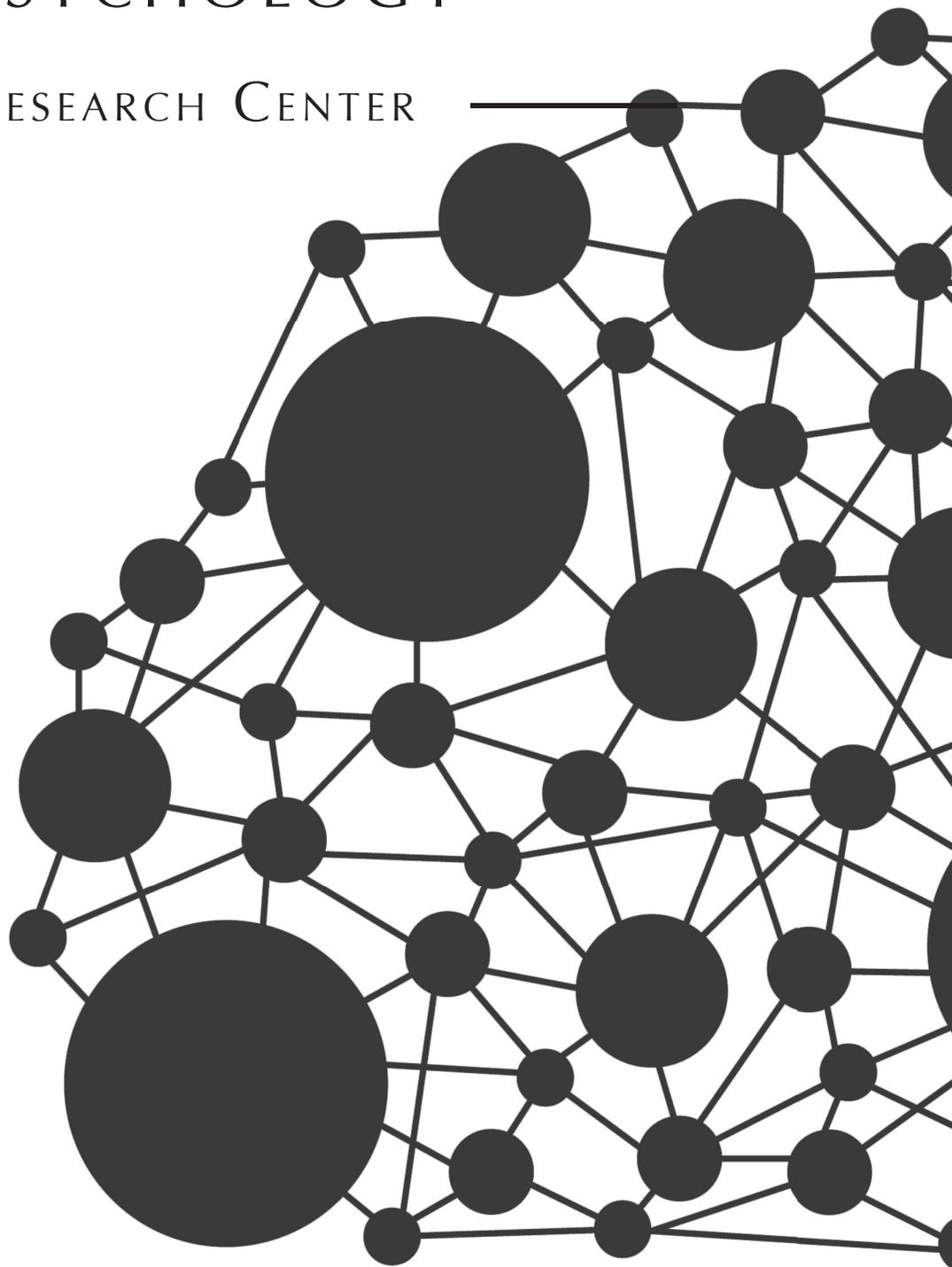


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EDITORIAL

STATISTICAL ISSUES IN PARAPSYCHOLOGY: HYPOTHESIS TESTING—PLUS AN ADDENDUM ON BIERMAN ET AL. (2016)

By John Palmer

Hypothesis Testing

In a previous editorial I described and defended my heretical views on how multiple analyses of empirical results should be addressed (Palmer, 2013). In this editorial, I express and defend equally heretical views on hypothesis testing. The issue of how hypotheses should be evaluated statistically is important for two reasons. First, confirmation of a hypothesis goes beyond confirmation of the effect itself because it supports, or at least should support, a theory or model. Second, a more lenient criterion of statistical significance is commonly applied to hypothesized effects than to other effects, which are often labeled “post hoc.” I have heretical proposals regarding both of these observations.

A tagline for my first proposal is that what is important is not whether an outcome is *hypothesized* but whether it is *hypothesizeable*. There are two key circumstances where there is a mismatch between the two. The first is: *An outcome is hypothesized that should not have been hypothesized (i.e., is not hypothesizeable)*. The fact that hypothesis tests are supposed to be tests of a theory or model implies that the author has an obligation to show how the hypothesis follows from the theory and/or previous empirical results related to the theory. In fact, this is one of the major purposes of the introduction section of a research report. This prescription is expressed as follows in the Publication Manual of the American Psychological Association (2010): “In empirical studies, [explaining your approach to solving a problem] usually involves stating your hypotheses or specific questions and *describing how these are logically connected to previous data and argumentation*” (p. 28; my emphasis). The word “argumentation” leads me to point out that the theory or model need not meet the formal requirements of such; any coherent and plausible conceptual scheme that fulfills this role should suffice. On the other hand, “hypotheses” that are ad hoc or based on hunches should simply be outlawed.

The second circumstance is the converse of the first: *The outcome is not hypothesized but should have been hypothesized (i.e., is hypothesizeable)*. I am sure that most researchers have had the experience of trying to interpret a significant post hoc effect and in the process of doing so realize that there was a sound basis for hypothesizing the effect. (The hypothesis would be the generalized form of a prediction of the effect.) However, I am equally sure that most parapsychologists would not retrospectively change the status of the effect from post hoc to hypothesized (and reap the rewards of doing so) because it looks like cheating. This is a powerful illusion, but an illusion nonetheless. As I noted above, the purpose of a hypothesis test is to provide evidence for or against a theory or model, but to fulfill that role, and for the purported hypothesis to legitimately be designated as such, the relevance of the hypothesis to the theory must have been established. If a proposition meets this test, a full interpretation of the effect requires that it be identified as a hypothesis; otherwise, the support that the confirmation of the hypothesis provides for the theory is obscured. Of course, it is the responsibility of the researcher to justify the reclassification in the Discussion section of the report, and referees can decide whether the author has succeeded. On the other hand, the argument against reclassification is based on the premise that a hypothesizeable effect should only be hypothesized if the researcher was astute enough to recognize that it was hypothesizeable before the study was conducted. This is clearly nonsensical. So my first heretical proposition is that demonstrably hypothesizeable post hoc effects not only can be, but should be, retrospectively reclassified as hypothesized if the researcher becomes aware of its hypothesizeability.

The practical consequences of adhering to my first heretical proposal is markedly reduced by adherence to my second. A major reason why a researcher would want a proposition to be classified as a hypothesis is that the criteria that the prediction(s) derived from it must meet for statistical significance to be claimed are more generous. There are generally two such criteria: (a) a one-tailed rather than a two-tailed significance test, and (b) waiving of the requirement for replication or a multiple-analysis correction. My proposal is that the significance criteria for a hypothesis test should be the same as for a post hoc test, namely, a two-tailed test and a multiple analysis correction or replication.

I have two arguments for my proposal. First, to confirm a hypothesis, a significant effect must be shown to be “real,” and this latter determination should be made irrespective of whether the higher-level proposition is classified as a hypothesis. To do otherwise is to assume what is at issue: that is, that classification of the proposition as a hypothesis is proper.

My second argument is similar to my objections to the use of Bayesian statistics, which I presented in a previous editorial (Palmer, 2011). What Bayesian statistics essentially does is to allow a more liberal criterion to be applied in assessing whether an effect is real if it can be shown that the effect has a high a priori probability of being real. In practice, the a priori probability is usually at least partly determined by whether the overarching theory or hypothesis is consistent with the “established” theory of relevance. Of course, this is the very standard that psi doesn’t meet, and, as I argued in the editorial, there are solid grounds for maintaining that a priori probabilities, including those based on theory, should have no influence whatsoever on how we determine the reality of an effect, and therefore, we shouldn’t use analysis methods that presuppose that the influence should be something other than zero. The case of hypothesis testing is similar, in that an effect is given an easier path to confirmation if the confirmation is consistent with the theory that the hypothesis is derived from.

The one exception to my proscription of one-tailed tests is when the “hypothesis” to be tested is a replication of a previous finding. The reason is that a significant effect in the opposite direction cancels out the original result leading to the conclusion that the effect is not real, the same conclusion one would draw if the replication outcome were nonsignificant. This is not the case for other hypothesis tests. I also maintain that replications are the only hypothesis tests that should be considered “confirmatory” in the sense this term is used by Kennedy (2016).

One-tailed tests are also appropriate for meta-analyses, at least insofar as they can be construed as a test of the replicability of a previous finding or set of findings, which I think is almost always the case in practice. I agree with Kennedy (2016) that they should be “prospective.” A particularly important question in this connection is how close the methodology of the replication needs to be to that of the original study to qualify for inclusion in the meta-analysis, and how uniform in methodology the original studies need to be for them to qualify as targets. I take a more liberal view on this matter than I believe Kennedy does, but I don’t have an argument for this preference. However, evidence from the meta-analysis of Bem, Palmer, and Broughton (2001) that only studies that closely followed the methodology of the PRL autoganzfeld series were collectively significant suggests that a more conservative approach might be a better tactic. Finally, I should note my reservations with using the Stouffer Z as a measure of replication (Palmer, 2013).

Bierman et al. (2016)

Bierman chose not to reply to my editorial in the last issue of the *JP* (Palmer, 2016) with a Letter to the Editor but he did reply to me privately (D. Bierman, personal communication, August 4, 2016). He makes some dubious statements in that letter that suggest to me I need to expand on a main point of the editorial, especially because he is likely to circulate these statements privately.

The key point in Bierman’s letter to me is that Bierman, Spottiswoode, and Bijl (*BSB*; 2016) were not making any claims about whether QRPs in fact existed in the database. Instead the purpose of the analysis was to say what the effect on the significance of the database would be if one were to assume a certain percentage of QRP studies in the database. As he expressed it: “Our conclusion was that assuming that parapsychologists did behave like main stream colleagues we could ‘explain’ a large fraction of the effect

size reported in that particular meta-analysis. Our conclusion was not a) the parapsychologists are as bad as the main stream experimenters (that was an assumption); b) experimenter X used QRP Y.” In other words, it’s all hypothetical.

If that’s all it is, the whole exercise was a monumental waste of time (and journal space), but the whole point of my editorial was to demonstrate why this is not in fact the case. I would like to add a few additional observations. First, Bierman’s assertion that his conclusion is strictly hypothetical is clearly refuted in the abstract of the BSB paper: “We conclude that the very significant probability cited by the Ganzfeld meta-analysis is likely inflated by QRPs, though results are still significant ($p = 0.003$) with QRPs” (Bierman et al., 2016). This does not describe a hypothetical, “what if” situation. Albeit it refers to a likelihood, but the likelihood is of something real.

Second, it’s very telling that in their article BSB never explicitly deny insinuating that there actually were a nontrivial numbers of fraudulent QRPs in the database and that QRPs were committed in particular studies. They would have to be imbeciles not to recognize that such inferences by the reader are possible (even likely), and given the seriousness of the potential charges, if their motives really were benign they would have bent over backwards to make this denial clear to the reader.

Bierman’s claim in his letter that “Our conclusion was not . . . experimenter X used QRP Y” is also problematic. It is explicit in their subsection on fraud that two particular studies were chosen for the QRP designation, to the point that with the aid of their disappearing supplement file I was able to identify who the author was. All a reader has to do to identify which authors committed a misclassification QRP (e.g., optional stopping or extension) is check the ganzfeld literature for studies with nonround N s. Of course, Bierman is correct that none of these attributions were “conclusions.” They instead were insinuations, and as I noted in the previous editorial, in my mind insinuations are worse than flat-out allegations. Indeed, this was the basis of my comparing the BSB article to the writings of Hansel.

It is important to recognize that the fraud insinuations do not generally appear in the description of the meta-analysis per se or of its results. For instance, the unjustified inference of misclassification from a nonround sample size informed only the estimate of the proportion of studies in which that QRP occurred. BSB try (unsuccessfully) to justify this inference by pointing out that that the percentage of nonround studies in the ganzfeld database is similar to the estimate obtained by John, Lowenstein, and Prelec (2012) for misclassification QRPs in psychology experiments.

Finally, it should be noted that the BSB paper was published in a psychology journal rather than a parapsychology journal, which means that its target audience was mainstream psychologists. I don’t need a crystal ball to tell me that these readers reached the conclusion expressed in the abstract, which I suspect is as far as many of them (and especially the media) went.

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AS IT OCCURRED TO ME: LESSONS LEARNED IN RESEARCHING PARAPSYCHOLOGICAL CLAIMS¹

By Chris A. Roe

Introduction

I have been involved in parapsychology for a little over 25 years (including my time as a PhD student), so in preparing this paper I've welcomed the opportunity to reflect on that experience to see if any of it is worth sharing with the wider community. In particular, I wondered if there were lessons I have learned the hard way that could be of benefit to early career researchers, so that their trajectory might be a little more smooth or fruitful than my own has been. I have selected some things that have occurred to me both in the sense of events that have happened and in the sense of realisations I have had based on those experiences. I hope they represent useful insights into what I regard as the art of scientific practice in parapsychology. They have shaped the kind of researcher I have become and perhaps explain some of my preoccupations and biases.

Early Beginnings

My first love is the scientific method. I first became interested in parapsychology when aged about 14, not through any powerful or vivid personal experience, or through a family history of paranormal beliefs or practices, but by discovering scientific research on the subject. In the UK a magazine was published called *The Unexplained* that promised to explore all sorts of fantastical phenomena, from alien visitations to spontaneous human combustion, and these unsurprisingly were very attractive to a young teenager with a fairly rich imagination but also a skeptical disposition. These tales were sobering in reminding us how much there is still to learn about nature and the phenomena that it permits. However, I quickly became disillusioned by a tendency for reports of investigations to end with the conclusion that the phenomenon remained a mystery, rather proudly declaring that things were inexplicable in terms of current scientific principles, and so cocking a snook at the hubris of the scientific mainstream by demonstrating that it didn't, in fact, know everything, but adding very little to what we did know. Surely demonstrating an anomaly was the *beginning* of the scientist's story rather than the end of it, indicating as it did the most potentially rewarding or revealing areas for future investigation? Thankfully, other articles in the magazine were more measured in tone, particularly, I recall, those highlighting Ed Cox's minilab experiments and Carl Sargent's ganzfeld studies at Cambridge,² and these adhered to more recognizably scientific methods that might be superficially less exciting, making slower progress and reporting only modest gains, but ultimately be more satisfying in providing trustworthy accounts of how things really are. And so, aged 14, I determined to become a parapsychologist—how difficult could that be?

Three years later, in 1984, I needed to decide to which universities I would apply. I had a meeting with my school careers adviser and mentioned my interest in parapsychology, and he remarked that he had just seen a press release about the appointment of a new Professor of Parapsychology at Edinburgh University. I can recognise an omen when I see one, and so it was that I applied to study psychology at Edinburgh. My original application to study for a Bachelor of Arts degree was returned to me, with the admissions tutor recommending that if I wanted to become a parapsychologist then I would be better to take a Bachelor of Science pathway, with its greater emphasis on training in experimental work. This was effectively a biological sciences degree with a major in psychology, so that I continued to study physics, chemistry and biology (my A-level subjects) alongside zoology and psychology.

¹A version of this paper was presented at the Parapsychological Association 35th annual convention, Boulder Colorado, June 19–24, 2016.

²See, for example, Cox (1984) and Sargent (1980), but also Hansen (1985).

The Sociology of Science

So it was that my education to that point was wholly in the natural sciences and had turned me into a philosophically naive positivist researcher—I had been taught that the scientific method was simply a process of disciplined observation that guarded against what Francis Bacon (1620/2015) called idols (of the Tribe, the Cave, the Marketplace, and the Theatre); that is, the ways in which we might deceive ourselves when making observations and when drawing inferences from those observations. From that perspective, nature might seem inscrutable, but it would give up its secrets with appropriate effort and diligence on my part. Science, I thought, is a process of increasing refinement and exactitude, particularly in measurement, and all meaningful properties of the world can be measured objectively and consistently if we are careful enough. I was soon to discover the limits of this philosophy when I came to conduct my own research.

I had a foretaste of things to come when I included in my undergraduate programme modules on the philosophy and sociology of science with members of Edinburgh's influential Science Studies Unit. Scholars such as Barry Barnes, Steve Shapin and David Bloor³ had a huge influence on me. I was introduced to the notion that even in the natural sciences scientific knowledge can be a function of its social and political time, that the scientific elite is inherently conservative and suppressive, and that, in Thomas Kuhn's (1970) terms, scientific practice is only rarely edging toward revolution and is more commonly engaged in "normal science" with its concomitant aggressive policing of the border between the legitimate and the illegitimate. The work of some of these philosophers and sociologists shows that, for most of the mainstream, parapsychology lies on the wrong side of that border, and is a victim of those processes (e.g., Collins, 1985; Collins & Pinch, 1983; Wallis, 1979).

This perspective informed my undergraduate dissertation project, which looked at how the quality ratings of the methodology of a parapsychology research paper (intended to simulate the journal review process) depends not on the described method itself but on whether the outcome and conclusions are congruent or incongruent with the assessor's own prior beliefs. In a classical instance of cognitive dissonance, participants were able to relieve tension brought about by being confronted with counter evidence by simply dismissing the evidence as invalid. I replicated the effect with students from St Andrews University and published the results in the *British Journal of Psychology* (Roe, 1999)—still my only publication in the flagship journal of the British Psychological Society.

This work drove home to me the extent to which people use their rational faculties for *post facto* justification rather than for genuine decision making, and it made the Jonathan Swift quotation⁴ "It is useless to attempt to reason a man out of a thing he was never reasoned into" one of my reference points. In my election statement for PA President I mentioned that I thought we spent too much time and energy engaging with sceptics whose reputations are too strongly associated with the counteradvocate position for there to be any realistic prospect of a shift in their public pronouncements, whatever the quality of methods or data we present. It is interesting to note, for example, that the substantive arguments offered by Ray Hyman (2010) and James Alcock (2010) in Krippner and Friedman's edited book, *Debating Psychic Experience*, are essentially the same as those they offered 20 years earlier in books such as *The Elusive Quarry* and *Science and Supernature*, respectively (Alcock, 1990; Hyman, 1989). That intransigence in the face of quite significant shifts in the methods and evidence base of parapsychology suggests—to me, at least—a preference for rhetoric over genuine engagement with the field. It seems that little has changed since Charles Honorton's (1993) damning critique of "the impoverished state of scepticism," which remains the most incisive criticism of the counteradvocate position.

I am not recommending that we ignore our sceptical colleagues but that we are clear in what we are aiming to achieve when we do respond to them. For example, in 2010 I wrote a rejoinder for *The Skeptic Magazine* to an article by Ray Hyman in which he asked the leading question "is Parapsychology dead or alive?" and I have appeared regularly with Chris French in the UK to discuss parapsychological claims before public audiences. One such event, a panel debate that also included Richard Wiseman on

³ For examples of their approach, see Barnes (1985), Barnes, Bloor, and Henry (1996), and Shapin (1996).

⁴ Attributed to Swift in *Scientific American*, Vol. 7 (Munn & Company, 1851), p. 338, but disputed and possibly apocryphal.

the apparent crisis in psychology and parapsychology at the UK skeptics' conference was attended by perhaps twice as many people as attended the joint meeting of the Parapsychological Association and Society for Scientific Exploration in 2016. My objective in each of these encounters was not, of course, to facilitate a shift in the sceptic's position, but to speak through them to a broader audience, some of whose opinions hopefully had not yet ossified, providing them with a more balanced picture of parapsychology's discoveries and emphasising that its practice was scientific "business as usual." Given the virulent way in which the Wikipedia entries on parapsychology are mismanaged, we have a difficult but important task ahead in ensuring that interested but discriminating members of the public can get access to accurate and balanced information about the state of the field.

Considering Normal Explanations

After my undergraduate degree I went on to study for a PhD at the Koestler Parapsychology Unit, and my research topic reflected the unit's emphasis on crossdisciplinary approaches, and on determining "what looks psychic but isn't" in order to better understand the processes that can lead to an attribution of paranormality. I explored the technique of cold reading by getting access to a set of arcane publications that represent how-to guides for would-be pseudopsychics and by spending time with a practitioner who had been working the circuit for over 30 years. Malcolm⁵ agreed to give readings to people he had never met before. The clients were asked to rate the accuracy of the readings and to make a judgement as to whether we should study him further, in more formal research. Two of the three sitters were very impressed by their readings and gave unequivocal recommendations. We videotaped the interactions and then recorded Malcolm as he watched the video and explained the stratagems he had been applying.

In analysing Malcolm's account and synthesising the descriptions from cold reading manuals, it became obvious to me that the technique was actually a set of techniques that varied according to how much information leakage was required from the client and how specific the reading material could be: the more leakage, the greater the specificity (see Roe, 1991; Roe & Roxburgh, 2013a, 2013b). This model argues strongly against the "heads I win, tails you lose" explanation offered by some sceptics, whereby unimpressive readings from mediums and psychics provide evidence that psi does not occur, whereas impressive readings from mediums and psychics are seen as evidence of the widespread use of cold reading and so also show that psi does not occur. The intention of the work was to show that we need to take into account the prevailing conditions when assessing whether communications of the specificity observed could be achieved through cold reading alone. It's interesting to note here that explanations in terms of cold reading make some assumptions about client behaviour, including their tendency to recall only the hits and forget the misses, and to elaborate on given material in ways that make the recalled version more specific to them; surprisingly, the only attempts to test these assumptions that I am aware of have been conducted by me (Roe, 1994)—rather than seeking experimental evidence for cold reading, sceptical researchers have been content to apply the method after the fact to given "real-world" data in a manner that would be scorned if done by a parapsychologist (e.g., Greasley, 2000; Underdown, 2003).

The Dynamic Interpersonal Nature of Research With a Sentient Subject and the Effect This Has on Outcome Consistency

As part of my PhD project I wanted to test whether general statements recommended by pseudopsychics were successful because they acted as Barnum statements (Roe, 1995). Barnum statements are statements that most people accept as true of themselves, but importantly do not recognise they are likely to be equally true for others, and so regard them as uniquely or especially pertinent. The classic Barnum statements were coined by psychologist Bertrand Forer in 1949 and were derived from a newsstand astrology book. They include items such as "At times you have serious doubts as to whether you have made the right decision or done the right thing," and "Disciplined and self-controlled outside, you tend to be worrisome and insecure inside." To assess whether the statements recommended by pseudopsychics work in a similar

⁵ A pseudonym used to protect his identity.

way I needed to use a standard clinical setting, similar to those used in hundreds of published tests of the Barnum effect, so I recruited participants to provide personal validation data on a new way of assessing results from the House Tree Person Test. This test unsurprisingly entails the participant being given a blank sheet of paper and a set of coloured pens and pencils and asked to draw a house, a tree, and a person. These drawings are passed on to an analyst who generates a personality description based on their interpretation of the images. This description is given to participants the following week so they can evaluate the analysis. Of course, everyone receives the same feedback, which consists of statements recommended in pseudopsychic manuals, and these show the same levels of acceptance as Barnum statements. Because the study contained an element of deception, I took great care during the debrief period to discuss the experimental design and to ask for their thoughts about participating; I was astonished to discover that quite a few of my participants *had been conducting their own experiments within my experiment!* They hadn't studied the House Tree Person Test before, but they suspected that certain features (such as large eyes, large people relative to the house size) might have particular meanings (such as a tendency to paranoia, or grandiosity) and so they included these elements to see if they would translate into their feedback. Many reported with satisfaction that they had—even though everyone received the same feedback, demonstrating people's capacity to see what they expect to see. More importantly for my thinking about research in general, and contrary to my experience of experimentation in the natural sciences, I realised that participants each created their own version of the experiment that I had designed. Any pretensions I had had to produce a consistent set of conditions across all the trials now seemed naively optimistic. Variations in outcome that I saw could have been a consequence of these variations in the psychological conditions under which each trial or study was conducted. But how the participants constructed the experiment was not a random, uncontrollable factor; rather, expectations about the study and its meaning were in part a consequence of the interpersonal dynamics between researcher and participant. These subtle but important effects seem to be especially relevant to parapsychological research, where the phenomena already seem quite sensitive to conditions (I discuss this in more detail in Roe, 2016b).

To illustrate, I led a project that was intended to tease out the similarities and differences in performance on ESP and PK tasks using a common platform. We developed a greyhound racing game that allowed us to include a condition in which participants acted as gamblers and simply had to choose which dog they thought might win the next race, so we were testing for ESP. In a second condition they acted as owners so didn't have any choice over which dog was theirs, and this allowed us to test for PK. Whereas the movements of dogs in the ESP condition were determined by a fixed list of random numbers drawn in advance of the study from tables, and so was potentially available by ESP, movements in the PK condition were determined in real time by reading data from a random number generator, and so the greyhound "movements" were potentially open to psychokinetic influence. In order to control for expectancy effects, we had half the PK trials in fact test for ESP and half the ESP trials test for PK. We thought the task was intuitive and engaging and were very hopeful that it would be effective. Unfortunately, across three formal studies (Roe, Davey, & Stevens, 2003, 2004, 2005) we found little evidence of overall ESP or PK effects, but—as seems usual in parapsychology studies—there were enough secondary effects to suggest that we weren't looking just at random noise in the data that we had collected. For the final study in the series (Roe, Davey, & Stevens, 2006) we speculated that the null results might reflect a kind of experimenter effect. To that point all the data had been collected by Russell Davey, a graduate research assistant of mine who was a bright and able student, but relatively inexperienced at running parapsychology experiments. In this last study, Davey was again responsible for the recruitment and scheduling of participants, but this time he ran only half of the sessions and the other half were run by me. I was more experienced and more invested in the goals of the project, having spent a lot of time developing the ideas and securing funding. Again, Davey's participants scored at or below chance expectation, but my participants scored significantly better in the disguised ESP and PK conditions and suggestively better overall. Most intriguingly, straight after the initial briefing we rated our interactions with each participant (that is, while the trial was being completed, when we did not know anything about the participant's actual performance) and our confidence of success correlated quite strongly with the outcome (Spearman's $\rho = -.43$, $p = .007$).⁶ We took this to suggest that there was some

⁶ Scoring on the task was such that a negative correlation indicates that greater confidence was associated with better performance.

ineffable quality of the interaction between experimenter and participant that cued us to the trial's likely success, though this was not some particular property we could identify more exactly or manipulate to ensure successes in subsequent trials. We have since conducted similar studies to look for experimenter-participant interaction effects in dream ESP, ganzfeld, and PMIR studies (Hitchman, Pfeuffer, Sherwood, & Roe, 2016; Roe, Sherwood, Farrell, Savva, & Baker, 2007; Sherwood, Roe, Holt, & Wilson, 2005), and although the findings are as yet equivocal, there are some grounds for optimism, particularly when looking at fluctuations of experimenter mood and expectations of success as predictors of performance. This reaffirms my suspicion that despite our best efforts, there are lots of ways in which one trial differs from another even when superficially we are keeping conditions constant.

This brings to mind Heraclitus's famous observation, "No man ever steps in the same river twice; for it's not the same river and it's not the same man." In the wake of the latest crisis in psychology brought about by the Open Science Collaboration's failure to replicate more than 36% of a set of findings originally reported in high calibre journals, I think it would be useful to consider whether it might be unrealistic to expect replication success to be simply a function of effect size and study power—as it might be in the natural sciences—since here we are working with subtle interpersonal factors and drawing on tacit knowledge. I talk about this at more length in a recent issue of *Mindfield* (Roe, 2016a).

The Tension Between the Need for Clarity and Control Versus Authenticity and Ecological Validity When Designing Studies

In my experimental work I had prided myself that my designs were reasonably sophisticated and did quite a good job in controlling for extraneous variables so that significant outcomes, if there were any, could be more unambiguously attributed to the variables I was manipulating. But I was becoming concerned that in my efforts to contrive an experiment with elaborate controls I had lost sight of how the work linked with people's everyday lived experience. This concern was driven home for me by Tony Lawrence, who during a talk at a conference of the Society for Psychical Research confessed to his sense of embarrassment when in social situations he met people who heard of his interest in parapsychology and volunteered accounts of rich and vivid psychic experiences or abilities of their own. He recognised that this sense of embarrassment reflected *his* shortcoming, not theirs. He was comfortable with psi as an abstract construct, and even with the occurrence of psi in experiments—so long as it was strong enough to produce significance levels that encouraged further research but not so strong that it challenged one's worldview. When he met people for whom psi was sufficiently rich to be an integral part of their sense of self and of their everyday lives, he found that he had nothing to say to them because his research didn't speak directly to their experience. That way of thinking about and managing research was clearly dysfunctional. Tony's conference presentation reminded me that as a psychologist, what I do in the refined conditions of experimental work *must* generate discoveries that can speak to people's lived experiences of psi, and for that to happen it must be founded on those lived experiences.

A case in point is telephone telepathy. Rupert Sheldrake has argued that telephone telepathy is among the most common types of parapsychological experience reported by the general public, based on surveys that posed the question "Have you ever heard the telephone ring or picked up the telephone and known who was on the other end without any possible cue, before they had spoken?" (e.g., Sheldrake, 2000). Such experiences are dismissed by the sceptically minded as reflecting the attribution of meaning to simple coincidence, implicit learning of behaviour patterns that might suggest when people are likely to call, and selective recall of confirmations and forgetting of disconfirmations; all of these are plausible given our susceptibility to such errors. To control for these explanations Sheldrake developed a simple protocol (Sheldrake & Smart, 2003a, 2003b) in which the callee is separated from a group of potential callers and one is chosen at random to make each call. The callees make their guesses once the phone rings but before they pick up the receiver, and their likelihood of success just by chance should be in proportion to the number of potential callers. Sheldrake has had some success with this protocol and it is an attractive way to introduce some of the principles of the scientific method to the general public, but I can't help feeling that he has missed the point of telephone telepathy in his rush to the laboratory, and this may account for some

of the difficulties that others have had in replicating (Schmidt, Müller, & Walach, 2004).

J. B. Rhine had some pertinent advice to those who hoped to study psi in the laboratory: “If you want to have rabbit stew, first catch the rabbit” (cited in Stanford, 1993, p. 129), and in order to be able to catch the rabbit then we need to know something of its behaviour and preferences in its natural environment or else we’ll be constantly chasing its tail. In other words, if we wish to study the action of psi in the laboratory then we need to ensure that the laboratory situation reflects the circumstances under which psi will ordinarily appear in the natural world. With only the most meagre sketching of the phenomenon of telephone telepathy as it occurs in situ, there is a real danger that sceptical accounts will fit insofar as one vague thing can be mapped onto another vague thing, and difficulties in replicating will arise because we have developed no real understanding of the necessary or sufficient conditions for its occurrence.

In this context I thought it important to ask people about the circumstances of their telephone telepathy experiences and of how they made sense of them. We conducted focus groups with people who had had such experiences and we thematically analysed their discussions (Roe & Smith, 2011). Among the themes to emerge were that there were often palpable emotional or physical changes during the experience that signalled to them that this wasn’t merely a coincidence (and by dint of which women were thought more likely to have the experience on the basis that they were more trusting of their emotional responses); also that the experiences usually were an expression of a sense of connection with particular people (that is, the telephone telepathy experience was a verification of something they already “knew” by other means); but most importantly that they saw these experiences as a minor subclass of phenomena that formed part of a cluster of experiences that confirmed for them that they were interconnected spiritual beings. They wanted to talk about instances of telephone telepathy in which they were the caller rather than the callee, about occasions when they felt a growing sense of foreboding or concern for someone close to them, whom they felt compelled to call, and who turned out to be in trouble or unwell. They saw this as an affirmation of their bond and obligation to each other. They also wanted to relate other experiences that they interpreted in terms of their lives forming part of some cosmic design or as reflecting the influence of some benign overseer. In that context the experiences were recognised as trivial in themselves, and attempts to reproduce them in formal experiments seemed facetious.

In an effort to overcome the kind of embarrassment that Tony Lawrence has described, I began to seek opportunities to spend time with practitioners, including healers who were part of the Confederation of Healing Organisations in the UK, but especially mediums who were members of the Spiritualist National Union. Along with my colleague Elizabeth Roxburgh, I attended church services with platform demonstrations, participated in development workshops, and talked with practitioners off the record about their abilities, how they experienced them, and importantly how they managed them. Qualitative methods had been quite alien to me given my background in the natural sciences and my research record over the first 15 years, which consisted almost exclusively of experimental work. However, it became clear that for some phenomena qualitative methods were a better fit for the rich, idiosyncratic and nuanced nature of parapsychological experience. They also appealed in privileging experiencers rather than researchers as expert concerning their own experiences, which I saw as an expression of the humility that I had always thought should be at the heart of empirical enquiry.

Our primary research focus has concerned wellbeing. Mediums report experiences that in other circumstances could lead to a diagnosis of pathology—they see things that others cannot see, hear things that others cannot hear, have embodied experiences that they attribute to the influence of (discarnate) others—and yet they present as more psychologically healthy than spiritualists who don’t have these experiences, and in fact also better than norms for the general public (Roxburgh & Roe, 2011). Roxburgh and I explored this paradox in interviews with practitioners, asking them about their life history, their practices and the experiences they led to (Roxburgh & Roe, 2013, 2014). Many reported seeing apparitions in childhood, and the reaction of significant others to these was key in determining how the experiences were processed and assimilated. It was important for them to be part of a community that not only accepted their experiences but valued them. They needed to have an interpretative framework that made sense of the experiences, gave them a sense of order and—importantly—access to ways of working with spirit communicators that made their experiences manageable, and not simply an indicator of psychosis. Thus the voices they heard weren’t

unwanted intrusions but were collaborators who could be negotiated with and whose time could be rationed so that they were able to have uninterrupted time to do mundane things. Some of these concerns are nicely illustrated in Sarah's account:

The first memory that I actually have was hearing voices after my father died . . . one night I went to bed and I woke up and I'd had these voices talking to me saying that my dad was fine, he was living, there wasn't a problem, he wouldn't want me to be upset and I thought I was dreaming, so I thought "pull yourself together" and as I turned over to go back to sleep the voices were still there . . . so I thought "I'm losing it, I'll go down and make a cup of tea" [this was an English person, and so making a cup of tea is the solution for most things] so I went down and all the while I was making this cup of tea these voices were still talking to me . . . so I went to the Doctor's and I told him what had happened, I said "I must be having a nervous breakdown," so he gave me some pills, as they do, told me to go away for a few days and just try and chill and relax . . . never took the pills because I don't take tablets, I don't believe in that sort of thing . . . I thought "Right this is me and now I need to cure myself to get better" so I just pulled myself together, blocked absolutely everything out, thought I've just really got to get back on track and I did that probably for about 6-7 years. [But then . . .] I started to talk to . . . [a medium] . . . and we sat just chatting about things and that is when my interest started because they were explaining things to me because that was my first real knowledge that somebody was talking to me (. . .) Steph and her sister were starting to explain all these things to me and all the little things that had happened over the years which you just put down as "Oh that must be that and that must be that" so you know, things just started making sense.

I have found that concerns about pathologisation and ridicule are quite widespread. Its consequences were impressed upon me by one experience in supervising an undergraduate project some years ago. I was approached by a mature student who wanted to collect new cases of near-death experience. She interviewed one woman, "Emily," who described an NDE that occurred after complications following childbirth perhaps 15 years earlier. Her NDE contained classic features, including an out-of-body element, an encounter with a loving light, and meeting deceased relatives. After the interview, my student was able to explain that these were typical of many NDE cases, and the experient's relief in having her experience validated and normalised was palpable. She disclosed that although the experience had happened so long ago, she had not shared it with anyone—not her husband or other family members, and certainly not any medical staff—for fear that she would be "packed off to the looney bin." She felt like she had been carrying a burden that had finally been lifted from her. It was clear to me that parapsychology has an obligation to support people such as Emily so that they are reassured that many so-called paranormal experiences are not abnormal, but reflect the normal range of human experience.

Our work with mediums has been part of an ongoing initiative to strengthen links between researcher and practitioner communities. When we first approached the Spiritualist National Union there was an understandable suspicion that our intentions were to prove that they were at best misguided and at worst fraudulent. It has taken time to demonstrate our sincere intentions and to build trust. Our strategy has involved a two-way exchange, attending and running workshops that have enabled us to learn about mediumship development and practice, and for practising mediums to learn about the principles of the scientific method in ways they can use for self-reflection and further development. This collaboration advanced significantly in 2014 when we were donated space at Stansted Hall to convert into a permanent on-site research laboratory. The Stansted Hall estate was gifted to the Spiritualists' National Union by Arthur Findlay in 1954 with the intention that the buildings be used to establish a "College of Psychic Science." It was renamed The Arthur Findlay College and has established an international reputation for its educational programmes in mediumship practice and philosophy. But despite spiritualism being among the most evidence-based of the major religions, these programmes have not always sought to engender a scientific approach to the evaluation of empirical evidence gathered during demonstrations, and the establishment of the laboratory was intended to rectify that.

Refurbishing the lab space was made possible by generous support from the Society for Psychical

Research, Friends of Stansted Hall, and local spiritualist groups, showing that this is a genuine collaboration. These donations have enabled us to create a facility that will be made available to serious researchers to work closely with a broad spectrum of practising mediums who visit the AFC. The monetary investment is quite modest but hopefully will have a significant impact on the amount of serious research with mediums that will take place in the UK.

The Negative Impact of our Funding Model Upon the Way in Which Parapsychology Is Organised

Speaking of investment, I'd like to acknowledge that all the research I've just described has been made possible by the generous support of a variety of funders, to whom I'm very grateful. But it's clear to me that the marginal status of parapsychology has made accessing funding much more difficult than for other, more mainstream topics, and I think this has had consequences for the kind of research that we see.

Firstly, because there is very little money to go around, relatively few people can be fully employed in research. The actual community of active researchers is remarkably small, so very little new research is conducted each year—barely enough to make publication in our journals as competitive as it should be. And those people who attract most of that funding tend to be innovators who have been successful in developing new protocols or adapting methods from other areas and in demonstrating “proof of principle” by reporting significant psi effects using such methods. We also have a number of “early adopters” who are quick to seize on new approaches and technologies and are responsible for the first wave of independent replications. However, relatively quickly the innovators lose interest in simple confirmations and move on to develop yet more methods and approaches, and the early adopters soon follow suit. I am sure that this pattern also occurs in other disciplines, but with their greater numbers they also include many able technicians who are willing to conduct modest replication extensions that allow them to do research in order to pay the mortgage. I suspect that parapsychology cannot afford many able technicians, so that interest in an original protocol (or effect) seems to wane as the caravan moves on. This gives parapsychology the appearance of a “butterfly science” that flits en masse from protocol to protocol as it falls in and out of “fashion” much as a butterfly flits from flower to flower (Roe, 2012). At best this is frustrating in diverting resources away from potentially fruitful avenues of research; at worst it looks suspicious to the outsider, who expects to see continuing and systematic work using a particular method for so long as it is productive, particularly when great claims were initially made for it. Why are there now so few micro-PK studies? So few ganzfeld studies? Has Hyman's prediction of an as yet undiscovered fatal flaw been fulfilled and hushed up? As a community we need to better coordinate our efforts to give rise to a more systematic and enduring programme of research, one that goes beyond proof of principle and the first wave of independent replications—the excellent Bem replications are a notable example of what can be achieved when critical mass is achieved (Bem, Tressoldi, Rabeyron, & Duggan, 2015).

Secondly, I think exaggerated competition for funding requires us to be creative in making the most of the limited resources available. As a senior academic in my university, I am simply too expensive these days to be employed directly in conducting research (the support of the Perrott-Warrick Fund is a notable exception). Instead, I have tried to invest the funds I have been awarded in young researchers. Professional development for those interested in a career in parapsychology is still extremely difficult in the UK, with very limited funds available to support one's study for advanced qualifications (MSc and PhD) that are necessary prerequisites to university employment. It seems inevitable that we are losing very talented individuals who simply cannot afford to opt for parapsychology when other branches of psychology can offer bursaries to defray living and research expenses. Employing a bright student to collaborate on a project, ostensibly as a research assistant but in practice as a co-experimenter who contributes to all stages of the research cycle from design to conference presentation and even journal publication, can be just the boost needed to maintain their commitment to parapsychology during times of adversity. Beneficiaries of this approach include people who have since received their doctorate, such as Nicola Holt, Sophie Drennan, and Glenn Hitchman, and people currently in PhD programmes such as Charmaine Sonnex, Callum Cooper, David Saunders, and Andrew Hodrien, names that may be familiar to you or will be in years to come.

I think that as a community we can do more to nurture young talent and give them the tools to become assimilated into the academy. Parapsychology will gain acceptance not by persuading established researchers to become involved in parapsychology, but by seeding the next generation with people who have direct experience of its rigour and caution, and are more open to consider the implications of its findings. That's why I think Bob Morris' strategy during his time as Koestler Professor has been so important for the field and why I think it is essential to continue it in his stead. When he was first appointed to the Edinburgh Chair, he confided to Jim Carpenter (2004, p. 425) that he intended to

take the long view with this post, investing the time and effort to build good relations with other academic disciplines, and developing a quality programme that could generate excellent scholars who would then go on to take academic posts at other universities, seeding the intellectual landscape of Britain and Europe with parapsychological experts in a way that had not yet proven possible in the US.

How successful was he in that endeavour? By my count he was able to mentor 26 graduate students to receive their PhDs, many of whom are still active in parapsychology, having been able to establish themselves in academia in university departments or private facilities (see Figure 1). Of these, 6 have supervised or are supervising their own PhD students, adding another generation to the "family tree." And among that generation 4 are themselves supervising PhD students, making 27 "grandchildren" and a further 9 "great grandchildren." By the standards of mainstream psychology this family tree is unexceptional, but in terms of ensuring the viability of parapsychology I think it makes a crucial contribution to the size and calibre of our research community around the UK. We need to find a way of replicating this in other countries.



Figure 1. Family tree of PhDs supervised by Bob Morris (representing three generations). Persons in bold have gone on to supervise doctoral students; italics indicate persons who have not yet graduated.

At a more local level, such mentoring can also contribute to the vibrancy of the research culture. I am a great believer in the importance of critical mass, having seen its consequences for myself. When I first joined the University of Northampton in 1995 I had colleagues who were supportive of my work but

knew little about it and were not interested in becoming more involved. As an isolated academic, progress was very slow. Things picked up when Christine Simmonds-Moore joined me to start her PhD under my supervision, and suddenly I had a knowledgeable person to bounce ideas with and hatch research plans; a task shared was a task halved. Slowly we were able to draw more people to us, so that today, Northampton has one of the largest research groups devoted to parapsychology and transpersonal psychology, and the diversity of interests and approaches ensures that each day with them is surprising and rewarding.

Conclusion

I started this paper by wondering whether my experience as a parapsychologist might have any value for those who are in the early stages of their career. What advice would I offer to the young researcher? What misconceptions could I point out? When I was starting out I think I had an unrealistic expectation about the degree to which parapsychology could emulate practice in the natural sciences. I would advise you not to be dismayed if exactitude of findings and consistency of replication in your own work is not what you expected. Recognise that our object of study (human experience and behaviour) is inherently more complex than is allowed by that model, given that it includes a subject matter with thoughts and expectations of its own that can have a dramatic effect on the outcomes, and that are highly sensitive to cues we give as researchers through our interactions with them. Alongside the sophistication of the research designs you develop, keep in mind that the art in scientific practice depends on your interpersonal skill to enable your participants to feel sufficiently comfortable with you to behave naturalistically, and to accept at an emotional level the possibility of a psi effect. Don't be beguiled by cleverness in research design and recognise the importance of authenticity; ensure that your work connects with lived experience and that it can have impact for real people outside the academy, particularly in addressing experiencers' concerns about ridicule and pathologisation. This requires you to understand the phenomena as they occur in the real world and to privilege the experient's account, unsullied as it is by exposure to the blinkers of previous research and theory. Recognise that although it is important to contribute to our research base, it is essential to invest in human resources. Strive to build community, to feel a civic responsibility to support others and to acknowledge the value of critical mass in ensuring the long-term survival of our discipline.

Above all else, don't be worn down by criticism of yourself or the field. Appreciate that the antipathy towards parapsychology is at least partly driven by social and psychological forces that are unlikely to be mollified by reason and further evidence. We are collectively engaged in interrogating phenomena that have the potential to cast light on our deepest and most fundamental capacities as human beings. The insights we gain might represent the merest of glimpses but their implications can be genuinely profound.

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The University of Northampton
Park Campus
Northampton NN2 7AL, UK
Chris.roe@northampton.ac.uk

IS THE METHODOLOGICAL REVOLUTION IN PSYCHOLOGY OVER OR JUST BEGINNING?

By J. E. Kennedy

ABSTRACT: Significant results from parapsychological experiments using standard psychological research methods motivated psychologists to recognize some widespread methodological deficiencies and the need for preregistered well-powered confirmatory research. Psychological researchers have not yet recognized several other common methodological weaknesses that can be expected to cause this cycle to be repeated. When confronted with the choice between psi versus overlooked methodological deficiencies, psychologists will recognize the need for methodological improvements. These overlooked methodological factors include: (a) deficient study registration practices, (b) bias from dropouts and incomplete data, (c) the need for software validation, (d) measures to prevent experimenter fraud, (e) appropriate statistical methods for confirmatory research, (f) failure to consider inferential errors with Bayesian analyses, (g) the weaknesses of retrospective meta-analysis and strengths of prospective meta-analysis, and (h) problems from statistical dependence for the outcome variables in statistical analyses. Psychological and parapsychological researchers can easily avoid this inefficient process of methodological evolution driven by controversies about parapsychological findings. Research practices that address these methodological deficiencies are available and will eventually be recognized as needed for psychological and parapsychological research. Recommended practices for addressing these methodological weaknesses are described.

Keywords: research methodology, incomplete data, software validation, experimenter fraud, statistical power

Parapsychologists have often argued that their research methods have been as good as the standard methods used in psychological research (Cardena, Palmer, & Marcusson-Clavertz, 2015, p. 1). With the publication of Bem's (2011) paper on "feeling the future," psychological researchers finally paid attention to this point and agreed. However, their conclusion was not that the research provided evidence for psi, but that the methods for psychological research needed to be substantially improved. With considerable publicity and declaration of a methodological revolution (Wagenmakers, 2015), psychological researchers discovered the need for preregistered well-powered confirmatory research that has long been the accepted practice for clinical trials.

However, psychological researchers are still struggling with methodological practices. Several key methodological issues have yet to be recognized and addressed. Psychological and parapsychological researchers can expect additional repetitions of this recent experience. When confronted with the choice between attributing experimental results to psi or to previously overlooked methodological deficiencies, psychological researchers will become advocates for additional methodological improvements.

An alternative strategy is to skip the inefficient cycles of implementing better methodological practices when skeptical psychologists become trapped by a choice between improved methodology and psi. Several methodological factors that psychologists and parapsychologists will eventually have to address can be easily foreseen and are discussed below. Methodological practices that address most of these factors have been implemented as standard procedure in regulated medical research. These practices are described.

Standards for Study Preregistration

Although the value of preregistration for confirmatory studies is now widely recognized, psychologists are still struggling with pitfalls in the registration process. Key practices for confirmatory

study registration include that: (a) the registry specifies requirements for the registration information, (b) all key methodological decisions that could affect the study outcome are registered, (c) each registration is independently reviewed for consistency and completeness, (d) the study registration is made irreversibly public before data collection begins, and (e) the registrations can be easily found and accessed when searching for studies on certain topics (Watt & Kennedy, 2015, also see the comments for that online article). The KPU Study Registry (2012) implements all of these practices, but, as yet, study registries for general use by psychologists do not incorporate all of these practices. Parapsychologists who use nonoptimal study registration can expect to have their findings eventually challenged when psychologists recognize the weaknesses of their registration practices.

Dropouts and Incomplete Data

Dropouts and incomplete data can introduce bias into experimental results but are often given little or no attention by researchers. Participants who are doing poorly in an experiment may tend not to complete the experiment or tend to make invalid responses. In general, dropouts and other types of incomplete data cannot be assumed to be independent of the experimental intervention and procedures—and therefore are confounding factors that can potentially cause bias. In addition, ad hoc retrospective decisions about handling incomplete data can create bias. The possibility that an ostensible finding is entirely or partially due to bias from incomplete data needs to be addressed.

In clinical trials the “intent-to-treat” (or “intention-to-treat”) principle requires that every participant who was randomized is included in the primary analysis, whether or not the participant complied with the protocol (U.S. Food and Drug Administration, 1998; Gupta, 2011). Intent-to-treat is a fundamental principle guiding study conduct and analysis and is generally considered necessary to avoid bias. If the inclusion or exclusion of incomplete data alters whether the study outcome is significant, the incomplete data compromise the study and the results are unconvincing.

A secondary “per-protocol” analysis is often also done and includes only participants who substantially complied with the protocol. The per-protocol analysis may provide a better estimate of the effect size for participants who comply with instructions, but the analysis can also be severely biased if, for example, those with poor outcomes tend to drop out or tend to provide incomplete data.

Psychological and parapsychological researchers often discard incomplete data without fully considering the potential for bias. In effect, a per-protocol analysis is used as the primary analysis rather than an intent-to-treat analysis. This practice can promote bias in favor of large effects. Any experiment in which the participants receive real-time direct or indirect feedback that gives an indication of their performance is potentially subject to biased data-selection due to incomplete data. This is one example of the larger problem of confounding due to incomplete data.

A general principle is that the exclusion of any data for participants who began an experiment introduces potential for bias that must be carefully evaluated. This includes data removed as outliers. The handling of incomplete data frequently involves tradeoffs between potential biases that underestimate (intent-to-treat) or overestimate (per-protocol) the effect being investigated.

The intent-to-treat strategy is conservative and is generally optimal practice for providing evidence that an effect occurs. For nonmedical research, this principle might be called *intent-to-participate* and would be triggered when a participant has been randomly assigned to a group or has provided initial data for the effect being investigated. At a minimum, any excluded data should be analyzed and reported to evaluate possible bias. However, the key question is not whether the results for the excluded and included data are different, but whether the overall result is significant if the excluded data are included—which is the question answered by the intent-to-treat or intent-to-participate analysis. For missing data, sensitivity analysis exploring various unfavorable assumptions about the missing data should be conducted to evaluate possible bias. Useful guidance on these points can be found in U.S. Food and Drug Administration (1998). For confirmatory research the handling of incomplete data should be specified in the study preregistration and carefully explained in the study report.

Software Validation

Documented software validation is essential for confidence in research results. This principle is well established for regulated medical research (U.S. Food and Drug Administration, 2002, 2003) but has not yet become widely recognized for psychological and parapsychological research. Psychological researchers recognize the need to empirically evaluate the validity of questionnaires and other measurement instruments but have been slower to recognize the need to empirically evaluate the validity of other aspects of research methodology, such as software.

Software validation involves testing and documenting that the software reliably and accurately fulfills its purpose. Theoretical discussions of software validation can be highly technical and filled with jargon. However, a practical, common-sense approach is more realistic in practice.

Validation of the software used for data processing and analysis is straightforward. Another researcher develops and/or applies independent programs that duplicate the analyses.

Validation of the software used to conduct an automated experiment and to collect the raw data is more challenging. Verification that the software functions properly must be based on tests of the experimental procedure rather than on independent programming. Watt and Brady (2002) described a software oversight that produced artificial positive results in a psi experiment. Such oversights and errors can easily occur with automated research—but can be avoided with basic, established principles for software validation. In the absence of software validation, the possibility of artifacts due to programming errors and oversights is a legitimate concern.

End-user testing is the final and most important step for validating software. This step is performed by a user or tester who did not do programming for the study. End-user testing verifies that the software operates as intended for the specific environment that will be used in the experiment. This includes the specific version of the experimental software, the specific settings and options for the software, the specific computer operating system (including updates), the processor type and speed, storage devices, other devices attached to the computer, and other software that may be running in the background. Verification that the software functions properly in the experimental environment is particularly important if timing for the stimuli or responses has a significant role in the experiment.

Competent initial end-user testing often discovers problems that the software developer(s) did not anticipate. The key questions for validation of software for automated experiments are:

1. Does the software accurately and reliably present the stimuli and/or feedback for the experiment?
2. Does the software properly generate the random elements in the experiment?
3. Does the software accurately and reliably record the human inputs and the conditions generated by the software?
4. Does the software properly handle unexpected, inappropriate inputs?
5. Does the software have these properties for all computers that will be used in the experiment?

Software validation is based on documented empirical evidence that these questions are answered affirmatively, rather than relying on optimistic assumptions that the software and hardware operate ideally. A review of the programming source code by a knowledgeable person is usually valuable but does not replace the need for this empirical validation testing. The final version of the software that is used in an experiment, not just the programming language or system for developing the experimental software, needs to be validated.

End-user testing should detect intentional programming errors (fraud) as well as unintentional errors. The optimal practice is for the person who developed the software to not have access to the computers used for collecting data, and reciprocally, for the experimenters collecting data to not have access to the source code for the experimental software.

Methods and steps that may be useful for developing a plan for end-user testing are described in the

Appendix. These steps focus on testing the software used to conduct parapsychological and psychological experiments.

The development of scripts and datasets that generate known inputs and outputs is a common practice in validated software environments. The validation package is run when the software is initially installed on a computer and when changes are made, such as modification of the experimental software, changes or updates to the operating system, or changes to software that runs in the background. The validation package can also be run periodically to verify that the operation of the software has not been altered by factors unrecognized by the experimenters. These validation packages typically include automated comparison of the observed and expected output, and they provide a report of the validation results that is kept as part of the research records. Automated validation packages typically require significant effort to develop, but relatively little subsequent effort to apply.

Psychological researchers can be expected to recognize the need for software validation after some widely publicized cases of research retraction due to invalid software and/or after parapsychological findings force recognition of the possibility of unintentional or intentional programming errors.

Experimenter Fraud

If measures to prevent and to detect experimenter fraud are not implemented in research, fraud will often be easy and tempting with little possibility of detection. Typical research procedures in psychology and parapsychology do not include measures to prevent and to detect experimenter fraud. Stroebe, Postmes, and Spears (2012) noted that detected cases of fraud are likely the tip of an iceberg of mostly undetected fraud. In the absence of measures to prevent and to detect experimenter fraud, meaningful conclusions about the rate of occurrence of fraud are simply impossible (Kennedy, 2016c). The uncertainty about undetected fraud compromises confidence in research findings and is increasingly recognized as unacceptable. However, practical, effective measures to address experimenter fraud have not yet been widely recognized by psychological researchers.

Independent replication and peer review have not been effective at detecting and deterring experimenter fraud (Kennedy, 2014b, 2016c; Stroebe et al., 2012). The primary symptom of fraud is inconsistent results among experimenters, but such differences are virtually never attributed to fraud. Inconsistent experimental results in psychology and in parapsychology are typically attributed to differences in experimental procedures and subject populations. These alternative explanations could be true and prevent independent replication from being useful for detecting and deterring experimenter fraud. Differences among experimenters have been prominent throughout experimental parapsychological research and bring into focus the need to routinely use research methods that prevent fraud (Kennedy, 2014b, 2016c).

Experimenter fraud typically has been by an individual experimenter who had opportunity to manipulate or fabricate data with little chance of detection. I am not aware of any cases of fraud that involved collusion among experimenters in academic or nonprofit settings.

Experimental procedures that make undetected data changes or fabrications difficult for one experimenter were a methodological standard in my experience working in regulated medical research (Kennedy, 2014b; 2016c). This standard has also been recommended for parapsychology (Akers, 1984; Dalton et al., 1996; Kennedy, 2014b, 2016c; Rhine, 1974, 1975). The goal is to make undetected experimenter fraud difficult rather than easy and tempting. Procedures that involve duplicate records and experimenters checking each other are relatively easy to implement once these become standard practices for research.

One basic strategy is to send a duplicate copy of each component of the data to a secure location as early as possible in the data collection process. This should be done before an experimenter has unblinded information that could allow the experimenter to bias the results. E-mailing or uploading a copy to a distant person or website that serves as a secure data repository is good practice given modern technology. The optimal procedures will assure that a person is never alone with access to data that could allow biased manipulation without detection. For example, if an experimenter obtains the random number for the target for a trial from an online source, a second experimenter can observe the generation of the

target, the transmission of the target to the data repository, and the entry or recording of the target into the experimental database.

Automated experiments are not immune from experimenter fraud. A person with knowledge of programming for the software used to conduct the experiment could create a fraudulent version of the software that would be used covertly during the experiment. A version without the fraudulent programming would be used for control runs and for software validation. Or, the software could detect whether a run is an experimental or control run and have fraudulent bias apply only during experimental runs. Alternatively, the data file created by the experimental software could be modified by a separate program or by direct editing.

As noted above, the optimal practice is for the person who developed the software to not be involved with data collection and to not have access to the computer(s) used to conduct the experiments. The end user experimenters who collect the data should validate the software as described above to assure that the software operates as expected in the actual experimental environment. These experimenters should not have access to the source code for the software. If software is transferred from one experimenter group to another, the software should be validated on the computer(s) for the receiving experimenter group. The source code may also be transferred and reviewed, but here too, the experimenters managing the software and the experimenters collecting data should be different and have restricted access to the other functions. In addition, when possible, the automated software should send a copy of the data to a data repository before the output file could be modified. A duplicate archival copy of the software should also be kept at the repository and ideally a process implemented to verify that the software used for experiments was not modified from the original archival copy. Technical innovations for assuring the integrity of experimental software may be developed.

Making the raw data available to others for independent analyses is also a useful, but secondary, strategy for deterring and detecting fraud (Kennedy, 2014b, 2016c; Stroebe et al., 2012). Accusations of fraud based on post hoc analyses will too often be irresolvable given the intrinsic limitations of post hoc analyses. Stroebe et al. (2012) pointed out that such accusations will sometimes be incorrect due to the probabilistic nature of the analyses. In addition, data can be fabricated or altered in a way that does not leave convincing signs of fraud. Making the data available to others does not eliminate the need for procedures that prevent fraud.

Statistical Methods for Confirmatory Research

The criteria for determining whether a replication study is successful have received much attention recently as the need for preregistered, confirmatory research has come into focus (Lakens, 2016; Open Science Collaboration, 2015; Simonsohn, 2015a, 2016). Typically, one or two confirmatory studies have been done and must be compared with previous studies that had more questionable methodology.

In recent decades psychological researchers have excessively focused on p values without adequate consideration of effect size, statistical power, and the distinction between exploratory and confirmatory research. One of the most widely discussed reactions to this excess is Cumming's (2014) "new statistics," which focuses on estimating effect sizes and advocates abandoning hypothesis tests and associated "dichotomous thinking."

Good confirmatory research, like good science in general, is based on making and testing specific predictions. Exploratory research focuses on estimating effect sizes without specific predictions. Cumming's new statistics attempts to set exploratory research methods as the standard for scientific evidence and avoids the dichotomous thinking associated with testing predictions. That is a major retreat from the basic principles of good science. Researchers who apply Cumming's recommendations will tend to interpret experimental outcomes that can easily occur by chance as evidence for an effect and will explicitly avoid confronting the possibility that the effect being investigated may not be valid. The limitations of the new statistics are increasingly recognized (Kennedy, 2016b; Lakens, 2016; Morey, Rouder, Verhagen, & Wagenmakers, 2014; Savalei & Dunn, 2015).

Researchers conducting preregistered well-powered hypothesis tests evaluate specific predictions that provide the most convincing evidence that the researchers actually understand and control the effects being investigated. With the new statistics, psychologists have shifted from an excessive, misleading focus on p values to an excessive, misleading focus on effect sizes.

The basic challenge for evaluating confirmatory research is that p value, effect size, and statistical power must all be considered. No one parameter alone adequately conveys the full outcome. Although statistical power has been generally ignored by psychologists for decades, it is the heart of classical analysis and is a key component of the statistical validity of a hypothesis test (discussed below, also see Kennedy, 2016b). As Cohen (1990, p. 1310) commented, “failure to subject your research plans to power analysis is simply irrational.” The statistical principles for regulated medical research clearly describe the importance of hypothesis tests, power, effect size estimates, and the distinction between confirmatory and exploratory research (U.S. Food and Drug Administration, 1998). These principles are useful guidance for anyone conducting experiments analyzed with statistics. Software such as the free program G*Power (Faul, Erdfelder, Lang, & Buchner, 2014) makes power analysis much easier than in the past.

One of the simplest and most informative presentations is the power curve or operating characteristics for a hypothesis test. This is a graph or table that shows the power of the test for different values of true effect size given the sample size in the study. This shows which effect sizes have low power as well as which have high power. For confirmatory research, the operating characteristics should be determined when the study is being planned.

As more experience is gained with power analysis, I expect that a statistical power of at least .90 (.95 when possible) will become the standard in situations when few confirmatory studies are expected, and in other cases when researchers want to use optimal methodology. A power of .80 may be appropriate when several or many confirmatory studies are expected and overall conclusions will be based on the results of multiple studies. The criterion for a successful confirmation will be a significant result in a study with a power of .80 or higher.

Properly designed confirmatory studies can provide evidence that the hypothesis of interest is false as well as true (Watt & Kennedy, 2015). If a study with a power of .90 or higher or multiple studies with a power of .80 fail to produce significant results, that outcome can be interpreted as evidence that the experimental hypothesis is false for the effect size used in the power analysis. The interpretation of nonsignificant results from studies with lower power or uncertain power is ambiguous because the results could be due to low power rather than to the experimental hypothesis being false.

A quantitative value for power is needed to provide evidence that an experimental hypothesis is false in the same way that a p value is needed to provide evidence that an experimental hypothesis is true. Without power analysis, a study cannot provide evidence that a hypothesis or prediction is false.

The effect size used in a power analysis when designing a study is a prediction about what will happen in the study. For a study with high power, a nonsignificant result provides evidence that the predicted effect size specified in the power analysis is false. A nonsignificant result does not provide direct evidence that the null hypothesis is true because a small, nonzero effect size could be true. The null hypothesis is basically irrelevant other than for developing a test for the effect predicted in the power analysis.

Researchers who abuse hypothesis tests focus on the null hypothesis and ignore statistical power. This prevents evidence that the experimental hypothesis is false and avoids the confirmatory question of whether the researchers can make reliable predictions about the effect being investigated. Most criticisms of hypothesis testing are actually criticisms of this abuse and are not applicable to proper applications of hypothesis tests with power analysis.

If a minimum effect size of interest cannot be reasonably specified for power analysis, the research is exploratory and the experimenters do not have the degree of understanding and control of the phenomenon that is needed for convincing evidence. Similarly, research is exploratory if the researchers ignore power analysis, as was common in the past in psychology and parapsychology. Exploratory research is typically the creative step that is the starting point for a line of research, whereas confirmatory research provides the convincing evidence that makes science valid and self-correcting.

If the power analysis and sample size for a confirmatory study are based on previous research, the uncertainty in the effect size estimates should be considered. As indicated by a confidence interval, the mean effect size from previous research can be assumed to have a 50% chance of overestimating the true effect size. The planned sample size should generally be based on an effect size on the low side of the confidence interval for the effects in the previous studies—such as the lower end of the 80% or 60% two-sided confidence interval (90% or 80% one-sided confidence interval). Allowance should also be made if the previous studies were exploratory and likely subject to bias.

Alternatively, the power analysis and sample size can be based on a theoretically meaningful effect size rather than on previous studies. For me an effect equivalent to a correlation of .14 or less (accounting for less than 2% of the variance) is basically meaningless in the social sciences except in the rare case that the effect has major health or economic implications. For many lines of research, an effect equivalent to a correlation of less than .2 (accounting for less than 4% of the variance) is inconsequential.

As discussed in Kennedy (2016a), studies with extremely high power (typically greater than .95) can give counterintuitive results if the p value for the outcome is significant but also is greater than one minus the power. This very rare situation is often pointed out by critics of classical hypothesis testing. The optimal practice in most cases with extremely high power is to set the alpha (significance) level for the study to be equal to one minus the power. Other options are discussed in Kennedy (2016a).

Multicenter studies can provide adequate power and optimal scientific evidence without placing a great burden on one research center. This is particularly important for parapsychological researchers who believe that prolonged testing by one experimenter group can cause the loss of the experimenter motivation and enthusiasm that is needed for successful psi results. Videos and other automated processes for interactions with participants can also alleviate concerns about declining experimenter motivation. The fact that such concerns raise serious questions about whether an ostensible psi effect is produced by the participants or by the experimenter is beyond the scope of the present paper.

Bayesian Analysis

Bayesian methods are becoming increasingly popular in psychology, but they are at a relatively early (honeymoon) stage of development. Bayesian methods currently have substantial uncertainties and potential pitfalls. These limitations will become widely recognized as experience is gained with these methods and as standards are developed (Kennedy, 2014a). One of the more prominent examples is that the common methods for Bayesian hypothesis tests have extreme sensitivity to the choice of a prior probability distribution (Kruschke, 2015, pp. 292–295, 346–348) and tend to be biased in favor of the null hypothesis, particularly for small effects and for diffuse prior probability distributions (Kennedy, 2015; Simonsohn, 2015b).

The development of operating characteristics for a statistical test as described above are needed for Bayesian hypothesis tests as well as for classical hypothesis tests (Kennedy, 2014a, 2015). The operating characteristics answer the fundamental question: If the true effect size is a certain value, what is the probability that the planned analysis will give the correct inference? These evaluations quantify the expected rates of inferential errors for a planned analysis and reveal unrecognized biases.

The evaluation of inferential errors establishes the statistical validity of a planned analysis. Statistical analysis is another component of research methodology that needs the validity of the planned methods to be evaluated and documented. Operating characteristics are expected when Bayesian hypothesis tests are used for regulated medical research (U.S. Food and Drug Administration, 2010).

Bayesian analysts who do not evaluate the expected rates of inferential errors for the predicted effect are continuing the statistical negligence that was common in the past among psychological researchers who applied classical statistics and similarly ignored power analysis. Psychological researchers have not yet widely recognized the need for these evaluations but are slowly moving in that direction. I expect that the debates about whether Bayesian or classical statistical methods are better will dissipate substantially when the statistical validity of the methods is quantitatively evaluated. In practice, rates of expected inferential errors are more important than philosophical ideas about the nature of probability.

Meta-Analysis

As discussed above, the appropriate standard for experimental evidence is that 80% or more of well-powered confirmatory studies produce significant results—which will usually make the occurrence of an effect obvious without a need for meta-analysis. Meta-analyses are most useful when adequately powered individual studies are not feasible or when deciding whether and how to pursue a line of research that has not yet obtained consistent confirmatory results.

A typical retrospective meta-analysis is similar to exploratory research because methodological decisions are made after the study outcomes are known. These decisions include which studies to include, what statistical methods to use, how to evaluate questionable research practices, and what moderating variables to investigate. These retrospective decisions provide opportunities for bias, and associated opportunities for challenging the results of the meta-analysis (Kennedy, 2013a).

A prospective meta-analysis is a form of confirmatory research because the methodological decisions are made before the included studies have been conducted (Watt & Kennedy, 2017). The statistical analysis plan for a prospective meta-analysis should be publicly preregistered before data collection begins for any of the included studies. Of course, the studies that may be included should also be individually preregistered. The registration for each study can be used to decide prospectively whether the study will be included in the subsequent meta-analysis. This process is described in the first such prospective meta-analysis initiated by Watt (2016; Watt & Kennedy, 2017). Prospective meta-analysis, with power analysis and adjustment for multiple analyses, will eventually replace retrospective meta-analysis as the preferred choice for controversial topics.

A prospective meta-analysis does not prevent or discourage research innovation or exploratory analyses during a meta-analysis. It simply prospectively specifies how a study will be handled for the preplanned analysis in a subsequent meta-analysis and clearly distinguishes between preplanned and post hoc analyses.

Statistical Dependence

Statistical dependence can be a problem in both parapsychological and psychological experiments. The basic issue is that human responses, both conscious responses and physiological measures, cannot be assumed to be independent, as is required for the dependent or outcome variable for standard statistical analyses (Kennedy, 2013b; 2014c). The effects of statistical dependence are difficult to predict, but a common effect is to make p values misleadingly small.

The traditional analysis for parapsychological experiments uses the random events as the outcome variable (Burdick & Kelly, 1977; Kennedy, 2014c). This strategy was selected specifically because the random events are independent (if properly generated) and thus avoid dependence problems. In general, an analysis that uses a t test or ANOVA with some type of human response as the outcome variable has potential for dependence problems. An analysis that uses a binomial or similar test with independent random events as the outcome variable is typically free of potential dependence problems.

The most severe dependence problems occur in studies with feedback to the participant on each trial, as occurs in presentiment studies that investigate physiological measures of precognitive anticipation of a random event (Kennedy, 2013b; 2014c). The participant's physiological responses may include reactions to the outcomes of previous trials and thus contain information about and be dependent upon the specific sequence of random targets or stimuli. This can introduce bias when the mean of the responses is found for each type of stimulus. Attempts to correct for dependence with statistical adjustments, or to do post hoc analyses to argue that certain types of dependence did not occur, are controversial (Kennedy, 2013b, 2014c). An alternative analysis that is consistent with the traditional strategy in parapsychology is to use the physiological measures to predict the random stimuli and use only data collected prior to feedback for a trial to make the prediction for the trial.

Bem's (2011) "feeling the future" experiments that had reaction time as the dependent variable are another case with potential dependence problems. However, these experiments are more complex than

the typical presentiment studies. Bem's studies involve random retroactive priming as well as random selection of stimuli, and also randomly intermixed forward (non-psi) priming trials. It is more difficult to imagine how bias from sequential dependencies could enter into the analyses of these data. However, it is also difficult to make a compelling argument that dependence problems cannot occur given the feedback on each trial.

Studies of remote influences on other persons (Schmidt, 2015) also have human responses as the outcome variable and thus have potential for dependence problems. However, these studies do not have feedback to the participant who makes the responses, which greatly reduces the potential for bias. These studies typically use the session as the unit of analysis rather than the epoch or trial, which further reduces the potential for dependence problems. Although I currently do not see potential dependence problems with these studies, the analyses are not as theoretically clean as analyses that use the random events as the outcome variable.

An alternative design and analysis for studies of remote influence has a session divided into sequential trials that each consist of two epochs. One of the epochs for each trial is randomly selected as the influence epoch and the other epoch is a control. The analysis is based on determining which of the two epochs for a trial has a response that is most consistent with the expected influence. That epoch is predicted to be the randomly selected influence epoch for the trial. A simple binomial analysis of the number of correctly predicted influence epochs would provide a theoretically clean analysis with no potential for dependence problems.

Experiments that have potential dependence problems place a cautious or skeptical scientist in the position of choosing between either psi or dependence problems as the most likely explanation for the results. Psi is unlikely to be the preferred choice. It is a safe prediction that skeptical psychologists will eventually interpret such psi studies as evidence for subtle dependence problems that need to be addressed. Parapsychological studies with the traditional analysis of random events as the outcome variable are much more difficult for scientists to dismiss.

Conclusions

The validity of all aspects of research methodology needs to be evaluated and documented for confirmatory research. This includes the validity of software and statistical methods as well as the validity of measurement instruments. Hypothesis tests that evaluate specific predictions about the experimental outcome provide the strongest evidence that researchers understand and control an effect. Quantitative evaluation of expected rates of inferential errors for the predicted effect sizes that the study is intended to detect is a fundamental component of statistical validity for both classical and Bayesian hypothesis tests. Procedures that prevent experimenter fraud are also an important component of valid research methodology, as is appropriate handling of incomplete data.

The methodological topics discussed here have not yet been widely recognized or addressed by psychological researchers. If parapsychological researchers adhere to the methodological practices of psychological researchers, they can expect that eventually skeptical psychologists will consider these methodological issues as more likely explanations for experimental results than psi. When confronted with this choice, the standards for research will be revised accordingly. In effect, this will repeat the cycle that has just occurred with the wide recognition that preregistered confirmatory research is needed. This inefficient process of methodological evolution can be easily avoided if psychological and parapsychological researchers proactively implement good research methodology.

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Broomfield, CO, USA
jek@jeksite.org

Appendix

Steps that may be useful in developing an end user validation plan for software used for collecting data are listed below. These steps are intended to give an idea of the possibilities and to stimulate thinking rather than to provide a fixed formula that is applicable in all situations.

Step 1. The validating experimenter or end user will initially familiarize himself or herself with the experimental software by acting as a participant for a few sessions.

Step 2. If the inputs to the program are from a keyboard or mouse, a keyboard and mouse recording program (such as JitBit Macro Recorder) will be set to capture keyboard and mouse events before the experimental software is run. (Some of these recording programs do not have the needed accuracy for the timing of inputs and playback—this should be tested.) If the experiment involves randomly selected displays, a video recording or video capture of the screen display (such as Replay Video Capture) will be used to capture the display. If timing is critical for the experiment, a video recording will often be preferable to software that captures the screen display (which utilizes significant computer resources and may interfere with the experimental program). If the software has settings that identify experimental runs versus control runs, the validation tests will be done with the settings for experimental runs.

A typical experimental session will be conducted by the validating experimenter. The data in the output file for the sessions will be verified by comparing it to the values captured by the keyboard, mouse, and display recordings. If a large amount of data is generated, it may not be feasible to check each data value. In these cases, it is important to verify the data for the first few trials and the last few trials in the experiment, and at the beginning and end of any runs or other subdivisions of the session. In addition, the data for randomly selected trials at other times can also be verified. If timing is important, video editing software can be used to examine individual frames and verify the timing. Also, the keyboard and mouse recording will include time between events, which may be useful information.

Step 3. If random processes are involved, as in a typical parapsychology experiment, the responses recorded for a typical session will be played back as input to the experimental software. This repetition of the session will be done at least 20 times to verify that the output appears to be random. The optimal practice when feasible would be to simulate the number of sessions in the planned experiment. If suspicious results occur, the sessions will be repeated until confidence is developed that the software either is appropriately random or is biased. The potential for interactions between the responses and random process can be evaluated by generating two sessions with extremes for the responses—such as one session with fast and/or biased responses and another session with slow and/or different biases. These sessions would be played back with the experimental software at least 20 times each (many more times if feasible) to verify random results. Unfortunately, if there is feedback for each trial and potential for biases from reactions to the feedback, the sessions testing for bias must be entered by a person.

Step 4. The validating experimenter will conduct a session that includes efforts to break the software. This includes a variety of unexpected responses, such as extremely rapid and premature responses, and slow and non-responses. Other tests include hitting extraneous keys, switching the Insert/Overwrite setting, switching the Caps Lock setting, holding down a key to make the input repeat, and exploring various other unexpected inputs. Any unexpected behavior of the software will be noted and the data output will be compared with the recordings of the keyboard and mouse events, and with the video of the display if relevant.

Step 5. In addition to these initial validation steps, testing can be done based on the results obtained during the experiment. For the sessions with the most extreme effects in the actual experiment, the responses and timing for the responses can be played back with the experimental software at least 20 times (more if feasible) to verify that the results are generally random. The optimal strategy would be to use an independent program to capture and play back the keystrokes and mouse events for all experimental sessions. However, if necessary the scripts for the playback could be developed from the data collected for the experiment.

Abstracts in Other Languages

German

IST DIE METHODOLOGISCHE REVOLUTION IN DER PSYCHOLOGIE VORUEBER ODER BEGINNT SIE GERADE ERST?

ZUSAMMENFASSUNG: Signifikante Ergebnisse parapsychologischer Experimente, die unter Verwendung üblicher psychologischer Forschungsmethoden erzielt wurden, haben Psychologen dazu veranlasst, einige weitverbreitete methodologische Mängel und die Notwendigkeit vorher registrierter konfirmatorischer Forschung von zufriedenstellender Teststärke anzuerkennen. Psychologische Forscher sehen jedoch noch nicht weitere allgemein verbreitete methodologische Mängel ein, von denen man erwarten kann, dass sie zum Anlass der Wiederholung dieses Zyklus' werden. Vor die Wahl gestellt, sich zwischen Psi und unentdeckt gebliebenen methodologischen Mängeln entscheiden zu müssen, werden Psychologen die Notwendigkeit methodologischer Verbesserungen vorziehen. Dazu zählen folgende übersehenen methodologischen Faktoren: (a) fehlerhaftes Vorgehen bei der Studienregistrierung, (b) Verzerrungen aufgrund von Abrechnern und unvollständigen Daten, (c) die Notwendigkeit einer Software-Überprüfung, (d) Maßnahmen zur Verhinderung von Experimentatorbetrug, (e) angemessene statistische Methoden zur konfirmatorischen Forschung, (f) das Versäumnis, mittels Bayesscher Analysen auf fehlerhafte Schlußfolgerungen zu achten, (g) die Mängel einer retrospektiven Metaanalyse und die Vorteile einer prospektiven Metanalyse und (h) Probleme aufgrund der statistischen Abhängigkeit der Ergebnisvariablen bei statistischen Analysen. Psychologische und parapsychologische Forscher können diesen ineffizienten Prozess einer methodologischen Evolution unter Berücksichtigung von Kontroversen über parapsychologische Ergebnisse leicht vermeiden. Forschungspraktiken, die diese methodologischen Mängel berücksichtigen, stehen zur Verfügung, und ihre Nützlichkeit für psychologische und parapsychologische Forschung wird sich am Ende erweisen. Die empfohlenen Praktiken zur Vermeidung dieser methodologischen Mängel werden beschrieben.

Spanish

¿HA TERMINADO LA REVOLUCIÓN METODOLÓGICA EN LA PSICOLOGÍA O APENAS PRINCIPIA?

RESUMEN: Los resultados significativos en experimentos parapsicológicos utilizando métodos de investigación psicológica estándar motivaron a los psicólogos a reconocer deficiencias metodológicas generalizadas y la necesidad de pre-registrar investigación confirmativa con suficiente poder. Los investigadores psicológicos aún no han reconocido varias otras debilidades metodológicas comunes que pueden hacer que este ciclo se repita. Cuando se enfrenten a la elección entre aceptar fenómenos psi o corregir deficiencias metodológicas pasadas por alto, los psicólogos reconocerán la necesidad de mejoras metodológicas. Estos factores metodológicos no contemplados incluyen: (a) prácticas deficientes de registro de estudios, (b) sesgo por abandonos y datos incompletos, (c) la necesidad de validación de los programas de computación, (d) medidas para prevenir fraudes por los experimentadores, (e) métodos estadísticos apropiados para confirmación, (f) no considerar errores inferenciales con análisis bayesianos, (g) las debilidades del metanálisis retrospectivo y las ventajas del metaanálisis prospectivo, y (h) los problemas de dependencia estadística en las variables de resultado en los análisis estadísticos. Los investigadores psicológicos y parapsicológicos pueden fácilmente evitar este ineficiente proceso de evolución metodológica causados por las controversias sobre los hallazgos parapsicológicos. Las prácticas de investigación diseñadas para resolver estas deficiencias metodológicas están disponibles y serán reconocidas como necesarias para la investigación psicológica y parapsicológica. Se describen prácticas recomendadas para abordar estas debilidades metodológicas.

French

EST-CE LA FIN OU LE COMMENCEMENT DE LA RÉVOLUTION MÉTHODOLOGIQUE EN PSYCHOLOGIE ?

RESUME : Des résultats significatifs obtenus lors d'expérimentations parapsychologiques utilisant des méthodes de recherche standards en psychologie ont poussé les psychologues à reconnaître des déficiences méthodologiques largement diffusées, et le besoin pour une recherche confirmatoire pré-enregistrée et dotée d'une puissance statistique suffisante. Les chercheurs en psychologie n'ont pas encore reconnu d'autres faiblesses méthodologiques communes qui pourront, selon toute attente, entraîner une répétition de ce cycle critique. Lorsqu'ils sont confrontés au choix entre "psi" et "déficiences méthodologiques sous-estimées", les psychologues vont reconnaître le besoin d'améliorations méthodologiques. Ces facteurs méthodologiques sous-estimés incluent : (a) pratiques déficientes en enregistrement d'étude, (b) biais liés à des données exclues ou incomplètes, (c) le besoin d'une validation de software, (d) des mesures pour prévenir la fraude expérimentale, (e) des méthodes statistiques appropriées pour la recherche confirmatoire, (f) l'incapacité à considérer les erreurs inférentielles avec les analyses bayésiennes, (g) les faiblesses des méta-analyses rétrospectives et les forces des méta-analyses prospectives, et (h) les problèmes de dépendance statistique pour les variables indiquant les résultats lors d'analyses statistiques. Les chercheurs en psychologie et parapsychologie peuvent facilement éviter ce processus inefficace d'évolution méthodologique contrainte par des controverses autour des résultats de la parapsychologie. Des pratiques de recherche qui font face à ces déficiences méthodologiques sont déjà disponibles et seront probablement reconnues comme nécessaires pour la recherche en psychologie et en parapsychologie. Nous décrivons ces pratiques recommandées pour contrer ces faiblesses méthodologiques.

SPECIAL BOOK REVIEW SECTION: DO WE SURVIVE DEATH? A PHILOSOPHICAL EXAMINATION

INTRODUCTION

By John Palmer, Editor

Several months ago, as I was looking for people to review books for the *JP*, I noted that a book by Michael Martin and Keith Augustine entitled *The Myth of an Afterlife (MoA)* came out at roughly the same time as Edward Kelly et al. came out with a sequel to their 2007 anthology *Irreducible Mind*, entitled *Beyond Physicalism (BP)*. *MoA* and *BP* deal with the same basic issue, the mind-body problem, but reach opposite conclusions. The opportunity to extend this confrontation to promote the education of *JP* readers on an issue so central to parapsychology was too good for an editor like me to pass up. I decided to frame it as a debate about postmortem survival. Even though *BP* does not for the most part deal with survival directly, the mind/body problem, a main theme of the book, is central to the survival issue for obvious reasons. Although I accept psi as a genuine Kuhnian anomaly, I am on the fence regarding survival, so I felt I could be an honest broker.

The script was as follows. I invited Douglas Stokes to write a critical review of *BP* and James Matlock to write a critical review of *MoA*. I then invited Kelly and Augustine to reply to the reviews, giving both the option to delegate some of the task to their coauthors. Augustine took advantage of the offer, hence the brief contributions by Ingrid Smythe and Claus Larsen. Kelly did not. For the first two stages, authors were free to make their own points even if they were not directly responsive to what was said in the piece they were reviewing or responding to (so long as they didn't overdo it), and there was no word limit. Kelly and Augustine responded to the latter in diametrically opposite ways, such that the replies of Augustine et al. are more than 10 times longer than the reply of Kelly. Finally, I invited Stokes and Matlock to reply to the replies, giving Matlock a limit of 15,000 words. Finally, I instructed myself to write a discussion section offering my take on the preceding contributions.

Augustine in particular was quite generous in providing page citations even when they were not required, and I decided to follow suit in my piece. When a page number is preceded by contributor initials, it refers to pages in this issue of the *JP*. If the page numbers stand alone, with no date, they refer to pages in *BP* (Part I) and *MoA* (Part II). There are a couple exceptions in which there are references to different sections of a paper by an outside author, but these should be clear from the context.

PART I: BEYOND PHYSICALISM

BEYOND PHYSICALISM: TOWARD RECONCILIATION OF SCIENCE AND SPIRITUALITY, edited by Edward F. Kelly, Adam Crabtree, and Paul Marshall. Lanham, MD: Rowman & Littlefield, 2015. Pp. xxix + 602 (hardback). \$60.07. ISBN 978-1-4422-4338-9.

THE ELUSIVENESS OF SOULS: AN ESSAY REVIEW OF *BEYOND PHYSICALISM*

By Douglas M. Stokes

Beyond Physicalism (BP) is a 631-page tome reporting the findings of a 15-year effort by an Esalen-based group of philosophers, scientists, and theologians to find a middle path between currently po-

larized scientific and spiritual approaches to the study of mind and consciousness. *BP* is a successor to an 831-page tome entitled *Irreducible Mind (IM)* published by the same group, which calls itself the Sursem group (E. F. Kelly et al., 2007).

At the beginning of his Introduction to *BP* and again in Chapter 1, Edward F. Kelly asserts that an overwhelming body of evidence supports the existence of psi phenomena that is “more than sufficient to demonstrate beyond reasonable doubt to open-minded persons who take the trouble to study it that the sheer existence of [psi phenomena] is a fact of nature with which we must somehow come to scientific terms” (p. 4).

In fact, nothing could be further from the truth. The evidence strongly suggests the opposite, namely that psi phenomena do not exist or at the very least have not been experimentally demonstrated to exist. I consider myself to be a fair-minded person and am intimately familiar with the evidence. I have read the parapsychological literature for over four decades. I reviewed the *Proceedings* of the annual Parapsychological Association conventions for the *Journal of Parapsychology* for decades, until the *Research in Parapsychology* series ceased publication. For over three decades I have served as an “internal critic,” debating both psi skeptics and psi proponents, but never coming to a firm conclusion regarding the existence of psi.

What finally pushed me over the edge and into the abyss of psi-denialism were recent revelations regarding the massive rates of experimenter misconduct and incompetence among researchers in orthodox psychology and biomedical research. If similar rates of experimenter malfeasance occur in parapsychology, one would expect to find nonzero overall effect sizes in meta-analyses, due to the presence of fraudulent researchers in the sample as well as the selective reporting of data. These small effect sizes would be expected to occur in the absence of psi and would be expected to reach statistical significance as more and more studies are added to the sample. Any meta-analysis that assumes that none of the researchers in the sample are fraudulent is based on an untenable fantasy. The same is true for any estimates of the size of the “file drawer” of unreported studies that assume that the average effect size in the unreported studies is zero. In fact, the average effect size would be expected to be negative, as significantly positive results would have been included in the meta-analysis sample and hence would never enter into the file drawer. Thus, the size of the file drawer of unpublished experiments needed to reduce the aggregate psi effect to nonsignificance is greatly overestimated in many if not most parapsychological meta-analyses. See Stokes (2014, 2015) for the details of this analysis.

Finally, if psi does not exist, one might expect to see a pattern in which initially flamboyant fraudulent investigators report novel and highly significant psi effects, establishing a new “paradigm.” This aggregate effect would then be expected to decline to nonsignificance as more and more honest and competent researchers attempt to replicate the study. I would submit that this is precisely the pattern we observe in parapsychology. For the record, I have always been attracted to the study of psi phenomena, and I am deeply disappointed by their seeming nonexistence.

If psi does exist, one might reasonably expect that in over a century of research, psi investigators would have discovered a technique for reliably eliciting psi phenomena. Alas, no such technique has been found.

Parapsychologists can take heart in the fact that a large proportion of the findings of mainstream psychologists and medical researchers are in many cases just as dubious as those in psi research and for the same reasons. For instance, in a nationwide attempt to replicate the findings of the top (most-cited) cancer experiments, the company Amgen could replicate only 11% of the papers, the company Bayer could only replicate 21% of cancer studies, and the MD Anderson Cancer Center could replicate only 45% of prominent cancer studies (Kaiser, 2015).

In an attempt to replicate 26 key psychological experiments, Brian Nosek and his coworkers were only able to replicate 10 of the original effects, a success rate of 38% (Saey, 2015).

Returning to the main theme of Kelly’s Introduction to *BP*, he states that the Sursem group is seeking a *tertium quid* (middle way) between scientific and religious approaches to the study of consciousness. He hopes that such an expanded science would recognize the genuine empirical realities underlying traditional forms of religion. He notes that a sizable fraction of the members of the American Academy of Religions appear to have embraced the current scientific orthodoxy of physicalism and are intent on explaining away most, if not all, religious experiences.

Kelly notes that the chapters in *BP* are roughly ordered from the more scientific to the more metaphysical. He suggests that the findings in *BP* point to some form of idealism (all is mental) or dual-aspect panentheism (all things are suffused with the consciousness of God, which extends beyond the physical universe) as the basis of the ultimate nature of reality.

Chapter 1 is also by E. F. Kelly, who reviews theoretical challenges to theory construction in the area of consciousness research and summarizes the arguments in the Sursem group's previous anthology (*IM*). Not one to be afraid of jumping right into the deep fray, he cites the levitations of St. Joseph of Cupertino as evidence for macro-psi.

He reviews the phenomena exhibited by mediums. It should be noted that very few methodologically sound studies of mediumship have been reported by parapsychologists. Remote viewing is another area in which sound methodology is rarely employed. Adoption of ganzfeld-like procedures would eliminate most of these deficiencies.

Kelly goes on to discuss the evidence for reincarnation, crisis apparitions, placebo effects and other psychosomatic phenomena. He states that, "for the theoretical purposes of this volume [*BP*] we will therefore assume the empirical reality of both [personal] survival and rebirth without any further discussion or argument" (p. 13). This is a pretty big first bite to swallow without any discussion, and it might require the cognitive equivalent of a Heimlich maneuver to put things right.

It should be noted that Sursem is not the only group to publish a massive tome in 2015. A group of skeptics have published a 706-page behemoth entitled *The Myth of An Afterlife: The Case Against Life After Death (MoA)*, in which they debunk the notion of personal survival.

There is an intimate relation between brain states and mental states that makes it difficult to conceive of how personal survival (survival of one's thoughts, memories, emotions, skills, etc.) could take place after destruction of one's brain. This evidence is reviewed extensively in *MoA*. Survival researchers in parapsychology for the most part ignore this evidence (or are ignorant of it), rendering their arguments empty and unpersuasive. Survival researchers should familiarize themselves with this literature. To ignore it any longer places them in a perilous position in which they appear to be scientifically illiterate in the eyes of the mainstream scientific community.

Incidentally, on the very first page of *MoA*, Keith Augustine notes that the volume addresses only personal survival and does not address impersonal survival, such as the survival of centers of pure consciousness or absorption into a "world mind." In my own analysis of the literature relating to survival (Stokes, 2014), I have deemed such impersonal forms of survival to be the most likely to occur in reality. These forms of subpersonal, impersonal, and suprapersonal survival are difficult to refute (and difficult, but perhaps not impossible, to test given the current state of our scientific knowledge).

It is too bad that the arguments put forth in *MoA* and *BP* do not "speak" to one another, as they are addressed to separate audiences. Both volumes represent a form of "preaching to the choir." This continues the counterproductive cognitive isolation between the literature of the skeptics and that of survival proponents.

Kelly argues against the prevailing scientific view that memories are based on traces in the brain.

He notes that consciousness may be divided in both a "top-down" manner (e.g., subcortical vs. cortical regions of the brain) or side-by-side manner (e.g., left hemisphere vs. right hemisphere). As further evidence for divided consciousness, Kelly cites cases of automatic writing with two hands, as well as alien-hand syndrome. (The latter is lovingly depicted in Peter Sellers' portrayal of Kubrick's Dr. Strangelove.) Kelly notes that subconscious states may manifest higher skills that are not possessed by the primary personality, as in the case of Patience Worth and cases of xenoglossy. Kelly also cites the well-known case of the Indian mathematical genius Srinivasa Ramanujan, who discovered a vast amount of mathematical knowledge, without much in the way of training in the field.

Kelly discusses the "binding problem," in which diverse brain activity is bound into a single conscious perception, which in his view is not achieved anatomically, but rather through a global neuronal workspace, a view endorsed by many mainstream neuroscientists and cognitive psychologists. It should be noted that the binding problem poses difficulties for all hierarchical theories of mind, including physicalism.

He attributes mystical experiences to direct knowledge of a higher mind. He notes that neurocognitive modules cannot function without homunculi, as they would otherwise lack what we possess, namely conscious minds. Finally he asserts that the theory of quantum mechanics shows that matter in the Newtonian sense does not exist, a conclusion that I wholeheartedly endorse.

Kelly's chapter is followed by a chapter by Paul Marshall on mystical states. He notes that both psi and mystical experiences are related to the psychological states/traits of transliminality, thin ego boundaries, and absorption.

He endorses the view that the brain and nervous system act as reducing valves or filters to screen out forms of higher consciousness and to focus attention on stimuli that are more directly attuned to the biological needs of the organism. These ideas are discussed by several of the contributors to *BP* (as well as several of the contributors to *MoA*, but of course from a more skeptical viewpoint). To avoid redundancy, these arguments are summarized below.

There are several versions of the "filter" theory (which are not often distinguished by their proponents):

1. The *filter model*, in which the brain is seen as limiting one's attention to the events that are most immediately relevant to one's biological survival.
2. The *transmission model*, in which the brain is seen as merely a transmitter for mental states.
3. The *instrument model*, in which the subject's mind "plays" the brain like a piano or other instrument.

Under the filter model, if the brain is damaged or destroyed, higher consciousness is no longer restricted and floods the subject's mind. One would expect brain-damaged subjects to manifest paranormal awareness, floods of memory, feats of high skills, and deep spiritual insights. Alzheimer's disease should make the mind clearer, rather than dimmer. Needless to say, this is not what is usually observed (except in some cases of deathbed apparitions and terminal lucidity).

The transmission model seems to be at odds with the findings of cognitive neuroscience, in which experimentally manipulated brain states often give rise to complex causal chains that involve mental events. Thus, the direction of causation is certainly not always top-down.

The instrument model, the transmission model, and physicalism all predict a degradation in communicated mental states if the brain is damaged or, Heavens forbid, entirely consumed by flesh-eating bacteria.

The occasionally reported phenomenon of "terminal lucidity," in which a patient suddenly manifests highly improved mental powers just before death, is often taken as supporting the transmission or instrument models. However, it actually contradicts them, unless the physical brain that acts as the transmitter or instrument of communication is somehow miraculously repaired before death, producing a sudden clarity in the patient's mental state. Such improvement would likely also be compatible with physicalism. Thus, one is faced with the problem of distinguishing between the transmission and instrument models on the one hand and physicalism on the other, on an empirical basis. Physicalism seems to be more parsimonious than the transmission and instrument models, in that it does not propose the existence of some sort of mental realm in addition to the physical world.

Marshall urges that theories of mystical states should not neglect psi, and vice versa. He notes that mystical experiences may give direct insights into the natural world (such as the structure of spacetime). They also point to the unity and timelessness of consciousness. Marshall does not cite the work of Lawrence LeShan (LeShan 1969, 1976, 1984; LeShan & Moreaneau, 1982) in this regard, a glaring omission. LeShan's name does not even appear in the index for *BP* (which is disconcertingly incomplete).

Marshall provides a very detailed summary of the types of mystical experience.

He cites idealism (the doctrine that the world is mental in nature) as being closest to his position on the mind-body problem. Similar views include panpsychism (the doctrine that all things are suffused with consciousness), pantheism (the view that God is in all things), and panentheism (the view that God

is within all physical things, and that God extends beyond the physical universe). Pantheism is the view endorsed by many if not most members of the Sursem group, almost to the extent that it becomes something of a “party line.”

The second chapter is by Michael Grosso and is devoted to the transmission theory of mind-body interaction, which has already been discussed. Grosso takes terminal lucidity as confirming Henri Bergson’s theory that memories are not destroyed by brain damage, but are merely rendered inaccessible. Please see *MoA* for a discussion of the overwhelming evidence that memories are stored as brain traces. This is another disconnect caused by the isolation of parapsychology from modern neuroscience. It would have been more illuminating if the essays in *BP* and *MoA* appeared in the same volume, or for a more long-term solution, in the same ongoing journal (see below).

Grosso cites the English philosopher F. C. S. Schiller’s argument that the brain is a “labor-saving” device that allows us to carry out automatic processes such as shoelace-tying and cutlery operation. In Schiller’s view, matter is not what produces consciousness, but rather that which limits it. Grosso provides a thorough history of the transmission and filter models, which have already been discussed.

Grosso cites favorably the philosopher C. J. Ducasse’s philosophy of hypophenomenalism, which holds that physical events are epiphenomena produced by mental events; thus, mind is primary and the physical world is merely a manifestation of mind. In a recent book, the physicist Max Tegmark (2014) has suggested that all mathematically possible universes are realized. In this view, God might be thought of as the ultimate mathematician, looking at his creations from every possible angle, much as a human mathematician may look at the magnificent graph of a fractal, zooming in and out to look at it from different vantage points. This might be thought of as a form of idealism.

Grosso reviews the work of the Hellenistic philosopher Philo Judaeus, who proposed that God is the mind of the universe, just as we are the minds of our bodies. Our physical minds are thus a part of a greater Mind. This should not be taken as implying that God has a human body, but that each mind contains a divine spark of God, which is a nonanthropomorphic interpretation of the doctrine that humans are made in the image of God.

Grosso closes with an endorsement of pantheism as an explanation of the mind-body relationship.

The next chapter is by E. F. Kelly and David E. Presti and is devoted to an analysis of the transmission model. They cite neuroscientist Christof Koch’s observation that subjectivity is too radically different from anything else for it to be regarded as an emergent phenomenon. Koch is a reformed reductionist who now subscribes to the philosophy of panpsychism. Kelly and Presti discuss the evidence linking transpersonal states to brain processes and personality traits. They also discuss the psychobiology of creativity and mysticism. Their review of these topics as well as of the instrument, transmission, and filter theories of the mind-brain relationship is very detailed, and I learned a lot from reading it.

The next chapter is by quantum physicist Henry Stapp. Stapp notes that quantum mechanics (QM) reinstates consciousness as a fundamental part of reality. The QM relation between psychological and physical events is basically top-down. If QM decisions are moved up the physical hierarchy to a suprapersonal level encompassing the universe as a whole, the observed mental events remain fixed in Stapp’s view. The mathematician and physicist John Von Neumann called such preserved aspects “abstract egos.” Stapp notes that the impassable gulf between objects and minds in Newtonian physics is spanned in QM. He observes that QM allows ontologically nonphysical minds or egos to inject logically required and causally effective inputs into the QM-described brain dynamics.

Physicalism comes in a great many varieties. Perhaps the most prominent version right now asserts that no laws of physics, including QM, are violated in mind-brain interaction. One might call this promissory physicalism, as it presupposes that the laws of physics, or some suitable future extension thereof, will be able to account for mind-brain transactions. Of course to the extent that such an advanced theory of physics partakes of novel entities, laws, and processes, it could be regarded as a defeat of physicalism (or at least of the promissory physicalism based on currently understood physical laws and entities).

If the extended physics postulates such novel entities, processes, and laws, these might be regarded as spiritual or mental entities rather than physical entities, especially if they incorporate the consciousness

of the observer (as in QM). QM may allow consciousness to inject logically required and causally effective input into the QM-described brain dynamics. As an example of how conscious observers might effect such a directive influence, Stapp cites the quantum Zeno effect. He argues that through repeated observation, a conscious observer could maintain a QM state indefinitely, as the QM state will be reset in each iteration. Thus the observer may delay QM vector collapse until a propitious moment, allowing top-down partial control of the brain. Stapp's argument relies on orthodox QM theory, and orthodox physicalists assert that no violations of the laws of physics will be observed in mind-brain interaction, including the statistical laws of QM. Of course, parapsychologists have amassed a vast body of evidence for psychokinetic (PK) effects on QM processes. However, as explained earlier, the existence of psi phenomena is not well supported by existing experimental evidence in view of the high rate of scientific misconduct and fraud in the scientific community at large (Stokes, 2014).

Stapp asserts that the human brain may be divided into tiny spatial regions, each containing a pure (uncollapsed) QM state. This is similar to my own theory postulating that there are likely a large number of centers of consciousness inhabiting a human (or animal) brain (Stokes, 2014).

Stapp asserts that the task of science is not to cast mental realities that are known to exist out of the scientific worldview or *weltanschauung*. Rather it is about reconciling man-the-object with man-the-subject.

Stapp notes that there are no faster-than-light (FTL) influences in classical physics, but FTL influences are required in QM (although it should be noted that such FTL influences in QM cannot be used to send a signal unless one postulates that conscious observers may psychokinetically impose a pattern on these random events). He notes that QM influence is global in nature and suggests that QM theory is akin to a form of idealistic ontology in which the primary reality is mental in nature.

He views the seemingly retroactive action of observation as eliminating records of unrealized events and alternative histories.

Stapp interprets Libet's finding of a buildup of readiness potential in the brain that precedes the subjective time of decision in voluntary choice experiments as reflecting the neural preparation that leads up to the decision to act rather than to the act itself.

Stapp then turns to the survival problem. He notes that one foundation of Von Neumann's interpretation of QM is that the thinking entity (ego) aspect of each of us is ontologically different from the QM-described physical universe. After death, such an ego could simply cease to exist. Alternatively, it could survive and have a stream of consciousness. Such an ego or center of consciousness will cease to be a factor in the physically-described world, unless some alternative mode of interaction is possible.

Stapp proposes that such egos or centers of consciousness may reincarnate or possess another human body (such as in mediumship). However, in such cases, one would need to explain how memories and personality traits could survive after the destruction of the brain.

Stapp then launches into a discussion of psi phenomena, including clairvoyance, macro-PK, and precognition, including a review of Daryl Bem's work on retroactive causation.

Stapp concludes with the observation that it is impossible for classically conceived reality to contain conscious experiences, as classical reality was purposely stripped of all conscious-like properties. He notes that QM brings mind back into the causal structure.

The succeeding chapter is by Harald Atmanspacher, a physicist and editor-in-chief of the journal *Mind and Matter*, and Wolfgang Fach, a psychologist (F&A). F&A begin by discussing the philosophy of dual-aspect theory and neutral monism, in which matter has both a physical aspect and a mental aspect, citing Spinoza as a prominent proponent of this view.

At this point, the reader's head may be spinning, trying to tease apart the philosophies of materialism, dual-aspect theory, neutral monism, panpsychism, pantheism, idealism, physicalism, and panentheism. Aren't they all the same, if observable matter conforms to the laws of physics, as it generally appears to do? There seems to be no way of empirically or even conceptually teasing them apart, at least not if the deity continues her nap. Thus, these grand theories of the mind-body problem may all be operationally equivalent to one another. Phenomena such as psi and personal survival would throw a monkey wrench into the whole

physicalist program insofar as they would violate the laws of physics or would at least be inexplicable by current laws of physics.

One form of physicalism is promissory materialism, which is the view that all mental activity will be accounted for based on current theories of physics or a suitable extension thereof. Given the current state of contradictory and muddled physical theories, I wouldn't count on this happening anytime soon. Psi phenomena are a notoriously difficult pill for promissory materialists to swallow, and that is why these phenomena have been so heavily attacked by physicalist scientists and philosophers. However, the evidence discussed above suggests that psi phenomena may not exist, or that if they do exist, they cannot be reliably elicited in experimental tests. However, if their beef is with physicalism, Sursem is correct to focus its efforts on psi phenomena and personal survival. But such phenomena are not necessary to support a belief in an afterlife. Logical arguments can be mustered in support of the survival of the soul, as shown below.

Before leaving this catalog of "solutions" to the mind-body problem, we should consider the philosophical doctrine of radical materialism, which asserts that conscious experiences and conscious selves do not exist, and epiphenomenalism, which asserts that conscious experiences and mental events are caused by physical events, but exert no influence upon them. These positions used to be widely held, for instance by Daniel Dennett and B. F. Skinner. However, these are self-refuting philosophies. The writings of radical materialists and epiphenomenalists are material events. Thus, if their theories are true, their writings cannot have been caused by mental events such as thoughts and reasoned arguments so why read them? If they have been caused by actual thoughts, this falsifies their doctrine that mental events exert no influence upon material events. Their philosophies are thus either vacuous or self-refuting. Also, mental events and conscious selves are directly experienced, and it is ontological madness to deny their existence. Most advocates of these absurd positions have abandoned them in the face of an onslaught of ridicule or have had the common decency to retire or die. These versions of physicalism are surely false. By the way, for you fledgling scientists out there, these cases illustrate that the rise to academic prominence is greatly facilitated by proposing an absurd theory and never abandoning it in the face of criticism. One's citation index and fame grow exponentially as critics pile up to attack this absurd position.

Strangely, even idealism, which denies the very existence of physical events, may be a form of physicalism, if the behavior of phenomenal objects conforms to the laws of physics as mentally implemented (presumably by some sort of mathematically sophisticated intelligence existing within a greater mind). Ditto for solipsism.

Psi phenomena would pose the greatest challenge to promissory physicalism, if they exist. However, if the behavior of ostensibly physical events conforms to the laws of physics, all the above solutions to the mind-body problem may be essentially the same for human observers and scientists. There is likely no way to test them empirically against each other at present. If new entities and processes need to be introduced to explain the phenomena of consciousness, it is possible that they may easily be assimilated into the laws of physics, and this assimilation might be taken as a victory for promissory physicalism. If such entities cannot easily be assimilated into physics, this might be taken as a strike against promissory physicalism.

This leaves out Cartesian dualism, according to which some higher cognitive functions such as thinking and memory occur in a nonmaterial mind, which in turn interacts with the physical body. Cartesian dualism would be supported over physicalism if such functions cannot be identified with brain processes or if such processes continue to be manifested after the death of the brain. Thus, the evidence for personal survival could be taken as refuting the doctrine of promissory physicalism based on the laws of physics as currently understood. However, there is overwhelming evidence that mental events are intimately dependent on brain processes, summarized in *MoA*, which renders the survival of personality elements after the destruction of the brain implausible. These comments are directed at the hypothesis that elements of one's personality survive death but leave open the possibility of nonpersonal survival of centers of pure consciousness, as discussed above.

Returning to A&F's chapter, they discuss the principle of complementarity, as proposed by Wolfgang Pauli and C. G. Jung, in which two seemingly incompatible views, such as the particle and wave theories of elementary particles, may both be valid in their respective domains.

A&F assert that measurement in QM may be seen as an intervention, decomposing a system constituting an inseparable whole into locally separable parts. They distinguish between ontic states and epistemic states. Ontic states are grounded in a holistic conception of reality that is empirically inaccessible. Epistemic states are grounded in an operationally accessible reality. A&F observe that in mainstream physics, consciousness is not involved in QM measurements. They cite Pauli's remark that the "autonomous action of the soul [is] something that is objectively psychical that cannot and should not be explained by material causes" (*BP*, p. 202).

A&F note that Jung saw reality as "psychoid in nature," placing him in or near the idealism camp. They assert that within a dual-aspect framework, it is wrong to interpret mental states as caused by material states or vice versa as mental events are simply physical events experienced from within.

A&F note that people have a model of their selves and a model of the world, which they compare to the dichotomy of mind and matter. The self-model can be accessed only internally. Although it is true that only the subject has direct access to inner events, external observers may have indirect access, such as through introspective reports.

The next chapter is by Bernard Carr and is devoted to hyperspatial models of matter and mind. Carr proposes that one's inner phenomenal world and the material world are different cross-sections of a five-dimensional space. He provides an overview of hyperspatial models, including Plato's cave, theories of higher dimensions in modern physics such as the Kaluza-Klein models and brane worlds, the theories of J. W. Dunne, C. D. Broad, John Smythies, Jean-Pierre Jordan, Targ, and Rauscher, and the dubious theory proposed by William Tiller that access to higher-dimensional realities are characterized by different rates of "vibration."

Of these theories, the most developed is the eight-dimensional spacetime model proposed by Elizabeth Rauscher and Russell Targ (R&T). As none of these hyperspace models offer any way of empirically measuring separation in the hyperdimensional spaces, these theories are untestable. R&T's theory is based on elementary mistakes in the algebra of complex numbers and a misinterpretation of space-time intervals in Minkowski spacetime (see Stokes, 2011, for a recent commentary). R&T have not corrected these mistakes, although I have pointed them out many times over the years.

Carr then discusses his own model. He notes that the experience of "time flow" is not accounted for in modern theories of physics and suggests that QM may be of help in this regard. He suggests that there may be a hierarchy of QM sensors associated with different "actuality planes."

Carr proposes that a "specious present" may be associated with 40-Hz oscillations of the brain (commonly proposed as a means of binding neural events into holistic perceptions). The concept of a specious present or "atom" of subjective time has been proposed by E. Robert Kelly as well as by William James. Multiplying the time interval associated with a 40-Hz oscillation (a "specious present" of 0.025 seconds) by the speed of light, Carr computes 10,000 km to be the distance associated with the specious present. This distance is the furthest a *classical causal signal* can travel in 0.025 seconds. Carr asserts that psi phenomena may occur within this distance (but why should psi signals be limited by the speed of light?). This gives rise to a hockey-puck-shaped "atom of subjective spacetime" with a height of 0.025 seconds and a radius of 10,000 km. He proposes that memories of physical events involve direct access to those events. This is somewhat confusing in view of the fact that Carr limits the specious present to 0.025 seconds.

Carr proposes that people's dreams occur within their own spacetimes. He asserts that apparitions may be located within normal space or may inhabit their own space, which he compares to Myers' metetherial space or H. H. Price's psychic ether.

The next chapter is by Gregory Shaw, a theologian, and is devoted to a consideration of Neo-Platonic philosophy. He notes that seemingly supernatural phenomena have been manifested by Neo-Platonic philosophers and that self-transformation is a central process in ancient philosophical traditions, as is the development of supernatural powers.

Shaw notes that Plotinus, the founder of Neoplatonism, recognized three levels of reality: (a) the One and the Good, which is beyond Being and is the hidden source and font of reality; (b) Being-Mind-Demiurge, the active principle of intelligence that shapes the current of divine power; and (c) the World Soul,

which is the manifestation of the Divine Mind as a living cosmos and the stream of physical reality.

Shaw states that the Plotinian procession from the One might be imagined as a series of expanding circles, originating from and returning to their source. As the One has no duality, it cannot be discursively known. This is too bad for those of us who are discursive knowers (including, one would think, Shaw, who discourses about the One).

Shaw notes that Plotinus assimilated the Gnostics' distrust of matter. The Plotinian soul deigns to never enter a material body, as the evils the soul encounters are caused entirely by matter.

Proclus viewed the gods as formless but noted the incapacity of potential viewers to imagine the formless without a form, which explains the popularity of religious icons and imagery.

Shaw notes that the physicalistic worldview under which we operate is born of Enlightenment rationalism and asserts that "the Cartesian split between mind and matter has drained the world of what the Neo-Platonists—in their imaginative-capacity—experienced as a living and breathing soul" (p. 306). He notes that unlike the Abrahamic religious traditions, Neo-Platonism does not have a Supreme Being, rather the One is the source of all being. A similar doctrine is on offer from Hinduism. Shaw's chapter provides a good, comprehensive review of these topics.

The ensuing chapter is by Edward F. Kelly and Ian Whicher (K&W) and is devoted to Patanjali's Yoga Sutras. They cite the philosopher Frank Dilley's observation that in the Samkhya tradition everything is material except pure awareness. They note that the siddhis (supernormal powers) claimed in Patanjali's Sutras include psi and extreme psychophysical influences. K&W note that parapsychologists have confirmed that meditation is a psi-conducive state. They view Patanjali's denigration of psi powers as a mistake. They note that he adopted a filter theory of mind-body interaction.

They go on to discuss mystical and contemplative traditions in Catholicism, including documented psi phenomena in the lives of saints. K&W's discussion of these topics is very detailed.

The next chapter is by Lorilai Biernacki, who discusses Abhinavagupta's (A's) Tantric teachings. Biernacki is the sole female contributor to *BP*. There are no Asian or Hispanic contributors, although there are two chapters devoted to Asian religions.

Readers will need a strong stomach for Sanskrit terms to work their way through Biernacki's text in places. We will avoid most of them here.

Biernacki notes that A adopts a position of panentheism in which the Divine permeates all things in the universe, as well as some things that lie outside the universe. She reviews A's teaching regarding the subtle body, which can affect material realities in ways that are outside the agency of the physical body. Biernacki discusses Tantric doctrines relating to reincarnation and disembodied entities. She notes that the attainment of siddhis is a primary goal in most Tantric traditions. A's Tantrism recognizes the innate nature of the self. She notes that under A's Tantrism, it is the subtle body that survives death and reincarnates. The subtle body is also less limited by space and time than is the physical body, allowing the manifestation of siddhis or supernatural powers. A tells us that the basic substance of what we perceive as physical matter is a nonmaterial and fundamentally conscious principle. Biernacki states that lower regions of the mind are treated as matter in Indian philosophy. The true basic distinction is between conscious and sentient events on the one hand and insentient material events on the other. Biernacki notes that there is a blurring of the distinction between consciousness/sentience and the material world in A's philosophy. In A's philosophy, mind operates nonlocally on matter, without intervention of local physical forces or causes. Mind and matter are seen as expressions of the same impulse. It is the consciousness of the experiencing subject that creates time as well as the possibility of cause and effect. Such acts of creation in A's theory are similar to the role of the observer and the notion of "becoming" in QM. In A's philosophy, the physical world is a kind of mental reality or consciousness. The material and the mental are fundamentally convertible to one another. The impetus toward matter entails a loss of indeterminacy. The mental may have large scale effects on the physical, resulting in the manifestation of siddhis, or supernormal powers. Biernacki claims that macro-PK (macroscopic psychokinesis) requires that there is no articulated sense of self within a body. She cites the Vedanta metaphor in which the ocean (Brahman or World Mind) gives rise to waves (individual selves). She notes that A's Tantric philosophy is a form of panentheism, and she compares the view in A's Tantra to

Stapp's model of mind-matter as described above. In particular, she compares Stapp's quantum Zeno effect, which requires sustained focus on a physical process, to the meditative technique of concentration.

I should point out that I generally subscribe to the philosophy of panentheism endorsed by the majority of the contributors to *BP*, but as an amateur theologian rather than a scientist. However, you can keep the subtle bodies, in view of the general failure to detect such bodies. I would be hard-pressed to propose empirical tests or even conceptual distinctions between many of the philosophical positions mentioned above. For instance, is the God of panentheism immaterial or physical? Perhaps these categories should be dropped in favor of discussion of the roles of the deity, consciousness, and matter. I am, however, ready to jettison radical materialism into the deep abyss from which it came. Fortunately, this philosophy has largely been jettisoned by the academic community at this point. May it never rise from the dung heap again.

Biernacki's chapter is followed by a discussion of a proposed Neo-Leibnizian model by Paul Marshall. Marshall presents a theory with two components: a reservoir of events that could potentially become conscious, and a filter that determines which events will become conscious. He proposes that the reservoir may house centers of consciousness, but that these are generally subliminal or subconscious, as they lie below the threshold of everyday awareness. I too have proposed the existence of such centers, which I call microsouls, minisouls, macrosouls, and megasouls (Stokes, 2014).

Marshall notes that monist systems such as idealism, dual-aspect theory, and neutral monism are of particular interest, as they challenge materialism by taking consciousness, mental perceptions, experiences, and feelings to be characteristic of the world at large. They may shed light on the furthest reaches of the subconscious beyond the individual minds and may help to explain supernormal phenomena, in Marshall's view.

Marshall notes that, in his philosophy of monadology, the famed mathematician and philosopher Gottfried Wilhelm Leibniz proposed that the world consists of monads or centers of consciousness. Marshall notes that Leibniz's monads are not composite and cannot be created or destroyed. Monads have a common origin in God. Leibniz asserted that, as well as being indestructible, monads have complete perceptions of the universe. This contrasts with the panexperiential approach of Alfred North Whitehead, who proposed that the world is composed of "occasions" or fleeting experiences. Each occasion arises from the occasions that precede it and dies upon giving rise to the occasions that follow it. There are no permanent souls in Whitehead's "process philosophy." Marshall classifies Whitehead's theory as a form of panexperientialism, and he sees his own contribution as a transformation and modernization of Leibnizian metaphysics, calling it a "Neo-Leibnizian" approach to explaining consciousness as well as psi phenomena.

In process philosophy, God is seen as creating and sustaining the universe. He does so from many perspectives within the world (each of which corresponds to a monad). Each monad has its own path of development and is associated with its own train of thoughts and perceptions, and both God and these monads see the universe from all perspectives. It is not to be supposed that monads are positioned in space or undergo changes in time. Each may be thought of as a center of pure consciousness. This Neo-Leibnizian monadology bears a similarity to cosmologist Max Tegmark's view that all mathematically possible universes are created, each of which may be explored by conscious observers (Tegmark, 2014). Each monad carries within itself something of all its past and future states.

The hierarchy of monads associated with a person may amplify and organize perceptions. Most primitive monads have basic bodies and are unconscious, giving the appearance of inert matter, although groups of simple monads are endowed with perception and appetite (desire). He notes that matter has mental properties, as opposed to the Cartesian view that matter is extended substance lacking consciousness (vs. mind, which is unextended in space).

There are causal processes associated with monads. Marshall notes that Leibniz's monadology is a form panexperientialism, "panperceptualism," or panpsychism, and he notes that monads provide a solution of the "binding problem" (how diverse sensations are bound into a single perception). Monads are complete wholes, each an expression of the universe in its totality. In Marshall's view, monads interact with one another. However, he rejects Leibniz's view that monads are placed in pre-established harmony by God (which is one of the main reasons I myself have balked at endorsing Leibniz's theory).

More sophisticated monads have sensory organs. Central or dominant monads are called “minds,” “rational souls” or “spirits.”

Marshall turns to the subject of psi phenomena. He notes that the philosopher H. H. Price referred to Leibniz’s concept of latent omniscience to explain psi. This omniscience is latent because much of this knowledge is subconscious. Only God is truly omniscient.

Marshall notes that Leibnizian monadology renders paranormal cognition normal.

He discusses H. H. Price’s distinction between the radiation hypothesis of psi phenomena and theories that appeal to direct acquaintance. He notes that Price’s theory involves a two-stage model, in which direct contact with the paranormal target is followed by the emergence of the paranormal knowledge into consciousness. This emergence is holistic. An ESP target card is not perceived atom-by-atom, but rather as the whole card. Psi perception involves direct acquaintance with the target at a subconscious level. He notes that Price proposed that this direct acquaintance takes place in the common unconscious, or shared unconscious regions of the participants’ minds.

Price asserted that the right question to ask is not how psi is possible, but why it does not occur all the time. With regard to Bergson’s filter theory of memory, we should ask not why we remember so little, but why we remember anything at all.

Price observed that clairvoyance in a monadological universe is essentially telepathy. He noted that retrocognition and precognition are explained by the fact that the present states of monads are filled with their pasts and pregnant with their futures. Marshall notes that, in a monadology, psychokinesis may be construed as a telepathic process rather than the force of “mind over matter.”

Marshall observes that monadology implies knowledge of a universal mind, as each monad has subconscious knowledge of the rest of the universe. Thus, mystical experiences are not surprising in a monadological universe; however, Leibniz was not a mystic and eschewed mystical approaches to knowledge. Marshall suggests that the Leibnizian view of the world as consisting of a few relatively distinct perceptions supported by a cosmic sea of indistinct perceptions should be overturned in favor of the view of the cosmos as composed of islands of indistinct perceptions surrounded by a sea of perfectly distinct perceptions. Marshall states that the Neo-Leibnizian approach he advocates is a form of idealism and panpsychism, and that all monads are perfect minds.

Under the Neo-Leibnizian view, phenomenal experience is already in the brain. Base matter has qualia (sensations, experiences, etc.), whereas under the radical materialist view, matter does not have felt qualities. This is in contrast to neutral monism, the philosophy embraced by Bertrand Russell, in which matter does have felt properties and qualia.

Marshall notes that the noumenal experiences reported by mystics encompass past, present and future, while phenomenal experiences have a transient quality. Each moment of a phenomenal experience is confined to a specious present. Marshall notes that while “we are fully conscious of everything in the universe at a subliminal level, we are conscious of very little at the supraliminal level because the phenomenal field of experience is very narrow” (p. 408).

Marshall then turns to the survival problem. He notes that Leibniz viewed monads as simple substances without parts, which can neither be created nor destroyed. The physical body, however, is composite and thus subject to dissolution and death. As monads are filled with the past, personality traits and memories might be expected to survive death at a subliminal level and might be reincarnated, as the monad is absorbed by another body.

The next chapter is by Adam Crabtree, who reviews Charles Sanders Pierce’s metaphysics. Pierce regarded matter as a more specialized and partially deadened form of mind. He viewed physical laws as habits a la Sheldrake or, perhaps more flatteringly, symmetry-breaking in modern physics. Pierce noted that the brain houses no central cell. He asserted that the unity of consciousness is not of physiological origin. Such unity must therefore be of metaphysical origin. Pierce contended that an evolutionary philosophy requires a personal creator. He notes that a human’s circle of society is a sort of loosely compacted person. Pierce distinguished between mind and consciousness. He asserted that mind is present in all reality, but consciousness is present only in higher animals. Crabtree views Pierce as a panentheist, which may be

difficult to square with this restriction of consciousness. Pierce contended that feeling is operative in protoplasm, as it is governed by teleological or final causes and exercises all the functions of mind. Pierce viewed death as a vacation, a time of freedom from the physical world. He accepted the existence of apparitions, but regarded them as mere shades. Souls, he asserted, are ghosts of their former selves.

I found most of Crabtree's chapter and his explication of Pierce's theory as well as the direct quotes from Pierce to be largely unintelligible.

The next chapter is by Eric M. Weiss, who explicates a view that he calls transphysical process metaphysics (TPM). Weiss asserts that psychokinesis is involved in every bodily action we take. (Please note that such "internal PK" is not generally considered to be a form of psi.) He rejects idealism, epiphenomenalism, and dualism in favor of a process-oriented Whiteheadian panexperientialism. In this, his approach is similar to that of David Ray Griffin (1994a, 1997). Weiss calls the worlds experienced in dreaming, lucid dreaming, and life after death "transphysical worlds." He views the everyday waking world as a very rigid and highly constrained dream. After death we survive in a transphysical body in a transphysical world. In the transphysical or parapsychological view, our inner activity is a direct causal expression of the thoughts and feelings of those around us in the waking world, as opposed to the isolated minds of the normal scientific view.

Weiss advocates a version of Whitehead's process theology in which elementary events, or "occasions," reflect the past and determine the future. Events are no longer classified as objective or subjective. Each of us is an actual occasion of experience. Every past event is an actual occasion.

At this point, I will state the reasons for my own rejection of Whiteheadian process theology. In Whitehead's theory the world is composed of fleeting events, with no provision for a continuing self. However, I seem to be directly aware of myself as a center of consciousness that persists over macroscopic time intervals. I would submit that this knowledge is direct and infallible. In a universe such as Whitehead's in which nothing persists for more than a nanosecond, there is no room for a self that persists for at least the few seconds it takes my brain to paraphrase Descartes, saying that "I sense this and then that, and therefore I am" (at least for a little while longer than a nanosecond or the "specious present" granted me under the Whiteheadian view). For much the same reason, I reject the Buddhist doctrine of No Soul (also called No Self or No Mind).

Incidentally, I have proposed an alternative model of consciousness based on a hierarchy of centers of pure consciousness many times over the past two decades. This theory differs from the model proposed in *BP*. See for instance my last two books (Stokes, 2007, 2014). Yet none of the contributors to *BP* cite these books or any of my other writings on the subject over the past decade. Unlike their models, mine does not rely on the existence of psi phenomena or the survival of personality elements after death, both of which are overwhelmingly rejected by modern scientists. As pointed out in Stokes (2014) and in *MoA*, the intimate dependence of mental events on brain states that has been discovered by modern neuroscientists makes it extremely implausible that personality elements could persist after the death of the brain. As shown in Stokes (2014, 2015), the experimental evidence for psi phenomena can be explained on the basis of the very high rates of fraud, data selection, and misconduct in the general scientific community. Thus, psi phenomena and personal survival provide shaky ground on which to base a general model of mental processes.

My hierarchical model involving centers of pure consciousness does not depend on the existence of psi phenomena or the survival of personality elements. What survives death in my model are centers of pure consciousness that do not carry personality traits such as memories. Thus, my model avoids the cost of appealing to phenomena that are overwhelmingly rejected by the scientific community (and with good reason). The version of panentheism I am peddling comes free of charge, as there is no paranormal price to pay. The "God" part of my panentheism is comprised of a nonanthropomorphic agent or group of agents existing in a realm that lies beyond our current ken. I am open to the possibility that the set of gods is the null set. Whatever creative agents exist, I do not believe they "keep a finger in the pie" by micromanaging what to them must be the lives of infinitesimal creatures. Thus, when it comes to God, I would more accurately be described as a deist (or atheist) than a theist. (Deist gods do not intervene in the universe after its creation.) Thus, in this respect, I might more accurately be described as a panendeist, rather than as a panentheist. We're on our own down here. For now.

The centers of consciousness in my model, unlike the occasions of Whitehead's process philosophy, extend over macroscopic time intervals. Their scope may be restricted to an elementary particle (microsouls) or may encompass the entire human race or even the universe itself (megasouls).

In my books, I suggest that these centers of consciousness may be un-soul-like in that they may be continually leaving one physical system and entering another, much like an oxygen molecule is breathed out by one person and absorbed by another. After entering a new body, such a center of consciousness may quickly fall under the delusion that it has been there all along, as it carries no memories of its former existence in the previous body. The reason that most of us believe that we are a soul embedded in our present body for life is likely due to a misidentification of oneself with the Person, which encompasses both the body and its phenomenal experiences. I have long argued that you are not your body, as the atoms that comprise your body are replaced from day to day, while you seemingly persist. Neither are you your emotions, memories, and beliefs, as these aspects of the personality change from day to day, while you persist. However, the fact that you are trapped in a physical body (if only momentarily) suggests that you are at least in part physical or are at least capable of direct interaction with matter, which brings you within the realm of matter. If an empirical theory of such interaction is developed, this could be construed as a victory for physicalists (even if the laws of physics have to be slightly modified) or a victory for the opponents of physicalism (for instance, if such laws refer to entities and principles that fall outside those known to current physicists).

Returning to Weiss' chapter, what seems missing from his account is the continuity of experience over macroscopic time intervals. Weiss reviews the work of other predecessors of his own view, including Henri Bergson, Pierre de Chardin, Sri Aurobindo, and Ken Wilber. He notes that Bergson, in contrast with Whitehead, puts greater emphasis on the continuity of, rather than discontinuities in, experience. Weiss states that through a personally ordered society of occasions, one can consider oneself to be an enduring object. However, collections or aggregations of entities are not generally considered to be enduring objects (consider a decaying body, for instance).

Weiss notes that personally ordered societies (of monads) need not trace continuous trajectories through spacetime. They may leave one location in spacetime and re-emerge at a remote location with varying degrees of continuity of memory and purpose. That's right, even though Scotty and you are dead, he can still beam you up (may you rest in peace among the stars, James Doohan).

Weiss distinguishes between high-grade and low-grade occasions, such as those reflecting an animal and those reflecting an electron. This corresponds roughly to the distinction between macrosouls and microsouls in my own ontology.

Weiss contrasts Whitehead's process theology with his own transphysical process metaphysics (TPM). He compares an atom to a cycling system of subatomic particles that cannot be actualized for any duration shorter than the cycle. He proposes that the length of time an entity may exist increases with the grade (level) of the occasion. In contrast to Whitehead, he proposes that "societies" of high-grade occasions may exist on their own entirely, in the absence of lower-grade occasions, with which they may sometimes interact. Weiss calls such higher-order worlds astral and vital worlds (if composed of medium-grade occasions), and mental worlds (if composed of higher-grade occasions). Weiss suggests that even medium-grade occasions, including those of biological cells, survive the death of their bodies. He proposes that higher-grade occasions existing apart from their physical bodies may interact, giving rise to worlds of experience that are in no way dependent on the physical world. Such worlds are creatively more powerful than the physical world. Weiss suggests that such worlds may precede the physical world and may incorporate entities that can be found nowhere in the physical world.

Weiss' TPM theory holds that medium- and high-grade occasions are not part of the physical world. For instance, the medium-grade occasion presiding over a biological cell belongs to the vital world and is not detectable by scientific instruments.

He asserts that higher-grade occasions are naturally telepathic, although this telepathic capacity is limited to images and feelings. He notes that if the participating occasions are restricted to insensate atoms, they cannot account for empathy and telepathy.

Weiss notes the parallels between his TPM theory and quantum mechanics (QM). Quantum events, like occasions, reflect the past and determine the future. Under TPM, psychokinetic events involve higher-order occasions imposing their will on lower-grade occasions outside of the body. Weiss asserts that neither vital (astral) worlds nor mental worlds are dependent on low-grade physical occasions, thus seemingly cutting mind off from matter altogether!

Weiss notes that in contrast to the process philosophy put forth by David Ray Griffin, under TPM all medium and higher-grade occasions never do exist in the waking physical world. He notes that because matter is held to have no characteristics other than those that can be mathematically expressed, there is no reason for matter to exist (kind of like short people in Randy Newman's cosmology).

He asserts that the astral and mental worlds of TPM render personal survival of death and even reincarnation possible.

The next chapter, by Ed Kelly, summarizes the book. He describes the models produced by the Sursem group as "Resonant Opening to Subliminal and Transpersonal Assets," or ROSTA. He states that the group has chosen the acronym ROSTA as it is not easily parodied and has no misleading or irrelevant connotations. This acronym is thus carefully and meticulously designed to protect the group from ridicule. (Perhaps "ROSTA-farians" is the word they were looking for.)

After rolling out the carefully-designed new acronym, Kelly discusses the observational theories in parapsychology. He notes that matter and mind can no longer be sharply distinguished in modern science and that there may be gradations between these two poles. He discusses subtle bodies and proposes that it is the subliminal self, or collective mind, of Myers that survives death. However, this is really a form of suprapersonal survival. My own hierarchical model (Stokes, 2014) provides a view closer to personal survival than this. Kelly notes that Gardner Murphy also proposed a form of survival in a collective or group mind.

Kelly proposes that consciousness may even be extended to plants. My own model (Stokes, 2014) which Kelly does not cite, also extends consciousness to plants.

He notes that William James proposed a version of "pluralistic panpsychism," which James preferred to idealism, which he called an unintelligible pantheistic monster. James resisted the interpretation that the higher self is God, preferring the view that evil originates from outside God. Instead, he embraced a form of pluralistic panpsychism, which he thought was not in conflict with science.

Kelly reiterates his support for panentheism, which he notes has been gaining traction in theological circles. He notes that panentheism is not a single, unified doctrine, and strands of it are present in all the major faiths. It is a doctrine that is more compatible with science than traditional views. In panentheism there is no such thing as matter as classically conceived. (I will note that there is no room for it in modern physics either.) Kelly notes that the Jung scholar Roderick Main has asserted that Jung's analytical psychology implicitly advances a panentheistic metaphysics. He notes that some form of idealism or evolutionary panentheism has emerged as Sursem's "central tendency." He calls for more investigation of spontaneous cases of psi. To paraphrase Smokey Robinson, I will second that promotion. However, one does not need psi to establish panentheism. Many have arrived at panentheism through philosophical and theological arguments that do not make recourse to paranormal phenomena.

The final chapter is by Michael Murphy, who presents an argument for panentheism. He sees panentheism as an emerging canon, which lurks in margins of academic science and religion. He sees panentheism as being evolutionary in nature and as an increasing manifestation of the Divine.

Murphy provides a review of the history and development of panentheism from a Christian slant. He cites Friedrich Oetinger's view that God emerges from Himself and returns to Himself. Murphy reviews the panentheistic philosophies of Sri Aurobindo and Ken Wilber. He discusses some Eastern views, including those of the yogic and meditative traditions. He asserts that the cosmic evolution of panentheism has a future that stretches beyond the mind's reach. (But will it remain after the sun becomes a red giant and swallows the Earth a few billion years from now? That is like next week on a cosmic timescale.)

Murphy invokes post-rationalist deconstructionists such as Jacques Derrida, whose views help to free us from the crippling habits of rational thought. He does, however, praise the increasing rationalism across world cultures. He notes that faith is becoming increasingly fact-based. He cites psi phenomena as

supporting the pantheistic worldview. He notes, however, that psi research and the study of mystical states are still rejected by the reductionist establishment. Given the present state of psi research, basing an argument for pantheism on psi is somewhat like throwing a spitball at a tsunami. It is no doubt more reassuring to view oneself as a piece of God rather than as a temporarily existing throbbing heap of atoms. However, if we are indeed God wandering in the desert of the physical world, we've got a bitching case of amnesia.

In summary, in *BP* the members of the Sursem group are led to endorse a philosophy of pantheism after examining a wide range of scientific, philosophical, and theological evidence. Evidence for personal survival of death (survival of personality traits such as memories, emotions, attitudes, skills) include such phenomena as past-life memories, and mediumistic communications. To the extent that this evidence involves the unexplained transmission of information, it could be explained by psi (assuming psi exists, which has by no means been proven). One step that would bring us closer to personal survival without psi would be to assume that some personality elements are retained in a trace such as the psychophore proposed by Ian Stevenson (1987) or the ψ -trace proposed by the philosopher C. D. Broad (1925). Such traces may be pictured as a collection of psychic flotsam and jetsam floating in the sea of a collective mind or even in the physical world itself. However, even if the existence of such traces is thought to bolster the survival hypothesis, an anomalous flow of information would still exist, and psi by any other name may smell no sweeter to the mainstream cynic. However, one does not need to accept psi phenomena to embrace the philosophy of pantheism. Many philosophers, theologians, religious leaders, and scientists subscribe to this philosophy without basing their argument on the existence of psi. I am one of them. I am also an adherent to panpsychism, pantheism, panentheism, panendeism, idealism, dual-aspect theory, solipsism, and non-ridiculous materialism. After intensive study over many years, I can no longer clearly see the differences among these positions. Due to the lack of any compelling evidence, I do not believe in the survival of personality elements (especially in light of the overwhelming body of evidence that personality elements are intimately dependent on brain activity). I also no longer believe that the existing body of experimental evidence establishes the existence of psi, in view of the high rates of experimenter misconduct in science in general, which are sufficiently high to explain the meager body of significant results that have been reported in parapsychology (see Stokes, 2014).

Pantheism and physicalism are not incompatible. In fact, it is likely that most people who describe themselves as pantheists (a word that likely falls outside the vocabulary of the non-cognoscenti) do not believe that their philosophical stance requires that the laws of physics be violated.

The physicalism which the Sursem members reject would seem to be promissory physicalism, the assertion that the laws of physics as they are currently understood or some minor modification thereof will be sufficient to explain mental activity. Of course it is widely recognized that these laws cannot explain, and in all likelihood will never be able to explain, such elemental facts as the existence of consciousness.

This explains Sursem's focus on such phenomena as psi and personal survival, as these phenomena are inexplicable by the current laws of physics and thus would be evidence against promissory physicalism. It should, however, be recognized that even Newton's deterministic billiard-ball universe is compatible with pantheism. As for the problematic "God" part of pantheism, discard the guy with the white beard and replace Him with a nonanthropomorphic cosmos generator, or with a committee of disembodied intelligences that somehow establishes the laws of physics for a (perhaps uncountable) multitude of universes and then somehow breathes life into them, so that these cosmoses, in the words of Steven Hawking, "go to the trouble of actually existing." These creators of cosmoses might then get lost contemplating the beauty (and exquisite horror) of each of their cosmological progeny. Panpsychism and hence pantheism finesse the intractable problem of how consciousness arose from insensate matter. It didn't; it was there all along.

Although the Sursem members provide thorough (albeit repetitious) coverage of the monadologies of Leibniz, Griffin, Whitehead, and others, they strangely avoid discussing my own very closely related but distinct model of a hierarchy of centers of pure consciousness. I suspect that this is because I am extremely skeptical of personal survival based on the vast body of evidence relating mental states to brain states amassed by neuroscientists over the past few decades. I am also skeptical of the experimental evidence for

the existence of psi based on meta-analyses, as such analyses are generally based on the assumption that none of the researchers are fraudulent. Given the vast amounts of scientific misconduct recently uncovered in the orthodox scientific communities, this is a preposterous assumption.

I abhor the coining of new terms in psi research, as this provides only the illusion of progress (think of the terms for psi introduced over the years). However, the struggle of memes for intellectual ascendance is no doubt greatly facilitated by a catchy name. Despite the fact that my hierarchical model of consciousness is directly related to, although different from, those presented by Sursem and has been widely published in the “mainstream” parapsychological literature, Sursem does not cite it. Indeed, my work is likely the only such directly related material to appear in the parapsychological literature in recent years. Yet, Sursem chooses to ignore it completely. Presumably this is because the Sursem members do not like, or do not know how to address, my skeptical arguments regarding personal survival. Neither do they address the vast body of evidence demonstrating an intimate relation between brain states and mental states, as documented in detail in *MoA*.

The *BP* authors have sometimes cited Bergsonian filters in rebuttal of opposing views, but this is not an adequate argument for dismissal, as discussed above. Indeed, it would be expected that there would be a great onrush of spiritual experiences and higher thoughts if the filter (brain) is damaged: the more the damage, the greater the onslaught. But this is not what is observed. The parallel publication of *MoA* and *BP* is symptomatic of the great divide between modern day parapsychologists and skeptics. Sursem seemingly prefers to return to the 18th Century views espoused by F. W. H. Myers rather than to deal with the last century of neuroscientific research. In fact, a CD of Myers' *Human Personality and Its Survival of Death* was included in Sursem's previous tome (*IM*).

It should be noted that tremendous progress has been made in our understanding of the mind-brain relationship since the time of Myers. For instance, I was a teaching fellow in an introductory psychology course at the University of Michigan run by James V. McConnell, who was famous (or perhaps notorious) for his theory that memories are encoded in molecules rather than in the structure of synaptic connections. McConnell claimed that one can teach a flatworm a trick, such as running a maze, grind it up, and feed the dust to another (conveniently cannibalistic) flatworm. This resulted in the cannibalizing, naive flatworm learning the trick much more quickly than would be expected. McConnell was a flamboyant orator and even published his own, often hilarious, journal, the *Worm Runner's Digest*. McConnell became an academic pariah when his results proved to be unreplicable. At that point in time, it was very unclear how memories were stored in the brain or whether they were stored in a material system at all. The prominent psychologist Karl Lashley was famous for his celebrated failure to locate any physical memory traces at all (probably due to the fact that memories are distributed globally in the brain rather than in localized traces).

When I was still a graduate student at Michigan, I taught a course called Physics and Parapsychology, taking the position that memories might well be stored outside of the brain, which was based on the then-current current body of evidence. Now we know that our brains are productively filled with Jennifer Aniston cells, a fine use of neural resources if ever there was one.

There exists a vast communication gap between parapsychologists and their critics. Skeptics primarily read and publish in skeptical journals and books, and parapsychologists primarily read and publish in parapsychological journals. Each side progresses in blissful ignorance of the other. This is not a situation that is conducive to a productive dialogue. At times, it seems that it is almost a definitional requirement that parapsychologists believe in psi or personal survival. However, I still consider myself a parapsychologist because I study ostensibly paranormal phenomena. Similarly, based on the depth of his work, I would consider Ray Hyman a parapsychologist, although it is doubtful that he would welcome an embrace into the fold.

In fact I believe in life after death, which likely puts me at the extreme left wing of psychical research. However, I believe in the survival of centers of pure consciousness, a subpersonal form of survival that the *MoA* editors specifically exclude from their analysis at the outset, likely because it is difficult to refute. For those who cling to the hope that the personality will survive death, I should tell you that I have found the prospect of nonpersonal survival to be exhilarating, and yet at the same time counterintuitive. Try it on, you may like it.

The same situation occurs within established academic fields. Theologians, religious leaders, philosophers, neuroscientists, psychologists, shamans, and laypersons have largely produced isolated islands in the sea of knowledge, with little in the way of communication among them. Indeed, the specialized jargons of their academic specialties often render the writings in one academic discipline to be cognitively impenetrable to those in another. For instance, the New Atheists may be ignorant of higher-level esoteric religious doctrines and thus devote their efforts to debunking arguments based on a literal interpretation of the Bible, which may be the equivalent of flogging a dead horse (at least for the Christian intelligentsia). A general-interest magazine aimed at the survival problem would explore diverse approaches, including scientific, religious, experiential, parapsychological, philosophical, skeptical, humanistic, meditative, shamanistic, humorous, and poetic approaches. The journal might even include fiction, cartoons, and reviews of books and movies, even music related to survival, as well accounts of personal experiences. The journal might be called *Charon: The Journal of the Afterlife* or something like that.

Such a journal or magazine would be open to all approaches and would be geared to the general (undergraduate) reader. Such a publication and or website would hopefully attract a wider audience than does dry academic parapsychology at the present time. This may be a way to entice a wider audience of readers and contributors to join the debate about survival and to give parapsychologists' writings a wider exposure.

A strong editorial hand may be needed to ensure that such a journal, magazine, or website would not deteriorate into irrationalism, to ensure that all viewpoints are represented, and that the contributions are of high quality. Would anyone like to join me in such an effort? Sursem members, *MoA* authors, theologians, religious leaders, parapsychologists, philosophers, psychologists, and other interested parties, including lay persons are welcome. (I am not sure how long my increasingly cybernetic body will hold out, so the road to Editor-in-Chief may be pretty unobstructed.) Let me know if you are interested.

The writers of both *MoA* and *BP* have provided good, comprehensive, erudite reviews of the literature in their areas, although the index for *BP* is woefully incomplete.

Both *BP* and *MoA* are "must" reads for serious scholars in the area of survival research.

424 Little Lake Drive #3
Ann Arbor, MI 48103, USA
Dstokes48103@yahoo.com

BRIEF RESPONSE TO DOUG STOKES (AND *MOA*)

By Edward F. Kelly

What will best serve the purposes of this exchange, I believe, is for me to put the key issues in larger perspective, highlighting the fundamental problems I have both with Stokes' essay/review of *BP* and with *MoA*.

BP is actually the second major product of a 15-year multidisciplinary collaboration, sponsored by Esalen Institute's Center for Theory and Research, focused on the contemporary dialogue between science and religion. Our diverse academic participants, several score in all, have generally been skeptical of the currently prevailing classical physicalist worldview but equally wary of uncritical embrace of any of the world's major religions with their often conflicting beliefs and decidedly mixed historical records. At the same time, we sense that emerging developments within science itself are leading inexorably toward an enlarged conception of nature, one that can accommodate realities of a "spiritual" sort while rejecting rationally untenable "overbeliefs" of the sorts targeted by critics of the world's institutional religions. We advocate no specific religious faith, and we aspire to remain anchored in science while expanding its horizons. We are attempting in this way to find a middle path between the polarized fundamentalisms—religious and scientific—that have dominated recent public discourse. Both science and religion, we believe, must evolve.

We focused initially on the question of post-mortem survival (hence the nickname “Sursem,” from “survival seminar”). This is a watershed issue theoretically, because survival beliefs are common to traditional faiths but cannot be true if physicalism is correct. Furthermore, there already exists—largely unknown to believers, skeptics, and the general public alike—a substantial body of high-quality evidence suggesting that survival *does* at least sometimes occur.

We quickly realized, however, that our task was really much larger, and that we needed to approach it in two overlapping stages: first, to assemble in one place many lines of peer-reviewed evidence pointing to the empirical inadequacy of classical physicalism; second, and far more challenging, to seek some better conceptual framework to take its place.

The first stage culminated in publication of *Irreducible Mind: Toward a Psychology for the 21st Century* (Kelly et al., 2007, henceforth *IM*). There we catalogued the serious challenges posed to physicalism by well-evidenced empirical phenomena such as psi (possibly including post-mortem survival); manifestations of extreme psychophysiological influence such as stigmata and hypnotically induced blisters; prodigious forms of memory and calculation; phenomena of human memory more generally; psychological automatisms and secondary centers of consciousness; near-death and out-of-body experiences, including experiences occurring under extreme physiological conditions such as deep general anesthesia and/or cardiac arrest; genius-level creativity; and mystical-type experiences whether spontaneous, pharmacologically induced, or induced by transformative practices such as intense meditative disciplines of one or another sort.

Collectively, these phenomena greatly compound the explanatory difficulties posed to physicalism by everyday properties of human mental life such as meaning, intentionality, subjective point of view, and the qualitative aspects of consciousness, all of which have recently been targets of intense philosophical discussion. In a nutshell, *IM* added a rich *empirical* dimension to what appears to be a rising worldwide chorus of *theoretical* dissatisfaction with classical physicalism as a formal metaphysical position. We seem to be at or very near a major inflection point in modern intellectual history.

Assuming now that classical physicalism is inadequate, as we firmly believe it is, what should take its place? We addressed this far more difficult question, the main target of the second phase of our project, essentially by struggling to understand how we individual human beings and the world at large must be constituted in order that “rogue” phenomena of the sorts catalogued in *IM*—and systematically ignored or derided by mainstream physicalist science—can occur.

On the psychological side we were already committed to what historically have been called “filter” or “transmission” or “permission” models of the brain/mind relation. As developed by pioneers such as F. W. H. Myers, William James, and Henri Bergson, such models portray the brain not as the *generator* of mind and consciousness but as an organ of adaptation to the demands of life in our everyday environment, selecting, focusing, channeling, and constraining the operations of a mind and consciousness inherently far greater in capacities and scope. A central aim of the first phase of our project was to review and reassess Myers’s model of human personality in light of the subsequent century of research in psychology and neuroscience, and in *IM* we argued that the evidence supporting such pictures has actually grown far stronger since his death. Myers and James themselves were of course soon pushed aside by the rise of radical behaviorism with its self-conscious aping of the methods of classical physics, and that influence persists in modified form even now in mainstream cognitive neuroscience (see *IM*, Chapter 1). In our view psychology has taken a hundred-plus-year detour and is only now becoming capable of appreciating the theoretical beachhead that our founders had already established.

The normally hidden region of the mind, Myers’s subliminal consciousness or “the more” of William James, is the wellspring of the crucial *transpersonal* phenomena—especially psi phenomena and mystical experience with their deep historical and psychological interconnections, postmortem survival, and genius in its highest expressions—which jointly demonstrate that classical physicalism *must* give way to some richer form of metaphysics. The work summarized in *IM* invites—in fact *demand*s, we believe—a radical overhaul of currently prevailing physicalist conceptions. Note that what is at issue here is *not* whether we will have metaphysics—because we inevitably will, whether conscious of it or not—but whether we will have good metaphysics or bad.

A central element of our strategy in approaching these larger issues was to investigate conceptual frameworks both past and present that explicitly make room for rogue phenomena of the relevant sorts. To that end, we assembled an interdisciplinary team including physical and biological scientists, psychologists, philosophers, and scholars of religion specializing in relevant forms of mystically-informed religious philosophy (*not* “theologians,” as Stokes repeatedly and incorrectly states). We approached the comparative-religion material, of course, not with the expectation that any of these ancient systems contain all the answers, ready-made, but in the interest of prospecting for common themes and useful clues as to how best to advance our theoretical purposes.

All of these efforts culminated in the publication of *BP* earlier this year. To cut straight to the bottom line, our collective sense is that theorizing based upon an adequately comprehensive empirical foundation that includes the rogue phenomena catalogued in *IM* moves inescapably into metaphysical territory traditionally occupied by the world’s major institutional religions. Specifically, we argue in *BP* that emerging developments in science and comparative religion, viewed in relation to centuries of philosophical theology, point to some form of *evolutionary panentheism* as our current best guess about the metaphysically ultimate nature of things.

In brief, pantheisms in general attempt to split the difference between classical theisms and pantheisms, postulating an ultimate consciousness as pervading or even constituting the manifest world, as in pantheism, but with something left over, as in theism. The version we tentatively embrace in *BP* further conceives the universe as in some sense slowly waking up to itself through evolution in time. Most importantly, the rough first-approximation picture we develop can be elaborated and tested through many kinds of further empirical research, especially research on meditation and psychedelics as pathways into higher states of consciousness. Although a great deal remains to be done both theoretically and empirically to flesh out the picture sketched in *BP*, we feel confident that we are headed in the right general direction.

What is ultimately at stake here seems nothing less than recovery, in an intellectually responsible manner, of vital parts of human experience that were prematurely devalued or discarded with the meteoric rise of modern science starting four centuries ago. And what is especially significant at this critical juncture, and the fundamental new factor that we think will finally allow this recovery to succeed after numerous previous failures, is that it is now being energized by leading-edge developments in science itself.

Turning to the specifics of Stokes’s essay/review with that larger context in mind, I must first comment on two key claims he advances repeatedly, both of which I view as deeply mistaken.

First, echoing the skeptical position he adopted in the new parapsychology *Handbook* (Stokes, 2015), he begins by challenging one of the major premises of *IM* and *BP*, declaring that psi does not exist or at least has not been demonstrated to exist. He arrives at this conclusion essentially by extrapolating rates of misconduct recently found in various kinds of mainstream research to experimental studies of psi, and especially to meta-analyses of the direct-hit rates reported in ganzfeld studies and the like.

This conclusion seems to me unwarranted. In the first place the extrapolation itself seems unwarranted, because statistical and experimental methods in psi research have from the beginning been subjected to unusually intense scrutiny, precisely because of the theoretical challenge psi phenomena present to mainstream physicalist thinking. Furthermore, Stokes ignores other kinds of systematic psi effects such as terminal salience, displacement effects, consistent missing, and grouping of hits, which have often been found or confirmed through re-analysis of datasets originally collected for other purposes. He also does not take into consideration the substantial literature of process-oriented experimental studies (which as recently shown by Carpenter, 2012, in *First Sight* reveals many parallels to effects discovered by mainstream psychologists and neuroscientists) and the extreme levels of statistical success obtained in many individual experiments involving exceptional subjects (which has always seemed to me the best way to study rare psychological capacities of *any* kind, including psi capacities). Last but certainly not least, he also completely ignores the enormous body of high-quality literature devoted to case and field studies of crisis apparitions, mediumship, and cases of the reincarnation type. The empirical case for the reality of psi remains extremely strong, in my opinion, and in fact I surmise that experimental parapsychology may well emerge from the current controversies over questionable research practices looking substantially *better* than many areas of conventional mainstream research.

The second recurring theme arises in connection with the possibility of post-mortem survival. Stokes repeatedly suggests or insinuates that we, like other pro-survivalists, must be oblivious to the skeptical arguments and fail to appreciate the dependence of mental states on states of the brain. He refers approvingly to *MoA* as making this case in great detail and even characterizes the Sursem group as preferring in contrast to return to the 18th Century with Myers rather than engaging with contemporary neuroscience.

That this is a gross distortion should be apparent from what I have already said above, but let me now put it in more personal terms: For me the first phase of our project went a long way toward dissolving what the great American psychologist Gardner Murphy long ago called the “immovable object” in the survival debate—the biological objection to survival: Specifically, if physicalism is true, and mind and consciousness are manufactured entirely by neurophysiological processes occurring in brains, then survival is impossible, period, as clearly acknowledged in the Introduction and Chapter 1 of *IM*, and as argued at great length in *MoA*. But the evidence we assembled in *IM* clearly shows, I believe, that the connections between mind and brain are in fact much looser, and can be conceptualized in the alternative fashion of filter or transmission models without violence to other parts of our scientific understanding *including in particular leading-edge neuroscience and physics* (see especially *IM*, Chapter 9). In this context it should be evident that the direct counterpart to *MoA* is not *BP* but *IM*, yet neither Stokes nor *MoA* has much of anything to say about *IM* itself, and Stokes chastises us repeatedly for not saying more in *BP* about matters that are treated at length in *IM*. To repeat: The work on *BP* began at the point where for us at least *that* discussion was over, and the need for an alternative metaphysics had already been established. *MoA*, by contrast, is a sustained polemic in support of classical physicalism, which is simply assumed from the outset to represent the truth of the matter. Indeed, the basic tone of the entire collection is set in its Foreword, whose author asserts that we *know* survival is impossible and hence that the main question of interest is why anybody would believe such crazy stuff.

I need say only a little more about the essay/review itself. Stokes provides chapter-by-chapter descriptions of the contents of *BP*, as a reviewer should, but his descriptions vary wildly in length and are often inaccurate, so I encourage interested readers to find out for themselves what is actually there. The “essay” aspect seems to consist mainly of increasingly strident appeals for greater attention to his own views, with which I must confess not being very familiar. Curiously, despite his repeated embrace of *MoA*’s physicalist polemic against the possibility of survival, Stokes himself apparently endorses survival (although not in personal form) and arrives at a nonphysicalist metaphysics having much more in common with ours than with theirs. He repeatedly congratulates himself for accomplishing this without appeal to the evidence for psi and survival, which of course he regards as defective. I will try to read his 2014 book, of which I had not known until reading his essay/review, but meanwhile my co-authors and I can certainly take heart from the convergence toward similar metaphysical positions, if it is real, while making no apology for our very different way of getting there.

Division of Personality Studies (DOPS)
Department of Psychiatry and Neurobehavioral Sciences
 210 Tenth St. NE
 Charlottesville, VA 22902, USA
 ek8b@virginia.edu

REPLY TO ED KELLY

By Doug Stokes

I will reply to Kelly’s points in roughly chronological order.

He states that Sursem subscribes to the filter, transmission, and permission models of mind-brain interaction. These three doctrines are not equivalent. My objections to those doctrines are spelled out in my review and I will not repeat them here.

With regard to our mutual embrace of panpsychism in its various forms, I would urge the adoption of the terms “pandeism” and “panendeism” in preference to the more widely used terms “pantheism” and “panentheism” in order to avoid the theistic assumptions of the latter two terms (which both Sursem and I reject).

Kelly cites serial position effects, psi missing, grouping of hits, and so forth as supporting the psi hypothesis. Does he seriously think that these effects have given rise to replicable, fecund, and progressive lines of research? He cites Carpenter’s (2012) writings on what he calls first sight to support the existence of psi. However, Carpenter assumes at the outset of his book that the existence of psi has already been proven, which it has not. Rather, he states that rather than presenting an analysis of the reality of psi, his book presumes the reality of psi (p. 6). Then Carpenter proceeds to engage in wild speculation based on this assumption that is not well-grounded empirically.

Many skeptics assume the reverse, that psi has been proven not to exist. In fact, in 2006 the leading skeptical organization changed its name from the delightfully-acronymed Committee for the Scientific Investigation of Claims of the Paranormal (CSICOP, pronounced PSI COP) to the Committee for Scientific Inquiry (CSI, a prosaic acronym already used by far too many TV shows). This name change was prompted by the skeptics’ feeling that criticizing psi research was like flogging a dead horse. They also noted that public interest in psi research has diminished from its heyday in the 1970s and 1980s, when CSICOP was founded. So they have largely abandoned criticisms of psi research in favor of other lines of research that they also regard as pseudoscience and/or controversial.

At this point, it will probably be useful to clarify my own views on whether psi exists or not. I was commissioned to write the “skeptical chapter” for the recently published *Parapsychology: A Handbook for the 21st Century*. I titled this chapter “The Case Against Psi,” and in it I tried to play the devil’s advocate for the anti-psi view. I may have lapsed into the role of the Devil himself (or herself) in places.

I do not believe that the existence of psi has been proven. It can never be proven through meta-analyses based on the unexamined and likely false assumption that none of the experimenters are fraudulent, especially in view of the recently discovered high rates of fraud in related fields such as biomedical and psychological research. Meta-analyses might provide such proof if the experimental designs can be tightened so as to eliminate the opportunity for investigator fraud. The problem of data selection can be addressed through the preregistration of experiments, an advance that parapsychology is quickly adopting. Thus, the field is moving in a direction that might one day result in solid experimental proof of psi. However, if one assumes that fraud and data selection occur at the same rates that have been documented to occur in the wider scientific community and assumes that psi does not exist, one would expect that parapsychological effects found in meta-analyses would be quite small and not easily replicable. Such meta-analyses may, however, show statistically significant overall psi effects.

Some prominent parapsychologists, such as Dean Radin in his book *Supernormal* (Radin, 2013) present an oversimplified version of hypothesis-testing by stating that the null hypothesis in a psi experiment is the hypothesis that psi does not exist (p. 198). Under this interpretation, when one rejects the null hypothesis in a statistical test, one is rejecting the hypothesis that psi does not exist. Ergo, psi must exist. This facile conclusion can be avoided once it is realized that the null hypothesis is much more complicated than this, as discussed in more detail in the next paragraph. Radin, also presents a Bayesian approach, but he mangles that in places. For instance he states that the odds against chance of obtaining an experimental effect that is significant at the .05 level are 20 to 1 (p. 32). In fact they are 19 to 1.

In my view, the existing pattern of psi results is about what one would expect under the hypothesis that psi does not exist. Mainly, one would expect that a few dastardly investigators (of whom there is currently no shortage in the scientific community) will engage in fraud and claim to have obtained spectacular evidence of paranormal wonders. Then the results of these lines of research will decline as more and more honest researchers attempt to replicate these effects. In any psi experiment, indeed in any scientific experiment, there are unstated auxiliary hypotheses lingering within the real null hypothesis. In most psi meta-analyses, such unstated assumptions include the assumptions that none of the experiments are fraudulent, that the experimental apparatus has not malfunctioned during the experiments, and so forth.

If psi does exist, one might reasonably expect that a replicable psi effect might eventually be discovered. Right now, the pattern of results strongly suggests that either psi does not exist or that psi cannot be captured experimentally using current methodology. We have tried a lot of things, and they haven't worked.

Spontaneous cases provide a strong suggestion that psi may exist, but the study of such phenomena is a somewhat neglected area at the present time. Ironically, if psi does not exist, this would only strengthen the existing evidence for personal survival, including that based on apparent memories of past lives, crisis apparitions, and so forth.

With regard to my own work, of which Kelly claims ignorance, I have developed my own models of mind-matter interaction over the course of my three books, *The Nature of Mind* (Stokes, 1997), *The Conscious Mind and the Physical World* (Stokes, 2007), and *Reimagining the Soul* (Stokes, 2014), as well as in numerous earlier articles in the academic journals in parapsychology, going back to the 1980s. As McFarland is arguably the leading publisher of academic books in parapsychology and as these books have been reviewed in all the leading parapsychological journals, my ideas are hard to miss. As the model of reality I have proposed in these works is so close to Sursem's, I was surprised to see no mention of it in either *IM* or *BP*. This may be due to the fact that I argue against the survival of personality traits, which is a substantive difference between Sursem's view and my own. (My reasons for doubting the survival of the personality are set forth in my review of *BP* and set forth in encyclopedic abundance in *MoA*). Sursem's failure to address my work may reflect a general tendency on the part of a substantial minority of parapsychologists to simply ignore criticisms of their work and to continue using their flawed methodology as if the criticisms never happened. A particularly egregious example of such practices may be found in Brenda Dunne's remote viewing research (see my article entitled "The Strange Odyssey of Brenda Dunne," Stokes, 2004).

If the parapsychological community simply ignores both internal and external critics and simply continues current practices while ignoring legitimate criticism, it will produce a recipe for a pseudoscience in the making.

Finally, on a personal note, I take a Fox Mulderian approach to the investigation of psi. I want to believe. Psi phenomena, if they exist, would be the coolest and potentially most important thing in the universe. But we cannot abandon rationality in their pursuit.

424 Little Lake Drive, #3
Ann Arbor, MI 48103, USA
Dstokes48103@yahoo.com

PART II: THE MYTH OF AN AFTERLIFE

THE MYTH OF AN AFTERLIFE: THE CASE AGAINST LIFE AFTER DEATH, edited by Michael Martin and Keith Augustine. Lanham, MD: Rowman & Littlefield, Pp. xxxi + 675 (hardback). \$79.31. ISBN 978-0-8108-8677-3.

THE MYTH OF MORTALITY: COMMENTS ON MARTIN AND AUGUSTINE'S *THE MYTH OF AN AFTERLIFE*

By James G. Matlock

Philosopher David Ray Griffin (1997, pp. 26–27) distinguishes three types of thinkers: paradigmatic thinkers, data-led thinkers, and wishful thinkers. Paradigmatic thinkers are those who have adopted a particular worldview and see everything through its prism. The facts they attend to and how they interpret those facts are conditioned by what their worldview tells them is possible or impossible. Data-led thinkers

are empiricists whose worldview is shaped by the facts before them. Their outlook is open and liable to change as new facts come to their attention; the facts determine what seems possible and impossible. The worldview of wishful thinkers is formed by what they would like to be true. Facts matter little to wishful thinkers and neither does logic.

Michael Martin, Keith Augustine, and the contributors to *The Myth of an Afterlife (MoA)* give every indication of being paradigmatic thinkers. They are committed to 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. They evidently regard all who disagree with them as wishful thinkers. Indeed there may be wishful thinkers among “survivalists,” in particular New Age partisans and those who on religious grounds believe in survival in a resurrection body. However, it seems to me that the majority of survivalists whose work is dismissed in this book are data-led thinkers. They have come to doubt materialism and mortality because they have seen evidence that takes them in another direction. The paradigmatic-thinking authors included here are not much interested in this evidence and ignore it, distort it, or dismiss it without what data-led thinkers would consider a fair hearing.

Griffin (1997) reveals himself to be a data-led thinker. He argues that psi is key to understanding how the brain and mind interact. After close examination of the relevant data, he concludes that the mind survives the body’s demise and that at least some minds reincarnate in new bodies later on. Griffin is not the only contemporary philosopher to reach these conclusions. Almeder (1992), Becker (1993), Braude (2003), Grosso (2004), Lund (2009), and Weiss (2012, 2015) also are persuaded by the evidence for postmortem survival and reincarnation. Psychologists Baruš and Mossbridge (2017), too, recognize the evidence for the survival and reincarnation of consciousness. Neuroscientists who have been led by the data to believe that the mind can exist independently of the body include Schwartz (J. M. Schwartz & Begley, 2002; J. M. Schwartz, Gulliford, Stier, & Thienemann, 2005), Beauregard (2012a, 2012b), and Woollacott (2015). Other scientists and theorists who have rejected materialism include Kastrup (2014) and Stapp (2009, 2015; Schwartz, Stapp, & Beauregard, 2005).

Academics from a variety of disciplines contributed chapters to two hefty books produced by Edward Kelly and his colleagues, *Irreducible Mind* (E. F. Kelly et al., 2007) and *Beyond Physicalism (BP)*. The former brings together an immense array of data relating to the mind’s independence of the body, while the latter explores the implications of these findings and seeks rapprochement between science and religion (or spirituality). Moreira-Almeida and Santos (2012) address similar issues in another edited volume, *Exploring Frontiers of the Mind-Brain Relationship*. The materialist position is questioned in three other recent collections, *The Waning of Materialism* (Koons & Bealer, 2010), *After Physicalism* (Göcke, 2012), and *The Soul Hypothesis* (Baker & Goetz, 2011). These works and others like them individually and collectively present a challenge to *MoA* and raise the question of whether it constitutes a rearguard action by the defenders of a paradigm under assault, a last-ditch attempt to rescue its worldview from an oblivion threatening to overrun it.

MoA contains 30 chapters and a Foreword. In this essay I take up the main arguments of the book, then outline a theory of survival and reincarnation consistent with proposals made by Griffin (1997), E. F. Kelly (2007), and *BP*. This theory depends on what might be called an evolutionary idealist panpsychism (Matlock, 2016a) and builds on the process metaphysics of Alfred North Whitehead (1929/1978). Martin, Augustine, and their contributing authors are preoccupied with attacking substance dualism and Christian conceptions of life after death. Nowhere do they address the idealist and process approaches to the survival problem currently in vogue among survival theorists. Several of the book’s chapters are reprints or light revisions of previously published material, adding to its dated feel from the research point of view.

Foreword, Preface, and Introduction

MoA opens with a 12-page Foreword by Steve Stewart-Williams, a psychologist. Stewart-Williams considers beliefs about the afterlife to be memes or cultural traits susceptible to a sort of natural selection that privileges elements with the greatest cultural resonance. Afterlife beliefs persist because (among other

things) they bring pleasure and because giving them up produces acute cognitive discomfort, much as going off an addictive drug produces withdrawal pains. Stewart-Williams observes that survival-related experiences are interpreted in a belief context. Certainly experiences occur against the background of established beliefs, but I think he overstates his case. Experiences and observations may engender changes in beliefs as well as reinforce them. Children's past-life memory claims not infrequently precipitate re-evaluations of views on the part of Western parents who encounter them, for instance.

Stewart-Williams concludes with a section on why it is important to accept that bodily death is the end of existence: "*because it's true*" (p. xxiii, his emphasis). His Foreword states the theme and sets the tone of *MoA*: Afterlife beliefs are religious beliefs, which science has debunked. Next comes a Preface in which the editors say that the book's purpose is to ask questions and explore reasons for thinking that death is the end. This pretense does not last long, however. It soon becomes apparent that this is an unabashedly polemical text, with the answers decided at the outset.

In Chapter 1, entitled "Introduction," Augustine (p. 1) contrasts the "extinction hypothesis" with the "survival hypothesis," or, as he quickly reframes the problem, the "dependence thesis" with the "independence thesis." The dependence thesis holds that the mind derives from neural activity and is fully dependent on the brain; the independence thesis maintains that the mind has a separate existence but interacts with the brain while embodied. Augustine asserts that various lines of evidence from neuroscience "have one thing in common: they demonstrate that profound changes in the brain radically alter the mind itself," which is "extraordinarily difficult to reconcile with the simple notion that the mind is mostly independent of the brain, requiring the brain only as a means to control the body" (p. 4). This naïve version of the independence thesis, repeated over and over in *MoA*, is a caricature of the filter or transmission model employed today. "The basic picture here is of a conscious mind which normally operates in close conjunction with its associated brain in a manner strongly dependent on that brain's functional state," according to Edward Kelly and David Presti (*BP*, p. 117). By not engaging with the independence thesis as it is advanced by its proponents, Augustine and his fellow authors set up a straw argument, the easier to defeat it.

Moreover, do the findings of neuroscience really indicate that "profound changes in the brain radically alter the mind itself" (p. 4)? That depends on how one defines "mind." If mind is truly and exclusively a product of neural activity, one would have to say yes, but if mind is construed in the way the independence thesis imagines it, the conclusion is far from obvious. E. F. Kelly and Presti (*BP*, Chapter 4) are careful to specify that it is the "conscious mind" which engages the brain. There is no doubt that alcoholic drinks, psychoactive drugs, head traumas, and so forth, alter one's conscious awareness, but is that the same as altering "the mind itself," much less altering it radically? Is conscious awareness all there is to the mind? Not if one follows F. W. H. Myers (1903) in distinguishing the "supraliminal" from the "subliminal." Myers' supraliminal mind is the seat of conscious awareness, whereas the subliminal is the home of unconscious or subconscious processes.¹ The subconscious plays a key role in modern transmission models but is rarely mentioned in *MoA*, whose contributors conceive of the "soul" in terms of substance dualism. Many philosophers (e.g., Baker & Goetz, 2011; Göcke, 2012) continue to champion substance dualism, but since it is not the basis of the transmission model represented by Edward Kelly and colleagues (*BP*, E. F. Kelly et al. 2007) the unrelenting attacks on the "independence thesis" mostly fall flat from the research perspective.

Augustine has no need for psi, which he considers to be an "unknown and purely hypothetical" ability (p. 33). He argues that psi and superpsi (which he understands to be an "unlimited" psi) are functionally indistinguishable and since superpsi tends to be invoked in an ad hoc way to explain the survival evidence, and is unfalsifiable, that that is true of psi in general. However, superpsi is best understood not as unusually extensive psi but as an unusually complex psi. We may say that psi involves the transfer of information between two minds, the acquisition of information by one mind, or the direct action on the physical world or a biological entity by one mind, in a single step. Almost all if not all instances of telepathy, clairvoyance,

¹ Myers' idea of the subliminal or subconscious stratum of mind was very different from the concept of the unconscious developed around the same time by Freud and later adopted by Jung as the personal unconscious (Cook, 1994). Myers (1903) also wrote about the greater mind as being the subliminal Self (E. F. Kelly & Presti, *BP*, Chapter 4; E. W. Kelly, 2007a), which takes in mystical states of consciousness, but I am more interested in his subliminal/supraliminal contrast.

and psychokinesis reported from both the lab and the field qualify as psi on this definition. Superpsi on the other hand involves the acquisition of information from more than one source or a combination of information transfer, information acquisition, and direct action, in a single step or sequence of steps. Accounting for birthmarks along with episodic memories, emotional memories, and behaviors linked to the previous person in a reincarnation case in terms of psi requires superpsi, because it is impossible to explain all these effects as the products of a single event. One might perhaps evoke a “magic wand” (Braude, 2003) but even magic wands attested to in other contexts (e.g., Eisenbud, 1970, 1983) do not produce such complex effects.

I would like to make one other preliminary point regarding psi and survival. It is clear that psi processing occurs in the subliminal stratum of the mind. Psi impressions are sent and received through the subconscious before being presented to conscious awareness. There is no reason to think that a discarnate mind would lack subliminal and supraliminal strata and no reason to think that psi operates differently when a mind is disembodied than when it is embodied. Disembodied minds would be able to communicate with each other and with embodied minds through psi, with no need for superpsi. Braude (2009) seems to think that disembodied perception and communication would require superpsi, but I am unaware of any situation in which one must posit deceased-agent superpsi. Superpsi is a supposed ability of living agents only. It is an attempt to explain phenomena suggestive of survival without evoking deceased agents. Psi, but not superpsi, has a significant place in the survival theory I outline in this essay.

Correlation and Causation

MoA is arranged in four parts, each introduced by a few pages from the editors. Part I, entitled “Empirical Arguments for Annihilation,” includes nine chapters by philosophers, psychologists, and neuroscientists dedicated to showing that various kinds of mental states are related to brain function. From this we are to infer that the brain produces the mind and that when the brain fails, so does the mind. However, all that this work demonstrates is that there is a correlation between mental and neural activity; it suggests that they are interrelated, but it does not prove that the neural activity gave rise to the mental activity.

That “correlation is not causation” is a widely-recognized counter to the dependence thesis. Several contributors acknowledge the issue. Some, like Terence Hines (Chapter 8, p. 193), admit that it is impossible from correlations alone to say which way the causal arrow runs. David Weisman (Chapter 4, p. 102) insists that the correlations are so tight that they should be regarded as causal. Augustine and Yonatan Fishman (Chapter 10, pp. 208–209) agree. Augustine (p. 31) argues that one must take into account the degree of evidential support and that since the preponderance of the “best evidence” favors the dependence thesis, it should be preferred.

Early on, Jean Mercer (Chapter 3) departs from the neurological emphasis to examine the genetic roots of temperament, tendency toward psychopathology, and intelligence. Although she confesses that this work has led to “confused and partial conclusions” she feels that it is more parsimonious to assume that personality is “entirely determined by biological factors” than that it is “determined by biological and spiritual factors” in tandem (pp. 79–80). She says nothing about environmental factors, which many psychologists have long believed to make a major contribution to personality development.

In Chapter 2, Matt McCormick tries to show that personality traits, cognitive abilities, emotions, conscious awareness, and self-awareness—“in short, the features that we attribute to the personal soul” (p. 64)—are brain-dependent. Carlos Álvarez (Chapter 7) supplies a more detailed look at the neural substrates of emotions and emotional processing. Terence Hines (Chapter 8) describes the specialization of language function within the brain. There is no doubt about the brain’s involvement in all these things, but it has become increasingly clear that the mind also has a role in their expression. Beauregard (2007, 2012b; Beauregard & O’Leary, 2008) has written about fMRI studies of emotional self-regulation. J. M. Schwartz (J. M. Schwartz & Begley, 2002; J. M. Schwartz, Gulliford, Stier, & Thienemann, 2005) has documented changes in the brain when adults with obsessive-compulsive disorder deliberately alter their behaviors. Baker (2013) discusses the limitations of neurobiology in explaining what Noam Chomsky (1966) called the “creative aspect of language use.” Noë (2009) makes a strong case that conscious awareness emerges

outside the brain, in response to environmental stimuli. His theory explains language, perception, thought, and cognitive skills generally as the result of engagement with the world around us.

Several contributors claim that the decline of psychological function in a compromised brain demonstrates that the mind cannot exist apart from the brain. Rocco Gennaro and Yonatan Fishman (Chapter 5) note that brain damage can affect a great range of mental processes, including perception; awareness, comprehension, and recognition; memory; personality; language; emotion; decision making; and moral judgment and empathy. David Weisman (Chapter 4) focuses on the dying brain, using case studies to show how progressive degeneration is related to lessening of awareness and conscious control. Gualtiero Piccini and Sonya Bahar (Chapter 6) describe the neural localization of mental function in the brain and conclude that there can be no mental life after brain death. In their treatment of neuroplasticity (pp. 159–160) they miss the important point that behavioral changes guided by will can sometimes bring about the neural reorganization (J. M. Schwartz & Begley, 2002; J. M. Schwarz, Gulliford, Stier, & Thienemann, 2005).

Given the tight association between neural and mental functioning, a correlation between brain degeneration and loss of awareness is to be expected. There is no reason to suppose that the impact on conscious awareness affects the subliminal stratum of the mind, however. Subconscious processing might continue despite the mind's ability to interact with the brain and, indeed, could help to account for responsiveness in locked-in and persistent vegetative states (e.g., see Owen et al., 2006). The persistence of subconscious activity could also explain cases of what Nahm and his colleagues (Nahm & Greyson, 2009; Nahm, Greyson, E. W. Kelly, & Haraldsson, 2012) call "terminal lucidity," even if conceived of as "lucid intervals" (Weisman, pp. 100–102) or "transient lucidity" (Augustine & Fishman, pp. 248–250). In these cases, although the brain may have been severely impaired by advanced dementia, tumors, strokes, abscesses, meningitis, and other disorders, often involving tissue destruction and lasting for years, patients suddenly become responsive, recognize and even converse with loved ones, usually shortly before dying.

Gennaro and Fishman (p. 122) ask, "if the soul can influence the brain, then why can't the soul 'will' away drug addiction or depression" or help to resolve other problems? Well, sometimes it can. In *Anatomy of an Illness*, Cousins (1981) described how he used laughter to cure himself of ankylosing spondylitis (an inflammatory disease that can cause spinal vertebrae to fuse). The well-known placebo effect furnishes other examples (E. W. Kelly, 2007b, pp. 139–148). One man's cancer went into remission when he was given an experimental drug but returned when he learned that the study was inconclusive. He recovered again after his doctor injected him with water, telling him that it was an improved version of the drug, but relapsed once more and died when he heard another report about its worthlessness (E. W. Kelly, 2007b, p. 145). Emotional states alone may have physical effects; the bereaved may die within days of a spouse's passing (E. W. Kelly, 2007b, p. 124). The mind may also affect the body in an injurious way. In stigmata, marks (accompanied sometimes by bleeding) appear on the hands and feet of devout Christians, corresponding to wounds Jesus is presumed to have suffered on the cross (E. W. Kelly, 2007b, pp. 152–156). See E. W. Kelly (2007b) for many other instances of psychophysiological influence.

Jamie Horder (Chapter 9) assumes that under dualistic interactionism the mind should be consciously aware and in control of everything that befalls the body. Since this is manifestly not the case in situations that include hallucinatory states, dreaming, and hemineglect (where one side of the body is not aware of what the other is doing or going through), there can be no mind apart from the brain, he contends. Horder does not acknowledge that the subconscious might have a role in dreaming and that our subconscious could preserve our identities when our conscious awareness is offline or confused for whatever reason. Absence of conscious awareness or control need not mean that there is no mind present at all.

Correlations between neural and mental events are as compatible with the independence thesis as with the dependence thesis. Is there any way of deciding between the rival interpretations? Augustine and Fishman (pp. 208–211) say yes, we should prefer the dependence thesis because the temporal precedence of the neural events is clear: the brain acts first, and the mind responds. However, as I have shown, that is not always true. There are many reports, some quite well documented, of mental events coming first. The only recourse materialists have is to dismiss these cases and studies on the grounds that they are old, anecdotal,

poorly described, do not meet the standards of our “best available data” (p. 204), “best findings” (p. 254), and “most reliable evidence” (p. 269), and so forth. This they must do and do do, otherwise they would have to admit that the dependence thesis is falsified and the independence thesis supported. The causal arrow between the mental and the physical may run in different directions at different times, but if any part of the large and diverse data set suggesting the mind’s temporal precedence is valid, at the least we know that the arrow does not always run from brain to mind.

Causal Openness

Part II is devoted to “Conceptual and Empirical Difficulties for Survival.” There are nine chapters, seven by philosophers, one by a psychologist (Susan Blackmore), and one by a biologist. We get a rehearsal of familiar arguments against substance dualism, with no attention to other forms of dualism or to nondual and idealist possibilities. A central theme is causal closure, a necessary starting point for any materialist position. To say that the physical world is causally closed is to say that for every physical effect there is a physical cause. There is no room and no need for mental causation. The physical is the only reality; the mind must in some fashion emerge from or supervene on the brain. The trouble for materialism is that it is not clear that the physical domain is causally closed at a fundamental level. Causal closure is a feature of classical or Newtonian mechanics, but quantum mechanics is probabilistic rather than deterministic. The standard model of quantum mechanics places consciousness outside physical systems and many physicists believe there is evidence that the mind can bias outcomes in certain directions (Stapp, 2011).

The argument from causal closure is meant to bolster the dependence thesis. None of the *MoA* authors give any indication of realizing that the physical domain may not be closed after all and consequently do not consider what this lack of closure might mean for mental causation and the independence thesis. They do not seem to appreciate that we have evidence of mental causation in the mind’s ability to impact its body for healing and harm, much less in psi effects generally. The role of psi in the making of reality may nevertheless be quite significant and psi may provide the answer to the biggest philosophical objection to dualism, the question of how a nonmaterial substance (the mind) might influence a physical one (the body). Gennaro and Fishman maintain that “no such explanation is forthcoming or is perhaps even possible” (p. 108). Several other authors in Parts I and II say similar things.

We are told again and again that it is impossible to imagine how a disembodied mind could perceive or act upon the physical world, unless with divine assistance. Theodore Drange (Chapter 12, p. 331), the only contributor to discuss psi in this context, contends that clairvoyance, telepathy, and psychokinesis require a body (not just a mind), although he does not explain why this should be. Drange cannot understand how a disembodied mind would be able to establish its own identity, let alone the identity of others. Perhaps identity could be based on memory, but the apparent memories of a disembodied mind would give no assurance of identity because they could have been “deliberately implanted by someone else” (p. 332). Since there is no way to ensure identity in the afterlife, it makes no sense to speak of personal survival, Drange claims.

Raymond Bradley (Chapter 11, pp. 301–302) asks how, given the “manifest dependency” of mental states on changes in physical states while embodied, there could be changes in mental states while disembodied. One answer is that changes in mental states are not necessarily dependent on changes in physical states. Rather, changes in mental states may arise directly from prior mental states or in response to external cues (Noë, 2009), and so they might occur while disembodied as easily as while embodied (given psi information transfer and acquisition). Bradley (pp. 304–306) challenges substance dualists to explain the soul in relation to evolutionary change. When in evolutionary history did souls first appear? Do nonhuman animals have souls? How exactly does the soul relate to the mind? These I think are problems for substance dualism (particularly the Cartesian variety targeted in *MoA*) that do not necessarily apply to other conceptions of postmortem survival. The theory I outline later embraces evolutionary change.

Another contributor concerned specifically with Cartesian ideas is Jaegwon Kim, whose Chapter 13 is reprinted from Corcoran (2001). I have already answered Kim’s titular question, “What Could Pair a

Nonphysical Soul to a Physical Body?” (it is psi) and will concentrate my remarks on an issue that I have not yet addressed, causation and space. Physical causation requires contact, hence spatiotemporal proximity. Psychokinesis does not require spatial proximity and perhaps not even temporal proximity. Would it be available to a disembodied Cartesian thinking substance and could it get around Kim’s objections that such a substance, lacking a spatial dimension, would be unable to interact with the physical world in the same spatiotemporal coordinate system? I am not sure and will leave that question to the philosophers. I think we can say, at least, that there would be fewer logical difficulties for a surviving mind that is localized in space (and time) and this is an additional feature of my survival theory.

In a paper reprinted from the *Journal of Consciousness Studies*, David Wilson (Chapter 14) complains that (in the words of his title) “Nonphysical Souls Would Violate Physical Laws.” This is followed by a chapter from David Papineau, excerpted from his chapter in Stone and Wolff (2000), in which he declares that “There Is No Trace of Any Soul Linked to the Body.” Leonard Angel (Chapter 16) appears next with a chapter entitled, “Since Physical Formulas Are Not Violated, No Soul Controls the Body.” The starting point for these three chapters is the alleged closure of the physical domain. Angel advises us that in order to come to the correct view of the matter, “learning intellectual history is required. It might take a bit of time to do that” (p. 378). He then helpfully supplies a condensed reading of the intellectual history to which he refers:

Pythagoras and Plato lived almost a century and a half apart, but Pythagoras recorded almost nothing that survives, whereas Plato wrote many surviving dialogues. Yet both Pythagoras and Plato seem to have been rationalists who believed in the importance of mathematics. Plato’s student, Aristotle, subtly developed an early synthesis of rationalism and observational empiricism. After Newton’s work, which occurred almost two thousand years later, modern syntheses of rationalism and observational empiricism developed. The developing processes came to fruition in the twentieth century (p. 381).

“Rationalists” seems to be used here as a synonym for “materialists.” It is a highly inapt appellation for Pythagoras, whom R. Martin and Barresi (2006, p. 10) liken to a shaman, and Plato. Pythagoras believed he could remember having lived before and taught metempsychosis and the direct apprehension of reality. His mathematics was a way into that reality. Plato did not claim past-life memories, but he had Socrates and others expound on reincarnation in several of his dialogues (R. Martin & Barresi, 2006; Obeyesekere, 2002).² Concluding the modern synthesis with Newton is just as misguided, and telling, ignoring as it does the revolution in physics that came with quantum mechanics (Stapp, 2011). Angel’s attitude perfectly captures the spirit of *MoA*. “The crucial thesis that nothing violates mathematical physics is not only *really hard* to deny, but evidentially *undeniable*; that some intransigent deniers refuse to accept it may simply be a reflection on their unmindfulness of the enormity of human history” (p. 388, his emphasis). The irony of the final clause requires no further comment.

Blackmore’s Chapter 17, “The Implausibility of Astral Bodies and Astral Worlds,” is excerpted from *Beyond the Body* (Blackmore, 1982), where it stands as the penultimate chapter and is entitled “Re-assessing the Theories.” *Beyond the Body* is about out-of-body experiences (OBEs) and this chapter is concerned with critiquing exteriorization theories of the OBE. Blackmore first considers physical theories (a physical double travels in the physical world), then “astral world theory” (a nonphysical double travels in the physical world), and finally “mental astral world theory” (a nonphysical double travels in an astral world). These theories introduce new concepts into *MoA*, especially the idea that the mind might survive death in a quasi-physical subtle (astral) body. Following Blackmore there are two chapters critiquing the notion of postmortem survival in a resurrection body.

² I am letting pass the relatively minor points that Pythagoras believed in oral teaching and left no writings whatsoever (R. Martin & Barresi, p. 10) and that Plato is not noted for his mathematical commentary. Perhaps Angel has in mind the passage in the *Meno* (84c–86a) where Socrates uses the mathematical prowess of his untutored slave boy to make a point about knowledge deriving from pre-existence and ultimately past lives.

Theological Conceptions Versus Theological Principles

A relatively short Part III, “Problematic Models of the Afterlife,” contains three chapters that point out “inconsistencies between theological conceptions and theological principles” (p. xxx). Michael Martin (Chapter 20) discusses “Problems with Heaven.” Bradley (Chapter 21) returns with the question, “Can God Condemn One to an Afterlife in Hell?” Ingrid Hansen Smythe (Chapter 22) supplies the only chapter that deals with a religious concept not derived from Christianity. Her “Objections to Karma and Rebirth: An Introduction” deals with a generalized concept of karma unrelated to any specific tradition, despite the sometimes considerable variations among the conceptions of different traditions (Krishan, 1997). Undoubtedly part of Smythe’s purpose is to undermine the idea of reincarnation by linking it to karma, but reincarnation does not entail karma. Karma has no place in reincarnation belief systems outside the Indic sphere (Obeyesekere, 2002), and, in fact, past-life memory case studies have found no sign of karma in the retributive (or juridical) sense that Smythe analyses (Stevenson, 2001, pp. 251–253). Her chapter has no bearing on the empirical question of whether reincarnation occurs.

Evidence for Survival

Part IV, “Dubious Evidence for Survival,” is the part of *MoA* most directly concerned with psychological research and cases suggestive of survival after death. Most of the eight chapters are reprints or revisions of previously published material.

Rense Lange and James Houran’s Chapter 23, “Giving up the Ghost to Psychology,” is a reprint of a 1988 article in *Skeptical Intelligencer*. It lumps together poltergeist disturbances and haunting experiences, interpreting them as “mistaken perceptions arising from an interaction of paranormal beliefs, paranormal experiences, and fear of the paranormal” (p. 508). This conclusion is based partly on the authors’ research, but they have the annoying habit of misrepresenting the findings of others. In their second sentence (p. 503), they cite Gauld and Cornell (1979) and Roll (1977) in saying that a poltergeist outbreak of any duration is called a haunting. In fact, Gauld and Cornell confirmed the standard distinction between a person-centered poltergeist and a place-centered haunting through a cluster analysis of 500 cases. Roll (1977) made the same distinction and introduced his theory of recurrent spontaneous psychokinesis (RSPK), which has become accepted as the explanation for the majority of poltergeist episodes. This, however, brings psi into the picture, and that is something Lange and Houran cannot countenance (pp. 505–506).

Chapter 24 brings back Blackmore on OBEs. Her “Out of Body Experiences are not Evidence of Survival” is reprinted from a 1998 issue of *Anabiosis*, now the *Journal of Near-Death Studies*. She says, “The main problem to face [in interpreting OBEs as literal out-of-body experiences] is conceiving of anything that” might be able to leave the body. “The ‘whatever it is’ . . . must be able to move, perceive at a distance, and to transfer the results back to the body. That is a very tall order” (p. 519). It seems to me, though, that describing the “whatever it is” is not so hard. It is not difficult to imagine a mind that separates from the body, views things via clairvoyance, records them in its subconscious memory bank, then uploads them into conscious awareness when it is associated with its body again. The harder question is, do we have evidence that this happens? On the latter point, I agree with Blackmore: apparently not very much. OBE sensations are very similar to autoscapy, the visual hallucination of one’s body, viewed as if from the outside. Most OBEs are likely no more than special altered states of consciousness. OBE perceptions become parapsychologically interesting only when they are veridical and even then it is often possible to interpret them as psi-based without having to assume the mind has left the body (E. W. Kelly, Greyson, & E. F. Kelly, 2007, pp. 394–405).

In Chapter 25, “Near-Death Experiences are Hallucinations,” Augustine (2007b, 2007c) presents a condensed and revised version of two of papers that appeared in the *Journal of Near-Death Studies* in 2007. Augustine has scoured the NDE literature for instances of inaccurate or confused perception and presents these as problematic for an exteriorization interpretation of the experience. Apparently he expects disincarnate perception to resemble embodied visual perception, but we are dealing with clairvoyance here. The

psi inputs are processed by the subliminal mind, whence the distortions. We see this with dream telepathy and remote viewing to an even greater extent than with OBE perception during NDEs. A recent book by Rivas, Dirven, and Smit (2016) brings out the psi angle very well, citing cases of veridical telepathy as well as clairvoyance. Rivas et al. (2016, Chapter 7) also discovered seven cases of NDEs related to reciprocal apparitions, in which experiencers were seen as apparitions at the sites to which they felt they were traveling out-of-body. These last cases more than the psi-related ones suggest a mind detached from the body, but we should remember that NDEs at their best tell us no more than that separation is possible. NDEs do not provide direct evidence of survival. Of greater relevance to the survival question are memories of the intermission in reincarnation cases (Matlock & Giesler-Petersen, in press; Sharma & Tucker, 2004). Interestingly, veridical perceptions of the terrestrial world have been reported in all stages of the intermission experience and are more accurate (have less distortion) than the NDE perceptions Augustine highlights (Matlock & Giesler-Petersen, in press).

Chapter 26, by Champe Ransom, a lawyer and research assistant to Ian Stevenson in the early 1970s, is based on a critique of Stevenson's research that Ransom prepared for Stevenson in 1972. This critique has a history. Writer D. Scott Rogo learned about it from an unnamed colleague and described it in *The Search for Yesterday* (Rogo, 1985, p. 79). Rogo's account brought it to the attention of Paul Edwards, who persuaded Ransom to write a summary of it for his *Reincarnation: A Critical Examination* (Edwards, 1996, pp. 276–277). The "Ransom Report" has since become a staple of the skeptical commentary on Stevenson, although it has never been seen in full. The *MoA* editors say that they were unable to get permission to reproduce it along with the response Stevenson had requested accompany it, so they asked Ransom to write the abbreviated version that they published.

The original report included 18 points, 13 of which Ransom chose to repeat here. Most do not concern Stevenson's research methods (Ransom never accompanied him into the field) but rather the way the write-up was handled in *Twenty Cases Suggestive of Reincarnation* (Stevenson, 1966), Stevenson's first volume of case studies. For instance, ". . .often the case reports are lacking in the details of when the statements (of a subject or witness) were made and in what context and to whom" (p. 640). Other points turn on hypotheticals, e.g., "Leading questions may have been used" (p. 641). Ransom raises some serious concerns, including subtle distortions of memory over time, the need to work through interpreters, and problems attendant with spending only brief periods with witnesses. However, these and many other potential pitfalls were acknowledged and addressed by Stevenson in the first chapter of *Twenty Cases* (Stevenson, 1966), so it cannot be said that he was unaware of them before they were brought to his attention.

Angel continues the criticism of Stevenson's work in Chapter 27. He declares that "Stevenson thought that anecdotal research was required for his project because it was practically impossible, he assumed, to do controlled experimental research on the subject" (p. 655). Angel believes that the correspondences between the present and previous persons, including the birthmark evidence that Stevenson adduces in *Reincarnation and Biology* (Stevenson, 1997), are all due to chance. What he means by "controlled experimental research" is demonstrating the statistical probability that a child's statements fit one person and no other. This has been done rarely, because the matches typically are so striking as to make quantification seem unnecessary, but he is wrong in his repeated assertions that no efforts of this kind have been made. Pasricha (1983) reports a statistical assessment in relation to the Rakesh Gaur case, and Mills (2004) attempted to quantify likelihoods in the case of Ajendra Singh Chauhan.

Angel reprises his assessment of Stevenson's case of Imad Elawar (Stevenson, 1966), originally presented in 1994 (Angel, 1994). The Imad Elawar case was not solved when Stevenson reached it and Imad's parents, in an effort to make sense of what he was saying, had strung his statements together in a way that turned out to be mistaken. What should be a strength of the case—the written record made before verifications were attempted—becomes problematic for Angel, who charges that Stevenson selected which information to credit and which not. In a careful re-analysis of Imad's statements before Stevenson arrived on the scene, Barros (2004) shows that they alone are sufficient to identify the previous person and supports Stevenson's interpretation over Angel's. Angel cites Barros's paper under the name "Siquiera" (his maternal surname) but does not take the opportunity to reply to his remarks on the case.

Chapter 28, by Claus Flodin Larsen, and Chapter 29, by Christian Battista, Nicolas Gauvrit, and Etienne LeBel, are devoted to research on mediumship by Gary Schwartz and Julie Beischel. Larsen opens with a history of mediumship, focusing on Spiritualism, drawn from skeptical and online sources. He explains the principles of cold reading and shows how the overinterpretation of reported statements by sitters could have influenced the results reported in *The Afterlife Experiments* (G. E. Schwartz with Simon, 2002). There is value in what Larsen says, but as so often in the skeptical treatment of parapsychology, he may be too quick to assume that there is nothing paranormal going on at all. Daryl Bem (2005), whose critical review of *The Afterlife Experiments* Larsen does not cite, identified a variety of statistical errors in Schwartz's work but believed that there was reason nonetheless to think that there was some psi involved, albeit psi on the part of the test mediums alone.

Battista, Gauvrit, and LeBel find methodological and statistical flaws in a triple-blind study by Beischel and G. E. Schwartz (2007). The statistical flaws appear to be corrected in a follow-up quintuple-blind study (Beischel, Boccuzzi, Biuso, & Rock, 2015), but one of the methodological issues remains. This is the practice of supplying mediums with the first name of the discarnate to be contacted, which Battista, Gauvrit and LeBel point out could provide information for the start of a cold reading. Beischel et al. (2015, p. 138) defend the use of this practice, and they are probably right that its impact would have been negligible; nevertheless, it would seem wise, as they acknowledge in their conclusion (Beischel et al., 2015, p. 141), to find a different way of ensuring the medium's "mental focus."

The final chapter, David Lester's "Is There Life After Death? A Review of the Supporting Evidence" is excerpted with minor changes from his *Is there Life after Death? An Examination of the Empirical Evidence* (Lester, 2005). Lester asserts that "the two major sources of evidence for contemporary research on life after death are reports of near-death experiences and ostensible reincarnation" (p. 636). He cannot understand why there should be cultural variation in either NDEs or reincarnation experiences. Cultural variation in NDEs also troubles Augustine (pp. 542–550). Both authors seem to think that if these phenomena are what they appear to be, they should be the same for all experiencers, everywhere. They give no reason for expecting such uniformity and it would be odd if it were found. Is there anything about human experience that is the same for everyone, everywhere? I do not have room here to treat Lester's other concerns about reincarnation in the depth required. I deal with Lester's concerns more thoroughly in a work in preparation (Matlock, 2016b).³

The chapters of Part IV do little to counter the evidence for survival. They do not even confront it properly. Most poltergeists have nothing to do with survival but rather concern the psychokinesis of living persons. OBEs and NDEs have no direct bearing on the survival question; at most they show that the mind can exist apart from the body and an incapacitated brain. There are features of NDEs, such as encounters with the apparitions of deceased persons, that are more directly suggestive of survival (Rivas, et al., 2016, pp. 221–237), but these are not discussed anywhere in *MoA*. In fact, apart from a few pages by Augustine (pp. 20–22), apparitions of any sort receive only passing mention. The treatment of mediumship is confined to studies by Schwartz and Beischel with mental mediums. There are no references to the decades of work with trance mediums by members of the Society for Psychical Research and the American Society for Psychical Research (see, e.g., Gauld, 1982). The reincarnation data also are much more complex and varied than this book suggests (Haraldsson & Matlock, in press). The failure to grapple with, much less to come to grips with, the breadth and depth of the evidence for postmortem survival is the greatest weakness of *MoA*.

A Process Theory of Personal Survival

In addition to insisting that correlation is tantamount to causation in mind/brain relations, that the physical realm is causally closed, and that all empirical evidence of personal survival is bunk, Augustine and Fishman (Chapter 10) discuss the requisite features of scientific theory and explain why they think the dependence thesis is more satisfactory than the independence thesis. A good theory should be testable and have predictive success; it should have wide scope (unifying power) and fertility (be productive of

³ This work is built around lectures for a course I teach online through the Alvarado Zingrone Institute for Research and Education (theazire.org/moodle).

research). According to Bayesian confirmation theory the strongest hypothesis (or theory) is the one with the best fit to existing data and background knowledge and the highest prior probability of being right. The more ad hoc auxiliary assumptions are brought in to shore up a theory, the more problematical it becomes. Simplicity counts.

Augustine and Fishman naturally believe that the dependence thesis is the winner of the contest with the independence thesis, because they assume that the mind cannot affect the brain and body and that the physical realm is causally closed. These starting assumptions constrain the estimation of prior probabilities and guarantee that the dependence thesis comes out ahead. If we reject the notions that the brain always acts antecedent to mental events and that the physical realm is causally closed, the calculus changes so that the dependence and independence theses are more equal in their prior probabilities; and 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, our background knowledge changes enough to tilt the balance in favor of the independence thesis.

Substance dualism is not the only alternative to materialism and mind/body identity in philosophy and it is not the alternative most favored by scientists who turn away from reductionism. As Barušs and Mossbridge (2017) note, “at the time that materialism is on its way out, it appears that panpsychism is on its way in” (p. 20). The core conception of panpsychism is that entities of all kinds at all levels have experiences associated with a mind or mind-like quality (Skrbina, 2005). Panpsychism does not necessarily require a rejection of materialism. Chalmers (1996), Strawson (2006), and Koch (2012) adopt panpsychist positions that recognize that awareness is not grounded in cerebral activity, yet do not forsake a materialist account of how the rest of the world works. Noë’s (2009) idea that awareness emerges in interaction with the environment is applicable all the way down the phylogenetic tree but does not entail wholesale rejection of the materialist world view either. Barušs and Mossbridge (2017), Woollacott (2015), E. F. Kelly (2007, 2015), and Griffin (1997), on the other hand, link panpsychism to idealism, the idea that consciousness is the primary force in the universe, and allow for postmortem survival and reincarnation (Matlock, 2016b).

Idealism is a form of monism diametrically opposed to materialism. From the idealist perspective, the universe consists of a “deep consciousness” from which “both physical and mental events arise” (Barušs & Mossbridge, 2017, pp. 181–182). Bringing idealism and panpsychism together we arrive at the proposition that all things are derived from the same deep background consciousness and so consciousness is a part of all things. One reason this idea has become popular is that it is consistent with quantum theory (E. F. Kelly, 2007; Stapp, 2011, 2015). All things are not alleged to have the same type of consciousness, of course. The idea is that material objects, including bodies, and minds are imbued with different grades of consciousness. There is a dualism of mind and body here, but it is an idealist property dualism. The difference between an idealist property dualism and substance dualism is important. If mind and body are not so different in their composition it is easier to understand how they interact (Griffin, 1997).

We may characterize the soul or mind as a stream of consciousness. A consciousness stream differs from body consciousness in various ways, chief among them that the former survives the annihilation of the latter. Also, a consciousness stream has supraliminal as well as subliminal strata, whereas other forms of consciousness enjoy gradations of the subliminal only; they are sensual but not fully sentient. I suspect that the reincarnation of the consciousness stream began with the inception of biological life and death and that the minds of all living creatures return in new bodies again and again. I think they do this more by habit than by requirement and that it is possible for there to be transmigration between different animal species, although as yet we have no good evidence that this occurs. By the same token, it should be possible for a consciousness stream newly sprung from the background consciousness to incarnate in the body of any species (including ours) without having been incarnated in that species before. Again, we have no good evidence of this, although it may be noted that popular metaphysics claims to be able to distinguish “new souls” from “old souls.” In a complementary way, a consciousness stream should be able to blend back into the background consciousness, much as would presumably occur in nonreincarnating types of consciousness when their hosts cease to exist.

We can assign certain capabilities or attributes to our human streams of consciousness, many if not

all of which may be features of consciousness streams in general. These include the capacity for attention, intention, and will (Stapp, 1999), psi (Stapp, 2015), and memory. Attention, intention, and will imply conscious awareness as well as agency and belong to the supraliminal stratum of the consciousness stream, whereas psi and the ability to form and store memories belong to the subliminal stratum. In memory I include our behavioral dispositions, our body image and body schema, and all the bits from which our personalities are constructed, in addition to the episodic, semantic, emotional, and other residues of our experiences. I call the subject or experiencer of these various attributes the self. I think of the self as contingent on experience and yet the self has a greater permanence than the attributes of the consciousness stream. We change continually as we go through life—and move from one life to another—yet the self remains constant (Matlock, 2016b).

I do not think that a consciousness stream requires the support of an astral body, which some analysts and critics (e.g., Broad, 1962; Flew, 1976) have believed necessary to hold a personality together after death and render postmortem existence intelligible. Broad (1962) and Flew (1976) missed a way in which a consciousness stream might cohere in a completely disembodied state, and that is through the concatenation of discreet experiential events (“actual occasions”) as per Whitehead (1929/1978). Whitehead believed that an individual’s experiential stream outlasted his death, resulting in a sort of “objective immortality” (Whitehead, 1929/1978, p. 351), but he seems to have thought of mental activity as having ceased then. I see no reason that the concrescence of actual occasions must end at death, though, and with this amendment Whitehead’s process metaphysics allows for the survival of dispositional traits and memory into the afterlife. This is especially true when we consider that the consciousness stream might continue at a subliminal level even in the absence of supraliminal activity. Weiss (2012, 2015) extends Whitehead’s proposal in similar fashion.

The persistence into death of a stream of consciousness means that the afterlife is better thought of as an altered state of mind than as a place. Our afterlife experience is a consequence of our consciousness no longer being constrained and mediated by our brain. Our personal histories and entrenched beliefs go with us when we die and help to shape how we perceive what we find in death, thus the individual and cultural variations in NDEs, intermission memories, and mediumistic portrayals of afterlife conditions. The afterlife experience has sometimes been likened to a dream, for example, by Price (1953), and Tucker (2013, Chapter 9), but this implies that the subconscious is more in control than appears to be the case from reported memories and mediumistic accounts. The fanciful elements and distortions in some of these narratives are better understood in terms of an increased permeability between the supraliminal and the subliminal strata of the mind, I think. On this issue, see E. F. Kelly and Presti (*BP*, Chapter 4, p. 121).

What would constitute the actual occasions, the experiential events, of the supraliminal postmortem state? Broad (1962, 1976) suggested that the dispositional aspects of personality are brought out through contact with other entities, an idea similar to Noë’s (2009) notion of conscious awareness arising in response to external stimuli. The stimuli could come from incarnate minds as well as other discarnate ones, via psi. Barušs and Mossbridge (2017) remark, “interactions with discarnate beings could become possible when we stop filtering them out of our experience” (p. 178). This seems to me exactly right. It may well be that the dead are all around us and we have only to open our minds to them. Mediums are practiced at doing this and it happens to the rest of us when we see apparitions or experience other afterdeath contacts (Arcangel, 2005; Haraldsson, 2012). NDErs participate in this discarnate reality, and for the same reason: Their brains are out of commission and material from the subliminal is more easily passed up to the supraliminal.

Other stimuli for a discarnate mind might come from clairvoyant perception of the terrestrial world. Earlier I mentioned reports of veridical perception not only during the OBE stage of NDEs, but from pre-birth or “pre-existence” memories and intermission memories (Matlock & Giesler-Petersen, in press; Rivas, Carman, Carman, & Dirven, 2015). An intriguing aspect of these perceptions is the impression they give of being from a situated vantage point, typically above the scene in question. It is as if the discarnate mind were present at the same time and place as the embodied persons. Sometimes its presence is seen, heard, or felt by the embodied persons (Arcangel, 2005; Haraldsson, 2012), perhaps because of a momentary letting down of the guard that normally prevents psi inputs from reaching conscious awareness. If disembodied streams of

consciousness are localized in this way we may want to conceive of them as extended in space, although this extension would be very different from the physical extension we are accustomed to considering.

From prebirth and intermission memories, as well as from mediumistic accounts, it appears that discarnate minds are capable of thinking and acting on their own volition. One way this shows up is in the selection of new parents in reincarnation cases. We see this in solved international cases, those involving reincarnation in a foreign country. A motive for reincarnating abroad is discernable in all 14 such cases known to me (Haraldsson & Matlock, in press, Chapter 27). There are indications also that a discarnate mind's deep-seated beliefs, carried over from its recently concluded embodiment, can influence its choices about where and when to reincarnate (Haraldsson & Matlock, in press; Matlock, 2016b; Stevenson, 2001, p. 180). This provides an explanation for cultural patterns alternative to the view (expressed by Lester in Chapter 30) that all such patterns are products of parents guiding their children in accord with their culturally mandated belief systems.

Reincarnation is the process of joining a previously embodied stream of consciousness with a new body and a new brain. It is perhaps best thought of as the long-term possession of a body. Generally the process begins in utero, but it can take place after birth, with the replacement of one stream of consciousness by another (e.g., Mills & Dhiman, 2011). When the replacement is permanent it may be preferable to term the cases "replacement reincarnation" rather than possession (Matlock, 2016b). Replacement in utero is theoretically possible and appears to have occurred in the case of Titu or Toran Singh (Haraldsson & Matlock, in press, Chapter 23). In any event, a crucial aspect of the reincarnation process is the resetting of the supraliminal stratum of the consciousness stream. This comes about thanks to its connection with the new brain, which has much to say about how conscious awareness is experienced. A person's previous-life experiences are still present in the subliminal stratum of his or her mind, whence they sometimes erupt into conscious awareness, but more often influence his or her behavior in an unconscious way (Haraldsson & Matlock, in press; Matlock, 2016b).

I must comment on one other feature of the reincarnation process—the transmission of physical traits and practiced skills. Imaged, verbal, and emotional memories might be recorded in the subconscious and inherited as part of the mind, but physical traits and learned behaviors are different. Although memories of them might be preserved in the subconscious, they themselves could not be. Physical traits include birthmarks and birth defects commemorating death wounds but also internal diseases and physical likenesses in stature, facial structure, eye form, skin color, and the like (Stevenson, 1997). Skills include speech performance and other "knowledge how," which some subjects have displayed (Haraldsson & Matlock, in press; Stevenson, 2001). Stevenson (1997, 2001) suggested that physical traits are conveyed to a new physical body in a subtle body he called a "psychophore," but could they not be produced more easily by the reincarnating mind acting psychokinetically on its new body? Similarly, neural pathways underlying skillful behavior might be imprinted on the new brain by the reincarnating mind (Matlock, 2016b). I do not imagine these things happening by conscious will; the physical traits and skills replicated tend to be ones to which the previous person was emotionally attached, and that implicates the subconscious. This proposal is novel but not unrealistic. We have a great deal of evidence of the mind's ability to influence its body and I am only pushing that influence back to the earliest stages of life.

I call my theory of survival and reincarnation the "processual soul theory" in acknowledgement of its debt to Whitehead's process metaphysics. It is unlikely to impress those committed to materialism and reductionism but other scientists and scholars may find it useful. It presents an alternative to substance dualism as a conception of mind and body, explains how the mind interacts with the brain, describes the nature of disembodied existence, and indicates how reincarnation works. The theory has a good fit with background knowledge in psychical research and parapsychology. Its scope is broad and unifying, integrating survival research into parapsychology at large by showing the role psi plays in the functioning of disembodied minds and in the interaction between embodied and disembodied minds. It has the potential to suggest new lines of investigation in parapsychology. The inclusion of reincarnation expands its scope and potential fertility well beyond parapsychology, however. Reincarnation has great explanatory power (Stevenson, 1977, 2000) and 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 when they come to be accepted. My ideas are in line with recent proposals from others—from *BP* and Barušs and Mossbridge (2017), in particular—and that time may be nearer than many people think. It is becoming increasingly clear that Martin and Augustine, et al., are fighting a losing battle.

*4 Booneville Cemetery Road
Fayetteville, TN 37334, USA
jgmatlock@yahoo.com*

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.⁴ (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

⁴ 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 psychical 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*.⁵ For example, we are told

⁵ 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.⁶ 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.

⁶ 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.⁷

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-

⁷After 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 conceptions⁸ (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.⁹

⁸ 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.

⁹ 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 thesis¹⁰—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.

¹⁰ 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.¹¹ 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).¹² 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:

¹¹ 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.

¹² 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.¹³ 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-

¹³ 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.¹⁴ 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.¹⁵ 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.

¹⁴ 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.

¹⁵ 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.¹⁶ (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-

¹⁶ 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.¹⁷ 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

¹⁷I 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.¹⁸ 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).

¹⁸ 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,¹⁹ 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.²⁰ 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.²¹ And if they do not exhaust them because a primarily nonphysical mind might also have a few physical

¹⁹ 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.

²⁰ 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).

²¹ 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

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,²² 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)²³ 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.

²² 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.

²³ 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,²⁴ 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

²⁴ 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 case²⁵ 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 wish²⁶ 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-

²⁵ 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).

²⁶ 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).²⁷ 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

²⁷ 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.

c/o Internet Infidels
711 S. Carson St., Suite 4
Carson City, NV 89701, USA
kaugust@infidels.org

RESPONSE TO MATLOCK

By Claus F. Larsen

Matlock points to the critical review by Bem (2005) as a different interpretation of some of the results and suggests that I should have included it in my critique, as it would supposedly have changed my conclusion that nothing paranormal was going on.

I was somewhat surprised that Matlock brought up Bem’s critique of Schwartz’s experiments, as Bem is pretty harsh on Schwartz’s methodology, and deservedly so. The only example of possible paranormal activity that Bem points to is a test that Gary Schwartz did with medium Laurie Campbell (LC):

To pit the afterlife hypothesis against the psi hypothesis, Schwartz conducted a follow-up experiment in which he created a list of 12 people, 6 living and 6 dead, writing the name of each on a separate index card. As LC sat in the same room with him, he pulled out an index card, concentrated on the person listed, and then asked her three questions: Is the person male or female? Young or old? Living or dead? Schwartz hypothesized that “the living subjects would not be aware of LC’s attempts to communicate with them, and therefore they would be unlikely to ‘communicate’ with her. So we expected she would receive more information from the deceased people than from the living” (p. 43). Even though LC did not know who was on the list of names, she correctly answered all 3 questions for all 12 people, a perfect score. If we ignore the problems of potential sensory leakage and concerns about randomization using a closed deck of alternatives, then this result favors the psi hypothesis. In fact, the most parsimonious interpretation of this study is that it is a (badly controlled) test of telepathy between the medium and the experimenter. (Bem, 2005, p. 175)

This is symptomatic of the poor standards that mar too much of parapsychological research and reveals just how deep wishful thinking goes: Bem acknowledges that the experiment was badly controlled, yet he concludes that telepathy, and not trickery, is the most parsimonious interpretation. Not so.

The reason may be hard to understand, but is also scientific to its very core: If we accept a badly controlled experiment as evidence of a paranormal phenomenon, whatever that may be, just how low

should the bar be set for us to come to the contrary conclusion? Carl Sagan popularized the phrase “extraordinary claims require extraordinary evidence,” and it still stands: We cannot, and should not, lower the bar for paranormal evidence, but instead set it higher.

As for the problems of sensory leakage, did it not strike Bem (or Matlock, for that matter) why Campbell suddenly could get a perfect score when tested for telepathy, when she was struggling to get hits when tested for mediumship? A *psychic* who scores perfectly when tested for *telepathy*?

Had I included Bem’s critique, as Matlock suggested, I would certainly not have focused on the one thing in Bem’s critique that is favorable, however unwarranted, to Schwartz’ results. I would instead have included everything else, which is strongly critical of Schwartz’ experimental setups, and especially his own interpretation of the results. In other words, I would not have focused on the—supposed—hit, but ignored the misses. Including Bem’s critique in my own words would thus not have altered my conclusion but only strengthened it: Schwartz did not find evidence of anything paranormal.

editor@skepticreport.com

REPLYING TO MATLOCK

By Ingrid Hansen Smythe

In replying to James Matlock’s review of my paper, “Objections to Karma and Rebirth: An Introduction,” I’d like to dive in with the one insurmountable problem that essentially renders all other considerations moot, which can be stated like this: Matlock’s entire elaborate metaphysical postmortem schema is built on the notion of the soul, which he imagines (and I use the word “imagine” with intent) as “a stream of consciousness” that differs from what he calls “body consciousness” because, unlike every single other thing that lives, without exception, *it survives death*. He then proceeds to speak knowledgeably about this alleged death-surviving stream of consciousness, sometimes employing impressive-sounding jargon—it has a “supraliminal as well as subliminal strata” (JM, p. 200), it is “sensual but not fully sentient” (JM, p. 200), it has a “capacity for attention, intention, will, and memory”, it has no need of an astral body, it begins “at the inception of biological life and death” (JM, p. 200), and “return[s] in new bodies” (JM, p. 200) by force of habit—on and on it goes, until one might well have forgotten, as Matlock appears to have done, that the entire idea was just sheer dreamt-up, made-up, knocked-off speculation in the first place—what, in actual fact, we would normally call fiction. I use the word “fiction” with intent, too, and reading Matlock on souls, I could well imagine that I might be reading a Wiki page on the topic—they’re immortal, intelligent, they have attention, intention, will and memory and so on and so forth—but all that can be said about Dementors (Rowling, 1997), or the Deep Ones (Lovecraft, 1928), or any one of a number of things that people have just made up. The point is that, epistemologically speaking, souls, Dementors, and Deep Ones (froggy fish-people who worship Cthulhu, the undersea Dark Lord of Madness) have *exactly the same epistemological status*. They’re fictions and there’s not one single datum of evidence for any of them.

So if souls aren’t real (and let’s not pretend to have proven what hasn’t been proven) then it doesn’t make a whole lot of sense to get worked up about what happens to them after we die. It’s like if I ask you to imagine that you’ve got a billion dollars, and then we conveniently forget the fact that it’s imaginary and spend the rest of our lives planning how we’re going to spend it. You can *imagine* that you have a billion dollars, or a soul, but to construct a life, or an afterlife, around it seems premature at best, and positively destructive at worst.

So this is how Matlock begins—with an unproven assertion—from which he spins out his elaborate conclusions. Interestingly, the no-evidence problem doesn’t seem to bother survivalists in the way that it bothers skeptics, and here I think we have a fundamental difference between believers and nonbelievers. Matlock gives it away in a different context when he mentions Norman Cousins, who claimed to have cured himself of ankylosing spondylitis with laughter and vitamin C. Matlock accepts Cousins’ claim at face value, in presumably the same way that he accepts the claims of those who say they remember their time

as slaves or kings in ancient India or Egypt or some other exotic locale. The skeptic, however, immediately puts the claim to the test and asks, “What other explanation might there be for this extraordinary claim?” The word “misdiagnosis” will probably pop into the mind of the skeptic unbidden, because it’s by far the simplest explanation and ever so much more likely (particularly if you understand what ankylosing spondylitis *is*) than Cousins’ fantastic assertion.

It seems to me that believers tend not to look very hard for other explanations. (I note that Matlock uses the phrase “otherwise inexplicable” on his website; Matlock, 2016a) and seem more dispositionally inclined to believe the mysterious, complicated, supernatural explanation than the slightly more (possibly disappointing) plain old natural one. They tend to be more impressed by anecdote than skeptics are, and especially by an accumulation of anecdotes that seem to point in the same direction. A million people may claim that their guru can levitate, for example (see Sai Baba), and those million claims will add up in the mind of the believer to be somehow more convincing than just one—whereas the skeptic will likely conclude that the probability of defying the law of gravity is so low that there must be another explanation (optical illusion, strings, some other form of good old-fashioned deceit), and a million claims don’t make it a million times more believable. They don’t make gravity a million times less powerful. Yes, on the one hand the guru *may* be levitating (and souls may be reincarnating) but—on the other hand—there could be an explanation that doesn’t defy everything we know about the nature of reality.

Skeptics, it might be said, simply have more respect for that other hand.

It’s evident that the skeptic simply demands a higher standard of evidence than the believer. There’s a higher bar to clear—as high as it would be, say, in a normal court of law. But sadly for survivalists, all they have in their arsenal are speculation, anecdote, and faith. That’s it. And because there’s no hard evidence, faith is really the beginning and end of the argument where souls are concerned, and here it’s instructive to keep Peter Boghossian’s (2013) superb definition in mind: “Faith is pretending to know things you don’t know” (p. 10). That’s why the skeptic remains unimpressed and asks, simply, for the actual evidence. This seems only fair. If I tell you, for instance, that I’ve got a thing (of some sort), which I define as (whatever you like), but I can’t produce it and have zero hard evidence for it, and further make sensational claims about it (it’s immortal!), you’re surely right to be entirely underwhelmed and to drift away, ultimately uninterested, until such time as I can give you a little more to go on. Until I can do that, it’s only sensible that the jury should remain out.

The other thing that the skeptic (hopefully) keeps forever in mind, which the believer somehow conveniently forgets about, is basic human nature and the fact that human beings are so profoundly flawed when it comes to even basic mentation. The alarmingly long list of cognitive and social biases that apply to *all* of us, believers and skeptics alike, to a greater or lesser degree, is humbling to say the least, and surely it must be considered when assessing anecdotal claims. Humans are a storytelling species after all. We’re given to motivated reasoning, exaggeration, suggestibility, sensationalized story-telling to gain attention and status, lying to please others, self-deception, memory distortion over time, outright false memory, herd behavior, confirmation bias, commitment bias, the illusion of truth effect, selective perception, wishful thinking, imagining patterns where no pattern exists—one might be forgiven for thinking that unintentional (or even intentional) distortion is essentially the modus operandi of the human mind. That’s why the skeptic is extra vigilant, relying on the scientific method and demanding facts and hard evidence when assessing claims. It’s because the trouble with people is that they’re people—not faithful, unbiased recorders and regurgitators of the unsullied facts.

People are also highly culturally conditioned and this further muddies already muddy waters. The idea that crackers turn to flesh during digestion, say, or that small winged horses are intermunicipal transport systems for prophets, or that ancient aliens flew what look for all the world like DC-8s into hydrogen-bomb-exploding volcanoes, or that there’s a thing called a soul that (a) actually exists! and (b) survives the demolition of its rental property, or that the foreskin of Our Lord ascended into space and formed Saturn’s rings—some subset of people believe one or more of these things, despite the lack of evidence, but it’s possible that one of them (let’s say the soul idea) may stand out as being somehow more likely than the others. I’d suggest that the believer consider the possibility that the soul is a living hypothesis as opposed

to a dead one (per William James) and thus the gut feeling that souls exist has a lot more to do with early cultural imprinting than with truth.

Knowing how human minds really operate also leads to a criticism of Matlock's opener—that there are paradigmatic thinkers as opposed to data-led thinkers, and quite naturally (according to Matlock) skeptics of the afterlife belong to the first group and believers to the second. But these are surely absurd caricatures—abstract and impossible types—because everyone who isn't insane is a combination of both. Nobody is exclusively paradigm-driven or data-driven. Without a paradigm, your data are numbers and noise—values without meaning. And if all you've got is a paradigm, until such time as data can be slotted in all you've got is speculation, and possibly just wild fantasy unmoored from reality entirely. All of this exists on an open scale, with some people more or less of either. The question is, to what extent is a person examining data that *doesn't* reinforce his or her own worldview? That's what *MoA* is all about and it's curious that Matlock dismisses this out of hand, claiming that the authors are mere paradigmatic thinkers, not data-led thinkers, like survivalists. This is wrong (and a fine example of another bias—the illusion of superiority). The authors—including me—have examined the evidence and found it lacking, that's all. We all await further evidence with heady anticipation.

However, let's take for granted, as Matlock does, that souls are real and that they survive death and are reborn in new bodies over and over. The question then becomes, *how* do souls do this? What is the mechanism? Here Matlock is missing a scientific theory entirely,²⁸ but in any serious scientific inquiry one has to deal with the *how* question, and the answer has to be an actual mechanism, not just more speculation. Thus, even if the no-evidence-for-the-soul problem were overcome, the proponent of reincarnation still has the *how* problem to deal with before the skeptic can be fully satisfied.

I end my reply to Matlock with a couple of lesser points, which refer specifically to my paper. Matlock says that I deal “with a generalized concept of karma unrelated to any specific tradition, despite the sometimes considerable variations among the conceptions of different traditions” (JM, p. 197). That's true, but might raise suspicions—are there kinds of karma that might pass the test if only I'd considered them? The answer, to relieve the suspense (!), is a flat no. I acknowledge in my paper's very first footnote that I'm using Karl Potter's CKTI (classical karma theory of India) as the standard model (Potter, 1987, pp. 109–110)—this was intentional—but the question is, what does Matlock consider to be a “considerable variation” and are the variations relevant? Karma has a definition, after all, and there are elements that all traditions have in common. Does it make any difference to the arguments against karma that, for example, some traditions allow karma, like some form of metaphysical cash, to be transferred, and other traditions believe this idea to be obviously preposterous? Not at all, because the salient point regarding any form of karma is that (like the soul, and here we go again) there is precisely no evidence for it whatsoever. Karma is like a unicorn in that way. It doesn't matter if it's the Indian unicorn or the lesser Southeast Asian unicorn—there is *no* variation of said unicorn for which there is a shred of evidence. Likewise karma.

“[R]eincarnation does not entail karma,” says Matlock, and adds that, “[u]ndoubtedly part of Smythe's purpose is to undermine the idea of reincarnation by linking it to karma” (JM, p. 197). No, I think what undermines the idea of reincarnation is not the karma problem but the zero-reason-to-believe-we-have-souls problem. Without a thing that reincarnates, reincarnation's a nonstarter. It's like imagining a spinning wheel that keeps on spinning long after the wheel itself has ceased to exist. Karma's just an added fantasy, and an example of yet another bias—the just world fallacy, which states that we all get what we deserve in the end. Matlock doesn't believe in this punishing form of karma but claims instead that souls are reborn in accordance with their disposition or character—what we might call charma as opposed to karma. This he bases on otherwise inexplicable behaviors and phobias and so on in the reincarnated individual, but whether the deciding factor is charma or karma makes no difference, just as it makes no difference if our billion dollars is in bills or in coins.

ihsmythe@shaw.ca

²⁸ “Fundamentally, a theory in science is not just a whim or an opinion; it is a logical construct of how we think something works, generally agreed upon by scientists and always in agreement with the available observations” (Sellers, 2016).

WHOSE PREJUDICE? A RESPONSE TO THE REPLIES OF AUGUSTINE, SMYTHE, AND LARSEN

By James G. Matlock

I thank Keith Augustine, Ingrid Hansen Smythe, and Claus Flodin Larsen for taking the time to reply to my essay review of *MoA*. I would also like to thank John Palmer for making possible this extended discussion of survival issues in the *Journal of Parapsychology*.

Not surprisingly, Augustine, Smythe, Larsen, and I have substantial disagreements. They are committed to a materialist world view and to a physicalism which claims that mental states derive from and are entirely dependent upon brain states, whereas I have become convinced not only that the mind has an existence independent of the body but that it survives the body's demise, persists for a time in a discarnate state, and then rejoins, or, as is commonly put, reincarnates into, a new physical body.

I have not always been as confident of this position as I am now. In 1990, I concluded a review of Ian Stevenson's reincarnation research and criticisms of it with the statement that, although the data then available justified continuing with the research, "it would be rash to declare that reincarnation has been shown to occur." I added, "Until the data and concepts discussed in this chapter can be assimilated to the rest of scientific knowledge, the data, at their best, will remain no more than suggestive of reincarnation" (Matlock, 1990, p. 255). As recently as 2011, I wrote, "although I think that current data point in the direction of reincarnation, we must be cautious in our conclusions, since it may turn out that our present ideas are not quite right and that another solution, which we cannot yet see, is the correct one" (Matlock, 2011, p. 809).

What brought about the change in my views? In part it was going over the research that had been conducted since 1990 and realizing how strong the case for reincarnation had become; in part it was developing a theory of survival and reincarnation that connected to other areas of scientific knowledge. I describe some aspects of my theory in my review. I deal with others in a book coauthored with Erlendur Haraldsson (Haraldsson & Matlock, in press), and I develop my ideas more thoroughly in a work in preparation (Matlock, 2016c). The evolution of my thinking shows what it means to be led by data in forming one's convictions about what our world is like. By contrast, Augustine, Smyth, Larsen, and the other *MoA* contributors appear to be paradigmatic thinkers who interpret data in light of a world view to which they are committed. Evidence that supports their world view is accepted and strengthens it, but evidence that runs counter to it is questioned and set aside.

I do not regard paradigmatic-thinking to be a pejorative label, as Augustine and Smythe seem to have interpreted it. Rather, I use it to indicate a particular cognitive style, different from the cognitive style of data-led thinking. This difference helps to explain why paradigmatic and data-led thinkers can look at the same evidence and come to different conclusions regarding its import. Of course, there are times that paradigmatic thinkers and data-led thinkers are not looking at the same evidence, because the paradigms to which paradigmatic thinkers adhere tell them that certain data are not worth considering, so they do not bother to familiarize themselves with them. Most survival skeptics are at best only superficially acquainted with the evidence for postmortem survival. Data-led thinkers, by contrast, are fascinated by data of all kinds and let the data tell them what to believe and not to believe.²⁹

Griffin (1997), from whom I borrowed the paradigmatic-thinking and data-led thinking contrast, suggested that paradigmatic thinkers need to have their paradigms confronted before they will consider data that conflict with them, and that is one reason I began my review with a consideration of two basic assumptions underlying materialism and physicalism on display in *MoA*, the inference from correlation to causation in mind/brain relations and the purported causal closure of the physical domain. I then dealt with the way survival research was handled in Part IV of *MoA*, and I sketched out a theory of survival and rein-

²⁹ Paradigmatic thinking should not be interpreted as implying a disregard for data. Paradigmatic thinkers consider data, and at some stage may have been data-led, but they have become solidified in their views, in contrast to true data-led thinkers, who continually test their positions against new information.

carnation to show that there is a way of imagining survival that surmounts many of the objections to substance dualism, the understanding of mind/brain relations that many *MoA* contributors attack and evidently presume to be the only way that postmortem survival can be conceived. I have organized this response around these same four themes in the replies of Augustine, Smythe, and Larsen. Although I do not attempt to respond to all of their points systematically, I believe that I address the main ones.

Brain and Mind

Several *MoA* contributors list ways that mental states are correlated with brain states, from which they infer that brain states bring mental states into being. Augustine and Fishman state, “We now have ample evidence that our mental lives are not merely correlated with brain activity, but positively caused by it” (p. 203). Three responses to this argument are available to survivalists. One is simply that correlation is no indicator of causation; this remains true no matter how vigorously mortalists maintain that correlations reveal a causal arrow running from brain to mind. Another response is that we have reason to think that the causal arrow may run from mind to brain as well as from brain to mind. Emily Kelly considers a range of studies that suggest the mind may impact the body under a variety of conditions (E. W. Kelly, 2007b).

Jeffrey Schwartz (J. M. Schwartz & Begley, 2002; J. M. Schwartz, Gulliford, Stier, & Thienemann, 2005; J. M. Schwartz et al., 2004) has demonstrated that the effort by sufferers of obsessive compulsive disorder (OCD) to change their behavior bring about changes in their neural organization, as portrayed in fMRI scans. Beauregard (2007, 2012; J. M. Schwartz, Stapp, & Beauregard, 2004) has documented the ability of people to self-regulate their emotional states. Augustine approvingly cites Clark and Dennett’s (2008) response to Schwartz’s and Beauregard’s work:

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. (Clark & Dennett, 2008, p. 22)

Now, is not whether “thought, attending and mental activity are . . . realised in material substance” the question at issue? Clark and Dennett’s argument holds only if we accept that they are. They are producing a tautology based on their preconceptions, rather than confronting the data presented by Schwartz and Beauregard, who draw the opposite conclusions from them.

A similar avoidance of the implications of the data is apparent in Augustine’s admitting the reality of psychophysiological influence, yet steadfastly refusing to believe that it represents the action of an independent mind. He says that my statement, “behavioral changes guided by will can sometimes bring about the neural reorganization” [is] “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”(KA, p. 215). True, but Augustine misses the point. What initiated the behavioral changes? It was an OCD patient’s decision and determination to carry the job through.

In his Footnote 9, Augustine proclaims:

. . . 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. (KA, p. 214n11)

This is a curious but revealing comment. Independence thesis supporters appreciate that because

one cannot read causality into correlation, one cannot take correlation as indication of mind-to-brain influence any more than one can take it as indication of brain-to-mind influence. However, independence thesis supporters also recognize that when cerebral changes are preceded by the will to do something, there is a sequence of events that suggests that the brain is changing in response to the willed action. This is very different from inferring, say, that because certain parts of the brain light up when one remembers something, that the brain is causing the memory to arise, or that the memory is reconstituted from a trace stored in the brain. Augustine dismisses the significance of precedence, but it is crucial here.

If mental actions can impact the brain, as J. M. Schwartz's work on OCD and Beauregard's research on emotional self-regulation suggest, or the body, as we see in phenomena as varied as the placebo effect and stigmata (E. W. Kelly, 2007b), then a mind must be able to influence the body to which it is related. It is not clear that Augustine grasps this point. Instead, he deflects attention to survival concerns. He says: "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*—has much of anything to do with the reality of life after death or mind-body separation" (KA, p. 209). I suspect that this is true even of people in parapsychological circles. Emily Kelly puts these phenomena forward as examples of psychophysiological influence, not as indicators of life after death.

Because Augustine does not address the real issue here, we don't know whether he denies the reality of the placebo effect, etc., or how he would explain these phenomena in physicalist terms, if he accepts that they occur. In attempting to square his brain/mind identity doctrine with his acknowledgement that causation can work both ways he cites a personal communication from *MoA* contributor Piccinini to the effect that "the issue is not about causation at all; it's about the synchronic metaphysical relation between mind and brain" (KA, p. 214). Just what sort of "metaphysical relation" is involved, Augustine cannot tell us, and it is not at all clear why events that proceed in sequence should be regarded as synchronic, anyway.

A third response to the claim of causal inference from correlations between mental and physical states is that it does not follow from the undoubted influence of ingested substances, brain damage, and so forth, on conscious awareness that the mind as a whole is affected by these things. Our minds include subconscious strata in addition to conscious awareness, and the subconscious allows the mind to carry on even when conscious awareness is impaired or interrupted. That is why we can awake from sleep after a period of being unaware. Augustine calls my appeal to the subconscious an "auxiliary assumption" (KA, p. 216), which it is from his brain/mind identity perspective. I, however, begin with the concept of a complex consciousness, so evoking the subconscious entails the introduction of no new, ad hoc variables.³⁰

Augustine discusses Horder's (Chapter. 9) critique of William James' suggestion that instead of producing consciousness, the brain transmits and filters it, like light passing through "colored glass, a prism, or a refracting lens" (James, 1898, p. 14). Horder calls on Lamont, who declares that, "if the brain is a stained-glass window and a beam of white light shines through it . . . then we are the colored light that results, not the white light itself" (Lamont, 1935/1990, p. 104). Why are we not both the white light and the colored light, though? The colored light is a version of who we are, no more than that. Augustine, however, endorses Horder's conclusion that, "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," Augustine avers (p. 214). True enough, but is his premise valid? Must one have a functioning brain to be aware of one's mental functions? That is the mortalist's assumption, but it is not the survivalist's assumption, and it is not the

³⁰ My concept of a complex consciousness comes from F. W. H. Myers (1903), who wrote about a "threshold . . . of consciousness," with a "subliminal" level below and a "supraliminal" level above. Myers understood the subliminal part of consciousness to include "not only those faint stimulations whose very faintness keeps them submerged, but much else that psychology as yet scarcely recognizes; sensations, thoughts, emotions, which may be strong, definite, and independent, but which, by the original constitution of our being, seldom emerge into that *supraliminal* current of consciousness which we habitually identify with *ourselves*" (1903, vol. 1, p. 14; emphasis in original).

conclusion drawn from the evidence by many data-led thinkers. Yes, there have been those who have questioned the degree to which conscious awareness and cognitive processes persist after death (e.g., Broad, 1925; Murphy, 1945), but others have found good evidence that they do in some instances (e.g., Braude, 2003; Carter, 2012; Ducasse, 1961; Gauld, 1982; Griffin, 1997). My view is that most of what makes up our personality, and our individuality, is carried in our subconscious, and that conscious awareness may not be as important when we are disembodied as when we are embodied, but I don't see how the existence of conscious awareness—and deliberative agency—in the discarnate state can be denied altogether. Much of the fragmented personality that comes through in mediumistic messages may be due to difficulties of psi communication between disembodied and embodied mental streams, rather than the absence of a coherent presence in the beyond.

Descartes (1644/2009) famously proposed that the mind—for him, an immaterial thinking substance—interacted with the brain through the pineal gland. That possibility was quickly ruled out, and with its rejection came doubts about his dualistic conception of mind (soul) and body. How is it possible for an immaterial soul to interact with a material body, it is asked? In *MoA*, that question is posed most directly by Kim (Chapter 13). I do not know who first suggested PK as a solution. Probably it was Thouless and Weisner (1947). Others have made the point since, most notably Griffin (1997), who assigns psi a central role in his panexperientialism. In my review, I put forward PK as an answer to Kim's titular question, "What could pair a nonphysical soul to a physical body?" Augustine counters that PK is a "mere placeholder for an explanation" and that "to say that psychokinesis is what allows nonphysical-physical interaction is just to say that nonphysical-physical interaction allows itself" (KA, p. 220). This makes no sense, unless one assumes that a physical basis for PK will ultimately be discovered. To the question, "what could pair a non-physical soul to a physical body?", PK—taken as an ability of mind, not brain—is an obvious and perfectly acceptable answer. If psychokinesis did not exist as a term and concept in this sense, philosophers would need to invent it.

Augustine says, "For such collections [as *MoA*] there is no expectation that any particular author be well versed in the diverse subject matter of other chapters that fall outside of his 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" (KA, p. 205). This charge is so obviously unfounded, I am not sure that it requires a response. I wonder if Augustine considers any research that is not materialist in design to be psychological research. I do not expect contributors to the first three parts of the book to be acquainted with, much less conversant with, the psychological research literature as such, but I think it is reasonable to ask them to address findings within their fields that conflict with their favored positions. A few contributors do this, although most do not, and it is to this deficiency that I was drawing attention.

Proponents of the dependence thesis are fond of moving from the assertion of brain/mind identity to the claim that they have shown that the mind cannot possibly survive bodily death, and they assume that defenders of the independence thesis hold the converse position, that because mental causation is suggested by some of the data, survival necessarily occurs. This assumption appears again and again in Augustine's comments. It is not a valid assumption, at least not in parapsychology and among survival researchers. Many parapsychologists doubt that the mind survives death, although those who take this stance have not made clear whether they are adopting an epiphenomenal position that understands the mind as emergent from cerebral activity, or if they believe that the mind comes into and goes out of existence along with the physical body. In any event, we may say that the mind's apparent ability to affect its body is consistent with the idea that the mind has an existence independent of the body, but that is as far as we can go. Survival issues have come up in the discussion thus far because Augustine has brought them up, not because the data have led us to them.

Quantum Indeterminacy and Causal Openness

A second major theme of *MoA*, and a prop in its physicalist argument, is causal closure. In my review, I define causal closure as the doctrine that "for every physical effect there is a physical cause" (JM,

p. 195). Augustine objects that this is “a little too strong” (KA, p. 218). He would prefer to define causal closure “as the idea that, for every physical event that has a cause, its cause is physical.” “Otherwise,” he explains, “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” (KA, p. 218).

Augustine makes a good point here, but his weaker definition of causal closure is not embraced by all of his fellow *MoA* contributors. McCormick (p. 63) says that causal closure means that “all physical events or effects are fixed in a fully physical prior history. The physical realm, as far as we can ascertain, is causally complete.” Kim (1993a, p. 280) has said that the doctrine asserts that, “no physical event has a cause outside the physical domain.” Kim’s definition is consistent with Augustine’s, in that it leaves open the possibility of physical events that have no cause, and merely stipulates that physical events cannot have causes that are not themselves physical.

The stronger form of the causal closure doctrine (espoused by McCormick) is consistent with Newtonian or classical mechanics, which is deterministic. If one knows how a physical process began, one can calculate how it will end. Classical mechanics was once thought to provide a complete description of physical reality. Physicists at the turn of the 19th Century believed they had nothing important left to learn and that the few remaining questions would be resolved within the same deterministic framework. They were wrong. The solutions to the outstanding problems (among them how light behaved) led at the beginning of the 20th Century to the realization that classical mechanics gave only an approximate description of interactions at the macroscopic level and failed utterly when applied to the exceedingly small and tremendously large. Quantum mechanics was developed to describe events below the level of the atom and its rules are very different from the rules of classical mechanics. The behavior of subatomic particles is random and the mathematics of quantum mechanics is probabilistic rather than definitive.

Although quantum mechanics works extremely well in practice, there is no universally accepted understanding of why, or of what it means. Augustine supplies a reading designed to bolster his brain/mind identity assumptions. Even so, he is forced to acknowledge that causal closure in its stronger form is untenable, and he can make the weaker form acceptable only by interpreting a central feature of quantum theory, the observer, as an object, rather than as a human being. To understand how he does this, and what the alternative is, we need to take a closer look at the standard or Copenhagen interpretation of quantum mechanics and its Von Neumann extension.

The Copenhagen interpretation was advanced by Werner Heisenberg and Niels Bohr, two of the founders of quantum mechanics, in the 1920s. From the start, in contrast to Newtonian concepts, quantum theory made room for consciousness. The founders recognized that experimental outcomes could be explained only if the actions of the experimenter (the observer) were taken into account. Because an experimenter’s actions are the results of his or her choices, determined by free will, and are not reducible to neural circuitry, this effectively brings an independent mind into the picture. Building on these Copenhagen principles in his 1934 book, *The Mathematical Foundations of Quantum Mechanics*, Von Neumann showed that all aspects of an experimental set up, including measuring devices and the body of the experimenter himself or herself, were part of the same quantum system. There is no break in the system until one reaches the mind of the experimenter, the only element that lies outside the system, and thus the only element able to have an external influence on the experiment (Stapp, 2011, 2015).³¹

Augustine joins other materialists in rejecting Von Neumann’s conclusion and insists that the mea-

³¹ This is a very rough summary of what Von Neumann worked out. Augustine refers to “the Von Neumann-Wigner interpretation” of quantum mechanics, merging Von Neumann’s demonstration with Eugene Wigner’s interpretation of it as consciousness “collapsing the wave function” in a quantum process. Wigner’s description is an oversimplification of Von Neumann’s, but associating Wigner with Von Neumann is sometimes used to try to diminish Von Neumann’s work. My account follows Stapp’s (2015) more conservative presentation of the process Von Neumann described. An observer’s decisions are of central importance, but so is a response from “nature,” which may or may not support the observer’s expectations.

suring device itself could make the “observation” that brings a quantum process to a head. Not only does this position ignore the role of the observer in initiating an experiment, but, because Von Neumann showed that the measuring device is part of the same quantum system as that which is measured, it provides no explanation for how an experiment plays out. It is important to recognize that the theory was never intended to be a general theory of the nature of reality, but only a practical experimental tool (Stapp, 2015). Still, what standard quantum theory suggests is that an independent mind may play a role in the construction of the physical world. Quantum indeterminacy makes room for, if it does not entail, causal openness, and that openness may be exploited by mental action. Moreover, contrary to what Augustine claims, quantum processes could well be involved in the interface of consciousness with the brain (J. M. Schwartz, Stapp, & Beauregard, 2005; Stapp, 2015), and the direct action of the mind on the brain certainly could influence behavior.

Augustine makes a great deal of the supposed absence of “interactive traces,” evidence of interaction between mental activity and the material world that would be expected if physical reality were causally open rather than closed. He refers to Chapters 14, 15, and 16 of *MoA* for support. These chapters (by Wilson, Papineau, and Angel) are devoted to descriptions of physical laws and declarations that “nonphysical souls” would have to violate them if they were to have causal effects on the world. There is no discussion of potential evidence, e.g., from psi research, that the laws are in fact violated under certain conditions, and hence that they may not provide the last word about the structure of nature. Interactive traces are simply declared to be absent, and impossible in principle, because they would violate physical laws. There is a profound circularity to Augustine’s reasoning here, which he appears not to recognize.

The causal openness of quantum indeterminacy has nothing to do with the question of whether the mind survives bodily death, except that unlike the doctrine of causal closure, it does not rule out survival a priori. This is important because, in conjunction with the rejection of the brain/mind identity, it changes the assessment of priors in a Boolean evaluation of the probability of postmortem survival. I do not see how this can be denied, yet in reply to my earlier comments to this effect, Augustine goes on at length about lesser factors he wants to take into account as well. These include parsimony (it is more parsimonious to believe there is no soul than that there is one), the uncertain nature of discarnate interaction, and the supposed incompatibility of survival with physical evolution. I will deal with each of these topics later. At this juncture, I only want to reiterate that because survival cannot be ruled out on the basis of brain/mind identity or causal closure, the antecedent probability of survival is considerably higher than it would otherwise be.³²

Augustine quotes from his chapter with Fishman, “we will charitably assign equal probabilities of 0.5 to the dependence and independence theses” (p. 260). This is quite charitable indeed, because if all the factors they bring into consideration are weighed as they would like to weigh them, the probability of survival should be near 0. In any event, I was not discussing Augustine and Fishman’s Boolean analysis in my comments, although inasmuch as I introduced the paragraph with reference to it, I see why my remarks were taken that way. All I intended was to make a point about the significant alteration in the posterior probability of survival if brain/mind identity and causal closure do not obtain. I believe that these are the decisive issues bearing on the survival question and with them settled, the probability that something survives is roughly equal to the probability that nothing survives. This sets the stage for consideration of the survival evidence. In my review, I argue that adding the evidence tilts the balance toward survival. Augustine disagrees.

³² Augustine asserts that I am confused about what constitutes a background consideration in a Boolean analysis. He says, “neither psychophysiological influences nor violations of closure (i.e., interaction) are prior probability considerations to begin with. Rather, they are facts to be explained” (KA, p. 217). He is interpreting my point too narrowly. I wrote, “If we reject the notions that the brain always acts antecedent to mental events and that the physical realm is causally closed, the calculus changes so that the dependence and independence theses are more equal in their prior probabilities” (JM, p. 200). The assumptions of brain/mind identity and causal closure are background considerations, and if assumptions regarding them are changed (so that the brain/mind identity doctrine is given up and causal openness replaces causal closure), the background estimation changes.

Assessing the Evidence for Survival

Augustine concedes that *MoA* could have treated the survival evidence more fully. The book has very little on apparitions or trance mediumship, two areas that have contributed large proportions of the relevant data. Apart from Angel's analysis of the Imad Elawar case (Stevenson, 1966), published originally by Angel (1994) and contested by Barros (2004), the handling of reincarnation concentrates on research methodology and does not attempt to come to terms with research findings. Much attention is given to poltergeists and NDEs, which are peripheral to the survival question. Yes, I should have acknowledged that many poltergeist effects have been shown to be fraudulent; nevertheless, the person-centered PK factor is thought by parapsychologists to explain many cases (Roll, 1972, 1977), with survival implicated in only a small number of them (Stevenson, 1972). Some NDE phenomena imply survival (Rivas et al., 2016, pp. 221–237), yet NDEs in and of themselves furnish no direct evidence for it.

The two chapters on mediumship focus on the work of Gary Schwartz and Beischel with mental mediums. In his reply to my comments on his chapter re Schwartz's experiments, Larsen insists that sensory leakage and trickery are more likely explanations for the reported results than some degree of psi, contrary to what Bem (2005) suggested in his review of Schwartz's book (G. E. Schwartz, with Simon, 2002). Larsen expresses surprise that I would call his attention to Bem, "as Bem is pretty harsh on Schwartz's methodology, and deservedly so" (CL, p. 231). Apparently Larsen believes that I am unable to be critical where required. The deficiencies of Schwartz's work are well recognized, and I am not going to defend it. Nor am I going to defend Beischel's work with G. E. Schwartz (Beischel & Schwartz, 2007), discussed in the chapter following Larsen's. The point I was trying to make, but could have been clearer about, is: Yes, sensory leakage could have played a role in these experiments, but even if some of the results might be interpreted as psi acquisitions, they would provide no support for survival. This is the problem with mental mediumship: Even in well-controlled experiments, the procedure makes it impossible to decide between psi and survival as an interpretation of positive results. Beischel herself has acknowledged this drawback (Beischel, 2013, p. 179). By including these chapters but omitting any extended discussion of trance mediumship, some of whose phenomena are much more difficult to explain in terms of either sensory leakage or psi (Braude, 2003; Gauld, 1982), *MoA* avoids confronting the better evidence for survival.

Augustine is absolutely right that

. . . 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. (KA, p. 226; his emphasis)

This is why it would be wise for survival critics to avoid using the concept of unlimited psi of living persons (commonly termed superpsi, after Braude, 1979)³³ as an explanation for the survival evidence. Augustine continues,

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. (KA, p. 226)

There is logic to this argument, and if we are not going to allow the theoretical possibility of an unlimited psi to undermine the evidence for a limited psi, we would do well to define psi and superpsi more carefully. In my review, I suggested that superpsi be considered not as an unusually extensive psi, but as an unusually complex psi. Regular and complex psi may be defined with greater precision. Regular psi, we

³³ Braude's concept of superpsi is an updating of the super-ESP construct introduced by Hart (1959).

may say, “involves the transfer of information between two minds, the acquisition of information by one mind, or the direct action on the physical world or a biological entity by one mind, in a single step,” whereas superpsi “involves the acquisition of information from more than one source or a combination of information transfer, information acquisition, and direct action, in a single step or sequence of steps.”

Defining psi and superpsi in these terms allows us not only to keep the concepts distinct, but to talk more exactly about the role of psi in survival cases. If living agents possess psi abilities, it is a safe bet that deceased agents (if they exist) do too, and a great deal of survival evidence is best explained by psi transfers between the living and the dead. Much of this evidence is susceptible to a living-agent psi interpretation, although some of it requires supposing complex superpsi on the part of the living, as opposed to a simple psi transaction between the living and the dead (Braude, 2003; Gauld, 1982). This is true especially of the reincarnation data, which (as Braude, 2013, repeatedly observes) provide more of a challenge to survival deniers than do the data of mediumship, even trance mediumship. Reincarnation cases include not only information about previous lives, but behavioral and physical correspondences between the persons of the present and past lives, features that are hard to explain in terms of psi acquisitions by the living (Matlock, 1990; Stevenson, 2001).

With reincarnation, we appear to be dealing with memory rather than psi. Augustine, of course, believes that memory is recorded in the brain and is lost when the brain is lost. He quotes Gauld as saying, “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), without noting that Gauld tries to show that this view is not justified. The fact is, neuroscientists have never been able to discover where in the brain memory is stored, despite decades of effort directed at the problem (Braude, 2006; Gauld, 2007). Lashley (1950) spent 30 years trying to locate memory “en-grams,” but he finally gave up, concluding that memory was not recorded in the brain after all. There is no doubt that the brain becomes engaged when things are remembered, and different types of memory activate different parts of the brain (Gauld, 2007), but all that is evidence for is a correlation between memory retrieval and neural activity, not the reconstruction of memory from traces stored in the brain.

The lack of evidence for the trace theory of memory led Pribram (1991) to advance the theory that memory is represented holonomically and distributed throughout the brain, but that proposition has failed to gain widespread acceptance. Given the uncertainty among neuroscientists about whether memory is engraved in the brain, psychical researchers seem to me justified in considering other possibilities, especially inasmuch as their data tell them that memory cannot really be there. A currently fashionable idea is that memory is preserved in a subquantum “Akashic field” (Laszlo, 2007, 2009), from which it is retrieved by psi. However, I stand with those thinkers and workers (e.g., Broad, 1925, 1962; Ducasse, 1961; Myers, 1903) who hold that memory is stored in consciousness, more specifically in the subconscious portion of the mind. I believe that this makes good sense and helps to explain how it is possible to form and retain memories when the mind is not fully engaged with the brain, as during NDEs and reincarnation intermission experiences (Matlock & Giesler-Petersen, in press), and how it is possible for memories to be transferred between lives in reincarnation cases.

Augustine defends the criticisms of Stevenson’s reincarnation research by Ransom and Angel. Ransom’s contribution is a summary of a critique of *Twenty Cases Suggestive of Reincarnation* (Stevenson, 1966) he prepared for Stevenson when he was working as his research assistant in the early 1970s.³⁴ Contrary to what I state in my review, it seems that Ransom did accompany Stevenson into the field on three occasions. Ransom’s critique is limited to *Twenty Cases*, so perhaps these trips came later in his employ

³⁴ Augustine repeats what is stated in *MoA* (p. 571) and I noted in my essay: he had wanted to reproduce in *MoA* Ransom’s full report, along with written comments from Stevenson, but was unable to get permission to do so. Why then did he not have Ransom summarize Stevenson’s comments, to accompany his summary of his critique? In published remarks on the Ransom Report as it was presented by Rogo (1985), Stevenson (1986) stated that he had correspondence in his files that showed that he “encouraged” Ransom to share his critique with others. He only asked “him also to show (with his critique) some comments I had made on [it], and Ransom readily agreed to this reasonable request” (Stevenson, 1986, p. 237). Nevertheless, Ransom has twice since—first for Edwards (1996), and now for *MoA*—publicly shared the substance of his report without saying anything about Stevenson’s comments on it.

with Stevenson. In any event, as I pointed out in my review, Ransom's major criticisms were anticipated by Stevenson in the introductory chapter of *Twenty Cases*, so his report contains nothing sensational or devastating to anyone acquainted with Stevenson's work. *Twenty Cases* was Stevenson's first case collection, and it was based on the early, pioneering phase of his field investigations. Stevenson's methodology and reporting standards improved over time, as he readily acknowledged (Stevenson, 1975, pp. 8–18; 2001, pp. 130–140).

Augustine says, “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 found between individuals in ‘solved’ cases defy what we would expect to find by chance alone (Angel, 2008)” (KA, p.227). Angel describes his “experimental” approach slightly differently in his 2008 article than he does in *MoA*. In the 2008 publication, he proposes taking 20 sets of “correspondences” between present and previous persons from actual cases, replacing names with initials to form a control group of 20 sets of correspondences, randomizing the resulting pool of 40 sets of correspondences, and presenting the entire pool to judges to see if they can distinguish between the original sets of correspondences and the control sets. If the only difference between the experimental and control sets is the form of the names, this should not be difficult, but whether or not the judges are successful, would the results tell us anything meaningful?

The purpose behind Angel's proposal for what he calls “practical, controlled experimental work” (p. 575) is clearer in *MoA*. He states: “My contention is that Stevenson and others doing so-called empirical research into reincarnation have not even attempted to show that there is anything that needs to be explained here” (p. 578). In order to establish that there is something in need of explanation, Angel suggests comparing sets of correspondences between two people alive at the same time with sets of correspondences between subjects and previous persons in reincarnation cases. The idea appears to have been prompted by a series of 21 correspondences Angel identifies between his life and Stevenson's. Both men were born in Montreal, both received B.A. degrees from McGill, both were married twice, and so on. I suspect that such a test would find a clear distinction between the two groups of correspondences. The correspondences in the control group (the living people) are likely to be of a noticeably more general nature than those in the experimental group (the reincarnation cases). I encourage Angel and Augustine to undertake this test, pitting their hypothesis (they expect there to be no measureable difference, clearly) against mine, and to publish the results. If they are right, and there is nothing to be explained here, the sooner the reincarnation research community recognizes that, the better.

Because *MoA* does not do a good job considering the survival evidence, it cannot claim to have disposed of that evidence. True, I have not substantiated my claim that the evidence tilts the balance toward survival, but neither have Augustine and his colleagues raised serious doubts about it. There is little chance that we can resolve our differences in the present discussion. We can, nonetheless, take up the last of *MoA*'s major themes that I treated in my review: the problem of how to conceive of what might survive death, if something were to do so.

Conceiving Postmortem Survival and Reincarnation

Smythe remarks, “what undermines the idea of reincarnation is not the karma problem but the zero-reason-to-believe-we-have-souls problem. Without a thing that reincarnates, reincarnation's a nonstarter” (IS, p. 243). I agree with her. For there to be reincarnation, something has to have survived death. I too am unconvinced that that something is a soul, or that souls exist, if by soul is meant an immaterial thinking substance on the Cartesian model. It is not clear to me that that is what Smythe intends by “soul,” but it is the definition of soul used—explicitly and implicitly—by many *MoA* contributors. There is the strong presumption among them that substance dualism, or dualistic interactionism in some form, is the only way a mind can relate to a body, so that by critiquing Cartesian dualism, they are defeating survival on a conceptual level. Substance dualism, however, is not the only way of conceiving survival, as I tried to show in my review. There is another possibility, a process alternative, that I believe both meets the major philosophical

objections to substance dualism and receives greater support from the empirical data.

The problem with the Cartesian proposal, as I see it, is not only the lack of direct evidence for an immaterial thinking substance, but that that substance is supposed to be eternally unchanging, to lack extension, and to have no location in space. Additionally, Descartes (1644/2009) held that a soul, or ego, is bestowed by God at conception, and allotted to human beings only, ideas no longer in vogue among non-Christian and even some Christian³⁵ advocates of substance dualism. The only way I can conceive of a soul is as the subject or center of consciousness, the experiencer of both conscious and subconscious states. I call that the self, rather than the soul, though, in order to avoid confusion with Cartesian concepts.³⁶ My self is unlike a Cartesian ego in that it does not possess attributes, such as the ability to reason, but is the experiencer of a stream of consciousness, which possesses these attributes. It is this stream of consciousness, comprising both conscious and subconscious levels, that survives death and reincarnates, I contend.

As Augustine notes, C. D. Broad understood the mind to be a compound of two factors, a psychic factor³⁷ and a bodily factor, neither of which “separately has the characteristic properties of a mind” (Broad, 1925, p. 535), but which together create one. Augustine takes this to mean that “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” (KA, p. 217). This is only one possible scenario envisioned by Broad (1925). In another scenario, he permitted the psychic factor to outlast bodily death, although he distinguished between its persistence and its survival. Personality fragments might persist, but survival requires that the psychic factor have an associated mental stream (Broad, 1925, p. 539). In 1925, he believed that the case data pointed to persistence rather than survival, but by the end of his career (Broad, 1958/1976, 1962), he recognized that in some cases there was evidence for a more robust survival associated with what he then called the ψ -component of the mind.

Although Augustine claims that a psychic factor cannot survive death in the sentence quoted above, elsewhere he draws attention to commentators (e.g., Murphy, 1945) who, like Broad (1925), have thought that the evidence suggests that if something continues on after death, it is a mere persistence of fragmentary personality traits, without an associated mental stream. There is no doubt that many mediumistic and apparition cases suggest persistence more than they do personal survival, but I am not sure that means what these commentators take it to mean. As I observed above, the apparent fragmentation may be a function of psi communication between the living and the dead. Alternatively, or in conjunction, it could be that there is a fuller survival of function at the subconscious level. Importantly, either possibility—persistence as defined by Broad or persistence as the surface reflection of a deeper flow of experience—is logically compatible with reincarnation. In regards to persistence and reincarnation, Broad wrote, “instead of a single mind which animates a successive series of organisms we should have a single psychic factor which combines with such a series of organisms to form a successive series of minds” (1925, p. 551). If that is so, if even fragmentary psychic factors may reincarnate, we cannot rightly deny that persistence would constitute a limited survival.

Broad was of the opinion that if a ψ -component belonged to a quasiphysical astral body, “it would be easier to grant that it might have a stream of personal experience associated with it, and that this might be continuous with the deceased person’s *ante-mortem* stream of experience.” That is because “the ‘astral body’ might be supposed to play much the same part in the way of supplying actual organic sensation,

³⁵ Swinburne (1986), for instance, grants that God bestows souls on nonhuman as well as human animals.

³⁶ Myers (1903) wrote about a “subliminal Self” that was not in conflict with the self as consciously experienced, but rather continuous with it and encompassing it. “I conceive that there may be,—not only *co-operations* between these quasi-independent trains of thought,—but also upheavals and alternations of personality of many kinds, so that what was once below the surface may for a time, or permanently, rise above it. And I conceive also that no Self of which we can here have cognizance is in reality more than a fragment of a larger Self, —revealed in a fashion at once shifting and limited through an organism not so framed as to afford its full manifestation” (Myers, 1903, vol. 1, p. 15; emphasis in original). In defining the self as the experiencer of both conscious and subconscious states, I mean by the term what Myers designated the subliminal Self.

³⁷ Augustine writes about Broad’s “psi factor,” but Broad (1925) called it the “psychic factor.” Later, Broad (1958/1976, 1962) referred to it as the ψ -component of the mind.

actual *quasi*-sensory perception of external things, and so on, as did the physical body during its lifetime” (Broad, 1962, p. 425; emphasis in original). The astral body idea faces serious hurdles, though, both conceptual and evidentiary. I concur with Augustine about these, and I agree with him that the astral body does no more than reframe the mind-body problem. However, I do not accept that the only alternative to an astral body is a “nonphysical soul” in the Cartesian mold. A surviving stream of consciousness, or a stream of personal experience, is not a soul in the Cartesian sense, and its relation to the body need not be conceived in terms of substance dualism or interactionism.

In my review, I draw on Whitehead’s (1929/1978) process metaphysics to explain how a discarnate stream of consciousness might continue into the afterlife, unsupported by an astral body. Whitehead supplies a detailed, psychologically-sophisticated account of conscious experience, and although he himself did not believe that streams of experience remained active after death, there is no reason other than a commitment to physicalism to discard the possibility. Augustine’s response to my proposal is: “Sure, if you amend [Whitehead’s model] so that it no longer entails [the implication of mortality], then it becomes at least compatible with personal survival. But that is surely true of any metaphysics if one amends it enough” (KA, p. 224). He adds: “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” (KA, p. 224). I am not sure what point Augustine is trying to make here. Whitehead’s position assumes physicalism (and hence mortalism), but I cannot see that it entails it, or why its status vis-à-vis physicalism should affect its value for my speculations about survival. For an extension of Whitehead’s metaphysics similar to mine, see Weiss (2012, 2015).

The philosophical advantage to treating the mind as a stream of consciousness, or stream of experience, extends beyond the possibility of considering the mind in process rather than substance terms. It relieves us of the need to introduce a new construct (a soul), and so disposes of the parsimony issue. Nevertheless, we should not forget Broad’s conception of mind as a compound of a psychic factor and a physical factor. It might be wise to restrict the meaning of “mind” to this compound, and refer to the part of a mind which survives as a psychic factor, ψ -component, or mental stream, rather than as a mind. Broad (1925) uncharacteristically introduced confusion in equating a mind with a surviving psychic factor that had an associated stream of consciousness. It is obvious that a disembodied mental stream of any sort would be different from an embodied mental stream, if for no other reason than that it would no longer come under the sway of a brain.

Embodied and disembodied mental streams would differ, but if they are continuous, how different would they be? Our disembodied mental streams would no longer be impacted by our diets, by our levels of stress or fatigue, by injuries to our heads, and so forth, but would the fundamental nature of our cognition have to change, would our cognitive abilities be altered or disappear altogether? They might be altered, but I see no reason to think that they would vanish (cf. Stokes, 1997, 2007, 2014). Stapp (1999) has argued that attention, intention, and will are properties of consciousness, and that they play key roles in quantum mechanics. If, as I have suggested, memories are registered in the subconscious, the ability to record and retrieve them would belong to a mental stream as well. Many other capacities and qualities, such as emotion, could be added to the list. The expression of all of them is mediated by physiological and neurological processes while we are embodied, but that does not mean that they are not available to us while disembodied. Also, without our bodies, we would lack eyes, ears, and so on, to receive information through perception and other sensory means; we would be unable to communicate with each other through speech; and we would lack physical means of acting on the world. However, a discarnate mental stream could compensate for these losses with psi.

I cannot take the space to illustrate these various capacities with case examples (for which see, e.g., Almeder, 1992; Braude, 2003; Carter, 2012; Ducasse, 1961; Gauld, 1982; Hart, 1959). By no means all cases of mediumship and apparitions suggest survival in the limited form of persistence, and for those which do suggest persistence as opposed to a more robust survival of consciousness, we must, as I have noted, consider the possibility that the limitations reflect the nature of psi-based interaction, or that substantially more activity is underway at the subconscious level than is manifested in the conscious awareness

of the deceased and communicated to the living. Myers' (1903) documentation of a permeable threshold between subliminal and supraliminal levels of mind strongly supports this latter possibility, and it has welcome theoretical implications. An unbroken stream of experience at a subconscious level would provide the continuity called for in Whitehead's (1929/1978) metaphysical scheme, and it would furnish the sort of container for fragmentary psychic items that Broad (1962) and other commentators have expected an astral body to supply.

Augustine says, "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" (KA, p. 216). He proceeds to draw from Broad (1925) the possibility that a psychic factor without an affiliated stream of consciousness might be reactivated upon its re-affiliation with a new brain after reincarnation,³⁸ and he makes this equivalent to the subconscious regaining conscious awareness when reunited with a brain. Thus, he concludes, I am being inconsistent with my "belief" in intermission memories with veridical perceptions of the material world, which Giesler-Petersen and I (Matlock & Giesler-Petersen, in press) found reported in all stages of intermission experiences. But I never said or implied that conscious awareness requires interaction with a brain. I believe that the structure of consciousness is the same in death as in life, and that conscious awareness certainly is possible in the absence of embodiment. The inconsistency is imagined by Augustine, who has read into my words something I never said, or intended to say.

Augustine has misunderstood me also in relation to the localization issue. Descartes (1644/2009) claimed that the soul has no extension and is not localized in space. Augustine quotes me as saying, "there would be fewer logical difficulties for a surviving mind that was localized in space (and time)" than there are for a Cartesian soul, but he interprets this as my support for the astral body concept, notwithstanding my clear rejection of that concept ("I do not think that a consciousness stream requires the support of an astral body. . . ."; JM, p. 201).³⁹ Augustine goes on to give reasons for doubting the existence of astral bodies and tries to use these reasons to undercut what he presumes to be my argument. But I was not referring to localization via an astral body. I was thinking of reports of phenomena such as veridical perceptions during intermission experiences and NDEs (Matlock & Giesler-Petersen, in press; Rivas et al., 2016) that suggest the localized presence of a disembodied mental stream.⁴⁰

Augustine believes that "discarnate perception, cognition, and emotion suggest an implausible break in our evolutionary continuity with other animals" (KA, p. 217). He quotes from his chapter with Fishman: "The independence thesis flies in the face of our understanding of the evolutionary origin and development of animal minds (p. 213)." Elsewhere in his reply, he asks: "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?" (KA, p. 208). Why is Augustine so adamant that survival is in conflict with evolution? I think it is because of the Cartesian assumptions that the soul is eternally unchanging and that God granted souls only to humans, not other animals. Another *MoA* contributor, Bradley, is explicit about this connection: "The intellectual stress-test on substance dualism becomes even more severe when we seek plausible, well-reasoned answers to questions about the evolutionary origins of minds or souls" (p. 304). It is not clear, though, why Augustine feels the need to hammer at the problem in his reply to my review. I explicitly distanced myself from all forms of dualism, and my process theory of survival specifically addresses evolution.

³⁸ Augustine actually says "after possession, reincarnation, or resurrection" (KA, p. 216) and cites Broad (1925, p. 536), but Broad does not mention either possession or resurrection.

³⁹ Later on, Augustine acknowledges that I do not support the astral body concept, but his comment suggests that he may not understand my process theory of survival: "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 personality traits could somehow be implanted/uploaded in a new biological brain before the old one dies—a minimalist kind a reincarnation in which there is no discarnate existence between incarnations" (KA, pp. 222–223n21).

⁴⁰ Other examples are: attempts to touch living people during intermission experiences and NDEs that are felt as physical contacts by those people; so-called reciprocal apparitions, in which an agent feels himself to travel to a distant location, and is there seen as an apparition by a percipient; deathbed visions in which dying people see deceased loved ones coming to meet them; and direct-voice mediumship, in which the disembodied voice of a communicator appears to emanate from the air near the medium.

Central to my attempt to show how the survival of consciousness is related to evolution are idealism and panpsychism. Augustine charges that I am wrong to state that “idealism is a form of monism diametrically opposed to materialism” (JM, p. 200). He maintains that idealism is the philosophical position that “only mental objects exist” (KA, p. 222), and its proper antithesis is realism. “For realism affirms what idealism denies, namely that physical objects exist” (KA, p. 224n23). He is using idealism in the restricted sense of Bishop Berkeley, whereas I mean idealism in the broader sense that the reality we know is fundamentally mental ontologically; it is not that only mental objects exist, but that the mental is the ground of all that exists. Idealism in this broader sense places the mental at the origin and center of everything, including the material world, and thus is opposed to materialism, which derives the mental from the physical (Marshall, *BP*, p. 388). This version of idealism may sound only slightly less extreme than Augustine’s, but I and many other researchers and theorists (e.g., Barušs & Mossbridge, 2016; E. F. Kelly, 2007, *BP*, Chapter 14; Marshall, *BP*, Chapter 11; Tucker, 2013) are turning to it partly because of its compatibility with quantum theory. As Marshall (*BP*, p. 390) points out, Whitehead’s metaphysics can be understood as idealist in the broader sense, although Whitehead was unhappy with the state of idealist thinking in his day (Whitehead, 1929/1978, p. 116).

Closely allied with this broader understanding of idealism is panpsychism. Again, there are different versions of panpsychism, but all affirm that “mind is a general phenomenon in nature” and that “all things have mind or mind-like quality” (Skrbina, 2005, p. 2). Most contemporary panpsychists refer to “experience,” rather than to “mind.” Chalmers (1996, pp. 293–299) equates consciousness with experience and experience with information management, allowing not only humans, mice, and slugs, but thermostats, stones, and electrons to have experiences. Whitehead (1929/1978) and Griffin (1997, 1998), on the other hand, distinguish between aggregates of low-level entities, each of which has experiences but which collectively have none, and hierarchically composed entities, which have collective experiences through their dominant members. Thus, each of the molecules that comprise a rock has experience, but the rock as a whole does not, whereas entities from atoms and molecules up to organic individuals such as ourselves share a collective experience. The latter type of panpsychism is often termed panexperientialism. Panexperientialism is, I think, the most reasonable sort of panpsychism. I link experience to consciousness, though not to mind (adopting Broad’s definition of mind as a compound of psychic and physical factors). If consciousness is at the root of everything, then it is logical that it would be a constituent part of everything, yet I would not want to say that inanimate objects have experiences, even granting that their experiences were of a very different nature than ours (Chalmers, 1996). Similar positions to mine are taken by many of the same workers who have embraced idealism (Barušs & Mossbridge, 2017; E. F. Kelly, 2007, *BP*, Chapter 14; Marshall, *BP*).

An idealist panpsychism or panexperientialism has important implications for the mind/body problem, survival, and reincarnation. In the first place, it means that mind and body are not composed of different substances. Mind and body are different, yes, but the basis of the difference is what I called in my review an idealist property dualism, not a substance dualism. This removes the essential philosophical obstacle to their interaction (the problem of how an immaterial soul can interact with a material body). The interaction could be effected through psi (Griffin, 1997). One might say, through PK, but from the idealist perspective, PK is a type of telepathy: “Because matter reduces to mind, ‘mind-over-matter’ is effectively ‘mind-over-mind,’ and so psychokinesis is *telepathic action*” (Marshall, *BP*, p. 399; emphasis in original). Internal psychokinesis or telepathic action may well go on all the time without us realizing it, and, as I pointed out in the review, it may have a central place in the reincarnation process. An incoming mental stream might influence its new body to produce congenital physical signs such birthmarks and lay down neural pathways in its brain to allow for the reproduction of skills and other learned behaviors. Once associated with a new body, the mental stream would join with physical traits to form a new compound mind. New bodies and new minds are what make us different people in each life, despite having inherited our mental streams from our previous lives.

Given the wide diversity of animate and inanimate matter in the universe today, it seems clear that consciousness has been evolving and differentiating since the universe’s beginning, and it stands to reason

that it would have evolved and differentiated in relationship to the entities of which it was a part. The evolution of physical form and the evolution of consciousness must have proceeded in tandem (E. F. Kelly, *BP*, pp. 514–515). The emergence of biological life and death would have brought with it the appearance of a new type of consciousness, a consciousness distinct from the living matter which was its host. Thus, I imagine, were born streams of consciousness, one of whose novel characteristics was the ability to animate one body after another. Reincarnation, I believe, has continued ever since, in an increasingly complex succession of life forms. I doubt that humans are the only animals whose mental streams return in successive bodies. Where there is death, there is survival and reincarnation of a mental stream, I am suggesting. As animals' brains became more complex, consciousness also became more complex in order to work with them, leading to the development at some stage of what Myers (1903) called the supraliminal mind (conscious awareness). Before that, what we now consider the subliminal or subconscious mind would have existed on its own, and it naturally would have undergone its own long evolution.

Smythe calls my speculation about these matters a fiction. She is right again. Like all philosophical or theoretical reflections of this order, it is a story designed to pick up where empirical evidence leaves off. My purpose in sharing it in my review, and reiterating it here, is to provide answers from the perspective of a process metaphysics to questions on which substance dualism is often said to founder (e.g., by Bradley, cited above). My story explains the origins of consciousness, of streams of consciousness, and of reincarnation; it suggests that consciousness has evolved in relation to the entities with which it is associated; and it claims that humans are not the only animals whose mental streams pass from one body to another over successive lives. How true my story is, we may never know, but I believe it furnishes a cohesive narrative that is amenable to testing by logic, if not by experiment.

Augustine says: “Traditional Cartesian dualism is the natural starting point for any consideration of *how* one might survive death without technological or miraculous intervention The question is how far beyond objections to it one need go in considering obstacles to personal survival. Not very far, I would argue” (KA, p. 222, his emphasis). Indeed, the only alternative he recognizes to substance dualism, which envisions wholly disembodied survival, is survival in an astral body. Thus,

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. And if they do not exhaust them because a primarily nonphysical mind might also have a few physical 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. (KA, pp. 222–223)

Substance dualism and astral bodies may exhaust the possibilities of “dualistically surviving death,” but dualism is not the only way to conceive of mind/brain relations and survival. The process approach I have outlined has clear philosophical advantages over substance dualism. Arguably, it enjoys greater empirical support too, which gives it a theoretical advantage. Augustine evidently did not understand what I was proposing, so he did not address it in his reply, other than calling idealism (which he characterizes in a different way than I do) an “extreme” position and asking of panpsychism, “Why bring it up at all?” (KA, p. 223). I take responsibility for not having made my argument clearer in my review. I hope I have done a better job this time around.

The State of Play

Augustine asserts, “Matlock displays a tendency to misrepresent others' views in order to create the appearance that they support his own” (KA, p. 223). He has this backwards: He is the one who portrays the positions of others as in line with his brain/mind identity notions, when in fact they depart from it radically. Augustine says: “[Matlock] misleadingly attributes to Noë (2009) the view that ‘conscious awareness emerges outside the brain, in response to environmental stimuli.’ But this is not what Noë's embodied/situated cognition approach maintains” (KA, p. 223). Yet in *Out of our Heads: Why You are not Your Brain*,

and *Other Lessons from the Biology of Consciousness*, Noë (2009) says things such as:

In this book, I advance this truly astonishing hypothesis: to understand consciousness in humans and animals, we must look not inward, into the recesses of our insides; rather, we need to look at the ways in which each of us, as a whole animal, carries out the processes of living in and in response to the world around us. The subject of experience is not a bit of your body. You are not your brain. The brain, rather, is part of what you are. (Noë, 2009, p. 7)

Consciousness isn't something that happens inside us: it is something that we do, actively, in our dynamic interaction with the world around us. The brain—that particular bodily organ—is certainly critical to understanding how we work. I would not wish to deny that. But if we want to understand how the brain contributes to consciousness, we need to look at the larger nonbrain environment in which we find ourselves. (Noë, 2009, p. 24)

I could easily multiply the quotations from Noë (2009), but these should suffice. Augustine writes: “Matlock similarly ascribes to Chalmers, Strawson, and Koch ‘panpsychist positions that *recognize that awareness is not grounded in cerebral activity*’ (emphasis mine). But nothing could be further from the truth. Each of their respective positions actually *entail* that awareness *is* grounded in brain activity” (KA, p. 223). Consider, then, the following, first from Chalmers:

If there is experience associated with thermostats, there is probably experience *everywhere*: wherever there is causal interaction, there is information, and wherever there is information, there is experience. One can find information states in a rock—when it expands and contracts, for example—or even in the different states of an electron. So if the unrestricted double-aspect principle is correct, there will be experience associated with a rock or an electron. (Chalmers, 1996, p. 297, italics in original)

If thermostats, rocks, and electrons have brains, presumably they are metaphorical ones. The same observation applies to Strawson:

I persist in thinking that “physicalism,” “real physicalism” is a good name for my position in the current state of debate, but it's already time to admit that my understanding of real physicalism doesn't even rule out panpsychism—which I take to be the view that the existence of every concrete thing involves experiential being even it also involves non-experiential being. (Strawson, 2006, p. 8)

Like Chalmers, Koch (2012) connects consciousness to experience to information, without tying consciousness to a brain:

By postulating that consciousness is a fundamental feature of the universe, rather than emerging out of simpler elements, integrated information theory is an elaborate version of *panpsychism*. The hypothesis that all matter is sentient to some degree is terribly appealing for its elegance, simplicity, and logical coherence. Once you assume that consciousness is real and ontologically distinct from its physical substrate, then it is a simple step to conclude that the entire cosmos is suffused with sentience. (Koch, 2012, p. 132; emphasis in original)

Augustine is not alone in wanting to portray his brain/mind identity stance as more unanimously held than it is. In my review of *MoA*, I pointed out Angel's attempt to bring Pythagoras and Plato into line with his own thinking. Under the heading of “Evidence that Physical Formulas are Not Violated”—in other words, causal closure is maintained—Angel says that “Pythagoras and Plato seem to have been rationalists who believed in the importance of mathematics” (p. 381). Augustine says:

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. (KA, p. 221n18)

If that is what Angel meant, he is confusing the Enlightenment rationalism of Descartes, Leibniz, and Spinoza, which referred to logical thought as opposed to empirical observation, with contemporary rationalism, which is a code for materialism and skepticism of phenomena considered paranormal by mainstream science. There is irony in making Descartes out to be a rationalist in the latter sense, given the strenuous opposition to his substance dualism in *MoA*. Beyond this, although Pythagoras and Plato might be placed in the same lineage as the Enlightenment rationalists, it should be recognized that Pythagoras was partly an empiricist who believed in the possibility of recalling previous lives. Moreover, neither Pythagoras nor Plato can comfortably be grouped with Aristotle, as Angel does in his next sentence: “Plato’s student, Aristotle, subtly developed an early synthesis of rationalism and observational empiricism” (p. 381). Angel then introduces Newton as the culmination of a supposed unbroken intellectual tradition.

Augustine charges that I represent “a lone voice in the wilderness” (KA, p. 223) in my promotion of idealism and panpsychism in relation to postmortem survival. He is very wrong about that. My ideas are not much different from the ideas of many others engaged in contemporary survival theory. Griffin (1997), Tucker (2013), Woollacott (2015), and Barušs and Mossbridge (2017), along with E. F. Kelly (2007, *BP*, Chapter 14), Marshall (*BP*, Chapter 11), Weiss (2012, *BP*, Chapter 13) and other authors in *BP* embrace idealism and/or panpsychism and allow for postmortem survival and reincarnation. Griffin (1997), E. F. Kelly (2007), and Weiss (2012, *BP*, Chapter 13) draw on Whitehead’s process metaphysics in addition. Survival theory is giving up dualism for idealism and panpsychism,⁴¹ but this trend is missed by Augustine et al. When I said that *MoA* has a “dated feel from the research point of view” (JM, p. 191), it is this, more than the several reprinted selections, that I had in mind.

When the movement away from brain/mind identity in psychology, biology, neuroscience, and philosophy is brought together with an increasing receptivity to postmortem survival and reincarnation, it becomes clear that the materialist world view represented by *MoA* is under sustained assault. As I have shown, survival cannot be ruled out on logical grounds, and according to Stapp (2009), it cannot be ruled out on scientific grounds, either.

Strong doubts about personality survival based solely on the belief that postmortem survival is incompatible with the laws of physics are unfounded. Rational science-based opinion on this question must be based on the content and quality of the empirical data, not on a presumed incompatibility of such phenomena with our contemporary understanding of the workings of nature. (Stapp, 2009, p. 16)

Stapp (*BP*, p. 181) has said that even reincarnation can be accommodated by quantum mechanics, with only minor adjustments to the mathematical formalisms. The main obstacle to the acceptance of the survival and reincarnation data is neither logical nor scientific. It emanates from adherence to the reigning paradigms of materialism and physicalism, which are buttressed by commitment to received opinion rather than evidence. We will have to wait to see which side wins the battle, which we can expect to be prolonged and hard-fought. The progressive forces are slowly gaining ground, however, and I have little doubt that they will prevail in the end.

⁴¹ This is the emerging and growing consensus view, but there are hold-outs, especially among philosophers, e.g. Almeder (2012) and Rivas (2012). Rivas’s paper (in Dutch), whose title may be translated as *The Self and the Reality of Mind*, is not yet available in English (T. Rivas, personal communication, December 6, 2016).

PART III: DISCUSSION

SURVIVAL AND THE MIND-BODY PROBLEM

By John Palmer

Offensive Ad Hominem Language

Augustine begins his reply by chastising me for allowing Matlock to opine in his review that Augustine is a “paradigmatic thinker.” It seems to me that Matlock was simply claiming that Augustine is a rationalist (as opposed to an empiricist) and I don’t think most philosophers would consider that an insult, even if they disagreed with the characterization. I think it is significant that Matlock avoided the “wishful thinker” alternative, which *would* be insulting, and which both Larsen and Smythe used in their replies to characterize the opposition. Although mortalists probably don’t wish to die, they certainly wish their theories to be true, so the epithet could in principle be applied to them. For what it’s worth, I think both Matlock and Augustine are data-driven but they are driven by different data.

When deciding whether language is offensive, I pay more attention to tone than to content. Although both Matlock and Augustine adopted a harsh tone at times, Augustine more so, I think both stayed within the bounds of proper discourse. On the other hand, I found the sarcastic, condescending, and self-righteous tone of Smythe’s piece to be quite offensive, and she undercut the apparent attempt of Augustine to project a more sober and scholarly image. Alas, her tone is typical of what parapsychologists have to put up with from psi-skeptics.

Finally, although not exactly a matter of tone, I found Stokes’ speculation in his review that the *BP* authors didn’t cite his book because they couldn’t answer his arguments and then in his reply to in effect call Kelly a liar for claiming he was unaware of the book, to be unjustified, unfair, and whiny.

The Psi Factor

In both his review and his reply, Stokes doubles down in telling us that he does not accept the experimental evidence for psi because of “. . . recent revelations regarding the massive rates of experimenter misconduct and incompetence among researchers in orthodox psychology and biomedical research” (DS, p. 170). I assume he is referring primarily to the John, Loewenstein, and Prelec (2012) analysis of fraud in mainstream psychology. In addition to Kelly’s reply and my observations elsewhere about the hazards of such inferences (Palmer, 2016a), it should be noted that the percentage of experimenters John et al. estimated to be fraudulent is 9%. I suspect that even if you removed the 10% or so most evidential studies from the parapsychology database, what is left would still be sufficient to establish the case for psi. At least the burden of proof should fall on Stokes to demonstrate that is not the case. The recent “meta-analysis” of the ganzfeld database by Bierman, Spottiswoode, and Bijl (2016) was an (unsuccessful) attempt to contribute to the realization of that objective (see also Palmer, 2016a, 2016b).

Survivalism Versus Mortalism

I decided that from here on the most efficient and economical way to proceed would be to present my own position on the survival issue, which the other contributors have helped me to clarify. For the most part, the only points of theirs I will address explicitly are those that are relevant to my thesis. Even in these cases I will generally respond only to points of disagreement about which I have something substantive to say other than “I agree or “I disagree.” Despite this policy, there is plenty of reference to the other contributions in what follows.

Consolidating the Ontologies

I want to begin with some terminological and conceptual housecleaning. I submit that there are four, and only four, fundamental, independent ontologies that are relevant to this discussion: the classic three (*materialism*, *substance dualism*, and *idealism*) and a fourth that I will label *semimaterialism* (and the substance, *semimatter*) because I am aware of no formal names for these in the literature. I will argue that the other relevant ontologies addressed in the debate are either equivalent to or versions of materialism or substance dualism.

I will first address those that reduce to materialism. The primary one is *neutral monism*, which is defined as follows: “Belief that both mental and physical properties are the features of substances of a single sort, which are themselves ultimately neither mental nor physical” (Kemmerling, 2011). Note that one can describe the identical substance in the following way: “Matter has mental properties” (materialism). Neutral monism as defined above is touted as parsimonious, and in comparison to dualism it is (as are all monisms), but in the more important sense it is unparsimonious, because, unlike my competing definition, it needlessly postulates a “new” type of substance. For this reason, it should be rejected. Adding psychological properties to matter is a legitimate move because it does not change the fundamental nature of matter; no intrinsic properties of matter must be denied to accommodate psychological processes. This is what most neuroscientists assume without batting an eyelash. Psychological properties have been implicit in the definition of matter at least since Descartes, because as a monism, materialism claims that there is nothing in the universe but matter, and thus matter has to account for the psychological functions that would otherwise be attributed to the Cartesian mind.⁴²

My rejection of neutral monism as an independent ontology assumes that the “new” substance has *all* the properties of matter, which opens the door for a truly independent ontology whose substance has only some of the properties of matter. This is where semimaterialism comes in. This ontology appears in the debate primarily in the reference to subtle or astral bodies. In this context, the existence of ordinary matter is not denied, and in most theories the astral body is thought to house a mind (i.e., semimatter is seen as third substance, an unparsimonious move.)

Having established that materialism refers to matter with psychological properties, other ontologies can be seen as equivalent to or versions of materialism. The most obvious is *property dualism*, which maintains that “there is only one thing, the person, that has two irreducible types of properties, mental and physical” (Honderich, 1995, p. 207). This seems identical to my materialism except that it refers specifically to, presumably, the brain. *Panpsychism* is defined as “the belief that everything in the world has some mental aspect, and that there is some degree of consciousness—however small—even to apparently inert bits of matter” (Kemmerling, 2011), again the same as my materialism with the added stipulation that all matter has mental properties. *Physicalism* is “the belief that all mental properties, states, and events can be wholly explained in terms of physical properties, states, and events” (Kemmerling, 2011). This directly follows from materialism, and my other source says quite bluntly that it is “also called materialism” (Honderich, 1995, p. 679.) This useless redundancy could be made useful by adding “only of the brain” to the end of the longer definition, making it a complement to panpsychism: to wit, materialism is either panpsychism or physicalism.

Whitehead’s metaphysics is particularly interesting, both intrinsically and to me personally. My understanding of Whitehead comes almost exclusively from David Ray Griffin, with whom I had personal contact when I was living in California many years ago. I persuaded him to write a summary of Whitehead’s ontology from a parapsychological perspective for a parapsychology journal (Griffin, 1993). I wrote a review of it in which I expressed the opinion that the substance in Whitehead’s ontology (*occasions of experience*, which have mental and physical *poles*) was exclusively mental and that the ontology was a form of idealism (Palmer, 1993); Griffin objected that this attribution was misleading (Griffin, 1994a). Then several months ago I had an opportunity to revisit Griffin’s original paper in preparation for a conference

⁴² This analysis ignores the logical positivists, who skirted the issue by proclaiming that psychological properties don’t exist. But if there is one thing I suspect all the contributors agree on, it is that logical positivism, which seems similar to what Stokes calls radical materialism, should remain in Stokes’ “dung heap” (DS, p. 178).

on Whitehead. Informed by the thinking described above, I concluded that the occasions of experiences are matter. But then in preparing this paper, I noticed that in his review Matlock (p. 201) cites Whitehead saying “that an individual’s experiential stream outlasted his death . . . (Whitehead, 1929/1978, p. 351),” which in turn implies that the occasions of experience must be semimatter. However, in his reply Matlock writes that Whitehead “did not believe that streams of experience remained active after death” (JM, p. 245), and Augustine proclaims that “Whitehead’s views entail mortalism” (KA, p. 224). So I turned to a paper by my authority Griffin (1994b), who cites Whitehead as saying he “. . . is entirely neutral on the question of immortality” (Whitehead, 1929/1957, p. 107). So I think it is safest to conclude that it is unclear whether the occasions of experience are matter or semimatter. Whitehead’s metaphysics also has a God construct, which qualifies it as panentheism, but God is declared to be “dipolar” (Whitehead, 1929/1957, p. 345), which seems to mean that it is of the same substance as the occasions of experience. Thus, Whitehead’s metaphysics is in fact monistic.

I will now turn to those ontologies that generally reduce to substance dualism. The most important of these for present purposes is *panentheism*, because it is the ontology advocated in *BP* and specifically by contributors Kelly and Stokes. What the various versions of panentheism seem to have in common is the postulation of a universal mind or supreme consciousness that seems to have the properties of a god (or God) and is sometimes explicitly identified as such. This of course fits the main theme or goal of *BP*, which is to reconcile science and religion.

Granted that matter is indeed material, and if we assume that the universal mind is not, panentheism as a form of substance dualism follows naturally. I do not have space to comment on all the versions alluded to above, but I will offer some remarks on the version presented by Stapp in Chapter 5 of *BP*, for three reasons. First, he postulates in addition to a universal mind, which he calls *nature*, an individual mind, which he calls *ego*. The role of ego is to make a binary choice of whether or not to take an action and the role of nature is to actualize the choice. A construct such as ego is necessary if one is to propose survival of an individual personality and not all versions of panentheism postulate one. Second, Stapp’s theory is based on quantum mechanics, which has been discussed at length in the debate. Third, Stapp is the one author to explicitly identify his theory with dualism:

This quantum ontology is basically in line with Descartes’ idea that reality is composed of thinking entities and things that occupy space. That is, the classical-physics ontology is expanded from merely the collection of physically described things that occupy space to a psychophysical space that includes our psychologically described egos . . . (*BP*, p. 181)

Finally, I will comment on Matlock’s *processual soul theory*. He begins by defining mind as a “stream of consciousness” (JM, p. 200). It seems to me that this is really a process and a better term would be “*streaming* of consciousness.” I strongly adhere to the view that processes and properties should be treated as predicates, and they cannot logically exist on their own; properties must be properties of something. Thus, I was glad to see that later on Matlock supplies that something, which he calls *self* (JM, p. 201). Thus, self is Matlock’s term for an individual mind. IMO, consciousness should be construed as one of its properties.

There is some ambiguity about what kind of stuff the self is. In his reply, Matlock writes: “The problem with the Cartesian proposal, as I see it, is . . . that substance is supposed to be eternally unchanging, to lack *extension*, and to have no *location in space*” (JM, p. 244, my emphasis). This suggests that the self is semimatter, but elsewhere Matlock rejects the notion of an astral or subtle body (JM, p. 245), which is what the quote above seems to entail.

Matlock insists that the ontology of his theory is not substance dualism, but instead what he calls *idealist property dualism*: “The idea is that material objects, including bodies, and minds are *imbued with different grades of consciousness*. There is a dualism of mind and body here” (JM, p. 200, my emphasis). Note that the highlighted phrase is equivalent to my statement above that “matter has mental properties.” It follows that the two substances in Matlock’s theory are mind and matter, and that the theory actually represents substance dualism.

However, his invocation of idealism raises an interesting point. Another statement I could have made that describes the same substance as neutral monism is “Mind has mental properties” (idealism). Thus, it would be legitimate for Matlock to identify his theory even as idealistic substance dualism. I ignored the idealism option in my discussion of neutral monism because (a) I think attributing physical properties to mind violates the essence of idealism much more than attributing mental properties to matter violates the essence of materialism and (b) the important point for the present discussion is that the particular substance in question (the brain) has the material property of mortality.

Substance Dualism⁴³ and Survival

I have always been under the impression that what dualists are saying is that the mind is the *indirect* cause of behavior and the brain is the direct cause (see Figure 1). I use “behavior” in a very broad sense to include—in addition to motor actions (e.g., writing)—cognitive processes (e.g., deductive reasoning) and physiological responses (e.g., electrodermal activity). This seems to be what Augustine is referring to as *interactive substance dualism* (ISD). In any event, I am going to borrow the term to label my version of substance dualism.

I have never understood why materialists keep citing brain-behavior correlations as evidence against dualism because dualists “predict” such relationships every bit as much as materialists do. When I was younger and more intellectually involved with this issue than I am now, I became so exasperated by this that I put a sign up in my office that read “Dualists believe in brains!” Thus, when I saw Augustine complain that survivalists pay no attention to such things as “degenerative mental disorders as patients approach the end of life” (KA, p. 208) as evidence for what he calls the dependence thesis, I had a profound sense of déjà vu.

mind → brain → behavior

Figure 1. Palmer’s ISD model.

Stokes’ discussion of the filter theory described by Edward Kelly in the first chapter of *BP* illustrates how the mind → brain part of the model might work (DS, p. 172). Stokes lists his three versions of his theory: the filter model, the transmission model, and the instrument model. The second two have nothing to do with filtering per se; they instead describe the processes that the filtering inhibits. The second two represent types of activation and can usefully be concatenated into one model, which might be called the activation model, and the filter model renamed the inhibition model to highlight the opposition of the filtering to the transmitting and instrumenting. It is important to recognize that these two mechanisms should not be considered mutually exclusive or that the processes they represent function all the time at maximum capacity. Stokes observes that if the filter were entirely incapacitated we would expect to see enhanced mental functioning in cases of brain damage, which we generally do not, because it is rarely entirely incapacitated. However, he goes on to discuss a case where we do see enhanced mental functioning, terminal lucidity. Survivalists would not see this as an application of Stokes’ transmission or instrument model, but rather of his filter model. Blackmore (1962, p. 65) offers an interesting materialistic interpretation of how the filtering might work in terminal lucidity cases, namely, that inhibitory neurons succumb to the effects of anoxia faster than excitatory neurons. She notes that this has been demonstrated in rat brains, but I am aware of no such evidence for human brains. To account for the rarity of terminal lucidity, it is necessary to assume that the timing differential is generally too short to create an experience long enough to be noticed, although I suspect terminal lucidity is grossly underreported.

But how is it possible for the mind to interact with the brain? This of course raises the classic objection to dualism, namely, that it isn’t possible. As Augustine puts it: “Cartesian dualism [requires] ap-

⁴³From here on I will refer to substance dualism simply as “dualism.”

parently inconceivable causal contact between completely nonspatial minds and spatial brains, ostensibly requiring violations of physical law” (KA, p. 225). This phrasing conveniently signals the problem with the materialist’s objection with its reference to physical law, which implies that all mechanisms in the universe must be physical, which assumes the validity of the very proposition that is in dispute. In a related vein, the word “inconceivable” in the quoted sentence brings to mind another slogan of mine, which I dubbed the rationalist fallacy: “If I can’t think of an explanation for it, it can’t be real.”⁴⁴

From a parapsychological perspective, the mechanism by which the mind influences the brain is psi, virtually by definition. Augustine appeals to the fact psi is negatively defined to point out that calling the mechanism psi doesn’t explain anything. But one does not need to explain a mechanism to justify its possible reality, a point that Augustine makes in a different context with the following metaphor: “. . .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” (KA, p. 214n12nx). On the other hand, IMO the most important contribution of quantum mechanics to our debate is the fact that it provides a potential explanation of mind/brain interaction that *is* physical in the sense that it conforms to currently accepted theory in physics (see Stapp’s chapter in *BP*).

So where does survival enter the picture? In my ISD model (and the theses of many survivalists, including Matlock) it is proposed that the discarnate mind expresses itself by interacting with the brain of another living person. This is what happens in both mediumship and reincarnation, for example. The mechanism by which it does so is the same mechanism that it used with its “own” brain when alive, namely psi, so in this sense the thesis is parsimonious. Note that the mind is independent of the brain only with respect to its *existence*, not with respect to its *process*, which I like to refer to as *function*. To put it another way, the fact that behavior is dependent on the brain in no way precludes the independent existence of the mind, as should be clear from Figure 1.

But this is where the touted brain-behavior interactions come back into the picture, for it would be unparsimonious to propose that the mind can perform these functions on its own, because why then would it bother using a brain? One good thing that behaviorism accomplished was to remind us that as scientists we can draw firm conclusions only from what we observe, which is behavior in the narrow sense of the term. However, I think it is reasonable to draw inferences from our observations to the cognitive operations and physiological processes that I incorporate in my broader definition of behavior. For example, if a brain-damaged person cannot report the correct answer to a math problem, it is reasonable to infer that he or she was unable to perform the cognitive operation that would have led to the correct answer. Strictly speaking, this means that functions that have been demonstrated by empirical research to be directly caused by the brain should be excluded from the list of functions that the mind can perform independently. However, I have a somewhat more relaxed criterion: I attribute to the brain any functions that we can plausibly expect a very “intelligent” computer to perform either directly or through attachment to the human body. What that leaves, in my view, is subjective experience, which I consider the only function mind can perform on its own. Since by “subjective” I mean conscious, I consider consciousness to be a property of mind. This issue, of course, is Chalmers’ famous “hard problem,” and I must confess that I have a hard time articulating why I believe that conscious experience cannot *in principle* be performed by a computer, which I guess has something to do with why Chalmers calls the problem hard.

Re the “causal loop” and correlation versus causation, the important point is that in my model mind-behavior and brain-behavior relationships are all totally causal in the billiard ball sense. This creates no problem whatsoever for substance dualism and in fact is the position taken by most survivalists. The one notable exception is quantum indeterminacy. I have yet to encounter an intelligible or convincing explanation for how indeterminacy at the micro level becomes determinacy at the macro level, so I was delighted to see physicist Stapp in his *BP* chapter challenge the conventional QM notion that “nature’s choices are purely ‘random’” (*BP*, p. 186). In any event, what happens at the macro level seems to me to be what is important for the survival question.

Augustine cites the absence of “interactive traces” as evidence against the independence thesis. But what does he mean by interaction? In my ISD model, interaction is what occurs all the time. I suspect what

⁴⁴ See Palmer (2011) for another example of the fallacy, this one involving psi and evolution.

he is referring to instead are those rare instances in which mind either bypasses the brain to affect behavior directly or interacts with the brain of another person. These are the cases that parapsychologists claim as empirical evidence for survival, which means that Augustine is simply saying that he rejects that evidence. Note that these cases require a revision of my ISD model to allow the mind, on rare occasions, to affect behavior directly. This makes the model unparsimonious, but as I noted above, data trumps parsimony.

I am probably the loudest of the parapsychologist critics of Bayesian analysis that Augustine refers to, and my main point was that a priori factors, especially “the extent to which a hypothesis is consistent with background knowledge that has been independently established by conventional science and history” (KA, p. 217), which I call the coherence principle (Palmer 1987), should have zero weight in determining the evidentiality of an empirical finding (Palmer, 2011). Thus, I was glad to see him assign prior probabilities of .5 to the dependence and independence theses in his Bayesian analysis. He is nonetheless able to get the desired result by entering a higher posterior probability for the dependence thesis because of the brain-behavior relationships. This is an illegitimate move because these relationships are “predicted” by both theses. However, in this case I do think that the survival hypothesis’ incompatibility with both the coherence and parsimony principles justifies the claim that the burden of proof falls exclusively on the survivalists. The other side of that coin is that the *only* way the critic can effectively attack the survival hypothesis is to address the empirical evidence for it.

Empirical Evidence for Survival

The best way to demonstrate the independence of mind is to establish the occurrence of cognitive activity when the brain is totally incapacitated. This is what survivalists claim for the famous Pam Reynolds case, in which Reynolds reported that she had an NDE during a window of time during surgery when physiological measures indicated she was effectively “brain dead” (Sabom, 1982). The problem is that the claim that the NDE occurred during that specific period is based on Reynolds’ obviously unreliable memory. There was also evidence of anomalous information acquisition during the surgery; for example, she reported hearing a saw which sounded very much like the real one. The problem here is that the mechanism for the acquisition must be ESP, and ESP doesn’t always occur in real time; in other words, she could have precognized the saw buzzing before she was actually brain dead. I see no way around these objections in this or any other such cases, past (Alexander, 2012) or future.

The most common approach in recent years has been a weak version of the above, to point to the demonstration of phenomena that are claimed to exceed the known capacities of a compromised but not “dead” brain (mostly enhanced cognitive functioning in NDEs) or a normal brain (e.g., ability of savants, genius, stigmata), a major theme of *Irreducible Mind*. There has been extensive, and IMO inconclusive debate, as to whether the NDE phenomena can be accounted for by conventional physiological mechanisms. As for the others, it should be noted that they are extreme cases of phenomena already linked to the brain or other physical causes. Even if mainstream scientists were to accept that current theory cannot explain these manifestations, their response would be to modify the theory without abandoning the underlying ontology; this is what normally happens when scientists confront new data that “don’t fit.”

A phenomenon that does not fall in the extreme case bin is psi. I am not among those parapsychologists who believe that psi per se is evidence against materialism, and May and Depp (2015) have recently proposed a materialistic model of ESP. Ironically, until recently survivalists have viewed (super)psi as a problem rather than a solution. An even greater irony is that mortalist critics may have to appeal to psi to fully explain away the Reynolds case.

Finally, I have suggested an approach grounded in the admittedly not very plausible assumption that the mind can do things when it occupies someone else’s body (after death) that it cannot do when it occupies its own—but again, data trumps the a priors. The prediction is that certain anomalous phenomena are found only in what I call survival-related contexts (e.g., mediumship, reincarnation). A class of phenomena that seems to fit this bill are anomalous skills (e.g., the ability to compose music like Litz) but it has to be established by research that such feats do not occur in nonsurvival contexts.

I don't believe any of these approaches have provided, or ever will provide, decisive "proof" of survival. However, I think that some of them (especially those represented in *Irreducible Mind*) are awkward for mortalists. I strongly believe that evidence is a matter of degree, and thus this evidence does increase my subjective probability (no, I won't give you a number) that we do survive death. However, for me the most persuasive "evidence" is Chalmers' hard problem, and my suggestion to survivalists is that they pay more attention to this in building their case.

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CORRESPONDENCE

To the Editor:

I am writing in response to J. E. Kennedy's generally favorable comments on my chapter "The Case against Psi," in the volume *Parapsychology: A Handbook for the 21st Century* (Stokes, 2015). Kennedy (2016) states that he is absolutely certain that paranormal phenomena occur due to several striking personal experiences of his own (which he does not present). This letter examines the claim that the existence of psi can be proven on the basis of my own personal experiences.

I myself have had three ostensibly paranormal experiences. One of these is a precognitive dream. The following account of this dream is taken from my book *The Nature of Mind*.

I dreamed that one of my star math pupils and I were seated in a dark theater. The student was eating the inner core of a tootsie roll pop, and I was eating the outside layers, which had become detached from the core. I related this dream the next day to a dream study group I was sponsoring at school, which was patterned after the experiential dream sharing/interpretation groups developed by Montague Ullman (1987). I had also made a written record of this dream for the purpose of reporting it to the group. About ten minutes after the dream group ended, I received a phone call from a reporter from the *Philadelphia Inquirer* asking me for my comments about an honor I had received. It turned out that the student of my dream had been named a Presidential Scholar (a very rare honor given only to about 140 students in the country) and that he had named me as his Distinguished Teacher (probably because he was a math whiz and I was his math teacher). We were both to travel to Washington at taxpayers' expense for a week of festivities, including a meeting with President Reagan. Thus, this was a very significant event in my life. The dream seemed to symbolize the fact that my student was getting the central award, whereas I was getting a satellite award. It would be hard to explain my "tootsie roll" dream on the basis of unconscious inference, as I had no way of knowing that my student was up for this award (in fact I did not even know what a Presidential Scholar was until the reporter called me). Also, no one at the school knew of the award prior to my receiving the phone call from the reporter (Stokes, 1997, pp. 18–19).

I have witnessed two ostensible instances of macro-PK involving the anomalous behavior of four malicious copies of Erich Fromm's *The Art of Loving*, which beamed me on two different occasions. In the first such incident, a copy of this book fell off the bookcase above my bed in the middle of the night and landed on my head while I was fast asleep, waking me. No one shared my bedroom and there were only three other people in the house (my nuclear family). This was in the middle of the night (ca. 3 a.m.) when everyone in the house (including me) was asleep. Earthquakes are rare in our area (Buzzard's Bay, Massachusetts), and there were no reports of seismic activity that night (or any other night that I can recall). A few years later, three copies of the same book fell off a high shelf and onto my head as I browsed in an academic bookstore. Perhaps some entity was trying to send me a message. At that point I bought and read the book but found it to have little in the way of personal relevance.

My sister and I experienced a collectively-perceived auditory hallucination of my mother's voice calling my sister's name in an admonitory tone when we were horsing around in the house. We were the only ones in the (stand-alone) house at the time.

I find these events difficult to account for on the basis of standard physics (excluding the bookstore event). The first flying book experience and the shared auditory hallucination immediately seemed anomalous and otherworldly to me at the times of their occurrence. I take them as genuine psi events. So, following Kennedy (and the rules of logic) I too should believe in psi. However, I feel squeamish about overthrowing the laws of physics based on the few experiences I have personally witnessed.

One of my students in a parapsychology course I taught at the University of Michigan told me his father was knocked off a park bench by an invisible blow to the jaw at the same time as his wife broke her jaw on some gymnastic equipment. If this experience occurred as described, it would pretty much prove the existence of psi all by itself (the coincidence hypothesis would not work in this case, as people are not frequently being punched in the jaw by invisible fists).

Although my own psi experiences may lead me toward a belief in the existence of psi, I do not believe that psi can be made to manifest on demand in the context of a controlled experiment or that psi has been experimentally *proven* to exist.

Mystical Experiences

As I emerged from a very serious depression as a graduate student, I experienced deep, repeated, and long mystical states in which I seemed to be merged into a benign collective consciousness. The world seemed to glow with brilliant colors. These mystical experiences deeply affected me (much more than my personal psi experiences as recounted above). They likely underlie my favorable disposition toward philosophical and theological views that are based on the theory of a group mind, world mind, or collective consciousness. In these mystical experiences, it seemed as though I was in direct contact with a higher, transcendent consciousness. Although it might be possible to attribute these mystical experiences to some sort of manic psychopathology on my part, these experiences are still with me and underpin my belief in a collective consciousness much more than a flotilla of flying books will ever do. Please note that these experiences do not constitute evidence of psi.

These numinous experiences also coincided with my decision to pursue a career in parapsychology. These experiences may be factors (along with philosophical analyses) that led me to my current view that the universe is based in mind rather than matter and to my embrace of mentalistic philosophies such as idealism, panpsychism, pandeism, and panendeism. (I find it almost impossible to tease these positions apart scientifically or philosophically.)

Aspects of modern physics such as quantum nonlocality would seem to open the door to psi, although for a different take on this, see Carroll (2016). The existence of a collective mind would also seem to grant an opening for psi phenomena. Thus, these mystical experiences may provide indirect support for psi. Unlike Kennedy, I am a little reluctant to say that I *know* that psi exists, but then I am a solipsist at heart, doubting the existence of almost everything (including the physical world, if taken at face value). Similarly, I neither believe nor know that psi does not exist (it being impossible to falsify an existential hypothesis in any event). However, the existing body of *experimental* data seems to fit the pattern that would be expected if psi does not exist or, more likely, is almost impossible to capture on demand in an experimental situation.

The Elusiveness of Psi

If psi exists, it occurs very rarely and unexpectedly. That's what makes psi events appear to be miraculous and surprising. The idea that one might be able to make psi appear on demand in a controlled experiment may be the height of audacity and undue optimism. It might be similar to a group of physicists setting up an array of instruments on the perimeter of a football field and waiting for a meteor to impact the turf. From a negative result, it would be foolish to conclude that meteors do not exist. The search should be widened to include meteorites that have already fallen (i.e., reports of spontaneous experiences). Although my own psi experiences may lead me toward a belief in the existence of psi, I am skeptical that psi can be made to manifest on demand in the context of a controlled experiment.

Going forward, the analysis of psi experiments should not be exclusively based on tests of overall statistical significance of traditional meta-analyses (in view of the fact that the assumptions underlying such tests, such as the absence of fraud or data selection in the database, are not likely to be met). I would recommend instead an analysis based on the effect sizes produced by individual experimenters or experimental paradigms. I would strongly encourage parapsychologists to revive the study of spontaneous cases along

with narrative approaches to the study of psi. Reports of such cases might be published in the main journals of parapsychology. Anthologies of the best spontaneous cases could be compiled. This might yield evidence for psi that will be far more convincing than meta-analyses of microscopic experimental effects. If anyone is interested in undertaking such a project, please contact me.

As for Jim Kennedy, I've shown you mine, now you show us yours.

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DOUGLAS M. STOKES

424 Little Lake Drive, #3
Ann Arbor, MI 48103, USA
Dstokes48103@yahoo.com

To the Editor:

Doug Stokes sent me a prepublication copy of the preceding letter that discusses his thinking about the role of personal spontaneous psi experiences in forming beliefs about paranormal phenomena. In the letter, Stokes said that he has had a few personal experiences that he takes as “genuine psi events.” But he also said “I feel squeamish about overthrowing the laws of physics based on the few experiences I personally witnessed” and “I am a little reluctant to say that I *know* that psi exists, but then I am a solipsist at heart, doubting the existence of almost everything.” He also stated “I do not believe that psi can be made to manifest on demand in the context of a controlled experiment or that psi has been experimentally *proven* to exist.” In addition, he said that some personal mystical experiences significantly influenced his views about the nature of reality.

Stokes's letter was inspired by my comment in a book review that I am certain that paranormal phenomena sometimes occur based on my personal experiences (Kennedy, 2016c). In his letter, Stokes encouraged me to provide more explanation than my brief comments in the book review. His letter motivated me to write this letter describing my views about psi and the role of spontaneous experiences.

I am less philosophically inclined than Stokes and for me the matter is relatively straightforward. I have had personal experiences that I am convinced were paranormal (outside currently accepted scientific understanding) and therefore I am convinced that such phenomena sometimes occur. Science continually evolves. I take it as a given that some phenomena are currently not understood. However, that does not mean that I believe that all or most claims about psi are true.

I agree with Stokes that psi has not been experimentally proven to exist and I think he is probably correct that psi cannot be made to manifest on demand in controlled experiments. However, I am not certain that he is correct about the latter point because the severely underpowered exploratory research methods used in virtually all past experiments make any conclusions questionable. I expect that the matter will become clear with preregistered, well-powered, confirmatory research and other related methodological improvements, including measures to prevent experimenter fraud (Kennedy, 2016b).

I described some of my personal paranormal experiences and how they affected my life in a paper published in 2000 that is available online (Kennedy, 2000). Prior to that time, I considered the best strat-

egy for handling personal psi experiences was not to talk about them. However, during the 1990s Rhea White frequently advocated that I write a paper about my experiences. Such writings were the basis for her approach to exceptional human experiences. Finally, at the end of the decade, I realized that my personal experiences shaped my thinking about psi. Given the lack of properly designed confirmatory research and the high probability of psi-mediated experimenter effects if the working assumptions for experimental parapsychology were true, I concluded that the available experimental research provided virtually no reliable insights about psi. The phenomena for which I had the most confidence were my personal experiences—and they should not be withheld.

Working on that paper brought into focus the fundamentally different worldviews about psi for spontaneous cases and for experiments. For spontaneous cases, psi is generally viewed as something external to a person that guides a person and is related to spirituality. For experiments, psi is generally viewed as something that a person uses to fulfill his or her personal motivations and is related to technology.

One major point of the paper was that the great majority of my experiences did not have the practical, motivation-driven benefits that would be expected if the assumptions of experimental parapsychology were applicable. The overall effects of the experiences were a sense that my life was guided and had purpose—psi guiding me (spirituality) rather than me guiding psi (technology). When I looked back on the experience that had the most striking practical benefit, I realized that the benefit could have been achieved more easily in a way that was much less dramatic and that was more normal for my behavior. In retrospect, the experience appeared to have been contrived to be a dramatic paranormal experience. A similar evaluation of alternatives would be a useful investigation for other ostensible paranormal experiences that appear to have tangible practical benefits.

I had many other personal psi experiences that are not described in the paper. The paper was intended to convey a sense of the types of events that influenced my thinking about psi. The experiences were not described with the level of detail that would be needed to attempt to convince readers of their paranormal validity. I do not believe that those who are skeptical of psi by disposition and experience will change their worldview based on reading about personal psi experiences of others. My current working assumption is that people basically live in different worlds with regard to the occurrence of and beliefs about paranormal phenomena (Kennedy, 2016a). Research that characterizes these different worlds for paranormal experiences and beliefs would be useful. I do not expect that scientific research will provide bridges between these different worlds in the foreseeable future. However, I hope that this expectation is proven incorrect, and I am supportive of those who conduct research with more optimistic expectations.

My original book review was substantially longer than the word limit for book reviews in the *Journal*. As a compromise, the published review was a condensed version (Kennedy 2016c) and the longer review was posted online (Kennedy, 2016d). The longer version has more explanation of my views and includes the reference with my personal experiences. Given the frequent misunderstandings of my views about psi, I posted on my website a concise (two-page) summary of my current conclusions about paranormal phenomena (Kennedy, 2013).

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J. E. KENNEDY

jek@jeksite.org

To the Editor:

This comment concerns your editorial, “Hansel’s Ghost: Resurrection of the Experimenter Fraud Hypothesis in Parapsychology,” in the Spring 2016 issue of the *JP*. In it, you noted that some people critical of parapsychology claim “. . . a significant number of psi experiments were likely successful because of experimenter fraud” (Palmer, p. 5). You examined the evidence, and in a paragraph titled Conclusion, made several important statements which I will reference here.

My comments are from the perspective of a director of an association focused on understanding the nature of transcommunication where the “trans” prefix indicates influence of intentionality across the presumed nonphysical-physical interface. Induced and spontaneous after-death communication and mediumship are forms of transcommunication. I will refer to the study of these as *survival studies*.

With the understanding that psi functioning is fundamental to possibly all paranormal phenomena, an attack on parapsychology is an attack on the entire paranormalist community. As a matter of citizenship in this community, it is important for those of us who are able and reasonably informed to respond. My guess is that you agree, else you would not have made your comments in an editorial for all to read.

You stated in the Conclusion of your editorial that “You can’t conclude anything reliable about the existence of fraud from inference in the absence of evidence,” and, “Fraud must be detected, not inferred” (Palmer, p. 12). This should be self-evident in the context of science where knowledge is assumed to be derived from well-considered research. However, it may not be so obvious when it comes to the society of people who practice and support the process of scientific enquiry. For example, journals of the three parapsychological organizations I monitor routinely include anomalistic psychology articles which ignore existing evidence of psi functioning and survival. Also included are exceptional experiences psychology articles, a study which is shaping up to be the same as anomalistic psychology but with more respect for psi research. The decision to ignore existing evidence is an expression of belief rather than good science. Such omissions are routinely ignored by the parapsychological community.

In the same paragraph, you also said that “Insinuations or allegations of fraud are a serious matter and not something to play mathematical games with, especially when there is any chance that the target persons can be identified” (Palmer, p. 12). This is an ethical question which our community needs to clarify. Public accusations of fraud bypass due process. I have worked with many practitioners and many forms of these phenomena. Of all, I have encountered only one person I felt might be more trickster than genuine practitioner. Yet, even with confidence that I am a reliable witness, I hesitate to claim foul in a public forum.

The reasons for my hesitation are threefold:

First, even assuming considerable experience and technical training, I am probably not aware of all the ways the involved phenomena might manifest. Nor am I aware of all the naturally occurring environmental and technological artifacts that might mimic paranormal phenomena. I submit to you that your detractors are likely no more knowledgeable about the ways the involved phenomena might manifest or technological artifacts.

Second, as you pointed out, real people are involved, and as a society organized by the rule of law, it is required of us to find a way to manage the situation without violating individual rights. Shouting fraud from the bully pulpit of academic publications is simply not acceptable.

Finally, on this far frontier of thought, we live in a glass house and every action is critically observed by mainstream society. As a Wikipedia editor, I played a small part in the arbitration cases in which Pseudoscience, Fringe Science and Paranormal were defined as official Wiki policy. (Access these by way of <http://ethericstudies.org/wikipedia-arbitration/>) (Incidentally, I was banned for life from editing the Rupert Sheldrake article based on my comments in support of what is deemed pseudoscience.) General con-

demnation of all things paranormal as dangerous pseudoscience was only possible because of the lack of pro-paranormal or moderate editors—and abundant accusations of bad science, fraud, and delusion published under the cloak of science. In effect, we gave them our sword!

This is a “what goes around, comes around” situation. You rightfully complain about poorly supported academic attacks on psi research but ignore similarly poorly founded academic-practitioner attacks. We all live in the same glass house.

A seasoned skeptic can easily make our internal squabbling appear to be proof that our research results are fraudulent. People who might have been neutral about things paranormal have little reason not to believe such skeptical opinion setters. Now with public support and the ready availability of labels like “pseudoscience” and “fringe science,” it becomes okay for those who fear disruption of their worldview to more aggressively attack paranormalists.

A social model has been carefully established by the greater skeptic community based on the Wikipedia definitions to show that pseudoscience is a danger to society. This has been accepted by some quarters of the federal government as policy guidance. Anomalistic psychology and exceptional experiences psychology support this model. From my perspective, parapsychology was once based on open-minded enquiry of all forms of these phenomena but is now evolving into exceptional experiences psychology. The result has been emergence of a caste system of sorts in which the PhDs whom laypeople look to for learned guidance concerning survival phenomena have become their detractors. Your editorial is right on but myopic. I invite you to consider the need to expand the scope of your concern.

Reference

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TOM BUTLER

tombutler@ethericstudies.org

ANNOUNCEMENT

CHANGE OF EDITOR

With the publication of this issue I will be retiring as Editor of the *JP*. My reason is the recent worsening of a chronic health condition. It's nothing "serious," as this term is ordinarily used in a medical context, but the annoying symptoms are exacerbated by stress, which makes even the routine stresses associated with editing and producing the journal quite onerous. Were I to try to "tough it out," I fear my job performance would suffer to an extent that would become noticeable to others. I will stay on as an Associate Editor to provide some continuity, but my role will be minimal.

When I checked my CV recently, I discovered that I have been Editor of the *JP* for almost exactly a quarter century. Although like any job it's had its ups and downs, I've found it to be an immensely rewarding ride. I am also aware that I am one of very few people who have been able to be employed full-time in parapsychology for most of their careers, and I am immensely grateful to my employers and to the parapsychological community in general for affording me that opportunity. I leave the editorship with no regrets, but it is time for new and younger blood.

My decision to retire was made easier by the fact that I was able to recruit an excellent replacement. The new Editor will be Dr. Etzel Cardeña, who holds the Thorsen Chair in Psychology at Lund University in Sweden. I got to see Etzel's editorial skills up close when I served as a co-editor of the recently published book *Parapsychology: A Handbook for the 21st Century*, of which he was the de facto editor-in-chief. I liked what I saw. His immersion in this project contributed to his broad knowledge of research and theory in the many subareas of parapsychology. This knowledge will help him make wise decisions about what should and should not be published in the *JP*, and to select qualified referees to assist in that process. Etzel also had the experience of being co-editor of the book *Varieties of Anomalous Experience: Examining the Scientific Evidence*, published by the American Psychological Association, and he has an academic specialization in hypnosis as well as in parapsychology. I expect that his interest and expertise in states of consciousness will influence how he approaches his future editorship, but I will leave it to him to inform us about his plans for the *JP* in the Spring 2017 issue. Finally, and most importantly, I have no doubt that Etzel will maintain the scientific standards for the *JP* that I have tried to uphold during my tenure.

John Palmer

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