analysis (p. 260; emphasis mine). And when we later summarize the upshot of that analysis, we note that “if we *charitably assumed equal priors* for the dependence and independence theses, Bayes’ theorem would [still] yield a vastly lower posterior probability for the independence thesis” (p. 270; emphasis mine). So it is blatantly false, on *either* the IBE or Bayesian analysis, that had we set aside prior probability considerations altogether, “the calculus changes so that the dependence and independence theses are more equal in their prior probabilities” (JM, p. 200). The real issue here is not prior probability at all, but the fact that the dependence thesis makes countless predictions that are *confirmed by observation* (pp. 218–244).

Thus Matlock is at his weakest when he hastily pronounces that “when we take into account all of the data relating to mind/body relations, not just those which conform to the expectations of the dependence thesis,” this is “enough to tilt the balance in favor of the independence thesis” (JM, p. 200). For this forces Matlock to defend the view “that the ambiguous parapsychological evidence for survival actually outweighs the virtually incontestable neuroscientific and other evidence for extinction” (p. 5; cf. p37n6). It’s one thing to say that the parapsychological evidence should be taken seriously (as it should); it’s quite another to say that it actually *outweighs* the neuroscientific evidence. On this issue, consider this telling concession by Gauld, who is hardly antithetical to psychical research in general or survival research in particular:

The data demonstrating connections between memory and brain function, which were already sufficient in Myers’s time to fill a library shelf or two, have now attained a detail and quantity that would fill a library or two. How within a broad canvas one might reconcile these data with the data ostensibly indicating that personal memories may survive death and disintegration is not easy to conceive, and the evidence for post-mortem survival of memory, though it has grown since Myers’s time, has not grown on anything like the same scale as the evidence for some sort of linkage between memory and the brain. (Gauld, 2007, p. 296)

Prior probability considerations like simplicity indeed count, but the data themselves count most of all; and when there is deep conflict between two different sources of evidence (which Gauld, at least, admits is not easy to deny), taking into account the *quality* of the data is paramount.

### **Apparent Causal Closure Is Incidental Corroboration**

Matlock defines the causal closure thesis as maintaining that “for every physical effect there is a physical cause” (JM, p. 195), but this is a little too strong. It would be better to define closure in indeterministic terms consistent with contemporary quantum mechanics. Thus I would suggest that causal closure be defined as the idea that, for every physical event that *has* a cause, its cause is physical. Otherwise closure would be immediately falsified by the widely acknowledged occurrence of uncaused physical events that have nothing to do with consciousness, such as the radioactive decay of an atomic nucleus, the spontaneous generation and annihilation of virtual particles within Planck-length time scales, and so on.

As noted in the previous section, the dependence thesis does not *require* causal closure to hold because mental states might depend only *in part* upon brain states for their existence, in which case *the combination* of brain functioning with the functioning of some *additional* entity that is not *itself* a mind might give rise to mental states. In that case the additional entity would be in interaction with the brain, but having a functioning brain would be no less necessary for having a mind. So detecting interactive traces would not falsify at least this minimalist sense of the dependence thesis. On the other hand, the independence thesis (in any form) *does require* violations of causal closure—and thus the existence of interactive traces—in order



for the mind to control the body.<sup>17</sup> Thus the failure to detect interactive traces constitutes evidence against the independence thesis.

At the same time, this failure also seems to indicate that a stronger form of the dependence thesis is true. Namely, it seems to indicate that there really is no additional entity in interaction with the brain, whether it be a nonphysical soul, an astral body, or even Broad's psi factor. Consequently, although evidence for mind-brain dependence is not *itself* evidence for the nonexistence of Broad's psi factor, at least, it nevertheless looks like his psi factor does not exist. And if that's right, that would corroborate the stronger idea that mental states depend for their existence *on a functioning brain alone*, which is just as fatal to dualistic personal survival as the idea that they only partially depend on a functioning brain for their existence.

What might the relevant corroborating evidence for this stronger form of dependence be? The fact that interactive traces are nowhere to be found, of course (see Chapters 14, 15, and 16 on what sorts of interactive traces we would expect to find were any additional entities actually altering our brain activity). If some additional entity *were* interacting with our brains, then there would be physiological changes to *the way that our brains function*. So the question of whether there are such changes is a question for neurophysiologists to answer. Unfortunately for independence thesis proponents, as Andrew Melnyk explains:

[I]f interactionist dualism of this sort were true, then there should be human behaviors for which no sufficient neurophysiological cause can be found by tracing efferent motor neurons back into the brain; and discovering such behaviors would clearly provide spectacular support for the dualist hypothesis. However, neuroscientists have as yet failed to discover any such behaviors, and my strong impression is that they do not expect to. (Melnyk, 2003, p. 187)

Melnyk is careful to point out that the significance of this failure is not that it *definitively disproves* mind-brain independence, as one can always come up with excuses for a lack of evidence. Rather, his point is that, had such traces been found, what amounts to a lost opportunity *would have* provided "striking confirmation" of the independence thesis (or at least Broad's compound theory). But as things stand, such corroborating data "have not in fact been uncovered; and no theory can be supported by nonexistent evidence, whatever might be the reason for its nonexistence" (Melnyk, 2003, p. 188).

One sympathetic proponent of the existence of such traces was the renowned neuroscientist Eccles, whose ideas on where interaction might occur in the brain—and thus be detected there—are systematically explored in Wilson's Chapter 14 of *MoA*. For now, I can do no better than cite Brooke Noel Moore's summary of what is most problematic about attempts like those of Eccles:

Such items as Betz cells and synapses ... clearly make their way into explanations of human behavior because of certain specific discoveries about the nature and functioning of the human nervous system. It seems altogether different in the case of Eccles' "conscious self," and it is too easy to think that the conscious self is brought into the picture not as a result of some specific theoretical physiological need but rather as a result of the psychological need of Sir Eccles to bring it in. (Moore, 1981, p. 40)

It is little wonder, then, that philosophers of mind and cognitive scientists (of all people) tentatively accept causal closure. They don't merely assume it; the evidence that we have suggests it. If that ever changed, so would their tentative acceptance. But there doesn't appear to be any reason to expect that to happen.

Turning from the *observational* problem that closure appears to hold, to the conceptual problem

<sup>17</sup>I am simplifying here by ignoring historical forms of substance dualism that *do* deny causal interaction in any direction, but have no defenders in the contemporary literature. Leibniz's parallelism pulls off this feat by invoking a metaphysical relation he called "pre-established harmony," essentially the idea that wanting to raise your hand corresponds perfectly with your hand rising because God determined that your hand would rise at the beginning of time at the same time as your intention, rather than because one event caused the other (an explanation that he extends to physical-to-physical causation as well). Nicolas Malebranche's occasionalism is similar.

of *how* minds lacking any physicality at all (i.e., having no spatial position, size, shape, mass, energy, momentum, etc.) could “push around” molecules in our brains, Matlock replies, “psychokinesis (PK) did it!” But since PK is a mere placeholder for an explanation—“nonconventional influence” is *not* any identifiable mechanism or process—I don’t see how PK could even begin to answer the question. No new information is being imparted in this semantic circle: to say that psychokinesis is what allows nonphysical-physical interaction is just to say that nonphysical-physical interaction allows itself. Matlock’s appeal to psychokinesis to solve Jaegwon Kim’s pairing problem (Chapter 13) therefore simply assumes what it needs to show, namely that some other kind of causation both exists and is what pairs a nonphysical soul to a physical body. So long as the mind is held to be *entirely nonphysical*, it’s not clear to me that the conceptual question *can* be answered, other than by grumbling, “It just happens!”

Now if this is the wrong way to conceive of the mind, and the mind is perhaps better conceived of as realized in some sort of physical astral body, then the conceptual problem above would evaporate. Hence I concur with Matlock that “there would be fewer logical difficulties for a surviving mind that was localized in space (and time)” (JM, p. 196), as I make clear when I summarize the remaining issues that make survival via a spatiotemporal/physical astral body *empirically* implausible (pp. 7–8). Unlike us (Chapter 17), however, philosophers of mind have paid little attention to this alternative, primarily because it does not even *aim* to solve the mind-body problem. Rather, it simply reframes it: The mind-body problem becomes the mind-astral body problem, merely pushing explanation back one step further instead of addressing the issue. That is, the issue is simply reframed from “How does the mind relate to the *normal physical body*?” to “How does the mind relate to the *astral body*?” This leaves all the attendant problems that have perplexed philosophers of mind untouched, such as how astral bodies could give rise to phenomenal properties (qualia), how they could be *about* or *represent* other things in a deeper sense than most appreciate (intentionality/representation), how their astral properties could impact or be impacted by purely mental properties (mental causation in the latter case), and so on. David J. Chalmers’ “hard problem of consciousness” (Chalmers, 1996) wouldn’t go away. And merely saying “psi” answers none of these issues, either.

It’s also unclear to me how quantum indeterminism might undermine causal closure (so long as one does not define closure deterministically at the outset). Nothing in quantum mechanics requires that some physical events have *nonphysical* causes; it merely requires that some physical events do not have any causes of any sort. When Matlock claims that its standard interpretation “places consciousness outside physical systems” (JM, p. 195), he is playing fast and loose with his words. The standard *Copenhagen* interpretation requires an “observer” to collapse the wave function, such that when one tries to measure a subatomic property such as the exact position of an electron, the very act of measurement changes the nature of the quantum system. But the act of measurement could be carried out by another physical system, such as a phosphorescent screen, and thus does not require a conscious mind at all, let alone one “outside physical systems.” This “observer” must be outside of the *quantum* system, but not the physical world altogether (as phosphorescent screens obviously are not).

Even *alternative* understandings such as the Von Neumann–Wigner interpretation, which *do* give consciousness a more central role, nevertheless do not entail any violations of closure:

This theory is certainly not universally accepted (for a start, it *presupposes* that consciousness is not itself physical, surely contrary to the views of most physicists), and I do not accept it myself, but in any case it seems that the kind of causal work consciousness performs here is quite different from the kind required for consciousness to play a role in directing behavior. It is unclear how a collapse in external perceived objects allows consciousness to affect physical processing within the brain; such theories are usually silent on what happens to the brain during collapse. And even if consciousness somehow manages to collapse the brain state, then all the above remarks about apparently random processes and their connection with behavior will still apply. (Chalmers, 1996, p. 157)

Chalmers’ closing comment refers to the fact that quantum processes are *inherently random*, and thus could not produce the *nonrandom* behavior of conscious beings even if someone like Eccles could find

a place for them in the brain—a point that Wilson underscores in his chapter of our volume (pp. 350, 355, 361–362) after showing that even the most nuanced proposals for interactive mechanisms would violate known physical laws. And *should* quantum processes take place in the brain, their microphysical effects on macrophysical neurons would be negligible anyway because brain function is dominated by *deterministic* physical effects that would drown them out, such as fluctuations in the blood supply to the brain (Jeeves, 1998, p. 94). So even quantum mechanics does not allow nonphysical minds to control human bodies. One must grasp at straws to find any kind of vague empirical support for violations of closure.

Given its dubious relevance to either life after death or how brains function, one wonders why Matlock brings up “the revolution in physics that came with quantum mechanics” (JM, p. 196) in the first place.<sup>18</sup> I suspect that he does so in order to give his belief in the existence of spirits an air of scientific respectability that it would not have otherwise. In certain circles there is a strong need to believe that myopic materialists are blinded by an outdated Newtonian understanding of physics (e.g., Carter, 2010, p. 32, 39), whereas antimaterialists are, of course, revolutionary trailblazers following the latest cutting-edge physics.

It does little good to caricature one’s opponents in this way merely for disagreeing with you. So in the interest of challenging the presumptuousness of such a position, a few simple questions are in order. Is it the consensus view of physicists in general, or quantum physicists in particular, that physical closure is violated? As a whole, do such physicists believe in deceased human spirits in particular, or genuinely paranormal phenomena in general? If they do not, how can one justifiably appeal to contemporary quantum physics to support the existence of such things? I suppose that it is always possible that *physicists themselves* are caught up in a myopic, outdated physics. But then who better to school physicists on physics than nonphysicists? In any case I have my doubts about just how representative those physicists who “believe there is evidence that the mind can bias outcomes in certain directions” (JM, p. 195) are of physicists as a whole.

### How Relevant Is the Metaphysics of Mind?

In order to address Matlock’s specific criticisms here, readers need to understand why the volume only briefly addresses the mind-body problem to begin with. Although the issue of which theory of mind is the correct one is not exactly *irrelevant* to whether or not we survive death, there is an important sense in which it is definitely *secondary*. Various solutions to the mind-body problem have *implications* for personal survival, either ruling it out entirely, or else at least permitting it. So if mental properties *just are* neural properties (reductionist materialism), for example, one’s mind cannot in principle survive the death of one’s brain. But if minds are nonphysical substances connected to brains (interactionist substance dualism), then one’s mind need not die when one’s brain dies, even though it still *might* die, say, by diminishing to the point of nonexistence in the absence of brain activity to sustain it (Lund, 2009, p. 85).

The same point applies to personal identity theory. If, for example, one holds to a brain/body criterion of personal identity, then in order for the same *person* John Doe one converses with today to continue to exist tomorrow, his brain or body would have to persist. Since his brain or body obviously does *not* survive bodily death, sans bodily resurrection the *person* he is could not survive death, either (Kagan, 2012, pp. 132–169). On the other hand, if one holds to a memory/psychological criterion of personal identity, and only his memories or psychological traits need persist in order for the same person to continue to exist, then he *might* continue to exist after his bodily death (depending on whether those memories or traits *actually do* persist after bodily death).

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<sup>18</sup> Matlock disparages Angel for “ignoring” quantum mechanics in his tour of intellectual history although in fact Angel explicitly mentions the need for scientists to have understood chemical compounding before they could complete the physicalization of chemistry (p. 383; cf. p. 389n9). In the same paragraph Matlock chides Angel for (accurately) describing Plato and Pythagoras as “rationalists,” which Matlock mistakenly takes to mean “materialists.” What Angel in fact meant was that Plato and Pythagoras are part of the same intellectual tradition that characterizes the rationalist epistemology of the late modern philosophers Descartes, Leibniz, and Spinoza, who believed that only reason, without the aid of experience, supplies genuine knowledge.

These are not the only (or even the received) mind-body or personal identity theories on offer,<sup>19</sup> but they are the simplest views to start with to get a sense of the underlying issues. I mention them here to answer the question, implicit in much of Matlock's review, "Why not argue from theory of mind X (or personal identity theory Y) to personal survival or extinction?"

The volume largely neglects such *theoretical* arguments for good reason: There is nothing like a consensus among contemporary philosophers as to which (if any) of these theories in the metaphysics of mind is the correct one (Bourget & Chalmers, 2009). Thus, it would be foolish to argue for personal extinction from a highly contentious issue (which theory of mind is correct) when a much less contentious one is available (whether brains give rise to minds). There is much more consensus about the latter because our most reliable *evidence* consistently points to a particular answer.

Thus to complain, for example, that the volume largely fails to engage criticisms of materialism (e.g., Hasker, 2015) as if this were some sort of deficiency widely misses the mark. *One does not need to presume materialism*, or indeed any theory of mind, in order to provide strong arguments for personal extinction. To complain that we failed to do so is to protest that we used stronger arguments when we could have used weaker ones, "the easier to defeat" (JM, p. 192). How could that possibly be a deficiency? By the same token, one need not address "idealist and process approaches to the survival problem" (JM, p. 191), either, for there is certainly no consensus about the cogency of those approaches, and the evidence for mind-brain dependence defeats them all the same.

Matlock correctly notes that there are survival-friendly theories of mind other than Cartesian dualism, and that we pay little mind to "other forms of dualism or to nondual and idealist possibilities" (JM, p. 195). It's true that the volume's contributors have little to say about non-Cartesian substance dualism (in which nonphysical mental substances are granted at least some physical properties), panpsychism (in which all physical substances have at least some primitive degree of mental/phenomenal properties), or idealism (in which, at least among concrete entities, only mental objects exist, and thus physical objects do not exist at all). The question is whether neglecting these particular alternatives constitutes any real deficiency in the volume.

Traditional Cartesian dualism is the natural starting point for any consideration of *how* one might survive death without technological or miraculous intervention, so it is little wonder why we start with it when considering conceptual and empirical obstacles to *particular ways of surviving death* in Part II. The question is how far beyond objections to it one need go in considering obstacles to personal survival. Not very far, I would argue.

If we treat the mind as *entirely* nonphysical, this Cartesian concept of mind generates certain problems that obviously do not apply to different conceptions. One alternative conception that allows dualistic personal survival views the mind as something that is harbored by an *entirely* physical astral body instead of by the brain.<sup>20</sup> If these two possible ways of dualistically surviving death—as either an entirely nonphysical soul, or else as an entirely physical astral body—exhaust the possibilities, then there is nothing more to cover.<sup>21</sup> And if they do not exhaust them because a primarily nonphysical mind might also have a few physical

<sup>19</sup> For example, "further-fact" criteria of personal identity go beyond bodily and psychological criteria. Other theorists hold that there is no fact of the matter about whether anyone remains the same person from one moment to the next, and some hold that preserving personal identity is not what matters in ensuring survival. See Heil (2004), Kim (2011), and Noonan (2003) for good introductory surveys of these and related issues.

<sup>20</sup> Incidentally, Matlock is not quite right that Blackmore's contribution to Part II newly introduces "the idea that the mind might survive death in a quasi-physical subtle (astral) body" (JM, p. 196). Hers is the only contribution to systematically explore the difficulties confronting such a view, but it is discussed in the Introduction (pp. 2, 7–8, 32, 37n14, 39n28), within a quoted argument critiqued in the brain damage chapter (p. 127), in the neural localization chapter (p. 137, 165n2), and a number of times in "The Dualist's Dilemma" (p. 211, 213, 226, 240, 244, 247, 276, 279n18, 283n66–67), as well as in contrast to purely bodiless minds in the opening of Drange's first Part II chapter (p. 329), and, implicitly, at the end of Kim's pairing problem chapter in a thoughtful final section on whether "souls" should be located in space (pp. 345–346). Blackmore also reiterates some of her earlier comments on the nature of astral bodies in her OBE chapter (pp. 520–521, 524), and I address what implication out-of-body discrepancies have for the reality of astral bodies in my NDE chapter (p. 550).

<sup>21</sup> Even apart from the possibility of fully bodiless minds, I concur with Matlock that a stream of consciousness doesn't *necessarily* need an astral body to sustain it, but only because it's logically possible that one's memories and personal-

properties and survive death, then at least some of the objections to astral body views will transfer over to this non-Cartesian form of interactionist substance dualism. Non-Cartesian dualism removes difficulties confronting a purely nonphysical mind, as it were, by introducing *other* difficulties that confront astral body views. So by explicitly providing objections to both Cartesian dualism and astral body views, one essentially splits the difference, implicitly providing objections to any middle way between them as well.

As we've seen in previous sections, Matlock displays a tendency to misrepresent others' views in order to create the appearance that they support his own. For example, he misleadingly attributes to Noë (2009) the view that "conscious awareness emerges outside the brain, in response to environmental stimuli" (JM, pp. 193–194). But this is not what Noë's embodied/situated cognition approach maintains. When Clark is not disputing Jeffrey Schwartz's and Beauregard's construal of psychophysiological influence (see the mind-brain correlations section above), he defends the approach's extended mind thesis—roughly, that when you use a calculator, your mental processes extend beyond just your brain processes to include processes going on inside of the calculator, too (Clark, 2008). Piccinini and Bahar describe the approach accurately, noting its irrelevance to their localization argument in the volume:

Sixth, it has become popular to point out that the mind is *situated* at least in part in the body and the environment.... From this some authors conclude that the mind is not *located* solely in the brain. Undoubtedly, the mind is so situated; but so is the brain. The brain is situated within the nervous system, the body, and the environment. Thus, the mind being situated may or may not conflict with it being located in the brain. Suppose, for the sake of the argument, that the mind is situated in such a way that it is located not only in the brain, but also in physical structures outside the brain. This highly contentious assumption would neither change the nature of our argument nor support the existence of nonphysical minds, let alone an afterlife. Since the situatedness of the mind makes no difference to our argument, we set it aside. (p. 138)

Matlock similarly ascribes to Chalmers, Strawson, and Koch "panpsychist positions that *recognize that awareness is not grounded in cerebral activity*" (JM, p. 200; emphasis mine). But nothing could be further from the truth; each of their respective positions actually *entails* that awareness *is* grounded in brain activity. Chalmers (1996, pp. 125–129) and Koch (2012, p. 152) self-identify as property dualists (*MoA*, pp. 108–109, 111), theorists who think that physical brains also have nonphysical mental/phenomenal properties; and insofar as mental properties cannot survive the destruction of the physical substrate in which they inhere, property dualism *entails* personal extinction. Strawson is a Russellian monist, one who thinks that physical brains have extrinsic physical properties that feature in physicists' causal explanations, as well as intrinsic physical phenomenal properties that do not feature in them (Chalmers, 1996, pp. 153–155). Strawson calls this "real physicalism," and its implications for survival are just as uncompromising.

But don't take my word for it—consult the works that Matlock cites. Chalmers (1996) wants us to "note that the real problem with consciousness is to explain the principles in virtue of which consciousness *arises from* physical systems" (p. 121; emphasis mine). Koch (2012) states that "the experience of being sad is a crystal, a fantastically complex shape in a space of a trillion dimensions that is qualitatively different from the brain state that *gives rise to* sadness," thereafter concluding that "it is not immortal. Once the underlying physical system disintegrates, the crystal is extinguished" (p. 152; emphasis mine). And Strawson (2006) writes that "I am happy to say ... that experience is 'really just neurons firing', at least in the case of biological organisms like ourselves. But when I say these words I mean.... that there is a lot more to neurons than physics and neurophysiology record (or can record)" (p. 7). Despite his desire to find support for his views in the wider scientific and philosophical communities, Matlock continues to represent a lone voice in the wilderness.

In any case, if an exotic possibility like panpsychism "does not necessarily require a rejection of materialism" (JM, p. 200), and thus is compatible with personal extinction, why bring it up at all? The

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ity traits could somehow be implanted/uploaded in a new biological brain before the old one dies—a minimalist kind of reincarnation in which there is no discarnate existence between incarnations. But even this bare possibility would not allow minds to persist in the absence of brain activity.

same point applies to Matlock's appeal to Whiteheadian process metaphysics—a view that he concedes sees “mental activity as having ceased” (JM, p. 201) at biological death. Sure, if you *amend* it so that it no longer entails that implication, then it becomes at least compatible with personal survival. But that is surely true of any metaphysics if one amends it enough. And while Alfred North Whitehead's views entail mortalism, mortalism itself does not require that his process metaphysics be correct, again making its relevance doubtful.

To complain that we don't consider a position as extreme as idealism seems to me misguided. If physical objects don't exist at all, and what we typically think of as a physical object (such as a human body) is really just a mental object—an idea in the mind of God, say—it is nevertheless an “object” that exists independently of the content of *one's own mind alone*, and so would continue to exist even one's own mind ceased to exist. There would still be an external world (a world external to one's own mind), it just wouldn't be a physical one. This of course removes any conceptual problems surrounding the causal interface between the mental and the physical since only the mental exists. But it escapes me why we should take the extreme that there are no physical objects at all (idealism) any more seriously than the opposite extreme, that there are no minds at all (eliminativism). Both positions “solve” the mind–body problem a little too easily—one by simply denying the very existence of bodies, the other by denying the very existence of minds—and so end up equally unattractive.

Of course, the idealist “solution” of maintaining that physical objects don't really exist raises the issue of why one should stop there; the natural next step would be to deny the existence of other minds, too, so that one ends up with the view that *only one's own mind* exists (solipsism). One can no more *directly* experience what's going on inside of other people's minds than one can directly experience physical objects,<sup>22</sup> so if being unavailable to direct experience is good grounds to doubt the existence of physical objects, it's just as good grounds to doubt the existence of other minds. Consistency would suggest doubting both or neither.

Now I would reject solipsism for the simple reason that, were I to find myself living in a mental universe of my own making, I would expect the world to be a whole lot friendlier to ensuring that my needs are fulfilled (and perhaps be less full of surprises) than it actually is. The road that idealism takes us down is one that few people are willing to follow—for good reason—so the absence of its consideration in a volume like ours is hardly surprising or any real deficiency. Insofar as both idealism and realism (the view that physical objects exist)<sup>23</sup> posit an external world of “objects” that exist independently of one's own mind alone, both would seem to be on a par, so there is no reason to favor idealism over default common sense even though one *could* do so. Whether you experience an actual physical environment or merely a “*Matrix*” simulation of one, either way something *external to you* causes your perceptions. Almost everyone accepts realism about the physical world by default, and pretty much every philosophical text about issues other than fundamental metaphysics assumes realism in the background as a starting point—so I don't see why doing so would be any more objectionable here than elsewhere.

Suffice it to say, I don't believe in any “deep background consciousness” (JM, p. 200) and survivalists need not believe in one, either. Nor does it necessarily help, since one's *individual* consciousness could just as easily cease to exist at biological death even if the deep background consciousness persists: does a rain drop persist once it merges with the ocean? Perhaps in some generic sense, but not *as an individual* anymore.

<sup>22</sup> The view that we *directly* experience only our own *mental representations* of physical objects (representational realism), rather than directly experiencing physical objects themselves, makes sense of the difference between genuine perceptions and waking hallucinations: Sometimes our mental perceptions represent physical objects “out there,” and sometimes they do not.

<sup>23</sup> Realism, contra Matlock, is the true contrast to idealism, *not* materialism. For realism affirms what idealism denies, namely that physical objects exist. And idealism affirms what realism denies, that physical objects do not exist. Materialism is a stronger view, one that maintains that *only* physical things exist, which is denied by more than one position. Platonic realism, for example, maintains that abstract objects such as numbers also exist. And substance dualism maintains that mental substances (pure minds) exist in addition to physical ones (like brains). These alternatives to materialism are compatible with realism.

Finally, starting from an idealist theory of mind, Matlock suggests that “If mind and body are not so different in their composition it is easier to understand how they interact” (JM, p. 200), which is obviously equally good reason to start from one or other materialist theory of mind. Since he mentions this possibility as a contrast to substance dualism, however, it is worth noting that virtually every theory of mind—any that distinguishes the mental from the physical on some level or other—faces difficulties with mental causation (Chalmers, 1996, pp. 172–209; Howell, 2015; Kim, 1993b). (Reductionist materialism, eliminativism, and idealism are the exceptions, but they avoid the problem “on the cheap” by simply rejecting the mental/physical distinction in the first place.)

But Cartesian dualism has *deeper* problems with mental causation than its “materialist” alternatives, for it *supplements* their problems with its own, namely requiring apparently inconceivable causal contact between completely nonspatial minds and spatial brains, ostensibly requiring violations of physical law, and failing to deliver on interactive traces that should turn up if it’s true, to say nothing of the difficulties facing any disembodiment of Cartesian minds after the death of the brain (pp. 5–7). Thus, here Cartesian dualism is at a disadvantage relative to “materialist” alternatives, rather than being on a par with them:

Imagine (with Eccles) that “psychons” in the nonphysical mind push around physical processes in the brain, and that psychons are the seat of experience. We can tell a story about the causal relations between psychons and physical processes, and a story about the causal dynamics among psychons, without ever invoking the fact that psychons have phenomenal properties. Just as with physical processes, we can imagine subtracting the *phenomenal* properties of psychons, yielding a situation in which the causal dynamics are isomorphic. It follows that the fact that psychons are the seat of experience plays no essential role in a causal explanation, and that even on this picture experience is explanatorily irrelevant.

Some might object that psychons (or ectoplasm, or whatever) are entirely *constituted* by their phenomenal properties. Even so, there is a sense in which their phenomenal properties are irrelevant to the explanation of behavior; it is only their relational properties that matter in the story about causal dynamics.... Indeed, nothing especially is gained by moving away from the causal closure of the physical. We still have a broader causal network that is closed, and it remains the case that the phenomenal nature of entities in the network is explanatorily superfluous. (Chalmers, 1996, p. 158)

In short, Cartesian dualism does not escape the mental causation problems that beset property dualism or Russellian monism, while simultaneously generating additional problems of its own. And as you can see in the quotation above, the shared problems that remain for the “materialist” alternatives are only conceptual, meaning that they are generated by combining certain assumptions (Kim, 2011, pp. 385–391), any one of which might be jettisoned (or supplemented with other ones) to solve the problem. These alternative theories of mind do not generate the *empirical* conflicts with physics and observation that Cartesian dualism produces. Moreover, the conceptual problems with “materialist” mental causation would seem equally applicable to something like geological causation: no *distinctively geological* properties of fault lines seem to have any causal impact over and above the impact already made by fault lines’ more basic physical properties, and yet we find it unproblematic to talk about the causal contribution of a tectonic plate’s geological properties to the severity of earthquakes.

### Theological Critiques Do Not Assess Survival Research

As the Preface and Introduction make abundantly clear, the purpose of the contributions to Part III is to examine “inconsistencies between principal theological conceptions of an afterlife and widely held and theologically central ethical principles” (p. xxx; cf. p. 11). Consequently, to portray Ingrid Hansen Smythe’s chapter on karma and rebirth as intended to “undermine the idea of reincarnation by linking it to karma” (JM, p. 197) *widely* misses the mark. Granted, “reincarnation does not entail karma” (JM, p.

197)—but no one here ever claimed otherwise. Smythe’s critique makes clear at the outset that her target is only internal inconsistencies within what the Indologist Potter (1987, pp. 109–110) has characterized as the common core “classical karma theory of India” (CKTI), noting that “it is surely impossible to analyze every aberration [from CKTI] in one paper” (*MoA*, p. 494n1). Since Hinduism is the oldest *major* world religion still practiced that affirms rebirth, I think that the relevance of Smythe’s critique to the stated goals of Part III is rather obvious. To complain that “past-life memory case studies have found no sign of karma in the retributive (or juridical) sense that Smythe analyzes” (JM, p. 197) thus misconceives the purpose of the selections in Part III. Matlock is absolutely right that her chapter has “no bearing on the empirical question of whether reincarnation occurs” (JM, p. 197)—nor should he expect otherwise. He could have just as well complained that Raymond D. Bradley’s chapter on whether a morally perfect being could in principle send people to Hell failed to engage research on “hellish” NDEs, as if such research were relevant to answering that conceptual question. Since Smythe has prepared a separate response, I refer readers to it after the current one.

### How Much Survival Research Ought One Critique?

Finally, I turn to what I imagine interests readers here the most, the ostensible evidence for personal survival provided by psychical research. I need to preface what I have to say about it with a few preliminary remarks about superpsi, however, since superpsi has the potential to deflate the significance of any such evidence.

Matlock is certainly free to conceive of “superpsi” differently than I do. I have no argument with his point that the label can be understood in more than one way. But I should note that the more pertinent point that I was raising in the Introduction still holds: Proposing explanations that invoke an unlimited kind of psi raises falsifiability issues that make unlimited psi explanations *ad hoc*, as it seems that any *conceivable* survival evidence could *always* be explained instead in terms of the unlimited psi abilities of living persons. An unlimited psi hypothesis that is compatible with every possible outcome doesn’t really explain any particular outcome since it is *guaranteed* that it will not contradict one’s observations even before making any. The reason that the possibility of unlimited psi makes even limited psi explanations problematic is that whenever limits are put on psi, the objection is invariably raised that we do not know that psi is subject to such limits, and so cannot rule out that a more extensive psi is at play. The end result is that there is never any particular evidence that *cannot* be “explained” in terms of an unlimited psi, rendering it unfalsifiable by any evidence that could be *imagined*.

In order to have evidence for personal survival, living persons would have to have no way of knowing certain things or of causing certain physical effects, so that by process of elimination we could say that information only known to the deceased, or purposive physical effects that living persons are not capable of producing, must be provided by the deceased. In other words, the dead would have to be able to know things or do things that living persons are not in a position to know or do. If the living are capable of knowing or doing *everything* that the dead could know or do, then it’s hard to see how we could ever have evidence that it is the dead (rather than the living) that come to learn or do some inexplicable thing.

With that caveat aside, let us turn to the state of the survival evidence itself, rather than problems with its interpretation that can be charitably sidestepped. Matlock writes that the Part IV chapters “do little to counter the evidence for survival,” particularly “the decades of work with trance mediums” and the “complex and varied” (JM, p. 199) ostensible evidence for reincarnation. It’s true that, outside of the Introduction, there is no discussion of the historical trance mediumship evidence, and that the chapters on reincarnation at best only give a taste of some of the problems confronting cases of the reincarnation type.

Nevertheless, this evidence is hardly ignored: A concise but wide ranging overview of the toughest difficulties that face taking each chief source of “survival evidence” to be *actual evidence of personal survival*—or indeed of anything genuinely paranormal—is provided in the Introduction (pp. 20–31), in which most of the features of this evidence that *conflict with* giving it a survivalist or paranormal interpretation were originally noted by psychical researchers themselves. And given that a systematic assessment of the



evidential value of each of these sources could fill a book in and of itself, I warn readers that “the part IV selections only appraise particularly telling features of the most evidential kinds of survival evidence” (p. 20). This is why I excluded my own assessment of specific cases of alleged veridical paranormal perception during NDEs (Augustine, 2007a), though I provide a brief update on the Pam Reynolds case in an endnote of the NDE chapter (*MoA*, pp. 559–560n1).

It is regrettable that before I completed the manuscript, I was never able to find a qualified contributor willing to summarize and critique the evidentially salient results of historical studies of apparitions (Gauld, 1982, pp. 230–260) or trance mediumship (Gauld, 1982, pp. 32–118; Sudduth, 2016, pp. 72–104), or more recently, cases of the reincarnation type (Matlock, 1990; Sudduth, 2016, pp. 105–133). Had I then been aware of Moore’s (1981, pp. 82–191) detailed negative appraisal of the chief survival evidence, I first would have invited Moore to contribute either a penultimate summary and critique of the overall historical survival evidence to complement Lester’s more contemporary summary, or else a chapter on the evidential significance of the classic mediumship studies alone. Had he been unavailable, I would have sought to reprint Moore’s (1981) multichapter evidential assessment of historical mediumship research as a single chapter with headings on Mrs. Piper and Mrs. Leonard (pp. 82–101), the cross-correspondences (pp. 102–114), and drop-in communicator cases (pp. 115–126). And had I been unable to get permission for that, I certainly would have cited his work wherever relevant. What the volume lacks with respect to evaluating the evidence from trance mediumship and reincarnation cases should be supplemented with Moore’s obscure work (that, as far as I can ascertain, no parapsychology journal ever reviewed—all the more reason to have included it).

Ideally, a volume like ours would include an overview and critical assessment of the *evidential* strengths and weaknesses of the key anecdotal cases from each of the five main sources of survival evidence—reports of sightings of apparitions of the dead, OBEs, NDEs, spontaneous “past-life memories,” and mediumistic “communications”—as well as summaries of attempts to gather whatever experimental evidence of veridicality one can get from these sources. Sudduth (2016, pp. 47–133) concisely provides an overview of key anecdotal cases for all of these save apparitions of the dead. Second best would be a critical assessment of the three cases (for each of the five sources) most touted by survival researchers for their evidential features, where copious details are available for critique (because these details were initially gathered in the first place, and because researchers made them fully publicly available), again coupled with an overview of any relevant experimental evidence. Third best would mostly be just an overview of the results of attempts to gather *hard experimental data* supporting postmortem survival or mind-body separation, such as direct tests of survival—attempts to detect a “double” during OBEs, attempts to identify visual targets during OBEs or NDEs, or assurances that one will attempt to pass along an encoded message to the living after one’s death if one is able.

Given that my options were limited, I settled on at least ensuring that the third best sort of critique was provided in the volume,<sup>24</sup> omitting a discussion of the experimental apparition detection studies that have been attempted for reasons that I will explain shortly. And of course I included Angel’s call for doing what little experimental work could be done for cases of the reincarnation type, namely performing experiments to determine whether the correspondences between individuals found in “solved” cases defy what we would expect to find by chance alone (Angel, 2008). Suffice it to say that the results of experiments designed to produce the most decisive evidence for personal survival that one *could* have—direct tests of survival—have not supported survivalist assumptions (*MoA*, pp. 218–223, 522–525).

As an editor of a multicontributor volume, one’s choice of coverage is constrained by at least two things: the willingness/availability of qualified authors to contribute something on a particular topic, and the accessibility of the data that needs to be critiqued. There is a common assumption in parapsychological circles that if an argument goes un rebutted, that is because no credible rebuttal is possible. More often than

<sup>24</sup> Of course, had we met the more manageable second best ideal that even Moore (1981) does not meet, many survivalists would invariably complain that even *had* we demonstrated that the selected cases, such as the now discredited Chaffin will case, fall short of providing convincing evidence for survival (Charman, 2013), we nevertheless neglected to consider *other* evidentially strong cases, such as the Cheltenham Ghost case (Lambert, 1958). Any *limited* assessment of the survival evidence is bound to leave some survivalists unsatisfied.

not, however, work like Beischel's laboratory mediumship research escapes critique because those with the qualifications to evaluate it have other academic priorities, and her research is simply not on their radar. I think that it is safe to say that there has been little scholarly interest in making the mortalist case<sup>25</sup> in general given the paucity of books that actually attempt to do so—not because one *could not* make such a case, but because few are interested enough to put in the work to do so. This is why the publication of our volume has been characterized as “a noteworthy publishing event” (Flynn, 2015, p. 14) where there was “clearly a niche waiting to be filled” (Dieguez, 2016, p. 60).

Once you beat the odds and find contributors willing to critically assess survival research for a mortalist work, however, your contributors are themselves constrained by how much data survival researchers make publicly available. If we couldn't even get permission to publish an old exchange between Stevenson and one of his research assistants on the *significance* of his data, for example, how could we ever get Stevenson's successors to share or make public their more recent full unpublished case files? (This is not a rhetorical question; we tried to obtain some.) What's a contributor to do when even published material lacks essential details about who said what, who (if anyone) can corroborate what, and so on? If there's not enough information to evaluate, then there's not much that one's contributors *can* say:

One of the most critical aspects of science is to report all of the data that you've collected. Beischel and Schwartz don't do that in their triple-blind paper. Although they collected item-by-item scores for their study, they never presented this data. Ironically, this is the best data that they have to analyze.... [I]t's considerably more objective, and it's a real shame that it's not included in the triple-blind paper.... Though in the 2007 triple-blind paper Beischel and Schwartz stated their intention to publish the results in the future, it is now 2014 and there [is] no sign of them. (*MoA*, p. 623)

Many of this journal's readers will undoubtedly see such explanations as excuses; be that as it may. But to protest that the Part IV contributions don't cover as much as one might wish<sup>26</sup> is not to deny the value of what they *do* cover, which is actually quite a lot. The volume *at least* represents a good start in this respect, and few (of the already rare) mortalist works canvass nearly as much of the survival evidence as we do (Blackley, 1986; Flew, 1987; Haynes, 1913; Lamont, 1935/1990; Lester, 2005; Moore, 1981; Musolino, 2015; Woerlee, 2013). There was no sense in making the perfect the enemy of the good when bringing a project of this scope to completion.

Outside of the Introduction (pp. 20–22), a discussion of a reciprocal apparition case (pp. 521–522), a small section of Lester's overview (pp. 633–634), and where apparitions occasionally figure in poltergeist research (p. 504), Matlock is correct that sightings of apparitions during the waking state “receive only passing mention” (JM, p. 199). The main reason for this is that I simply could not find a contributor willing to critique the historical apparition research for the volume. In addition, a discussion of inconclusive experimental attempts to detect the presence of “apparitions” (Maher, 1999, 2000; Maher & Hansen, 1992, 1995, 1997; Maher & Schmeidler, 1975; Moss & Schmeidler, 1968; Schmeidler, 1966) was ultimately cut. This discussion was excluded because, at best, any consistent “detection” over time by observers or instruments at a particular location might be nothing more than the result of common instinctive or socially conditioned expectations about what constitutes an eerie location, or else be due to sensitivities to drafts, pressure changes, contaminants, static electricity, infrasound, artificial electro-

<sup>25</sup> Despite widespread scholarly interest in exploring the *implications* of accepting mortalism (e.g., Benatar, 2016; Fischer, 1993; Kagan, 2012, pp. 205–233; Kamm, 1998, pp. 13–71; Metz, in press; Perry, 2014; Warren, 2004).

<sup>26</sup> Should readers suspect that this was intentional, note that many planned selections for Part I had to be abandoned because of a lack of interest on the part of potential contributors (such as a long-sought-after selection on the physiology of memory responsive to survivalists' claims—such as Clarke, 2015, pp. 57–65), or because of the failure of slated contributors to follow through on writing them. The bulk of the reprints in Part II similarly filled gaps that I could not get original contributions to fill. And since contributors are obviously motivated to write about what they care about most, I certainly cannot compel them to discuss every issue that I might like them to tackle.

magnetic radiation, or naturally occurring geomagnetic radiation—in short, a detection of anything but an apparition (Stokes, 1997, pp. 175–176).

Matlock poorly characterizes encounters with deceased persons during NDEs as apparitions. But unlike NDEs, apparition sightings occur during the normal waking state when one is ostensibly still “in the body,” so the conflation is dubious and presumes a survivalist interpretation from the start. More importantly, however, his claim that NDE encounters with deceased persons “are not discussed anywhere in *MoA*” (JM, p. 199) is demonstrably false: the NDE chapter includes a general discussion of such cases followed by comments on the evidentially more interesting Peak-in-Darien cases (pp. 556–557).

Matlock also claims that for the most part, “poltergeists have nothing to do with survival but rather concern the psychokinesis of living persons” (JM, p. 199). Although this does seem to be the prevailing interpretation of psychical researchers, it is not an uncontroversial one—Irwin and Maher, at least, find the reasons for this preference to be rather questionable and recommend leaving the interpretation of such cases open (Irwin, 1999, pp. 194–198; Maher, 2016, pp. 329–330). Moreover, pioneering poltergeist-experience researcher Roll (1977) analyzed both historic and modern “poltergeist” cases and found that many unequivocally involved fraud, and Houran’s (1997) review concluded that many so-called phenomena associated with such cases can have a wide variety of conventional etiologies. Thus, Matlock’s assertion that psychokinesis rather than survival has been established and accepted as the correct explanation of most cases is dubious. Houran and Lange obviously think at this time, based on the available evidence, that most cases are best explained as misperceptions of ambiguous stimuli rather than in terms of either discarnate intelligences or recurrent spontaneous psychokinesis (pp. 505–506).

Matlock is of course correct that OBEs and NDEs can at best only “show that the mind can exist apart from the body and an incapacitated brain” (JM, p. 199), but even just establishing *that* would be quite a discovery. The most compelling evidence militating against personal survival is evidence that *prima facie* shows that mental activity cannot persist in the absence of a functioning brain. Strong evidence to the contrary would thus seem indispensable to making a survivalist case, even if it did not demonstrate *postmortem* survival *per se* (p. 218).

Outside of parapsychology, I suspect that few people would be impressed with Matlock’s suggestion that disembodied perceptions might be “processed by the subliminal mind, whence the distortions” (JM, p. 198), an attempt to explain away discrepancies between what out-of-body NDErs report seeing and what’s actually happening in the physical world, particularly *invented* rather than misperceived details or mere omissions (see p. 22 for examples), encounters with fictional or still-living persons while ostensibly glimpsing an afterlife realm, and the failure of NDE prophecies to accurately forecast future events on Earth. Certainly one can conjure up an almost infinite number of alternative explanations for such cases, as one can do in nearly any attempt to neutralize contrary evidence. Perhaps when we dream at night our subconscious mind also distorts our perceptions of an *objective* world that we all visit in our dreams, where we encounter actual persons instead of just dream characters. This parallel possibility is, of course, *compatible* with our dream experiences, but its bare possibility in no way makes it *plausible*. Outlandish counterexplanations for ostensibly hallucinatory NDEs are no less implausible given the likelihood that some NDEs *look like* hallucinations because that is what they *are*.

On the wide cross-cultural diversity between NDE accounts, Matlock writes that Lester and I “give no reason for expecting” that NDEs “should be the same for all experiencers, everywhere” (JM, p. 199). This is of course an exaggeration of what we say, but it’s not true that we give no reason to expect, were a survivalist interpretation of NDEs true, that there should be substantial uniformity among minimally contaminated NDE reports from different cultures. First, such uniformity would bolster survivalist interpretations if present, for it would be *surprising* to find it if NDEs were hallucinations (despite arguments that we might all be hardwired to hallucinate the same way when the brain is dying, or at least when we think it is). Second, the wide variation actually found in reported NDE content across cultures is surprising on the assumption that something literally leaves the body and travels elsewhere. For in that case we would *prima facie* expect virtually every NDE to include an OBE component, when in fact only about a quarter of NDEs just within the West even include OBEs (van Lommel, van Wees, Meyers, & Elfferich, 2001, p. 2041, Table

2).<sup>27</sup> We might also anticipate that the *transition* that NDErs report making from “this world” to “the next one” would essentially be the same across NDErs, such as proceeding through a tunnel or darkness toward a light; but in fact, the Western tunnel-and-light motif is quite rare in NDE reports elsewhere. And we certainly would not expect, even limiting our consideration just to Western NDEs, the wide variation reported in the “astral forms” and novel “abilities” of different OBErs and NDErs during their experiences if some part of them literally left their bodies (pp. 23, 549–550).

Matlock goes on to suggest that counterexplanations are similarly available for cross-cultural differences among cases of the reincarnation type, but I conceded this at the outset when offering “underreporting” or “investigative focus” as possible alternative explanations (p. 26). Here again, that a “discarnate mind’s deep-seated beliefs ... can influence its choices about where and when to reincarnate” (JM, p. 202) is undoubtedly a *possible* alternative to “parents guiding their children in accord with their culturally mandated belief systems” (JM, p. 229), but surely it is a more outlandish alternative to the simple explanation that culture itself is what generates reports of “past life” memories—an explanation that does not even require us to posit discarnate minds within the furniture of the universe to begin with.

On our condensed version of the now famous Ransom report, Matlock concedes that Ransom raises significant concerns about Stevenson’s investigative methods, though he wrongly denies that they really concern his methods at all rather than just “the way the write-up was handled” (JM, p. 198). (Ransom himself says: “The way the write-up was handled is the reader’s only way to assess how the research was done. If Stevenson’s actual research was done in a more thorough and cautious way, why would he have failed to write it up that way?”; C. Ransom, personal communication, September 15, 2016.) And it’s hardly irrelevant that Stevenson obscured weaknesses when presenting *particular cases* by merely noting their presence in a general way in introductory sections of his works. Matlock then proceeds to wave away these concerns by pointing out that they were earlier “acknowledged and addressed by Stevenson” (JM, p. 198). This substantially misses the point of Ransom’s critique: Acknowledging and addressing a shortcoming is *not* equivalent to *eliminating* it, and some of his concerns may not even be eliminable given the testimonial nature of the evidence that Stevenson collected. Matlock also wrongly claims that Ransom never accompanied Stevenson into the field, but in fact Ransom observed Stevenson’s interviewing techniques on at least three separate occasions (once in both Juneau and Anchorage, Alaska, and twice in the American South in two different states; C. Ransom, personal communication, November 6, 2016). Suffice it to say that Ransom is not the only person who worked with Stevenson to find Stevenson’s methodology wanting.

Although it is true that the two chapters on mediumship are (largely) limited to assessing the laboratory mediumship research of Gary Schwartz and Beischel, the joint chapter by Battista, Gauvrit, and LeBel nevertheless refers readers to the rich and inconclusive history of this research and its criticisms at the outset (pp. 615–616). (I will leave it to Claus Flodin Larsen to respond to Matlock’s comments on his chapter in his separate response.)

More importantly, the joint chapter proceeds to do readers a service by contributing to this literature the then only rejoinder to Beischel and Gary Schwartz’s (2007) most touted study for which there was sufficient data to perform an evaluation. The newly recognized methodological problems noted in the chapter go far beyond what Matlock characterizes as merely those concerned with “supplying mediums with the first name of the discarnate ... which could [then] provide information for the start of a cold reading” (JM, p. 199). In fact, the contributors canvass how Beischel and Schwartz use two different ways to describe the same data in order to overstate the force of their results, their use of statistically invalid analyses and concepts that render their results “statistically meaningless,” their failure to disclose the only statistically meaningful data that they have, their use of procedures prone to “inflate the rate of false positives,” the openness of their experimental design to merely “collecting data until positive results emerged,” and how optimizing the differences between sitters’ actual readings and their control readings “essentially rigged the experiment to produce the result that they wanted” (pp. 619–625). While Matlock believes that the statistical flaws present in their triple-blind study “appear to be corrected in a follow-up quintuple-blind study,” there is no

<sup>27</sup> Of course, one can always posit that NDErs are amnesic about having left their bodies, but as Marsh (2010, p. 60) points out, that possibility would seem to be in tension with NDErs’ insistence that “separation from the body ... markedly heightens subjects’ perceptiveness.”

way for anyone to know since “the details of its implementation have never been published” (p. 616). The chapter ends on a constructive note by offering recommendations on how to get the experimental design right, as it were, when doing mediumship research using the small sample sizes typical of it (essentially by modeling it on memory research conducted on amnesia patients). Following these recommendations would eliminate problems that afflict psychological research in general, not just parapsychological research.

Matlock closes his review on the *promissory* note that “the reincarnation case data that have been amassed over the last 50 years will bring about a major revolution in our biological and psychological sciences.... [and thus] Martin and Augustine, et al., are fighting a losing battle” (JM, p. 203). That, of course, remains to be seen. But it is worth pointing out that the sort of tried-and-true data canvassed in *MoA* are themselves just the accumulation of discoveries that have *already* prompted a number of previous scientific revolutions. In this sense they have already earned their keep, unlike purported evidence for reincarnation. To the extent that bold suppositions about reincarnation grounded in questionable testimonial evidence require us to reject a mountain of well-established scientific knowledge, perhaps Matlock would be better off hedging his bets.

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