

Pondering a Journey to the Far Reaches of Mind: An Essay Review of *Transcendent Mind*¹

Rex Stanford

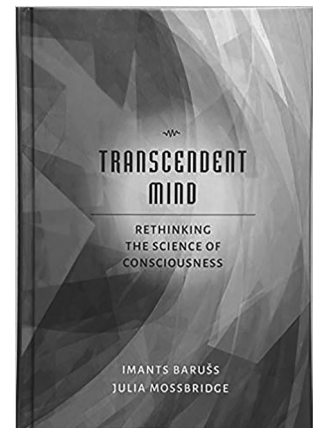
Professor Emeritus, St. John's University

Abstract: This review reflects at length on a landmark publication by the American Psychological Association, a volume whose argumentation, bolstered by reviews of empirical evidence from parapsychology and other disciplines, brings into question the view, presumably unquestioned among many psychologists, that mental functioning, including consciousness, is solely a consequence of interactions of the physical world, what the book's authors term "materialism." Placing their very divergent thinking in the public debate arena, these authors boldly espouse the metaphysically idealistic view that the world is essentially mental or consciousness-based. Their discussion—perhaps truly alien to many psychologists—seems to this reviewer generally careful, clearly articulated, and deeply reflective. Enhancing the volume's high educational value, the authors usually note alternative interpretations of observations. The reviewer felt obliged to note, though, specific instances in which this generally laudable scholarship upon occasion lapsed, such as inaccurate rendering of others' work, empirically unjustified claims, and failure to identify certain researchers whose work was discussed. It may be hoped that this conceptually engaging, rewardingly provocative volume might help awaken psychologists and other interested parties to there being reasonable grounds to start raising questions about the seemingly unquestionable.

Keywords: materialism; idealism; consciousness; time; meta-analysis

In *Transcendent Mind* Barušs and Mossbridge let the world of psychology know that, like it or not, the materialistic view that the brain is the basis of consciousness—traditionally accepted as something of a matter of faith by many (most?) psychologists—is, on grounds of both empirical evidence and logical considerations, in the process of being replaced by the view that consciousness is primary and that physical circumstances depend on it. This bold, wide-ranging book ventures into philosophy, ancient and modern physics, serious psi research, and, on occasion, mind-boggling anecdotes.

¹ *Transcendent Mind: Rethinking the Science of Consciousness*, by Imants Barušs and Julia Mossbridge. Washington, DC: American Psychological Association, 2017. Pp. 249. \$69.95, hardcover. ISBN 978-1-4338-2277-3. The author gratefully acknowledges the kind help of Miguel Roig, Ph.D. Send correspondence to: Rex G. Stanford, Ph. D., 1402 S. Border Ave., Apt. 753, Weslaco, TX 78596 USA. Email: calidris.bairdii@gmail.com



The Introduction describes the book's purpose as exploring "what consciousness looks like when we do not automatically assume that consciousness must arise from the workings of matter" (p. 3). The fundamental thrusts of their eight chapters are sketched. The book is intended for professionals in areas related to mental function and for others ready to consider new directions for understanding consciousness.

Chapter 1 (Beyond Materialism) begins with a historical examination of the concept of materialism and characterizes its historical form as the "*billiard-ball version of reality*" (p. 8, their italics), a wholly mechanistic account of reality. The authors name and describe six allegedly fundamental elements of that view.

The Disappearance of Matter argues the indefensibility of materialism, asserting that modern physics undercuts the idea of matter as described in the materialistic model. Major elements of both theoretical physics (quantum theory and Einstein's relativity theories) and some of the related research are said decisively to contradict one or more of the six assumptions of the materialistic perspective, and some explanation is provided for those claims. Having argued that quantum theory refutes the historical version of materialism, they also note that it is needed to understand certain aspects of brain function (e.g., quantum mechanical tunneling at synapses and any consequences thereof). They argue that these quantum effects in the brain may open neural function to currently unexplained influence(s), including extrasensory ones related to other minds (telepathy), physical information not accessed sensorially (clairvoyance), and future events/information not inferable from sensory information (precognition). Special emphasis is given to changes in understanding time in modern physics and to the evidence from psi research that events, in the absence of any apparent sensory or inferential bases, can be anticipated (as reflected in cognitive, behavioral, or physiological expressions) prior to their occurrence. The authors repeatedly write about "shared mind," by which they mean what most psi researchers call telepathy. Empirical demonstration of telepathy is, despite their repeated use of the "shared mind" term, conceptually muddled by the problem of empirically differentiating influences due to telepathy, clairvoyance, and precognition, which they admit (but nonetheless use "shared mind" as the title of Chapter 2).

Variations on Materialism describes and critically discusses some conceptually somewhat divergent, more recent, versions of materialism. One version given strong critical emphasis is *physicalism*, which proposes, as the authors describe it, "that the world contains just those types of things that physics says it contains" (p. 12). Their critique of physicalism is that physics is always changing due to needing to revise theories in the face of new evidence, and that physicists often disagree on how to interpret the data. Therefore, physicalism is itself always changing or at least in question. My comment: *What scientist really believes in physicalism as the authors describe it?* Every scientist knows that the current view in any science is tentative and may need replacement or modification. That is how one moves away from misconceptions. I do not see why holding such a view, ready for, even inviting, revision *in the face of high-quality new evidence* should be devalued or treated as dogma. Such a view seems, in principle, admirably intellectually open and honest. On the other hand, what kind(s) of evidence would convince the authors that their espoused metaphysical idealism (p. 179) is wrong?

There is, though, a particular materialistic framework that seems invoked widely nowadays, especially with the strong, almost-religious belief that the reality of the human mind, including consciousness

and its subjective qualities, can be encompassed by present or future findings based on neuroscience (or cognitive neuroscience). Barušs and Mossbridge term this belief *neuroscientism* (p. 13), tagging it as a strong belief system. Prior to discussing that idea they describe four uses of the term *consciousness*, derived by Barušs from reviewing the consciousness literature. I will discuss only two of those here and note that these authors say that, unless they indicate otherwise, they will use *consciousness* to refer to “a combination of subjective consciousness₂ and consciousness₃” (p. 15, and note their subscripts). Their *subjective consciousness₂* refers to “the contents of experience” (p. 14) and *consciousness₃* to “the sense of existence that a person has” (p. 14) for self. They define *mind* as “the aspect of the psyche that embodies consciousness along with all nonconscious cognitive processes” (p. 15). (This definition does not clarify what are the aspect(s) of the *psyche* not subsumed by *mind*.) Finally, *transcendent mind* “refers to the notion that mind is ‘transcendent’ in nature, that it cannot be adequately characterized in physical terms” (p. 15).

They use *existential qualia* to refer to “the subjective feelings that anything is going on at all” (p. 15). Their rejection of neuroscientism as a suitable basis for consciousness research, derives from their denial that it makes sense to try to explain existential qualia on the basis of neurons and neural function. They argue that the effort to bridge the gap between conscious experience and the functioning of neurons, even given that neuronal function shapes subjective experience, has not been convincingly successful and seems, thus far, an impossible dream.

The False Promise of Computationalism discusses and dismisses *emergentism*, which, in somewhat different formulations (from different authors), proposes that brains and/or computational systems of suitable complexity (with proper connections to the environment) necessarily give rise to existential qualia, although some commentators want to limit this to biological systems. Barušs and Mossbridge argue that there has never been a convincing explanation of how one would get from a computational system, whether biological or technological, to existential qualia and other attributes of consciousness.

The Failure of Emergentism suggests that, consequent to the failure to derive qualia by means of computational systems of whatever kind, some commentators have resorted to affirming that consciousness, along with its qualia, are a fundamental property of all matter (or, for some, only of biological matter). This is termed *panpsychism*. Commentators espousing panpsychism have proposed somewhat differing ideas but, according to Barušs and Mossbridge (p. 20), it is not clear that any would qualify as materialism under their definition of that construct. This is because such advocates posit existential qualia as a fundamental characteristic of what exists. In some sense, then, such commentators would seem to acknowledge consciousness as foundational for all things. But do such claimants view consciousness in anything like the way it is viewed by the authors (e.g., creating and able to act directly on matter)? This is not discussed.

Anomalous Phenomena are defined by the authors as “phenomena related to consciousness that are incongruent with materialism” (p. 20). Psi research findings that they claim undercut materialism are introduced in Chapter 1, but the nature and strength of evidence supportive of their reality are discussed in Chapters 2 (Shared Mind), 3 (Rethinking Time), and 6 (Direct Mental Influence). Their *anomalous phenomena* term subsumes phenomena traditionally called telepathy, clairvoyance, precognition, and

psychokinesis. Contemporary commentators (including, apparently, Barušs and Mossbridge) recognize that although these four terms have traditionally been used, empirically differentiating them is impossible because they have not been defined as hypothetical constructs (i.e., as having differential testable implications), but as operationally defined terms. I can see no reasonable justification for definitionally sequestering these traditional constructs as “related to consciousness” (p. 20). It seems unnecessary and probably unwise to put this conceptually loaded assertion into the definitions of these phenomena. It is usually said essentially that these phenomena seem inexplicable in terms of well-supported current scientific constructs. What, by the way, is meant in their definition by “related to consciousness”? How does one identify such a case, *given that these authors deem everything as being related to consciousness, as literally created by or out of it, including our own brains?* What is NOT, therefore, related to consciousness?

Scientific evidence for so-called psi events, supplemented by findings/constructs from quantum physics, including entanglement, seem to comprise the cornerstone of the authors’ efforts to understand the real-world ramifications of what they term consciousness. The anomalous phenomena section of Chapter 1, though, serves largely to: (a) introduce such phenomena and their claimed importance for consciousness research because they are thought to undercut materialism, and (b) discredit eight generic ploys used by skeptics to foster dismissal of evidence for such events and to prevent funding and/or publication of related research.

Materialism as Dogma alleges a wide range of adverse consequences for academic students and professors interested in or actively involved with psi research, thanks, the authors allege, to dogmatic rejection of such work by other academics wedded philosophically to materialism. That may be. They strongly decry those kinds of reactions (but not legitimate methodological criticism). Might it not be, though, that making specifically metaphysical claims on the basis of psi research plays a role in fostering those kinds of attacks? The authors go so far, as noted above, as to *define* anomalous interactions in metaphysical terms (i.e., as not congruent with *materialism*), rather than by the far more modest, traditional assertion mentioned above.

Beliefs about Consciousness and Reality sketchily reports and interprets selected research findings from the work of Barušs and other colleagues. The major thrust of a survey study by Barušs and Moore involved using multivariate statistical analyses to study, with over 300 consciousness researchers as participants, the relation(s) between beliefs about consciousness and beliefs about the nature of reality. The researchers, using multivariate statistical analyses, found evidence of a single dimension (labeled “material-transcendent”) underlying beliefs about reality and, along that dimension, three “positions” (p. 27), namely *materialism*, *conservative transcendence*, and *extraordinary transcendence*. Some items empirically characterizing those three positions were mentioned. Psychometrically inclined readers might wish to know more about how this single dimension and its three “positions” were operationally derived.

Also discussed was a single finding from a study of 75 university-student volunteers (Jewkes & Barušs, 2000) that the personality trait of “understanding” was “correlated with transcendent beliefs” (p. 28). Readers will be unable to know the strength and statistical significance of this relation because neither the correlation coefficient nor its probability under the null hypothesis were provided.

Next discussed were findings from a later study (Lukey & Barušs, 2005²) that examined whether transcendent beliefs about consciousness relate to intelligence, but that, alas, involved a far too small and selective sample (39 university students serving in a subject pool) to allow generalization to a broader population. There were some encouraging trends, but I presently see—based on the limited information in this chapter—no basis for statistically grounded, even potentially generalizable, inferences about intelligence and personal views on the nature of consciousness.

It was gratifying that Barušs and Mossbridge stayed with just describing results from this set of studies and wisely did not lay claim to generalizability (or general truth) of the findings just mentioned. Given the nature and (usually) small size of the samples, I fear that in the absence of a specific caveat about generalization, some readers might not have noticed or understood the thoughtful, but implicit, caution reflected in the authors' wording.

Chapter 2 (Shared Mind) provides an extended, in depth, look at what these authors deem some of the best evidence for anomalous reception of information, either mind-to-mind (i.e., telepathy) or in regard to the physical world (i.e., clairvoyance). They acknowledge, as do most (all?) psi researchers, that empirically differentiating these two nominal (i.e., operationally defined) kinds of receptive psi is not possible, given our present state of knowledge, and that such resolution may turn out not be possible in principle. Nevertheless, Barušs and Mossbridge opted to name this chapter "Shared Mind," although that term seems strange (or unjustifiable) as an umbrella intended to include both telepathy- and clairvoyance-paradigm results. They also sometimes use this idiosyncratic term elsewhere in this book to refer collectively to telepathy and clairvoyance.

Shared Mind in Psychology is an engagingly thoughtful journey through the fascinatingly intricate mind of Sigmund Freud, in this case his conflict relative to what kind of public face to adopt in regard to telepathy. Freud's major focus in his ultimate public concession regarding telepathy derived from its apparent appearance in dreams that the patient shares with the therapist (or, occasionally, vice versa). The section ends with a stress on the need for laboratory demonstration of receptive psi to allow solid research-based conclusions. Some have disagreed about such necessity (e.g., Ian Stevenson and others, discussed by Stanford, 1992, pp. 220 - 222). My own view is that study of possible psi outside the lab may suggest ideas about psi function that profitably can be investigated further in both non-lab and lab settings. Some things we cannot learn about in the lab. In the lab or elsewhere, research quality is a *sine qua non*.

Laboratory Evidence of Shared Mind moves the discussion to the laboratory, but the intriguing research by Carpenter (2002) aimed at demonstrating practical application of psi through taking advantage of multiple individuals calling the same target (i.e., presumed redundancy in psi access to signal) might not have been a particularly good way to begin a chapter on laboratory evidence of psi. There were elements of this work's highly complex and unusual methodology that make it difficult for others to replicate methodologically and that might discourage those considering a replication effort. No independent replication attempt was mentioned.

Remote Viewing primarily, but not exclusively, discusses work with the remote viewing paradigm.

² Quoted references not in the review's bibliography are listed in the book reviewed.

That involves no special apparatus or induction procedure—unlike, for example, ganzfeld or hypnosis—but involves directly and simply asking the participant to view a distant target site and to describe and draw it. The viewer is sometimes alone or may be with someone else who knows neither the target nor its location, but who may encourage the viewer's reporting (the paradigm has had various operationalizations). Remote viewing was at the heart of research efforts on behalf of the intelligence community some years ago, although that was not its exclusive use. The authors discuss statistical outcomes with this paradigm as contributed by more than one investigator (or set of investigators), noting thousands of such trials have shown “that *trained* (reviewer's emphasis) remote viewers describe the target location or image significantly more often than nontargets” (p. 36). Readers might have appreciated some information on the nature of this claimed training. Jessica Utts (1996) is reported as having noted that the highly successful remote-viewing research she was reviewing used “remote viewers who had been *screened* (reviewer emphasis) for their ability to receive and report this kind of information” (p. 37 of reviewed book) and that such screening might have been central to the notable success of those studies.

To illustrate the potential value of selecting talented psi participants to get evidence of receptive psi, Barušs and Mossbridge discussed five series of receptive-psi studies from among 15 series done from 1969-1971 by the Psychical Research Foundation (PRF, *but not part of Duke University, contrary to the book's claim*), Durham, NC. All of this work was with Lalsingh (aka Sean) Harribance (henceforth, LH), an apparently very psi-talented individual, who guessed in each run whether each of 10 randomized cards had a female or a male face on its front side. The five series of such runs selected for discussion were relatively late series deemed superior in obviating sensory communication. The authors did not cite primary sources in their discussion of any of the PRF work with LH, or of his EEG work. The single source cited on this work was a 2015 book chapter by B. J. Williams.

The authors' seeming failure to consult primary sources for the LH work might have been the cause of their not having discussed a quite dramatic but peculiar finding in one of those five receptive-psi series mentioned above, a finding that might have pragmatic and conceptual significance related to implicit psi-mediated knowledge. On all three series designed to allow telepathy or clairvoyance, LH had highly statistically significant success. The two other series were intended to examine clairvoyance. In one (Series 9), the earlier of the two clairvoyance series, LH produced highly statistically significant success *that was objectively the highest level of success in all of his receptive-psi work in Durham* (Roll & Klein, 1972, p. 111). On the other hand, his performance on the subsequent clairvoyance series was about as close to mean chance expectation as one could imagine (Klein, 1972).

What might explain this dramatic difference in clairvoyance performance in these two series? The dazzlingly high-scoring clairvoyance series (Series 9) was the earlier of the two tighter-condition clairvoyance series with LH, who was reported to have regarded such clairvoyance testing as difficult: He had received no feedback about outcomes until all 100 of the 10-trial clairvoyance runs (i.e., 1000 trials) had been completed (Roll & Klein, 1972, p. 106), although in earlier work he had received feedback after 10 runs (100 trials)! Series 9 had thus been a challenging, novel situation, but he came through this 3-day clairvoyance series with outstanding success. The later clairvoyance series (Klein, 1972) included 500 clairvoyance and 500 telepathy trials, done in 10-trial runs with runs of telepathy and clairvoyance regularly alternated, *but LH was not informed that both clairvoyance and telepathy runs were involved*

until the entire series had been completed. In each session there were 10 runs (with 10 trials per run), and there were two such sessions each day, so the 1000-trial study required five days. The results were clear-cut: (a) The 500 clairvoyance trials yielded a miniscule deviation from mean chance expectation (MCE), a dramatically flat chance performance; but (b) there was very highly significant statistical evidence of positive receptive psi in the telepathy trials.

LH normally had worked there under telepathy-allowing conditions, and he strongly preferred such work. Possibly the anticipated (but on half the runs, nonexistent) telepathic agent was for him a psi-mediated turn off. Might implicit knowledge of this deception have elicited psi-mediated psychological reactance, a motive to reassert his freedom-to-choose in the face of an unannounced threat to it? At any rate, on the clairvoyance runs his psi either simply was not activated or perhaps was unconsciously reactively suppressed. Perhaps, as suggested in Judith Klein's summative remarks at the end of her report on this work (Klein, 1972, p. 72), LH was exhibiting a psi-mediated preferential response. It is regrettable that the aforementioned major methodological differences in the clairvoyance methodology of the two studies were neither discussed nor mentioned by Barušs and Mossbridge. Commenting on the failure to find clairvoyance in the clairvoyance-telepathy design, they simply suggested that LH, "likely had 'on' and 'off' days" (p. 37). It is not, though, credible to explain the second study's dismal clairvoyance results on an off-day basis because telepathy and clairvoyance runs were alternated in closely time-locked fashion each day due to run-by-run alternation of task type in every session.

It long has been established (Poulton, 1973) that sensorially presenting conditions (or stimuli) in close temporal order can produce task-juxtaposition effects often involving more dramatically different reactions to each circumstance than when it is presented alone (and order of presentation can make a difference, too). *The LH data just discussed conceivably evince this, if we consider the possibility that task-juxtaposition effects can be mediated via receptive psi acting as implicit knowledge.* Task juxtaposition effects potentially could have major importance in psi research, whether the task juxtaposition is sensorially known (Stanford, 2003, pp. 40-45; Stanford & Stein, 1994, pp. 255-258) or is extrasensorially accessed as implicit knowledge (as might have happened in this work with LH). An exciting possibility emerges from the research with LH that, if real, has important implications for designing psi studies: The researchers' efforts to sensorially blind LH to the presence of agent-absent runs might have been vitiated by unconscious psi-mediated affective response to the realities of the two-condition test situation.

Telephone Telepathy was selected for discussion because it focuses on one of the circumstances in which persons' minds "are most likely to be shared" (p, 38), and mind sharing, as the authors term telepathy, is a major quarry in their search for consciousness-related outcomes. Telepathy might occur when one individual intends to call another by phone. This work focuses on the situation in which someone is getting ready to answer the phone that often is reported as a time when the individual being called is apt to feel that a particular person is calling. Barušs and Mossbridge delineate three reasons why persons at times may mistakenly think telepathy has happened in this situation. The originator of this research is Rupert Sheldrake and his later, better-controlled, research protocols are described by these authors. Two studies with positive outcomes are briefly discussed, one of them dazzlingly statistically significant, but statistical significance does not demonstrate a particular cause. Barušs and Moss-

bridge stop well short of endorsing the idea that telephone telepathy was demonstrated by these two studies and provide two justifications for that decision (p. 39).

Neurophysiological Measures of Telepathy reflects the idea that the brain activity of the intended telepathic receiver may differ in some recordable way when the sender is attempting to send some kind of information than when that is not the case. Some investigators believe this may provide a more sensitive measure of receptive psi than studying conscious responses to telepathic information. Barušs and Mossbridge discuss in detail some pitfalls that might arise in such work and consider how some of them might be obviated or made less likely. In this important discussion the authors cite several useful resources related to such cautions.

Barušs and Mossbridge claim there is a widespread “bias” (p. 39) that if research includes neuroscientific measures it is judged of higher quality or more rigorous, but the research cited (Weisberg, Keil, Goodstein, Rawson, & Gray, 2008) did not address that issue. It examined instead the hypothesis that including irrelevant neuroscience information in presenting an explanation for a psychological phenomenon can reduce the inclination to examine critically its underlying logic, even with a poor-quality explanation. That effect was found with individuals not expert in neuroscience, but not with experts.

Statistical Evaluation of Cumulative Laboratory Evidence largely concerns meta-analyses of outcomes of multiple experiments on telepathy when there is a receiver and a sender (aka telepathic agent) with receiver mentation based either on (a) recall of dream content elicited immediately at the end of an episode of REM sleep or (b) recipient utterances during relaxation in the Ganzfeld sensory-uniformity setting. Details of these procedures are well characterized. Glad to say, it is again noted that although such studies allow the occurrence of telepathy, one cannot be sure that, if there is success, the target material was not accessed by clairvoyance (alone or in combination with telepathy).

Also discussed as possible bases of some reported psi-research findings are questionable research practices (QPRs). QPRs include a range of practices that can mislead about the nature or quality of one’s published data. Three QPRs were mentioned by the authors at this juncture: (a) *selective reporting* – researchers electing to publish only studies that support their desired outcomes; (b) *data culling* (my term) – rationalizing, subsequent to data analysis, the discarding of data from subject(s) who performed contrary to a hoped-for outcome, this either rationalized in the investigator’s mind by a circumstances-justified discarding rationale or done as callous fraud, but either route to such culling may entail creating a public rationale for ditching data; and (c) *optional stopping* – tracking significance level during one’s data collection and deliberately stopping testing at a time when the p -value for a desired effect is statistically significant. Barušs and Mossbridge also noted that selective reporting “is implicitly encouraged by psychology journals that refuse to publish reports of nonsignificant effects or replications of previous experiments” (p.41). Publication policy in psychology journals might, though, recently have been changing due to the widespread realization, evoked by the recent crisis of confidence in psychology, that such a policy can be inimical to honesty in scientific practice and reporting. This would have been an opportune juncture at which to note that in 1975 the Parapsychological Association (PA), the international professional society of psi researchers, enacted the requirement that any journal to be considered a PA-affiliated journal must not reject an empirical paper primarily on grounds of not having obtained statistically significant support for the propositions it was intended to examine.

Research discussion first focuses on findings from the dream-ESP (especially telepathy) research paradigm developed and extensively used at the Maimonides Medical Center from about 1966 to 1972 ; Barušs and Mossbridge apparently relied, for their discussion, largely, if not exclusively, on Child's (1985) publication. Child's detailed, cautious, paper introduced psychologists to this innovative and exciting dream-ESP work and exposed boldly gross misrepresentations of this research in several books that were widely read by the psychological community. Barušs and Mossbridge provide an informed account of the part of the Maimonides dream-ESP work that provided perhaps the best opportunity to study telepathy in dreams due to a telepathic agent attempting to communicate a target during the recipient's REM sleep and an awakening at or near the end of each REM cycle to obtain the recipient's recollection of dream content, with the same ESP target throughout the night. Van de Castle (1977) provided a very clear methodological description of this work.

Child, for good reasons considering the goals of his statistical inference, did not present an overall statistical analysis for the series (total = 130 trials) that was of special interest to Barušs and Mossbridge, but they did so. The results yielded a very respectable p -value, but such a value, based upon pooling hits across studies and receivers of several kinds seems inadequate to support conclusions other than that some anomaly might have been manifest. Based on their usually cautionary posture, Barušs and Mossbridge might share that view of restricted generalizability. I share their view of the importance of cross-laboratory efforts to replicate the dream-telepathy/dream-ESP findings.

Although these authors stressed the need for cross-laboratory efforts at replication of the Maimonides dream-GESP work (i.e., general ESP task, which operationally allows telepathy and/or clairvoyance), they did not mention the unsuccessful effort at a sleep laboratory at the University of Wyoming (UW) by Belvedere and Foulkes (1971, as cited and discussed in Van de Castle, 1977) to replicate, using the full Maimonides protocol, the results of the highly successful eight-night GESP series at Maimonides (Ullman & Krippner, 1970) that had Van de Castle as the receiver. The UW investigators implemented the objective features of the Maimonides protocol, including having Van de Castle as receiver in their eight-night dream-GESP study, but the study failed to produce statistical significance. Mention by Barušs and Mossbridge of this failed effort at replication would have filled in an important gap in their reporting of work with the full Maimonides protocol during the time frame (1966 – 1972) of the Maimonides dream-GESP work. Inclusion of the UW episode along with some discussion of Van de Castle's reflections on that unsuccessful study (Van de Castle, 1977, pp. 491-492) could have apprised their readers of how very important might be the interpersonal milieu in psi research. Foulkes, one of the two UW investigators, provided his own highly revealing and strongly stated portrayal of the cognitively/socially stressful atmosphere surrounding the UW study (Van de Castle, 1977, p. 491).

Near the end of their discussion of dream-ESP research, Barušs and Mossbridge mention that there has been subsequent work on dream-ESP for which new meta-analyses (Sherwood & Roe, 2003) involving multiple laboratories provided evidence of the dream state being one in which telepathy can occur. That cited report was not focused specifically on the Maimonides protocol, and it included studies in which the dreamer was at home and thus, unlike in the Maimonides work, there was no electro-physiological monitoring and hence no awakenings for dream reporting at the immediate end of REM periods. The absence of these closely timed dream-reporting opportunities conceivably was, as Sher-

wood and Roe suggested (2003, as discussed in Baptista, Derakhshani, & Tressoldi, 2015, p. 206-207), one of the several cogent reasons that the post-Maimonides studies might have evinced, as a group, significantly lower psi-task performance than was found in the Maimonides studies. Nonetheless, there was independent statistical significance of receptive psi based on reported dream mentation for both the Maimonides and the post-Maimonides studies (Sherwood & Roe, 2003; Baptista, et al., 2015). Not surprisingly, the post-Maimonides studies in that review varied considerably in methodology and thus, potentially, in quality. Sherwood and Roe discussed shortcomings of methodology in some of this work, and Barušs and Mossbridge (p. 43) remark that the diversity of methodologies in the post-Maimonides work make it difficult to determine whether problems in the conduct of the studies might have been responsible for the overall statistical significance. That circumstance is not mitigated by the fact that quality-related coding of studies—and cross-database comparisons of them—for the Maimonides and the post-Maimonides dream-ESP studies were not available (Baptista et al., 2015).

The first Ganzfeld-ESP meta-analysis (Bem & Honorton, 1994; henceforth, BH, 1994) discussed by Barušs and Mossbridge provided, however, data from only one laboratory, Psychophysical Research Laboratories (under Charles Honorton), but that paper specifically examined 11 studies providing 354 sessions conducted by 8 experimenters, including Honorton. After accurately reporting the cumulative hit rate for this meta-analysis, based on 10 of the 11 studies, Barušs and Mossbridge stated: “These results were obtained after removing a study that produced very impressive results but did not follow the same protocol as the other 10 studies.” True. But then they continued, “That study used ‘optional stopping,’ one of the QRPs that should be avoided” (p. 44). But wait! Here is a blanket, flat-out accusation of a QRP, but no support was brought forth for that claim. The study in question was Study 302 (not thus identified by Barušs and Mossbridge), and although its data properly were not included by BH (1994) in reporting the cumulative result of their meta-analysis, its data (including its necessary response-bias-adjusted hit rate and statistical outcome) were provided in Table 1, along with data for the 10 other studies (p. 11). Although the results for Study 302 did not appear in the general accumulation, its omission had nothing to do with optional stopping: The reasons for treating Study 302 separately were: (a) The other 10 studies in the autoganzfeld database had involved both dynamic and static targets (BH, 1994, p. 11), *but Study 302 had, by design, been intended to examine four dynamic target-clips on which there had been outstanding numbers of hits in earlier series* (p. 10), *making it a very different kind of study*; and (b) *Study 302 was incomplete, for it was stopped prematurely because of the impending closure of the Psychophysical Research Laboratories*. In sum, contrary to a claim by Barušs and Mossbridge, Study 302 certainly would appear, on the basis of the report, *not* to have involved optional stopping. Had the authors any credible evidence in support of their optional stopping claim, that evidence should have been presented, rather than publishing a claim of a QRP without adducing supportive evidence and without providing any counterargument in regard to the circumstances that were clearly spelled out in BH (1994).

The BH (1994) Ganzfeld-GESP meta-analytic report described the autoganzfeld research paradigm, outlined the character of each of the series involved, noted its inclusiveness of the work done, and provided a meta-analysis indicative of robust statistical significance in line with the assumption of psi being present. The paper also reported secondary analyses to examine the typical characteristics of the most successful Ganzfeld-GESP performers and of success on target types (dynamic versus static),

essentially confirming findings from earlier research. All of the experimental work was said to meet the methodological, statistical, and reporting criteria agreed upon earlier by both Honorton, the premier figure in Ganzfeld-GESP research, and Ray Hyman, a leading skeptical critic of parapsychological claims. BH (1994) strongly motivated later, extensive autoganzfeld-GESP work by new investigators. This was a major step forward and the autoganzfeld series played a role in subsequent meta-analytic reports discussed by Barušs and Mossbridge.

Barušs and Mossbridge would have benefitted their readers' understanding of the success of the meta-analyzed autoganzfeld-GESP work by BH (1994) had they apprised them of the systematic efforts by the PRL's Ganzfeld-GESP experimenters to create a psi-favorable milieu via a carefully planned recipient orientation procedure and by providing a warm, friendly social milieu as described in detail in another paper (Honorton et al., 1990). Readers of BH (1994) were told where they could find those details (i.e., Honorton et al., 1990).

Barušs and Mossbridge next ponder a meta-analysis by Milton and Wiseman (1999; henceforth MW). Our book's authors reported that this meta-analysis involved "30 *autoganzfeld* studies performed between 1987 and 1994" (p. 44). Factually, the MW meta-analysis contained, by deliberate design, all the retrieved ganzfeld studies ($N = 30$), *whether they were autoganzfeld or not*. MW provided an elaborate rationale (p. 388) to justify their decision to include all ganzfeld studies in their meta-analysis. Also, the final year for the conduct of the MW database's studies was not 1994; the work ran at least as late as March 1996.

Barušs and Mossbridge correctly reported that the MW meta-analysis failed to find statistically significant evidence of "telepathy" (p. 44). (Given the GESP paradigm, this actually was a failure to find significant evidence of receptive psi more generally.) Although BH (1994), which MW (1999) had sought to replicate, had computed statistical significance using the exact binomial test of the overall hit rate—a very precise statistic for overall hits—MW (1999) used another analysis, the unweighted Stouffer method, which did not show statistical significance. Readers of the reviewed book might have appreciated knowing that if MW (1999) had used the exact binomial test (for overall hits), as had BH (1994), *the hit rate for the MW database would have shown statistical significance* (Storm, Tressoldi, & Di Risio, 2010, footnote, p. 473, which attributed this information to a personal communication from Jessica Utts, a statistician). One might have supposed that if MW wanted to see whether they had replicated the BH (1994) success, they would have used the same statistical test. Nothing was said by MW to justify their divergent choice of overall statistical test, they simply reported that the test was specified a priori (p. 388).

Barušs and Mossbridge point out that the studies in the MW (1999) database differed in a potentially important methodological way from those in the BH (1994) meta-analysis. According to our book's authors (citing Baptista & Derakhshani, 2014; Baptista, Derakhshani, & Tressoldi, 2015), in the BH database almost all of the receivers had been preselected on characteristics that in earlier work had been shown to be viable predictors of ganzfeld success, but in the MW meta-analysis only 10% of the 30 studies used preselected subjects. Barušs and Mossbridge also discuss ganzfeld-ESP meta-analytic evidence that collectively appears to support the claim of receptive-psi appearing with some regularity in ganzfeld over several decades and in both autoganzfeld and other ganzfeld work (Baptista, Derakhshani, & Tressoldi, 2015; Storm et al., 2010). We still, though, do not know why ganzfeld has been ef-

fective because experimental process-oriented work intended to explain the success has been woefully lacking. Just finding that certain types of persons tend to succeed in ganzfeld provides no explanation of their outstanding success! Stanford (1987) discusses conceptual hypotheses remaining unaddressed relative to explaining both ganzfeld and hypnosis receptive-psi outcomes.

The final discussion of Barušs and Mossbridge on the cumulative laboratory evidence from the ganzfeld-ESP paradigm seemed, despite some potentially confusing language, to be saying that we really do not have any insight into the underlying nature of the anomaly (or anomalies) evinced in the ganzfeld-ESP paradigm. Many psi researchers, myself included, would heartily concur. They also might have been suggesting that although we talk of test situations allowing both “telepathy” and “clairvoyance,” the lack of any insights on the mechanism(s) involved means that nothing can be ruled out at present, including that there is only one fundamental process involved, whatever that might be. They might have done well at this juncture to have emphasized the crying need for creation of and research based upon genuinely testable hypothetical constructs. Investigating the construct of *mental or behavioral influence of an agent, MOBIA*, (Stanford, 1974b, pp. 343-348) can examine the idea that the mind of the agent might play an active role here, perhaps helping to extricate us from this conceptual morass. Perhaps not wishing to leave readers in the conceptual doldrums, the authors promise next to discuss “what we do know” (p. 47) about the mechanism of either telepathy or clairvoyance.

What Underlies Shared-Mind Phenomena? seems not to address in any effective way what we know about these phenomena, and if it did, it would have had to be vastly longer. About 1.5 pages of the section’s 2 pages are devoted to what seem to me to less than well-developed ponderings about the kind of information actually being received or its source.

Despite these deficits, this section later explores some intriguing topics that seem worth pondering, given how little we know about receptive psi. Barušs and Mossbridge seem to be looking for some new, different ways of thinking about receptive psi, although this exposition does not deliver on the earlier promise in regard to what we do know. The first topic considered is the concept of *long body* from the Native American Iroquois, an idea that transcends the concept of individual (and perhaps of individuality) but assumes some kind of confluence of ancestors, contemporary tribal members, their land, and whatever is among their belongings. It reportedly is this extended entity from which information may guide the decision making of the tribe. This idea was explained by the late William G. Roll, for whom a 1980 publication (in an Australian journal) was cited. If the long body idea (including its possible relationship to receptive psi) seems weird to those of us from our predominantly individualistic culture—we look, for example, for information in the minds of individuals—it may be because we wear conceptual blinders brought to us by our culture. Possibly the long-body idea contains the rudiments of something about reality to which we have not had open minds. Alternatively, perhaps reality, including psi, functions somewhat differently in different cultures because of differing perspectives. These and related ideas discussed briefly by Barušs and Mossbridge might fruitfully be considered in the interest of broadening the bases of conceptualization.

Discussion next turns to an “information-access view” (p. 48), but gaining a clear idea about this perspective from reading this brief and somewhat rambling discourse was not, for me, possible. I wish the authors had backed off and tried to bring it into clearer focus. My limited understanding is that

the authors were trying to get away from the idea that the information developed by recipients during receptive psi necessarily is the result of accessing an individual mind or even, in, say, “clairvoyance,” an individual object. They say, “The idea here is that we could be sharing access to a larger pool of information, like a unified, larger mind” (pp. 47-48). It is when they try to explain how this works in specific circumstances that the concept starts to lose traction in my mind. This does not seem to me the stuff from which research studies might easily be developed, and the authors, glad to say, called it simply a “view” (p. 48), not a theory or even a model. Apparently in an effort to support the utility of this construct they cite non-psi work whose relevance to the earlier psi-related discourse seems to me at best analogical or stretched.

Far easier to grasp was the next topic, EEG-ESP research with an outstanding receptive-psi performer mentioned earlier, Lalsingh Harribance (hereafter, LH). Discussion focuses on the relation between ESP-task performance and the percentage of time LH showed predominantly alpha (i.e., 8-13 Hz) rhythms in the occipital lobe(s) of his brain (henceforth, PTA). The issue at hand is whether an abundance of alpha rhythms may indicate a neural state favoring ESP-task success. Barušs and Mossbridge focus their major discussion on two studies, apparently Series 10 and 11 (Morris, Roll, Klein, & Wheeler, 1972). They cited a secondary source and gave no study-author names and dates, so I could not check on details related to their single caveat (p. 49) related to additional EEG-ESP work with LH. Aside from their caveat, I would voice two others about Series 10 and 11: (a) These studies successfully used ESP performance to predict PTA, but we do not learn whether PTA predicted ESP-task performance; knowing both might be instructive, for if ESP performance predicts PTA, but not vice-versa (or in reliably lesser degree), alpha might be necessary (or helpful) but not sufficient for ESP success; and (b) correlational findings such as these cannot decisively evaluate the veracity of the hypothesis that PTA has a favorable causal influence on psi-task performance. Mention of one or more of those considerations would have been helpful.

Regrettably, there are substantive errors in Barušs and Mossbridge’s remarks related to non-psi EEG research by Kounios et al. (2006). Barušs and Mossbridge suggested that alpha rhythms may indicate brain activity “related to the process of ‘looking inward’ and retrieving an answer” (p. 49), but in a supportive lead-up to that suggestion they erroneously reported, “Alpha EEG activity has been found to *increase significantly* (reviewer’s emphasis) just before people discover an insight leading to a solution to a problem—for example, just prior to the “aha!” experience that comes with solving a word problem (Kounios et al., 2006)” (p. 49). Unfortunately, the quoted statement is erroneous in regard to its implications about what Kounios et al. (2006) (a) sought to study, (b) their methodology, and (c) what they found.

Contrary to the statement by Barušs and Mossbridge, Kounios et al. did not attempt to study whether there was *an increase in EEG alpha activity just prior to participants’ discovery of an insight solution to a problem*. That was neither the intended purpose of their study nor something that emerged from it serendipitously. Kounios et al. (2006) wanted specifically to know whether a particular brain state *prior to encountering the problem* would play a preparatory role in favoring an insight problem solution over a noninsight one. Accordingly, they did not measure or intend to study brain state *subsequent to knowledge of the problem* (and just before the “Aha!” experience). Specifically, the EEG recording that

was the focus of their report was obtained during the 2-second interval *between* the time the volunteer was queried as to readiness to see the problem and the actual presentation of the problem. *Thus, all the EEG recording preceded problem presentation*, so the study could not possibly have addressed the issue framed in the remarks of Barušs and Mossbridge. In sum, the remarks by Barušs and Mossbridge related to the Kounios et al. (2006) work are decidedly off-base relative to those investigators' expectations, methods, and findings.

The Creativity Link is brief, but the topic undoubtedly has importance due to empirical findings already described. Barušs and Mossbridge report, "People who manifest musical and artistic ability seem to perform better as receivers in Ganzfeld telepathy (*sic*, really, GESP) tasks than nonselected participants" (p. 49), citing Baptista and Derakhshani (2014), who discussed meta-analytic findings. Baptista and Derakhshani (2014, p. 73) went so far as to opine that "artistic populations"—but types not specified—would seem the most promising for replication of psi-success in ganzfeld.

Many artistic types who volunteer for psi experiments may have, also, potentially psi-favorable attitudes and personal experiences, and these characteristics may operate interactively to foster psi-task performance. For example, a creatively functioning mind may *allow* the influence of receptive psi, but personal positive interest in the psi aspect of the study may be important to engage that creative capacity in the deliberate psi-task situation. Dalton (1997, as cited in Baptista and Derakhshani, 2014) deliberately used, in a high-quality ganzfeld-ESP study, artistic volunteers who believed in psi and reported such experiences. The overall hit rate was dazzling and strongly significant. This was one of the highest hit rates recorded in such work! That does not prove that the creative and psi-related personal characteristics functioned interactively to foster psi-task performance, but it is compatible with that hypothesis.

Understanding Access to Anomalous Information is the stated purport of this section, but this discourse may have as its strategic aim to make readers more comfortable in entering the deep waters of the subsequent discussion of time in Chapter 3. That is important because Chapter 3's punch revolves, in my mind, around the topic of precognition, which may be at the heart of much that we ponder in psi research but that is a tough conceptual pill for some folks to swallow! Might Barušs and Mossbridge's discourse fend off an aversive response? It may for some. Theirs is a thoughtful effort, even if it does not, I think, actually provide a scientifically tractable, explicit basis for understanding anomalous cognition. It seems more like a lead into the metaphysics central to their discourse. Positing some boundaries for one's phenomena are required for scientifically tractable explanation. So far, such boundaries have not been found. Barušs and Mossbridge may be saying that the reason is that we have been looking in the wrong places (i.e., looking for physical limits). They seem to feel that to find the lawfulness in this domain, we shall have to look deeply inward. They consider that consciousness is behind what we observe in regard to psi and otherwise; so, "to understand consciousness, we must attempt to understand the natural laws that describe the behavior of the mental, experiential reality we each inhabit," and "these natural laws of the mind must include laws related to time" (p. 51).

Chapter 3 (Rethinking Time) is a long chapter and certainly one of the most stimulating for individuals who appreciate a thoughtful exploration of research at the forefront of scientific advance. What better topic for that than time? Barušs and Mossbridge introduce their rethinking of time by juxtaposing

our everyday experience and sense of time and its apparent flow into the future with the deep kinds of questions many of us, in a pensive moment, ask ourselves. Phenomenologically speaking, time is in life our present companion—certain altered-states excepted—but Barušs and Mossbridge take us on a journey beyond that into the diverse claims of self-nominated spokespersons for time who have tried to conceptualize its real nature or, sometimes, its nonexistence—philosophers, physicists, and mystics, among others—and venture onward to discuss anomalous phenomena, including precognition, both from life and from the laboratory. The authors promise in our escorted journey to travel “from physics, through psychology and neuroscience, and finally to discussions of anomalous phenomena” (p. 54). They further promise to introduce us to their constructs of “*apparent time*” and “*deep time*” (p. 54), the former referring to how we normally experience time and the latter to something that “structures the nature of consciousness and physical manifestation, and a possible relationship between the two” (p. 54). They also promise to revisit those two constructs in their final chapter where they will present their model of consciousness.

The Notion of Time in Physics reports that individual physicists vary widely in their conceptualization of time and that for some it does not exist. What is not made clear is whether or in what degree these sometimes radically divergent views have testable implications and whether research has helped decide between them. An exception would be Albert Einstein’s special theory of relativity as it treats time. According to Barušs and Mossbridge that theory implies that “time slows down for someone who is moving through space relative to a stationary observer” (p. 55); they cite two experiments reported in prestigious journals that claimed validation of that prediction. Citing implications of the special theory of relativity that the speed of light does not vary, but that at the speed of light time has stopped entirely, they maintain that these facts are “consonant with experiences of timelessness” (p. 56), experiences they promise shortly to discuss. This would, though, seem to be argument by analogy, which is a very fragile form of argument. In this case, that is at least in part because it conveys nothing clear about how what we know of the relativity-related-finding could help us to understand, say, a mystic’s experience of timelessness. In other words, how one kind of timelessness would map onto the other is not specified and does not seem obvious.

They consider the construct of a *block universe* with three spatial dimensions and a fourth consisting of time with this universe encompassing all that has happened and that will happen, so that we have a totally deterministic world. This essentially rids the world of what might be called “real time” (p. 56). My sense is that Barušs and Mossbridge tend, instead, to envision a world that reflects experiential reality, in which the world is not constrained in its very nature. They tend to think that mind is behind manifest reality. If so, it is no wonder that experience perceives something quite fluid about the world. This may become clearer in their final chapter.

This section about the concept of time specifically in physics also broaches the topic of retrocausation with extended discussion of precognition/presentiment looming ahead in the same chapter. They do so by describing two experimental settings, the two-slit experiment and the delayed-choice two-slit experiment, but I will not spoil the book reader’s fun by explaining here those intriguing studies. Suffice it to say that the delayed-choice experiment’s results will seem to some a possible case of retrocausation (backward causation) initiated by an experimenter’s choice regarding procedure, but an-

other interpretation has been in terms of a quantum-theory concept by which until the experimenter's choice is made, alternative event sequences exist but are in a state of *superposition* until one sequence terminates upon experimenter choice. That is how I understood this discourse, but I am not a physicist.

What emerges as I read this section is that the authors have no desire to ditch the concept of time, even if some philosophers and some physicists apparently have done so. Instead, Barušs and Mossbridge reintroduce the concepts of *apparent time* and *deep time* (p. 58), adding that both constructs can refer to something subjective and to something objective, thereby giving rise to a four-part view of time that, in the interest of concision, I will not describe here (but descriptions appear on pp. 58 - 59 top). These may take some repeated reading and thought.

Subsequently, Barušs and Mossbridge note that physicists in general have not devoted much thought to the idea that events in the future may exert influence on earlier circumstances. The notion of cause emanating essentially from the past might have been overemphasized, they suggest, considering the mounting evidence of future circumstances affecting the present. They then ask what we really mean by the concept of causation. They opt for the statement, "Event A causes event B if the occurrence of event A is *necessary* for the occurrence of event B" (p. 59, reviewer emphasis). This definition seems problematic because it implies that *if event A is absent, event B will not occur*. Alas, we know that there are events that have more than one sufficient cause, so any one of them, acting alone, can bring about the effect. Therefore, if we wish a definition of causation that is sufficiently broad, it cannot require event A to be necessary for the occurrence of event B. Modifying the definition to say, "Event A causes event B if the occurrence of event A is *sufficient* for the occurrence of event B" helps greatly, but this seems a bit narrow relative to causation. Sometimes an event is causal in relation to another event but that causal role is evident only when that event is in the company of one or more other circumstance(s). In other words, there is such a thing as interactive (i.e., conjoint or multiplicative) causation. For example, a psychologically very stressful event may be deemed to cause an episode of seriously maladaptive symptoms, but only for an individual genetically disposed to such episodes (i.e., a "diathesis/stress hypothesis"). Perhaps terminology should recognize more than one kind of causation.

The Centrality of Time to Consciousness is extremely brief (1 page) and amounts to a strong statement that the authors take very seriously the subjective experience of time as involving past, present, and future and a sense of flow and that these experiences have their own validity. Thus, we need not be concerned about whether as viewed from a physicalist perspective what we thus experience is or is not an illusion. Barušs and Mossbridge deem that such experiences can and should be studied in their own right. The issue then becomes whether one can develop ways to do that.

Conscious Awareness of Subjective Time discusses the time-related constructs of duration (how long does the stimulus seem to last), order (which of two items comes first), and flow ("the common waking experience of one event smoothly transitioning into the next," p. 60). The authors begin by discussing duration and move on to temporal order. In regard to duration they briefly sketch certain methods for studying it and then some factors empirically demonstrated to influence it. This makes for interesting reading. They summarize several easily understandable reliable findings (p. 61) and conclude the topic with a very nice quotation from William James and their own brief overview. Also discussed are *temporal order experiments* that, for example, disclose how close in time two different-pitched tones must be before the start

of one no longer can be distinguished from the start of the other. Additionally described are *synchrony experiments*, which examine, for a given sensory modality, how far apart in time two stimuli have to be for one to experience them as separate rather than as synchronous. This is known as assessment of *temporal acuity*. They discuss some findings with such methodology. Finally, discussion turns briefly to the experience of *flow*, making it clear that very little is known about this common experience.

Nonconscious Processing of Events in Time very briefly touches on the remarkable way in which our sensorimotor system can respond almost instantly and very proficiently to emergency situations, including those which we have not previously encountered. Many of these response schemas are the result of prior practice, as in driving a vehicle, and executing them comes to require minimal attention and none that has to rehearse what should happen. The authors make the very important point that actually paying attention to some of these overlearned responses can impair their execution! Also discussed is the possibility that some persons learn unconsciously the various circumstances in the environment that together signal what will happen next, as in sports, at least on the part of some outstanding players. Not surprisingly, they at this juncture introduce the idea that nonconscious functioning can, *even in the absence of sensory cuing or the possibility of inference*, alert us to—and perhaps even help prepare us for—future events, perhaps, especially, circumstances that have emotional impact. In this regard they provide a basic description of the experimentation on *presentiment*, focusing on work by Dean Radin (and later by others) that involves measurement of physiological responses prior to presentation of randomly ordered visual stimuli that are either calming or emotion-arousing. The objective is to see whether the latter stimuli are associated *before their sensory presentation* with greater evidence of emotional arousal than in the case of calming stimuli. The authors report that the cumulative outcome of a meta-analysis of such studies (Mossbridge, Tressoldi, & Utts, 2012) is “robustly statistically significant” (p. 65). They appear to think that some persons’ rejection of the possibility of presentiment might, in part, be based on a failure to recognize or to learn that, anomalous response aside, “the nonconscious mind can do things the conscious mind cannot do” (p. 65) and that recent research has documented this on a number of fronts (and they cite related work). Theirs is a very quick, but nicely informative, introduction to such developments. They suggest that lack of familiarity with the data relevant to anomalous cognition also plays a role in rejection of such claims. I would note that unconscious processing of sensory information seems to have much in common with the organism’s management of psi information, given that both involve processing of very subtle, unconscious information (Stanford, 1990, 2015). Knowledge of this might help convince non-psi researchers of the lawfulness of psi events. Parapsychological theorization in the mid-’70s about the unconscious processing of psi information presaged much of the present-day discussion of nonconscious processing of sensory information (Stanford, 1974a, 2007) and of presentiment.

Finally in this section the authors open, albeit briefly, discussion of what they call “conscious precognition” (p. 65), as distinct from the other contexts of anomalous influence of future information. Their emphasis here is that both unconscious and conscious functioning must collaborate in order for success to occur.

Precognitive Dreaming begins, as these authors often do, by mentioning findings from outside the laboratory and then moving on to what are largely laboratory-based studies or meta-analyses of such data. They discuss a couple of surveys of potential precognitive dreaming that provide percentages

of respondents reporting such dreams and then move on to a paper by Nancy Sondow (1988) concerning her personal spontaneous dreams that seemed to be precognitive. She reported analyses indicating that her seemingly precognitive dreaming about an event was far more frequent for the next day's events than for later ones, with a systematic drop-off in frequency of such events as temporal distance increased between dream and corresponding circumstance(s).

Barušs and Mossbridge's treatment of Sondow's report should have noted, but did not, that their statement that "around 40% of the predicted events occur on the day following the dream" (p. 66) was based on the carefully recorded and annotated dreams of a single individual. This circumstance could raise the issue of whether personal beliefs about time and dreaming might have influenced the outcomes; personal orientation toward time itself might also be a factor; and results may not generalize across individuals. Readers of the authors' brief report on Sondow's work had shortly beforehand read some results of two surveys of psi in dreams, and they might on that account—with no warning of this shift of content—mistakenly have assumed that the Sondow paper reported survey results across studies, but that was not the case. The authors next mentioned two alternate explanations mentioned by Sondow for the temporal pattern that she had found: (a) failure to notice or identify a precognitive dream because a later potentially confirmatory event is not recognized due to deterioration of memory for the dream due to elapsed time; and (b) "some general mechanism underlying precognition" (p. 66).

Barušs and Mossbridge next claimed support for that general mechanism hypothesis when they said that a meta-analysis (Honorton & Ferrari, 1989) of all the forced-choice precognition studies in English-language sources from 1935-1987 "indicated a clear drop-off of the effect as the time between the participant's guess and the subsequent feedback increased from less than 1 second to minutes, days, months, and years" (p. 66). Unfortunately, their statement contains two errors in reporting of methodology; plus, they make a decidedly unwarranted causal interpretation of the significant (albeit weak) negative correlation between *temporal interval* and precognition performance.

First, two substantive errors appear in the statement quoted above:

(a) Very important, the temporal interval actually studied of special interest to Barušs and Mossbridge in regard to general mechanism was NOT "the time between the participant's guess and the subsequent feedback" (p. 66), but "The interval between the subject's response and target selection" (Honorton & Ferrari, 1989, p. 297, bottom). These are different matters, which is very clear from the cited, well-written, meta-analytic report.

(b) Less important, but "years" (plural) (p. 66) is factually wrong as a statement of the maximum temporal interval from the Honorton and Ferrari (1989) meta-analysis; the maximum interval in the analysis was 1 year as per these meta-analysts' report (p. 297). Nonetheless, this descriptive error is a large-scale one in terms of time.

Most important, the authors advanced a clearly unwarranted causal interpretation of the small, but statistically significant, negative correlation of temporal interval and precognition performance. Neither the outcomes of this meta-analysis nor the remarks of its authors support the idea that this correlation can legitimately be deemed to reflect a general mechanism for precognition in relation to

that time interval. Honorton and Ferrari, in fact, strongly suggest that one or more confounds might well explain the covariation of precognition performance and temporal intervals in this work. The correlation between temporal interval (between participant response and target creation) and precognition performance reached significance (i.e., longer intervals were associated with lower precognition performance), but the effect was small and accounted for just under 4% of the variance. There are very serious problems in interpreting this finding because such an analysis is intrinsically correlational in nature and thus *cannot in principle sustain casual inference on account of potential confounds in a cross-studies database such as this* (Eagly & Wood, 1994, pp. 491-493; Stanford, 2003). In regard to examining precognitive performance as a function of temporal interval, one must keep in mind that the studies in this database were not intended to examine the possible consequences for precognition performance of temporal interval, and the meta-analysis report mentioned no systematic effort across studies to ensure that participants were sensorially masked to the temporal interval. The length of that interval varied widely across studies, but it was not varied experimentally within them. *Consequently, any effort to examine whether temporal interval covaries with psi-task success was forced to do so by examining the temporal-interval variation across studies, each done with its specific temporal interval and with its own unique set of multiple other circumstances that might have affected psi-task performance.* Therefore, in examining across studies this potential moderation of precognition performance by temporal interval, one must recognize that one is simply doing a correlational analysis and that justifiable causal inference related to the potential moderator (temporal interval) is not possible. That is true of any meta-analysis in which one is studying a potential moderator as it varies across studies (rather than within controlled-manipulation experimental studies where the effort is to hold everything else constant).

Beyond that, in the database examined by Honorton and Ferrari (1989) there were two important—but, fortunately, known—situational variables that potentially were confounded with (i.e., covaried with) temporal interval because they varied freely and widely across studies. These were *preselection of participants* based on psi-task performance in prior work and *degree of feedback* to participants on psi-task performance. In this meta-analysis: (a) studies with preselection of individuals produced significantly greater effect sizes than studies without such preselection (pp. 294-295); and (b) degree-of-feedback evinced a significant positive relation with precognitive effect size (p. 297). *Indeed, Honorton and Ferrari (1989) state outright (p. 298) that degree of feedback was confounded with temporal interval.* That is psychologically a potentially very serious confound! Also, when they examined studies with preselected individuals and compared them with those with unselected ones, they found that the negative relation of temporal interval and precognition performance was due solely to groups with unselected participants. Studies with preselected individuals, on the other hand, showed no such effect but a positive, albeit nonsignificant, correlation between temporal interval and precognition effect size. For Honorton and Ferrari (1989) this set of findings related to the preselection variable “suggests that the origin of the decline over time may be motivational rather than the result of some intrinsic physical boundary condition” (p. 299), and they added that “The relationship between precognition ES (effect size; reviewer clarification) and feedback also supports this conjecture” (p. 299).

Considering that causal interpretation of outcomes based on cross-studies analyses is unsustainable due to possible confounds from cross-studies extraneous variables, and given, additionally, evidence of actual confounding of the temporal-interval finding by identifiable variables, *it should be no surprise*

that Honorton and Ferrari did not include the temporal-interval/precognition correlational finding in their *Summary and Conclusions*. Possibly for the same reasons, Baptista, Derakhshani, and Tressoldi (2015) in their meta-analytic chapter's review of the Honorton and Ferrari (1989) report, likewise did not mention the temporal interval/precognition correlation. It is unfortunate that Barušs and Mossbridge endorsed an unjustified causal interpretation of this temporal-interval finding in four places in their book (pp. 66, 68-69, 71, and 157), thereby potentially creating and then reinforcing, however unintentionally, the groundwork for a meta-analytic myth. The most justifiable conclusion in regard to this matter is that the cross-studies nature of the database in the Honorton and Ferrari (1989) meta-analysis made impossible a valid test of the assumption endorsed by Barušs and Mossbridge. Very different studies than those meta-analyzed by Honorton and Ferrari (1989) would be needed to address the temporal-interval/precognition issue and to support meta-analytic examination of the related cumulative outcomes.

Returning to possible precognitive dreaming, Barušs and Mossbridge provide brief, but clear, helpful statements of three cogent reasons why spontaneous cases do not provide a clear scientific case for actual precognitive dreaming. I suggest that another important consideration is that the degree and kinds of correspondence (or lack of it) between dream content and ostensible confirmatory events often are made uncertain by a failure of the dreamer to have recorded in detail and put somewhere on record the dream content shortly after awakening. The authors opine that controlled studies of precognitive dreaming (e.g., as in sleep-lab studies) may be more helpful than spontaneous cases in gaining insight into precognition in dreaming. I would add that we need not trade one research milieu for the other. Both have their special kinds scientific of value, and efforts of both kinds may be mutually enriching (Stanford, 1992).

Next the authors provide a very thoughtful, brief discussion of what they term "controlled precognitive dreaming studies" (p. 66). Six studies of that kind are mentioned (along with citations of all), three of which found significant evidence for precognitive dreaming and three did not. The authors ponder the sources of this divergence, suggesting that in one instance the failure might have been due to reduced statistical power, given the relatively small sample of participants and trials for each, as contrasted with another study by the same senior author that was significant. Barušs and Mossbridge make a potentially important point by noting that: (a) in all three of the significant precognition dreaming studies, the participants were later exposed only to the target pictures, not to the foils (because they were not asked to judge correspondence in this study; external judges were used); and (b) in the statistically nonsignificant studies the participants themselves did the judging and thus were exposed both to targets and foils. They suggest that in a precognition study with volunteers as judges any precognitive focus on the target might be counterbalanced by possible precognition of the foils, for they also are seen during judging. This hypothesis merits research to learn whether it can be replicated in experimental work. Another factor that might have played a role is that when volunteers judge their own transcript against target and foils, for some there might be an element of egocentric threat that could adversely affect the judging process.

Barušs and Mossbridge's deliberation about the success/failure of dreaming-precognition studies misses a very important point: Two of the three significant precognitive-dreaming studies had as sole subject Malcolm Bessent, a British sensitive. He had a personal history of apparent spontaneous precog-

nition and contributed, in addition to his two significant precognitive-dreaming studies, two significant force-choice precognition ones. He never had an unsuccessful precognition series! Thus, Bessent produced significant success in each of his four laboratory precognition studies, two involving precognitive dreaming (Krippner, Ullman & Honorton, 1971; Krippner, Honorton, & Ullman, 1972; both cited by Barušs and Mossbridge, but Bessent's name appeared nowhere in their textual discussion); and there were his two, significant forced-choice precognition series (Honorton, 1971; Honorton, 1987; neither cited by Barušs and Mossbridge). Honorton (1987) randomly intermixed, by computer, forced-choice precognition and real-time runs with both Bessent and the experimenter masked to temporal condition in each run. Bessent statistically succeeded with precognition, but not with real-time targets; his precognition performance was significantly superior to real-time (as also in a cross-studies contrast of his earlier work; p. 294). *His significant four-study dreaming/awake contributions to precognition research are a stellar episode in psi-research history—the more remarkable because each study differed in design.* Mentioning neither Bessent's role in two (of the three) significant dreaming-precognition studies nor his broader precognitive successes was a surprise, given the authors strong interest in precognition.

Implicit Precognition refers in this review to a specific exemplar of that category, namely a retroaction methodological paradigm that was the basis of 7 of the 9 implicit precognition experiments published in the *Journal of Personality and Social Psychology* (Bem, 2011) in a landmark paper that probably caused at least as much intellectual uproar as any paper ever published in that journal. In general terms, these 7 studies investigated potential retrocasual (i.e., time-reversed) influences on affect and on cognition. Bem's bold research centered on whether four well-documented effects in traditional psychology could be elicited in *reverse temporal order* by placing the circumstance whose effects were to be measured subsequent to the time of their measurement.

This carefully-designed work with largely significant results was presented and argued in a way that made it hard to ignore. Nor was Bem "behind the door" when the consequent flak was flying and after replication efforts had been forthcoming. Bem, Tressoldi, Rabeyron, and Duggan (2016) met those challenges head-on via meta-analysis, reporting striking significance overall for the hypothesis of retroaction.

In discussing the Bem et al. (2016) meta-analysis, Barušs and Mossbridge correctly noted that the retrocasual studies requiring fast-thinking by participants, as contrasted with those requiring slow-thinking (Bem et al., p. 8), fared far better in regard to significant replication. Barušs and Mossbridge mentioned three possible explanations for this pattern: (a) reduction of opportunities for psi-blocking or psi-information-alteration driven by rational thought and analysis, thanks to time pressure in the fast-thinking protocols, essentially the hypothesis favored, in somewhat different terms, by Bem (2011) and Bem et al. (2016); (b) use of "more emotionally charged stimuli" in the fast-thinking protocols, which might make them "more engaging" (p. 68); and (c) that "implicit precognition may follow a similar time course as that found for direct or conscious precognition (Honorton & Ferrari, 1989)" (p. 68). But the latter argument goes nowhere, relative to support from the cited source, because Honorton and Ferrari (1989), on very cogent grounds, did *not* claim that their meta-analytic data had supported the idea that precognition declines as a consequence of temporal distance. *Nor can a causal role for temporal interval or for any other possible moderator be reasonably inferred from the potentially multiply confounded*

cross-studies comparisons of decidedly different research designs in the studies meta-analyzed by Bem et al. (2016); that is, from studies contrasting fast- and slow-thinking paradigms, which presumably varied in far more ways than speed of response. Such correlational data can only suggest hypotheses that may be worthy of experimental follow-up. Hypothesis (a), that rational analysis can interfere with receptive psi, has in other contexts received substantial support, both experimental and correlational (reviewed by Stanford, 1975). Alternative hypothesis (b), use of more emotionally relevant stimuli, seems interesting and might merit experimental investigation. Barušs and Mossbridge's tendency to provide interpretive alternatives at numerous junctures in this book is a major strength of their discourse.

Precognitive Remote Viewing was the next topic discussed, and the major focus of that discussion is the intriguing possibility that specific types of target material are superior for precognition, at least in the remote-viewing situation. The authors report that Ed May proposed that in precognitive remote viewing targets are more accurately perceived when associated with high-energy change, and according to his research that effect can be had even if the energy-change scenario is not itself the target but is in the area (May, 2013, and May & Lantz, 2010). The crucial construct here has been called *thermodynamic* change. Also mentioned in this section is *informational* change within the target. Targets evincing greater informational change (e.g., high contrast plus shifts in shape and luminance) should, according to this hypothesis, be relatively successful ones (May & Spottiswoode, 2014). Barušs and Mossbridge state (p. 69) that this informational-change hypothesis related to precognitive remote viewing has been supported by research.

Barušs and Mossbridge's discussion of thermodynamic and informational changes and their possible enhancement of target efficacy is confined to precognition in remote viewing. I was left wondering whether any work has examined whether these presumed effects are present *only* in a precognition protocol or whether they also appear in other psi protocols. Study of such issues would seem essential before any statements that claim or imply that this is some special property of the precognition domain are credible. We may have at least a good start on the answer. Dynamic targets (i.e., video with sound, such as film excerpts), which reflect high informational change, as contrasted with static targets (i.e., still pictures), were found to produce significantly better performance in autoganzfeld GESP work, based on 10 studies using both target types in each session (Honorton et al., 1990), but there have been some failures to replicate this finding (see Milton & Wiseman, 1999). Although, "the jury may still be out on this," the results seem promising, and more work—especially more conceptually refined work—on this issue would be useful. I wonder whether the informational change proposal was based on the known characteristics of how the organism processes change-related sensory information; if not, then the hypothesis of psychological advantages of change might be an alternative explanation. Sensory change garners attention—that is how we are set up by evolution—and changes in a dynamic target may help the telepathic agent in GESP to focus on and respond to its information; and changes in incoming psi-mediated information may be especially likely to garner conscious attention and, hence, be remembered and help interpret the incoming information. This psychologicistic model implies that the effect should be present in perceptual-type receptive-psi tasks generally.

Potential Mechanisms of Nonconscious Processing of Events in Time portends, by its title, to promote possible understanding of nonconscious processing of events in time. It would be good to say

the discourse delivered on that, but that is not the case. There may be readers who will feel differently, but from the start the task of understanding the effects involved is not easy. The very first sentence of this section is a confession that whatever is behind nonconscious acquisition of information about future happenings is not understood. What follows in regard to the laboratory work is a statement that what might at first look like precognitive response to the later-generated affect-relevant target might, instead, be psychokinetic influence on target selection such that the target thus matches prior ongoing physiology. The authors state that a controlled laboratory study to differentiate advance perception (aka, precognition or presentience) from psychokinesis has not been developed.

Discussion shortly moves back into the authors' often-repeated assertions about how time as we ordinarily perceive it consciously in the waking state may differ radically from nonconscious time, with the latter remarks quickly moving into the realm of what they call *deep* time. They seem to be expressing the hope that useful ideas relating deep time and the experience of time during anomalous phenomena in altered states of consciousness may be developed. Nothing is said about what, if any, hypotheses might be forthcoming and how a study in this domain might be operationalized.

Lacking in this section are specific proposals of "potential mechanisms of nonconscious processing of events in time." Readers instead get some discussion of potential approaches to gaining "clues about what underlies apparent time" and "about a possible deep time and its relationship to apparent time" (p. 71). These goals seem worthy of pursuit, but some readers may feel disappointed, considering what was portended by this section's title. Positing something like deep time is not the same as positing a specific mechanism, although these authors' discussion of deep time suggests that relevant construct(s) might be more evident in certain altered states than in the waking one. Deep time earlier had been described as that which "structures the nature of consciousness and physical manifestation, and a possible relationship between the two" (p. 54). That tells what deep time is supposed to do—and bites off a very big bite—but provides no clue as to how it functions. A mechanism must entail something far more specific.

Life Reviews is the first of three sections that relate altered states to the experience of time. Life reviews are reported in near-death-experiences (NDEs) by persons who came close to death but survived with memory of having experienced a review of personal life events; these sometimes also occur with persons who simply believe they are close to death. Such reviews are said to be reported relatively commonly by those in which there was no prior expectation of death, often due to an accident, and less frequently when the NDE was occasioned by illness. This brief (3.5 pages) treatment nonetheless provides an interesting and very informative overview of the substantial variety of such experiences and it thoughtfully raises important questions bearing on their nature. I will not summarize this material except to note that the emphasis was heavily and almost exclusively on the phenomenology of such experiences, including that NDEs sometimes are said by the experiencers to have included future events. They note the need for careful checks to ascertain whether supposed future events actually can be (or had been) verified. Also important, they note, is checking, when possible, on the validity of reported NDE memories. They do not assume that what is reported as memory in an NDE necessarily is veridical memory (i.e., is knowledge of the objective past), and they note the need for careful study to evaluate veridicality. They emphasize the sense of reality of the experience for the experiencer and that experiencers sometimes feel strongly that time is either nonexistent or is not as commonly experienced.

Altered Temporality describes altered states in which the experience of time is radically changed. Discussion begins with the imbibed psychedelic intoxicant known as *ayahuasca*. The authors cite work by Benny Shanon that involved interviews with many people to learn about their experiences during a large number of ayahuasca sessions. Time-related experiences with the drug are the focus of Barušs and Mossbridge's discussion, and they note that Shanon (2001, 2002) found that "every element of temporality can be altered" (p. 75). They report that during intoxication supposed historical events sometimes seem to be observed from within their historical time and sometimes from the observer's present-time perspective. They note that a present-time perspective on a historical event also can occur in non-intoxicated, wide-awake individuals.

The authors next devote over a full page to some very dramatic accounts by Phyllis Atwater (2011, 2013) related to what Atwater (and the authors) call *future memory*. I will not detail or even characterize these experiences, except to say that they are reported as wholly real and lifelike, as emerging suddenly and dramatically in otherwise everyday life, and as allegedly showing up in later life experience. Barušs and Mossbridge discuss these reported experiences, seemingly taking at face value the paranormality of the claims. I have no idea what kind(s) of documentation, if any, Atwater put into her published accounts, but I found bothersome the failure of the authors to comment on the availability in her book of the specific kinds of information needed to arrive at a reasonable judgment about the credibility of these dramatic claims as genuinely anomalous. This lapse was surprising because they generally do point out the ambiguities in interpreting such accounts and often point out information not supplied that is needed for reasonable judgment.

If accounts of putative future memory are not enough to keep one's boggle button buzzing for some time, then presentation of what looks on the surface like it might represent the stoppage of time for some minutes might be a help in keeping it duly exercised. The account given by the authors is based on a book by Cynthia Larson (2012). I will not describe here this temporally concatenated scenario but am happy to report that Barušs and Mossbridge this time do provide some alternative explanations that seem credible and not nearly as farfetched as the stoppage of time.

The authors seem deeply interested in any evidence that appears to evince altered temporality, suggesting that, in some contexts or manifestations, time is not the fixed, inevitably flowing thing that one experiences in everyday life, but is far more pliable, contractible or expandable, perhaps with objectively verifiable ramifications. In that regard they next ponder the manifestation, via "channeling" through Pearl Curran, of a personality—they say, "an entity" (p. 77)—named Patience Worth, a manifestation evident for almost 25 years that eventuated in over 4,000 pages of material. This personality/entity claimed to be the spirit of an English woman from the 17th century. A major reason for the authors' excitement about the Patience Worth phenomenon is that investigators sometimes requested from her sensorially unexpected very demanding types of verbal material and after a reportedly very short delay the requested production began and proceeded at an astonishingly rapid pace—as fast as the material could be transcribed. It was also claimed that the quality of the productions was very high. Barušs and Mossbridge speculate about three possibly distinct ways of thinking about these seemingly time-challenging productions.

The Experience of Timelessness consists largely of a series of quotations from three persons who experienced something like timelessness. They perhaps will help readers to gain some hint of what such

a state might be like. The concluding comment in this section essentially says that when one considers the time-related implications of the studies on anomalous foreknowledge, conscious or unconscious, along with the experiences of time as reported by mystics, drug users, and some others, we need to reconsider the nature of time, for it starts to look like what one sees in “the redacted time of physics” (p. 80). But whose redaction, espousing what specific view?

Rethinking Time allows Barušs and Mossbridge to make some fundamental and important points related to deep time, nonconscious processes, waking state, altered states (including timelessness), subjective apparent time, and anomalous functions related to time. This discussion is at the heart of their expanded view of consciousness, which they have worked hard to explain. It merits serious, thoughtful reading more than once. Does this rethinking section provide some sense of cognitive closure? I think so, although I do not see it at this point as likely to engender conceptual advances in the domain of empirical psi research. For that, further development is needed. This discourse may, though, help the reader gain a sense of possible relations between some very interesting and important sets of observations from what at first may seem very different domains.

Chapter 4 (Interactions with Discarnate Beings) begins in such affirmative style in its very title that some readers may wonder if its authors will be able to live up to their pledge that “. . . we will move ahead to logically examine the empirical evidence as dispassionately as possible” (p. 84). The title may make some wonder if, for the authors, spirit survival is a foregone conclusion.

Spontaneous Contact with the Dead does not let off on the strongly affirmative language, but the discourse provides evidence on the frequency with which survey respondents—several surveys—report what they deem or suspect were encounters with a deceased person. This information is interesting and may be surprising to some, but it seems unclear to me what proportion of the cases provided here truly should be regarded as spontaneous. After a loved one has passed away, surely it is not uncommon for a closely attached survivor to wish for, expect, and/or pray for some clear contact with the deceased. If so, a subsequent episode of perceived or actual contact with the deceased may be something less than spontaneous and might even have been fostered, if not created, by this emotional and cognitive push toward such contact. Did the surveys actually provide the kinds of question(s) that would tap into the spontaneity issue in this sense? Did the surveys ask about any surprise element in the incident and how strong was that sense of surprise? We cannot know from the frequency information provided, so it seems unclear what proportion of affirmative responses regarding such experience(s) were something like truly spontaneous. *Spontaneity is conceptually interesting in part because a case being arguably spontaneous might convey to the mind of the experiencer (and to others) a stronger a priori sense of the event's possibly having been initiated by the deceased individual referenced by the event.* Certain cautions are needed in interpreting surveys because the reported percentage of cases showing a given feature may not be typical of the population intended to be surveyed. One can only survey volunteers, who may differ in a variety of ways from non-responders, and even volunteers may not always be fully frank on what they experienced (or did not experience). Therefore, the specific numbers reported may be less important than that they signal that there are plenty of cases out there that merit attention.

Discussion next turns to the deeply interesting and diverse phenomenology of such reports, which upon occasion have included reports of possibly anomalous physical happenings. Of special interest

may be a listing of six features of such cases, based on Erlendur Haraldsson's collection (2012) of over 400 case accounts from Iceland that, if one takes at face value and views their features as genuine, convey the impression of paranormality and often in ways seemingly compatible with the survival hypothesis. Of course, compatibility with a hypothesis is not, logically speaking, proof of its validity. Not having read Haraldsson's cited book, I make no judgment of the cases' credibility.

Deliberate Contact with the Dead provides a different perspective on cases of potential contact, the possibility that these experience(s) might have a constructive, therapeutic role to play for grieving individuals, at least in part due to a continued sense of a continuing bond with the deceased. The authors seem to endorse such a view and they would seem—at this juncture at least—to have no concerns about the idea of a therapist actively trying to induce such experiences as a part of grief therapy. They refer to that kind of therapeutic approach as based on a “continuing bonds model” (p. 89), and very affirmatively extend their discussion to the effort of therapists to strive deliberately to induce such experiences in their patients/clients for therapeutic purposes.

Let us suppose that some patients/clients do become convinced through therapy that they are in contact with deceased individual(s). Depending, in part, on what the “spirit” says and/or does, there might be salutary effects for the believing recipient or effects that are distinctively adverse. What is more, just encouraging the belief that the patient's/client's life is being contacted from the spirit world might be sufficient to drive some unstable individuals into a serious episode of mental disturbance (with its social, medical, and, conceivably, legal consequences). Some individuals might subsequently assume that they are being (or might be) “invaded” by other spirits, given that they have been led by a professional to believe that that have sensitivity to spirits. It is good that, in regard to therapy, Barušs and Mossbridge acknowledge in this section and elsewhere that case improvement can come for reasons unrelated to the ideas championed by the therapist.

Mediumship begins with a definition of the term *medium*, somewhat broadly defined in that the proffered definition extends beyond the domain of communication with the spirits of deceased individuals, but certainly not beyond the claimed transactional realms of some mediums.

The authors then describe a tape-recorded interaction in which the sitter, Barušs, told the medium “that a recently deceased friend with whom he had played ice hockey had been on his mind” (p. 91). Despite the authors' statement that “the medium was ostensibly communicating correct information that she could not reasonably have known through ordinary sensory processes,” p. 91), this report seems less than convincingly evidential because of: (a) a range of verbal communication with the medium, both spontaneous from Barušs and in response to medium queries; (b) ample opportunity for nonverbal information; (c) information-shaped guessing, and (d) inability to establish chance baselines for relevant frequencies.

The authors next assert that it is “easy to design appropriate experiments” (p. 91) to learn whether a medium(s) can acquire correct anomalous information. The described proposal has numerous good features. I would add that in work focused on a special subject, such as a psychic or a medium, some planning may be needed relative to the legitimate needs or preferences of the individual to be studied.

The authors cite several reports that found significant evidence of discrimination by sitters of medium-uttered transcripts intended for them and those intended for controls. According to Barušs and Mossbridge's brief remarks on the significant studies, it would appear that sitters blindly "chose" (p. 92) which transcript was for them after reading transcripts intended both for them and for controls. This forced-choice task (p. 92) was indicated for both of the studies (Beischel, Boccuzzi, Biuso, & Rock, 2015; Beischel & Schwartz, 2007) for which accuracy percentages were provided by Barušs and Mossbridge. The forced-choice methodology might have turned the task into an implicit ESP task for the sitter. Having to select one of the two transcripts as one's own, perhaps especially if the differences seems minimal (or nonexistent), could invite unconscious or conscious extrasensory intervention by the sitter. This makes uncertain any claim that the forced-choice result assuredly was due to the mediumistic material. Fortunately, two non-forced-choice evaluative methodologies also statistically supported anomalous reception and thus might arguably be less likely to involve sitters' psi-mediated decision-making (two studies cited above, but as discussed by Beischel & Zingrone, 2015, p. 306). Barušs and Mossbridge did not mention this.

A sensory threat to the claim of anomalous information in those two studies also was not mentioned by Barušs and Mossbridge: The first name of each target discarnate was provided to the medium (Beischel & Zingrone, 2015, p. 306). This provides potentially effective sensory information, unconsciously and/or consciously, in moving the medium's production in the correct direction (or away from error). There seems no realistic way to assess this sensory threat's magnitude, and on that account this remains a bothersome methodological issue vis-à-vis construct validity.

Super-Psi versus Survival considers the problem of what would be needed to allow differentiation of anomalous productions as to whether they derive from living persons (even in a séance circumstance) or from a surviving spirit of a deceased person (usually, via a medium). Much of the discussion here consists of evidence from two rather elaborate mediumistic cases (pp. 92-93) in which mediumistic communications allegedly might have produced, among other material, evidence of skills, expressed through mediumship, that might better fit the assumption of survival (and communication) of a deceased, identified individual with such skills than of living-source psi being involved and/or possibilities of fraud. One case ostensibly pitted, via mediumship, the chess skill of the alleged spirit of deceased Hungarian chess grand-master Géza Marczy against a living individual, Victor Korchnoi, ranked third in the chess world. The game was truly leisurely, 92 months before "the ostensible Marczy resigned at the 48th move" (p. 93). I have a serious methodological concern about this case: The 79 of 81 biographical questions about the embodied life of Marczy successfully answered by the ostensible spirit Marczy cannot, in my view, reasonably be taken as decisive evidence of anomalous communication. This is because: (a) the medium knew early on who would be the ostensible competing deceased chessmaster and (b) conceivably might have chosen the ostensible spirit chessmaster because there was an opportunity to proclaim—based on a proffered list of 15 deceased grandmaster candidates supplied to the medium—one of them as willing to play the game. These circumstances make it difficult—given the methodological information I have at hand from the reviewed book—to rule out advance study potentially relevant to answering biographical questions about Marczy. This circumstance does not prove or even suggest that fraud occurred. It simply means that the interpretation of the startling mediumistic success with the biographical questions may be deemed equivocal, if my understanding of the methodology is

accurate. Barušs and Mossbridge did not mention this potential threat to the construct validity of the effort to assess anomalous biographical knowledge. The other mediumistic case (“*Scole experiment*,” p. 93) thought to suggest the talent and style of an alleged spirit-of-deceased convincingly resembling that of a known formerly living individual seems to me decidedly weak in a very different way. To my mind, nothing was reported by Barušs and Mossbridge showing that the allegedly paranormally written German poetry on photo film (of potentially dubious validity due to control issues; p. 93) was decidedly in the style of the deceased poet, R ckert, who some thought it might be, based on subjective stylistic judgments. That name apparently had not even been provided through the medium. The potential R ckert identity apparently was simply a hypothesis based on subjective impressions of similar style between the photo-script words and published material by R ckert. Barušs and Mossbridge were left asking, “Did R ckert create a poem after his death to show that he is still around?” (p. 93). My comment: Oddly, if so, for the ostensible communicator apparently never claimed to be the deceased R ckert or that R ckert was the author of the photo-script poetry.

Also discussed by the authors is the potential value of *drop-in communicators* in reducing the likelihood that the psi effects observed are related to needs of the medium rather than of a spiritually surviving entity, and suggests that the deceased sometimes appears to take over the medium’s body, thereby expressing much more fully through nonverbal behavior the demeanor of the deceased during physical embodiment. The authors consider such skill or style-related behavior as potentially strong evidence of the deceased still living and as decisively threatening the super-psi hypothesis. But how can behavioral-style commonalities be scientifically evaluated relative to examining the anomalous-information issue? Statistical significance of the correspondence can inversely depend on how common in the population are the characteristics of the target individual. How many other persons’ style might they match? Also, how can behavioral style be meaningfully conceptualized and measured in the first place, given its complexity and that it may be holistically perceived?

The authors still hold out the hope of finding empirical bases to address the super-psi/survival issue in mediumship and conclude this section by noting four kinds of evidence deemed most relevant to that issue. On the other hand, Beischel and Zingrone (2015) opine that this perennial information-source issue presently cannot be empirically resolved and suggest greater utility in investigating various other unresolved issues related to mediumship. Coming from Beischel, a leading figure in contemporary mediumistic research, this is an admirably forthright acknowledgment of the difficulty of trying to refute the super-psi hypothesis. The proposals of Barušs and Mossbridge would have been more interesting and credible had they been explicit about rigorous methodologies that might be used to implement their style- and skill-related proposals for vanquishing the super-psi hypothesis. It is one thing to say what needs to be done and another to delineate plausible, defensible ways of doing it.

Unwanted Intrusions is exceptionally long (about 4.5 pages). The authors ask, as a rhetorical question, whether such spirits may be capable of influencing us, and, enhancing the drama, also ask whether some of them might be something other than ex-humans, “disembodied entities of varying intelligence, character, and morality” (p. 97). Might there be enough in this particular section and later discussion to disturb the already mentally disturbed, if they should read it and take it seriously? If so, might it be ill-advised for mental health professionals to leave a book such as this lying around in the waiting room?

This section begins with the often scary phenomenon of *sleep paralysis*. This occurs as someone is falling asleep or at the start of awakening. It involves flaccid muscle paralysis, mediated by the same neural mechanisms that produce such paralysis during rapid eye movement (REM) sleep (which generally involves dreaming). Sleep paralysis can be very scary, in part because one is relatively conscious, thinks of oneself as such, but is unable to move when one tries. Given that one does not know what is happening, the experience may, in initial episodes, be unfamiliar and highly anxiety producing. Those experiencing such things may desperately seek understanding of them. Cultural beliefs, one's prior beliefs and belief systems, and even recent well-publicized spooky stories, including abductions by space visitors, may therefore play a role in the individual's interpretation of and possibly in the phenomenology of sleep-paralysis-related experiences. Some may even believe they have lived an objective episode that might instead be a self-generated fantasy during sleep paralysis. Such matters are broached in recent book chapters (Appelle, Lynn, Newman, & Malaktaris, 2014, pp. 226-228; Watt & Tierney, 2014, p. 248). Barušs and Mossbridge ask, in a seemingly serious tone (p. 98), whether some episodes of sleep paralysis might represent opportunities for unwanted intrusion by actual spirits of some kind(s), as some who experience these things come to believe, and whether at least some of the hallucinatory entities, as traditionally construed in psychiatric medicine, might really be intrusions of spirit entities. They opine that psychological health may be necessary to ward off these "apparent intrusions" (p. 98).

In search of more evidence that may relate to unwanted intrusions from the spirit world, the authors next ponder *dissociative identity disorder* (DID), sometimes colloquially known as multiple personality, and they ponder the possibility that, at least sometimes, it might be due to spirit possession. This discussion focuses heavily on the experiences and ideas of a psychiatrist, Ralph Allison (1980). Barušs and Mossbridge discuss in some detail a complex DID case from Allison's book. Allison felt he could find nothing in psychology to explain this set of circumstances. I wonder if he looked far enough. Anyhow, the authors would have provided some conceptual balance in their discourse had they cited and discussed an important review by Spanos (1994), which provides an in-depth sociocognitive analysis of multiple personality enactment and multiple personality disorder based on findings from experimental, cross-cultural, clinical, and historical sources.

Barušs and Mossbridge note that they have examined several types of seeming interactions with "invisible beings" (p. 100) and have found some evidence of consistency across those types. They do not discuss the circumstances of such observations, the nature(s) of the consistency observed, or whether they have any thoughts on what might underlie particular forms of consistency. The authors acknowledge that these consistencies may or may not have anything to do with the spirit world (p. 100).

They provide a somewhat creepy cautionary note near chapter's end: "Contrary to the claims of some grief therapists that interactions with the deceased are always benign, that is not true of interactions with apparent discarnate entities in general" (pp. 100-101). They apparently believe that going through grief therapy with claimed spirits of deceased persons can be decidedly risky because of the spirit-related possibilities just mentioned. They stress the need for research intended to promote an understanding of both: (a) mental mechanisms that allow for interactions with discarnate entities (of whatever kind or intention); and (b) mental mechanisms that protect from unwanted intrusions. No thoughts are provided on what those mechanisms of either type might be or how they might be researched. They

also note that whatever interpretations one may wish to put on such experiences, “There are already lots of cases in which people have ended up in serious trouble by stumbling into various practices for which they were not prepared” (p. 101; and they cite Allison, 1980; and Barušs, 1996). This assertion is troubling and disturbingly vague. What kind(s) of serious trouble? What kinds of practices are meant? Puzzling discourse! Barušs and Mossbridge also say that the aforementioned research is needed “before people subject themselves en masse to an onslaught of apparent discarnate entities” (p. 101). The types of research advocated seem conceptual and long-range, given so many unknowns. Have there been efforts to assess potential risks in and the efficacy of the kinds of practices mentioned, and whether certain type(s) of individuals are more at risk? Have there been professional-scholarly publications discussing these practices (e.g., in clinical journals)? Are those providing such alleged therapy licensed as psychologists? Some answers to such questions, even if negative, would have been welcome.

The chapter’s end turns to *deathbed visions*, which involve visions of deceased persons known to the dying individual. Conversations of the dying with envisioned deceased individual(s) may occur, even as the dying converses also with those physically in the room. The authors briefly characterize the phenomenology of such episodes, relying on two books (Fenwick & Fenwick, 2013; Osis & Haraldsson, 1997). They also mention three reassuring, heartening themes that Fenwick and Fenwick said they had found repeatedly from persons who had been present at such deathbed episodes. They then note that the materials reviewed in the chapter suggest post-mortem survival of memories and personal experiences and lead to the possibility that physical existence “may not ever be necessary for certain types of consciousness” (p. 101).

Chapter 5 (Separation of Mind from Brain) considers several sets of circumstances assumed to differ in the degree to which brain activity can support particular kinds of mental function.

Mind in a Compromised Brain begins with a discussion of *terminal lucidity* (i.e., lucidity at impending death), in which memory and clarity of mind return shortly before death. The discussion lists a series of medical circumstances deemed to work against these cognitive functions and that presumably obstruct their return. Several interesting cases of terminal lucidity are described, and they raise questions of what made them possible, which may differ in different cases. The frequency of such lucidity suggests to me that unknown neural/physical processes, starting up or shutting down in the process of dying may temporarily unblock the situations that had been impairing mental functioning. There also may be consciously or unconsciously discoverable alternate ways to activate an impaired function. Those possibilities are speculative. We may not know enough to rule out any particular possibility for explaining these transient effects, including the one seemingly favored by the authors.

Discussion next turns to the claim that “enhanced mentation is *characteristic of NDEs*” (p. 105, reviewer emphasis). This seems, though, a misstatement if the figures given thereafter are correct, *because less than half of the NDEers deemed their thinking unusually clear, and less than one-third deemed their reasoning unusually logical* (E. W. Kelly, Greyson, & Kelly, E. G., 2010, p. 386).

Barušs and Mossbridge report that studies in this domain have shown that memory reports based on NDEs evince “significantly more detail and emotionality” (P. 105) than those related to imagined emotional events (Palmieri et al., 2014; Thonnard et al., 2013). They also report that NDE memories do very well in terms of details, relative to emotional events recalled from life experience. I wonder whether

this comparison might be contaminated by the NDE being recalled sooner after its occurrence than in the case of emotional events from life experience.

Next is discussion of “visual elements of NDEs of the blind” (p. 105), based largely, if not entirely, on a report (Ring & Cooper, 1997) on NDEs in what are termed “blind” individuals. This discussion seems somewhat confusing and unclear, possibly on account of the unavailability of clear information on what blind persons are experiencing when they talk of things in their NDEs or even in their dreams. I suggest caution in making broad generalizations about experiences of “the blind,” given that people with a variety of different vision-related neurological deficits—and who show differently on various objective performance tasks, including blindsight tasks—may all report no vision (at least in certain parts of the retina, sometimes throughout it). An overview of blindsight, including neurological issues, is provided by Gazzaniga, Ivry, and Mangun (2002); it may have relevance to the topic at hand.

Explaining Near-Death Experiences begins with, but carries only briefly on, psychological explanations, quickly dispensing with them in a very few lines intended to counter the claim that all NDEs may be defensive reactions to fear of death.

By contrast, multiple pages are devoted to various physiological efforts to explain experience during NDEs. The discussion is well written, ponders a variety of interesting empirical data, and merits careful and thoughtful reading. The authors conclude that, contrary to the decrement in detailed, conscious experience one might expect in the face of the various physiological impairments associated with the approach of death, one gets remarkable, detailed reports of NDE experiences, including in cardiac-arrest cases. The latter might be expected to impair brain function due to lack of oxygen. The authors suggest that such impairment may allow consciousness to be free of encumbrance by the brain and to function in its own way, including, possibly, opening one up to experiences and information (including psi-mediated) to which it might not otherwise have access.

The authors discuss the self-reports from NDEers of large-scale, usually very salutary, changes in personal beliefs, orientation on life, understanding others, caring about others, and other claims. These claimed improvements would, they note, be easier to evaluate if ancillary information were available from others who had known the individual for considerable time prior to the NDE. Barušs and Mossbridge discuss in a very serious way some important construct validity issues. They suggest the value of proactive studies where patients planning to undergo cardiac surgery entailing deliberate heart stoppage would contribute psychological data beforehand and after surgery, allowing assessment of change. That sounds good, but it involves inputs only from the individual whose psychosocial well-being one needs to evaluate. Such self-assessment would potentially have substantial value, but the self-reports might be influenced, especially in the post-NDE phase, by impression-management issues and/or by a felt need to give answers confirming perceived investigator hopes and expectations. Careful structuring and prefacing of the enquiries might help to obviate such concerns.

It is important to keep in mind that even with a very positive, uplifting initial NDE there sometimes are seriously untoward subsequent developments for the experiencer on account of a range of potentially problematic psychosocial adjustments that may be needed following a deeply mind- and perspective-changing NDE (Greyson, 2014).

Mind in a Silent Brain describes anecdotal reports alleging that persons experiencing NDEs later were said to have recalled seeing, during cardiac arrest, details, some allegedly quite unexpected, of what was transpiring in the operating room. Barušs and Mossbridge deem it reasonable to conclude that such cases involved what most of us call ESP. The adduced evidence, though, does not seem persuasive that such cases have provided the level of quality assurance (including systematic gathering, recording, and protecting of evidence—not to mention statistical probability issues) needed to firm up the evidentiality of such important cases. There also are questions about the possibilities of those reporting such details somehow having sensorially gained, however unintentionally and however unconsciously or consciously, some relevant information about the surgery/resuscitation scenario at some point after resuscitation but before having to report their NDE.

The authors concede that such reports do not necessarily mean that there actually was a separation of mind from brain at the time during which some details of the medical scenario apparently were acquired by anomalous means. They mention the alternatives of the information having been acquired ahead of time (precognitively) or afterward (retrocognitively) but dismiss those two possibilities, seemingly on grounds that time-displaced receptive psi would be very improbable if the NDE experiencer had no prior history of precognition or postcognition (p. 114). This improbability assertion seems conveniently supposititious. It rests on two very questionable implicit premises: First, how can one possibly know that these individuals *never* had such experiences in other contexts? Were they even queried? Such experiences might, anyhow, have gone unrecognized or even forgotten, perhaps as being coincidences. Besides, time-displaced receptive psi (like present-time psi) can occur without conscious effort and without conscious recognition of that (e.g., Bem et al. 2016, meta-analysis showing adaptive, unconscious retroactive psi; Mossbridge et al., 2012, meta-analysis of presentiment cases), and can affect present-time memories (Stanford, 1970, 1990, 2007). Second, even if an NDEer actually had no known such prior experiences, time-displaced psi might be driven into deployment by a disposition to gain information about the scenario involved with personal life or death.

The authors also discuss the several subsequent unsuccessful prospective studies intended deliberately to look for evidence of anomalous reception during out-of-body experiences in NDEs in medical settings. Unusual targets were placed somewhere in the room. Resuscitated patients reporting NDEs recounted their experiences, and it was hoped that some might include the target. Only in the latest of these studies, that of Sartori (2008), were conditions right to provide a test of anomalous reception during out-of-body experiences during NDEs with targets somewhere in the room. In Sartori's work there were eight such NDE/OBE experiences, but with none was there successful anomalous target retrieval. Pondering these failures, Greyson (2014), like Barušs and Mossbridge later, noted that a potentially major problem in that prospective work might have been the use of target material lacking interest for those undergoing an NDE/OBE.

Mind without a Brain begins with mention of the possibility of using a medium to allow the spirit of a deceased individual to provide information on the presumably nonmaterial world of its present existence. The authors point out (p. 116) that there is no way to verify that information. The next-mentioned approach to this topic is *instrumental transcommunication* (ITC), which involves trying to get communications from discarnate entities through electronic means. The work on this comes largely from

nonacademic settings and is reported in nonacademic media, making it difficult to evaluate meaningfully. Those wishing more information on ITC and on the state of related research might, I suggest, find useful a recent book chapter (Leary & Butler, 2015).

The final section topic is so-called *prebirth communication*. If, as the authors seem inclined to believe, we are spirit and can survive death, then the question arises as to what our status was before we were born. The first scenario discussed is that of not yet born (or perhaps even not yet physically conceived) beings supposedly communicating with an embodied person through dreams, feelings of a presence, or a vision of some kind. If true, one might wonder toward what end these seeming communication efforts are intended, but Barušs and Mossbridge seem preoccupied with what such children-to-be might tell us about their situation in the spirit world. The reader may be left wondering why these communications should occur, in the service of what purpose(s). One possible reason derives from the work of Ian Stevenson on supposed reincarnation cases whose reports occasionally include an *announcing dream*. Such a dream (Stevenson, 1975, pp. 67-68) is one by a pregnant woman (or someone close to her) in which a deceased person appears to communicate *the intention to be reborn through the pregnant woman*. Such cases occasionally appear in supposed reincarnation reports from various cultures.

Their next suggestion for tapping into the mind of the unborn is to use hypnosis or guided imagery to regress persons to the period before they were born. Frankly, this approach seems scientifically vacuous and potentially risky for some participants. The procedure, anyhow, places individuals under strong implicit social pressure to produce reports of prebirth events, but they presumably have no idea what these might be. The pressure comes from their having come for the study, from strong demand characteristics for reporting something, from the social prestige of a scientific investigator, and, very saliently, from the researcher having invested time to prepare them for such a production (via hypnosis or whatever). With highly hypnotizable/fantasy-prone individuals the output might be unbridled fantasy—possibly experienced as vivid and real by them, however potentially disturbing that might be—but might be conscious fiction with some non-hypnotizable (and some hypnotizable) individuals. In experimental work on hypnosis-induced past-life “regression,” Spanos, Menary, Gabora, DuBreuil, and Dewhirst (1991) found that hypnotizability predicted the subjective intensity of the reported past-life experiences, but that the subject-reported credibility of the experiences as being past-life in nature depended on such factors as prior belief in reincarnation and whether the hypnotist defined such experiences as imagination or as past-life. In responses to queries from the hypnotist about things that seemingly should have been generally known at that regression-claimed historical place and time, responses frequently were wrong and sometimes egregiously so (Spanos et al, 1991). It would be surprising if similar influences were not present in requested regression to a pre-birth scenario, *but of course, there is no way to ascertain veridicality of reported pre-birth circumstances (unlike, potentially, with an embodied past life at a historical place and time)*. Also, requests to regress to a pre-birth time are less structured because no prior embodiment is invoked, no constraints are placed on what might emerge, and the uncertainty might open the door for anxiety. Therefore, this requested experience might pose particularly serious psychological risks for individuals already psychologically vulnerable and perhaps do so even for some not already disturbed. Also, given that this is a regression procedure to the pre-birth time, one might even wonder about reactivating, along the way, mental traces (if they exist) of possible trauma during gestation, birth, or shortly thereafter (e.g., circumcision)—or fantasies of such circumstances. Subjecting

persons to a regression-to-prebirth scenario may put some of them at serious psychological risk but with no clear scientific justification (due to no verifiability of things reported).

Next for discussion is the possibility of learning something about the supposed world of spirits of the deceased on the basis of what they can and cannot do, in regard to memories and skills, in communication through a medium. They rehash Chapter 4 discussion of the Géza Marczy chess-playing case (pp. 93-94) and throw in some additional details that appear to relate to memory of Marczy in his supposed incarnation. They make some generalizations from that case about the possible proficiencies (or lack of them) in the spirits of the deceased but note the importance of getting convergent information through mediums from other deceased individuals before supposing one has at hand a valid generalization. Again, convergence supporting generalizability is one thing, justifiable interpretation of its cause(s) is quite another.

Mind in the Afterlife ponders what life after death might be like and is in considerable degree based, as the authors note, on the ideas of David Fontana (2005). Four major topics are discussed by Barušs and Mossbridge: (a) retention by deceased's spirit of its embodied individuality, reflected in dispositions, affective character, habits of thought, and more; (b) mental pliability of the afterlife environment; (c) experience of time in the afterlife; and (d) deceased's attachment to experiences craved in embodiment and possible enjoyment of them in the afterlife through still-embodied persons.

All of these are discussed, making for interesting reading, perhaps especially for those who have never previously read sources (generally, nonscientific) discussing such ideas. The thoughtful discussion in this section is stimulating, the more so because it ponders alternative views on a variety of these intriguing topics. Barušs and Mossbridge might agree with me that this speculation should not be regarded as necessarily true, that it should be acknowledged that part or all of it may be wrong. Also, if there is an afterlife, its nature might differ from person to person (and/or culture to culture).

The authors throughout the book ignore a major contemporary topic of the scientific literature on possible survival, the substantial research and discussion of evidence that may be deemed suggestive of reincarnation. This is a major deficit from the scientific perspective and in regard to balanced coverage. Readers do not learn of the very extensive serious, scholarly research on the possibility that persons presently embodied can show the influence of prior incarnation in several ways (e.g., memories of people, places, and circumstances, plus acquired dispositions including attractions, aversions, or phobias). Most of the high-quality work in this domain was for decades that of Ian Stevenson (1918-2007), an eminent psychiatrist and psychical researcher. Updates on this line of work, its methodology, its findings, the criticisms, and related conceptual issues may be had from two book chapters by Antonia Mills and Jim B. Tucker (2014, 2015), who have continued the tradition of careful study of possible past-life experiences that characterized Stevenson's investigations. This research arguably has potential relevance to the interests of psychologists and psychiatrists because of its possible relevance to understanding presently living individuals (Stevenson, 1977).

Chapter 6 (Direct Mental Influence) discusses evidence claimed to show that the mind can directly influence the physical world (i.e., psychokinesis, PK). It covers a broad range of systems on which such effects have been claimed to have occurred. Some reports are substantially better evidenced than

others. Some of the evidence is from controlled laboratory conditions with some evidence of replicated success, but the quality of the evidence offered for some of the other reported observations leaves very much to be desired, given the claimed large-scale effects and less than adequately controlled settings of some of the observations.

Random Event Generators reports on individuals trying to use psychokinesis (PK) to influence the outputs of random event generators (REGs). The REGs are of two types, one based on the timing of radioactive decay and the other on the electrical outputs of a noise diode, which should, like radioactive decay, depend on quantum-mechanical processes and therefore should provide truly random outcomes. The randomness of REG outputs is assessed from non-PK-effort trials. Although some consistency has been evident in some phases of the work, the overall impression created by their review is, to my mind, that statistically successful work has not been easy to replicate. Much of what is discussed are ad hoc explanations for replication failures. In general, the level of replicability in this work seems notably lower than in receptive-psi ganzfeld and autoganzfeld work. In at least some of the PK work, according to Barušs and Mossbridge, a relative handful of participants contributed most of the statistically significant PK evidence.

Much of the discussion near the end of this section seems to me unbridled speculation seemingly capable of explaining any failure of replication. The worst-case scenario is, to my mind, the “entire-world explanation” (p. 131), which I term “unbridled” because it knows no limits of possibility; it posits no boundary conditions and seems untestable in principle. By extension, one wonders how, if the world is as malleable by mind as this proposal suggests, one can explain the remarkable advances in other sciences where strong differences of opinion often have existed among scientists in a given discipline. There are ways to obviate or reduce some possibilities for psi-mediated experimenter influence, and those in psi research and beyond should use them (Stanford, 1981).

Two-Slit Experiment reprises and expands discussion of a quantum-physics-based paradigm that was considered in Chapter 3 and that will re-emerge in Chapter 8. This work (Radin, Michel, & Delorme, 2015) is relevant to the experimenter-psi issue. Due to space limitations, suffice it to say that in this work the experimenter’s expectation for the outcome was in the opposite direction to the outcome, for which the volunteers were given auditory feedback to signal success (although they did not know for what, objectively, reinforcement was given). Under these circumstances the outcomes were in the direction reinforced by the feedback to the participants, not that expected by the experimenter. It would seem that what was important to affecting the optical system was the incentive value of the feedback signal to the volunteers. This study may, though, not be as easy to replicate conceptually as one might wish because what the experimenter thought about the meaning of the feedback was wrong, because of a “mistake” (p. 132) in which the computer code had, unknown to the experimenter, switched the empirical meaning of the feedback given.

Remote Healing refreshingly advances the breadth of potential mind-related physical influences, examining them in the midst of organismic needs. This chapter section is very informative in the variety of cited work and provides some thoughtful cautionary notes. Various research reports are briefly mentioned, but the highlight, in my view, was the discussion of the results of two meta-analyses by Roe, Sonnex, and Roxburgh (2015) of non-contact studies. One of their meta-analyses was devoted to “non-

whole human biological systems such as cell cultures” (p. 134), to non-human animals, and to plants and seeds. The other, to whole persons. Very important, methodological standards for the meta-analyses were strict, including a priori outcome-masked ratings of methodology being used to determine includability. Significant positive findings were reported from both meta-analyses for work meeting strict quality criteria.

The Pauli Effect is short and includes a humorous episode. The effect concerns the well-known physicist Wolfgang Pauli, who had a strong reputation for some form of accident occurring wherever he happened to be. These accidents, which varied widely in character, were said never to have harmed or discomfited Pauli. Many witnessed them. The authors’ discussion, next focuses on physical events alleged to happen around some NDE experiencers, subsequent to that experience. Barušs and Mossbridge called them “Pauli-like effects” (p. 135), but, based on the information in their book, these events seem likely to have occasioned some degree of distress or real inconvenience for the experiencing individual, unlike in the case of Pauli’s events.

Poltergeist Activity provides a very short, but thoughtful, introduction to this immensely complex topic. It ponders public reactions to reports in this domain, which naturally leads into discussing efforts to explain these events, such as: (a) fraud; (b) misconstrued natural events; (c) belief that things cannot happen that contravene physical laws; (d) viewing the events as unconscious psychokinetic expressions or due to some form of psychological tension pent-up in the individual; and (e) poltergeist-type activity that centers on a specific physical site often viewed as due to “whatever discarnate entities might be out there” (pp. 137-138).

In the authors’ poltergeist discourse there is a misleading statement about terminological usage. In discussing the “psychological explanation” (p. 137) of what brings about the poltergeist events, they say “In such cases, the phenomenon has sometimes been called *recurrent spontaneous psychokinesis* (Irwin, 1994; B. Williams & Ventola, 2011)” (p. 137). Ironically, the RSPK terminology was expressly intended by the term’s creator, W. G. Roll, to be used for recurrent instances of apparently anomalistic physical events *regardless of how the investigator should elect to interpret the status (embodied or not) of the mind serving as the presumed agency for the effects* (Roll, 1972, p. 9, top; Roll, 1977, p. 383, top). This confusion is conceivably based on their using secondary source(s) rather than Roll’s work. The poltergeist discussion opens with the Saucie poltergeist case, a major one, but I found no citation of the detailed treatment of this case afforded by A. R. G. Owen (1964), who personally investigated it, obtained witnesses’ testimonies, and authored the scholarly, thoroughgoing, poltergeist treatise just cited.

Surprising to me was Barušs and Mossbridge’s seriously speculating at chapter section’s end that “discarnate entities” (p. 138) might have been behind the PK success on REGs contributed solely by “only about 2% of the participants” (p.138). They went further and suggested that those who showed psi-missing might “have a mischievous entity that deliberately misbehaves (p.138).” These two suggestions were not accompanied by any hint of how they might be tested.

A Fictitious Ghost discusses the conceptually unusual and methodologically non-traditional, sitter-group PK research of I. M. Owen (1976) in Canada. Much of that work was inspired and conceptually guided by the sitter-group work of Kenneth J. Batchelder in England. Such groups try to elicit PK

(usually, such feats as non-contact table tilting and table levitation), but they neither attempt to call up actual spirits nor involve known mediums. Some imagine a fictional spirit. This chapter section is a short introduction to that work. Readers wishing further discussion may find very interesting a paper by Batchelder (1984) that thoughtfully discusses elements of social dynamics and individual psychology that may affect PK success or failure in sitter groups. These are ideas, he suggests, that might be useful in laboratory work. Research-informed discussion of the psychology (conscious and unconscious) of PK events, including of RSPK and sitter-group phenomena, may be found in Stanford (1974b).

Macro-PK exposes the reader to dramatic and, sometimes, bizarre claims of what the authors deem possible macro-PK. Discussion begins in the not-so-bizarre vein with the reported ability of Susan Padfield to move a light mobile housed inside a glass bottle, making it rotate in a pre-specified direction and, even, sometimes, to a specified angle of displacement. Baruš and Mossbridge cite only a book chapter by Padfield (1980). Citation of report(s) from independent investigator(s) might have been useful.

The next case of alleged macro-PK involves reports of a series of truly bizarre-appearing effects associated with Thomaz Green Morton Souza Coutinho from Brazil. The claimed feats are described by Baruš and Mossbridge in some detail, but the level of scientific evidence needed to make such claims reasonably credible is, in my view, absent from their discussion, even if they claim there is “good documentation” (p. 141). I found in what the authors wrote about these particular claims no personally satisfactory basis for any conclusion, positive or negative, about the authenticity of the claims. This review will also, on similar grounds, stay away from any judgment about the claims advanced for the individual known as the “gold leaf lady” (p. 142) (Braude, 2007).

The final case in this chapter is the story of Anita Moorjani (Moorjani, 2012), who, suffering and near death, reportedly fell into a coma and awakened from it in a profoundly altered state of consciousness much like that reported in mystical experiences. The reported outcropping of the experience was a shift in perspective that seemingly led to healing of the experiencer’s cancer. Baruš and Mossbridge consider this “the macro-PK of disappearing cancer cells” (p. 143). This was an uplifting way to conclude the chapter, but to what degree the healing was due to anomalous physical influence might be debated.

In regard to the consciousness-as-creator idea favored by the authors, one set of spontaneous observations seems eminently apposite but was not mentioned despite its extensive, credible documentation: There may be no more profoundly interesting set of replicated evidence suggesting a direct, automatic, relationship of intensive inner experience and anomalous physical happenings than the numerous well-witnessed reports of the levitation of the human body reported to have occurred spontaneously with certain Christian saints during mystical rapture. These usually occurred during meditation, prayer, or spiritually inspiring sights, and the levitators described them as unsought, seemingly automatic, and sudden physical accompaniments of their often sudden spiritual rapture. My remarks focus on levitating individuals canonized by the Roman Catholic Church because such reports are scrupulously critiqued during canonization proceedings to see if the scrutiny sustains or discredits the claims of miraculous events and of the deceased candidate’s saintly character as reflected in life. Herbert Thurston (1952) provides in-depth, scholarly discussion of such reports.

Chapter 7 (Reintegrating Subjectivity into Consciousness Research) stresses the importance for all scientists—and even more so for those in consciousness research and their research participants—of enhancing skills related to observing one’s inner life (called “first-person observation,” p. 147), that is, introspection. Barušs and Mossbridge advise that to be an effective scientist of any kind one needs to be aware of one’s own mind because this can help one recognize and obviate the undue influence of one’s own, sometimes unrecognized, presuppositions, biases, and tendencies to ignore things counter to expectations. They promise to describe methods to improve introspection. Whether such methods will help make persons more effective, genuinely open, and self-reflective as scientists and/or clinicians is an empirical question that, so far as I know, has not been researched.

The first chapter section is **First-Person Observation at the Core of Science**. Unrelated, though, to that topic, early in this section there is the insightful and very important statement that exact replications of research studies provide enhanced confidence of the findings but no grounds for enhanced confidence in the interpretation originally placed on those findings (p.148). As the authors note, studies with exact replication will replicate any artifacts and threats to construct validity in the original study. At this juncture, discussion could have taken a usefully positive turn by mention of three strategies that can take research beyond trying to exact-replicate a study: (a) Use improved methods to test the same hypothesis; (b) test more than one prediction logically derived from the conceptual hypothesis; and (c) test conceptually diverse (i.e., alternative) hypotheses by examining their contrasting predictions based on their explanations for the same set of empirical observations. More about strategies for conceptual advance may be found in Fiedler, Kutzner, and Krueger (2012), a well-argued, insightful discourse that stresses the importance of creating and investigating *clearly articulated alternative conceptualizations of research findings*, especially those having the explanatory breadth needed to support broader applicability.

The remainder of this section involves a somewhat repetitious and not always clear discourse about how as adults we learn to ignore, undervalue, and even mistrust our own inner experience, coming to rely, instead, on valuations placed on the world by adult society. There is hope expressed that individuals can learn to reconnect with their own inner experience and that this might move them away from reliance on default valuations that the community has tended to impose on them. This seems somewhat abstract, and some examples of how it might work in practice might have helped.

Some Problems with Subjectivity in Psychology begins with an overview of problems related to ostensible psi experiences for which there are no additional witnesses. In such a case, some may wonder if the experienced event is delusional. How does one make a judgment in regard to the delusional-experencer explanation? In that regard, the authors describe four reasonable-sounding decision-relevant factors.

In regard to studying internal experience, the authors make the important point that people seem generally better at reporting their inner experiences than in making accurate attributions about their causes. They then note that even when we are reviewing or reporting on our personal inner experience, there are three limiting factors to be considered.

Commenting on independent verification of subjective observations and finding lawfulness in at least some domains, the authors report that some private events show remarkable agreement across

individuals, and they provide an illustration of this. They promise more in an upcoming chapter section, **Psychophysics**. The authors note that what often is needed is a combination of methodologies and careful study to learn whether the results converge in support of a particular hypothesis.

In regard to observers forgetting what they later wish to report, the authors' discussion is brief and, perhaps of necessity, superficial relative to the interestingly complex issues related to retrieval failure. Given their interest in altered states, it is good they at least mention *state dependent memory* (SDM), which they render as "state-specific memories" (p. 153). In introducing SDM, they say that people may "unintentionally forget" (p. 153, *reviewer emphasis*) an experience that occurred in a different mental state than that in which they attempt to recall it. "Forget," alas, sounds like an active process, unintentional or not. More apposite would have been an explanation saying that people may "fail to retrieve" an altered-states experience while in ordinary consciousness because in it one may lack the specifically altered-state-related cues that during encoding became associated with the target experience, thereby becoming potential retrieval cues for it. Human work on SDM has illuminated a variety of memory-retrieval issues (reviewed in Eich, 1989): (a) revealed the importance of interactions between circumstances of encoding and retrieval circumstances in influencing retrieval; (b) new ways of thinking about memory in several clinical circumstances; and (c) pinpointed issues important to theorization about SDM, including its boundary conditions. Some conceptual discussion of SDM would have seemed merited by its importance, conceptually and pragmatically, for consciousness research.

The authors suggest that one should record experiences while in the altered state or shortly thereafter. Aside from catching a still-vivid memory, I would add that early recording may obviate the many changes that can occur in trying to remember (i.e., mentally to reconstruct) an episode during later reflection. The authors' discussion of distortion in such memories very briefly mentions some things that may lead to distortion of memories. The discussion chiefly, though, focuses on biases in what will and will not be reported, but no guidance or external reference was forthcoming about obviating these untoward social influences or detecting their presence if they occur (but see Aronson, Ellsworth, Carlsmith, & Gonzalez, 1990, Chapter 10).

First-Person Methods in Psychology addresses the issue of need for independent verification of reports of internal events. This sounds reasonable in principle, but verification may, I suggest, be far from a simple matter, depending on the specific problem broached by consciousness research. The brief introductory section concludes by naming three methodologies said to have been "rigorously used" to cast light upon the "substrate and content of conscious awareness" (p. 154): phenomenology, psychophysics, and psychophysiology, are the foci of the next three chapter sections.

Phenomenology is represented here by the *experiential method* and *descriptive experience sampling*, with helpful examples of each. Their very different methodologies and objectives are well explained.

Psychophysics begins with the authors extolling the level of reliability found in many studies that fall within its rather big methodological umbrella. They claim that psychophysics has produced the most replicable results in psychology and provide three examples said to have yielded "powerful insights into subjective experience" (p. 156).

It was gratifying to see some remarks on *signal detection theory* (SDT), mentioning its potential usefulness in a variety of disciplines. The authors did not mention the SDT model's potential usefulness in receptive-psi research. Stanford (1982) provided a précis of the basics of SDT, along with seminal references and commentary on its potential usefulness in psi research.

Barušs and Mossbridge report that psychophysical principles have been used “sparingly” (p. 157) to study psi. They do cite a 2015 study by David Vernon that reportedly documented, in one respect at least, retroactive repetition priming.

It was troubling again (p. 157) to see the authors' unjustified claim—already voiced by them twice with citation (p. 66; p. 68) and referred to another time (p. 71)—that a meta-analysis (Honorton & Ferrari, 1989) supported a temporal decline of precognition. To declare this methodologically untenable claim an example of using “the principles of psychophysics . . . to examine the characteristics of psi” (p. 157) is far off the mark and regrettably short of the rigor and methodological precision typical of psychophysics.

Psychophysiology basically informs readers that studies in this domain do NOT bear on the issue of “whether physiology creates our conscious experience” (p. 158) and, further, that the authors are “making no assumptions about the direction of any potentially causal relationship between mental experience and neurophysiology” (p. 158). Nonetheless, the authors are very interested in studies that help to elucidate relations of mind and body, and have special interest in what Varela (1996) called “*neurophenomenology*” (p. 158). This discipline records neurophysiological data as specially trained observers “keep track of their phenomenal experiences” (p. 158). Barušs and Mossbridge unequivocally assert that psychophysiological findings can aid in understanding the relation of mind and body. They seem especially interested in EEG characteristics as related to awareness (e.g., in meditative states).

Toward an Integrated First- and Third-Person Approach emphasizes that a science of this kind must have trained observers of their own minds. Barušs and Mossbridge suggest that problems of potentially biased or idiosyncratic observations can be addressed or mitigated by: (a) looking for convergence of observations across observers (i.e., intersubjectivity), and (b) examining the correlation of physiological measures (e.g., EEG) and phenomenological reports to assess potential convergence of brain state and inner experience. I consider item (b) an example of the important principle that a hypothesis should be tested in more than one way (in the authors' example, reported phenomenology and finding related indications in EEG indices). The more conceptually convergent evidence one can find, the stronger the support for the relevant conceptual hypothesis. In that spirit, I offer a third example: An altered state may be further evinced by (c) conceptually relevant behavioral indices of information processing (e.g., reduced call balancing or fewer sequential constraints in forced-choice receptive-psi tasks; or less rational justifications in reporting experiences). Obtaining raw behavioral data for analysis of mode(s) of information processing often can proceed *without doing anything intrusive or reactive* (Stanford, 2003), by unobtrusively recording the relevant behavior for later analysis. Such analysis (e.g., Stanford, Frank, Kass, & Skoll, 1989) can assess predictions based on models or theories; it also can be exploratory in the interest of developing conceptual hypotheses or models for later testing.

Developing First-Person Observation Skills focuses initially on becoming aware of and reducing one's biases, which may be conscious, unconscious, or involve elements of both. The authors' primary approach to this involves meditation, which can be used as a means of knowing one's own mind (and hence having the chance to control it). What follows in regard to remediating individuals' biases of particular interest to consciousness researchers is inadequately developed. The term "bias" can refer to a wide range of things. The kind(s) of "bias" of particular concern to consciousness researchers are not specified here, but effective work toward remediation arguably would have to vary substantially depending on the targeted kind of bias. The cited justificational research was related to implicit social biases (especially gender and race/ethnicity) and their remediation. On what grounds such social-psychological work, its conceptual underpinnings, and its methods should be deemed germane to the specific interests of consciousness researchers was not explained. In the absence of such explication, citing these social bias studies is less than satisfying. Also, conceptual preferences in some domains (e.g., precognition or survival of death) often are strongly conceptually linked to how an individual conceives and relates to the world and living in it—including via religion and/or philosophy of life, central systems involving beliefs and values—so trying to change reactions to such broad-implication abstract constructs may be difficult and may require very different approaches than what may be needed to change the perception of or feelings toward a certain social group.

The authors next discuss meditation, emphasizing its potential role in training one's mind for introspection. They provide some research examples to illustrate that experienced meditators know what their minds are like at the time of meditation and that such insight is supported by physiological recording taken during meditation. They note that for some individuals such ventures into self-awareness pose serious risks, including complications that can necessitate professional clinical assistance. They then begin discussion of some techniques or strategies that can be used to open one's inner experience to careful self-observation. They stress that meditation focuses the mind on awareness, which, they explain, entails clearly viewing experience, moment to moment, with no effort to analyze or explain it.

They discuss *focused awareness* meditation and, citing a report by Lutz, Slagter, Dunne, and Davidson (2008), they seem to endorse several claims about its salutary cognitive effects. Various meditation approaches involve focused attention, but a common denominator is to focus on a single object and, if the mind wanders, to let go of the distractor and bring attention gently back to the focal object. Having mastered this focus-related meditation, one then takes the next step (*open monitoring* meditation), which is no longer to focus on an object but instead to observe, steadily and without analysis, the flux of one's conscious awareness. Barušs and Mossbridge see such meditation as potentially very useful in the training of consciousness researchers and in enabling them to help others to observe phenomenology.

Self-Development for Consciousness Researchers begins with a reminder, including a dramatic real-life example, that various types of meditative discipline can pose serious risks for some individuals. Much of this section involves speculation on how one might know if one is ready to confront those self-examination related risks and the possibility of reducing the chance of harm from them. No research evidence was discussed bearing on the predictive validity of the self-actualization assessment suggested for self-screening relative to potential risk involved in such a meditation regimen. More

fundamentally, are psychiatrically troubled individuals capable of validly assessing where they fall on Maslow's hierarchy of needs, especially given the scale transparency and the social-desirability of believing oneself self-actualized?

In the final chapter section, **Accurate Intuitions about Events in Consensus Reality**, the term "consensus reality" means the sense of the world that most of us have on an everyday basis. To me, the more interesting parts of it begin with the last paragraph on page 167 and end at chapter's end (p. 169). The earlier parts are likely to have less interest for many readers. They seem more like filler, given the fundamental thrust of the book, because they have doubtful relevance to its central emphases. That first part does, though, have relevance to intuition as psychologists tend to use that term, but lacks demonstrated specific relevance to anomalous cognition, which sometimes seems to manifest as intuition. The remainder of the chapter I found more interesting, and it integrates some ideas central to the book's theme of transcendent mind.

That remainder of the chapter first turns to some research by Roney-Dougal and colleagues that examined Tibetan Buddhists' numbers of years of meditation training as a predictor of their precognition-task performance. Barušs and Mossbridge do not specify the correlation between these variables but say it was strong and positive (citing Roney-Dougal, Solfvin, & Fox, 2008). But this is only part of the work by this team in regard to years of meditation and receptive-psi performance (or sometimes yogic attainment and psi). The several studies of Roney-Dougal with various colleagues that correlated number of years of meditation practice (or yogic attainment) with receptive-psi performance were cited and summarized by Roney-Dougal (2015, pp. 131-132), with what seems to me decidedly mixed results. Even had the results strongly supported a positive correlation of receptive-psi success and years of meditation practice, the interpretation would be equivocal. Years-of-practice potentially could be confounded with personalistic and/or life-history variables that influence whether people stay with or drop out of practice and such factors might themselves influence psi-task performance. It is possible that years of practice may not be as conceptually appropriate a predictor as some measure(s) of spiritual/meditational attainment. Further, successful studies comparing meditators and non-meditators on receptive-psi performance cannot be assumed clearly to support the proposition that involvement with meditation per se is responsible for superior psi-task performance. Those who elect to be involved with meditation may be different kinds of individuals from the start than those who do not, and they may be individuals who tend to be relatively psi-receptive. Another possibility: Meditation practice may enhance deliberate psi-task performance only with certain kind(s) of individuals. Some of these issues are discussed by Roney-Dougal (2015).

Chapter 8 (Transcendent Mind) begins with **Quantum Mind**. Its first paragraph provides the section's gist by stating that quantum theory is not "very useful" in helping to conceptualize "a greater mind beyond the brain" (p. 172). The authors assert that quantum theorists have not faced up to the reality of research findings (e.g., the two-slit studies addressed in other chapters) that point toward the influence of mind on the physical world. These authors seem to have an ambivalent relationship with quantum physics, treating it as potentially opening the way for anomalous influence in the world but resenting that quantum theorists have not, for the most part, recognized the role of consciousness in shaping the world.

Filter Models propose the nervous system as a filter system that blocks from awareness unspecified source(s) of information that otherwise would flood experience, preventing focus on pragmatically needed information. Its proponents cite instances such as near-death experiences in which the brain has been rendered dysfunctional (in at least some respects), so its filtering function is presumed compromised, allowing into consciousness material of which one normally would be unaware. Baruš and Mossbridge go further by dubbing as the “unconstrained mind” (p. 177) this whatever-it-is that is thought normally blocked by brain filtration and by presuming in the model that it “is directly connected to other minds” (p. 177) and perhaps other kinds of information. Further, they suggest unconstrained mind as a source of anomalous action, treating PK as, for example, the potential enabler of physical speech in a dying body. In addition to anomalous reception and action, unconstrained mind is suggested somehow to be involved in transcendental or mystical experience when the filter is absent or dysfunctional or one’s source of subjective experience manages to get beyond it. If the filtering is entirely passive, they seem concerned about the possibility of “unwanted influences” from “discarnate beings” (p. 178). They make two important points: (a) altered “transcendental states” produced by psychedelic drugs may differ from nondual states of awareness during meditation; and, relatedly, they consider (b) the possibility that the concept of a “passive filter is too simplistic” (p. 178) and that there may be ways of managing the brain so that opportunities are created for emergence of “transcendent abilities and contents” (p. 178, citing E. F. Kelly & Presti, 2015).

The Brain as a Byproduct of Consciousness poses three questions related to metaphysical idealism, which they explain as “mind” being the fundamental basis of all that is, including the so-called physical world. They seem to prefer the term *consciousness* in lieu of *mind* and to assume that “consciousness of some sort is the fundamental substance of the universe and that everything else is made out of consciousness” (p. 179). The authors’ three key questions (p. 179) at this juncture are (my wording): 1. How does consciousness beget the brain? 2. Why should brain activity correlate demonstrably with conscious awareness? 3. Why should we have a brain? Their answers and related thoughts are found on page 180, but an answer to question #3 is *not* provided. They instead note that materialists are similarly perplexed in explaining why we have consciousness.

Flicker-Filter Model proposes a model, but not, apparently, a testable one. It is said to explain (or permit) a wide range of things relative to time, psychokinesis, and anomalous functioning generally. However, it is easy to make after-the-fact assertions to “explain” something that has already been observed, especially if one’s constructs imply no boundary conditions. *A useful (i.e., testable) theory or model must not only subsume the known, it must logically imply new findings observable in specific, predicted conditions if the proposal is valid.* The discussion never gets there. The authors state, “Given its speculative nature, perhaps this is as far as we should go at the moment in working out the flicker-filter model” (p. 184). They note that “it is necessary to test this theory empirically to determine its continued utility for understanding the nature of consciousness” (p. 184). But, for the present, what kind(s) of novel observations under what circumstances does it predict? If what its authors were saying is that their model needs further development to be testable, I concur.

Guidelines for Future Research on Consciousness provides 10 research-related suggestions intended to promote innovative, more adequate models of consciousness. Each of these seems interest-

ing, some seem very thoughtful and creative, and some, wildly imaginative. Research would be needed to assess the value of each suggestion.

Implications for Clinical Practice examines clinical ramifications of topics discussed earlier and some new ones. It succinctly addresses five important issues that should interest anyone concerned with clinical practice. Also briefly mentioned is the possibility of training psi-related abilities for use in the healthcare and other helping professions.

Implications for Scientific Discovery as a section title would seem to portend some exciting things, but I did not find them here. There is extended recounting of the authors' real-life effort to elicit, from the ostensible spirit of a deceased quantum physicist, Richard Feynman, via a medium, the numerical value of the "fine structure constant" (p. 193). The authors, in the end, admit that this story "does not prove anything" (p. 194). I heartily concur but wish they had left out this long, overwrought episode, presenting, instead, some thoughtful discussion of the chapter section topic. The idea of free-loading scientific advance by sitting on one's intellectual rear end soliciting verbal tips from *soi-disant* discarnate spirits of eminent scientists talking via mediums seems far from what successful scientists always have done or likely will do. If Barušs and Mossbridge's transcendent mind is a reality, what is wrong with allowing it to access directly for the investigator's inquiring mind the nature of whatever aspect of reality one may be working to understand? Coming to grips with such access may, of course, require a prepared mind if the information is to be recognized, understood, communicated, and used. That means that real prior work on the problem domain may be a prerequisite. Deep insights usually seem to come to those who have worked hard on a problem, even if they may come at a moment of release from the struggle.

The book's final chapter section, **The Nature of Consciousness**, is a succinct, very welcome, well written, and important one for providing an overview of the central themes of this remarkable volume. Its tone is upbeat and hopeful.

Editorial Concerns

A substantial number of publications are cited and referenced whose authors are not listed in the index. Index lapses can frustrate readers and seem unfair to those whose work is used but whose names are not indexed. Readers eager to learn more may be frustrated by the citation of many sources for which the authors *provide no idea of their specific relevance to the context in which they are cited*. How can one assess the potential interest of a cited source if the specific locus of its interest for those citing it is undisclosed?

Reflections

This book was intended by its authors to place before a broad range of psychologists and interested others empirical evidence and related arguments that the authors deem forcefully to challenge the view of mind as simply the functional apparatus provided by the nervous system. Although a wide range of topics is covered, the discourse is usually clear, coherent, and fair offering, however briefly, alternative

interpretations of many of the observations discussed. Sometimes, though, the authors do not discuss topics that arguably merited or even needed discussion. A very important lapse, given the emphasis on survival of bodily death, is the conspicuous absence of the intensive and culturally extensive work by Ian Stevenson and colleagues on cases of possible reincarnation and that work's continuation by his contemporary successors. Strongly contrasting with that particular survival-related deficit is a very strong focus on spiritistic entities, concepts, and related observations. The nature of time runs like a leitmotif throughout the book. Some readers may, like this reviewer, appreciate that emphasis and find it exciting, but thinking deeply about time may be a bit unsettling for some. Whatever the topic, the very thoughtful, information-rich character of this book calls for careful reading and some deep, intellectually honest pondering. For those who welcome such challenges, it can provide very rewarding reading.

I noted instances of problematic scholarship of the following kinds: (a) inaccuracy in rendering the work of others; (b) empirically unjustified claims; (c), failure to identify certain researchers whose work was discussed; and (d) an unsupported and factually contestable charge concerning a questionable research practice. The substantial treatment of meta-analyses was usually good, but some exceptions were noted. There was considerable use of secondary source(s) related to some important anomalous reception studies, which might explain why the authors sometimes seemed to lack primary-source information that might have helped them to understand some important elements of that research. In discussing two important topics (i.e., dissociative identity disorder and hypnotic age regression procedures) there was no mention of some important theoretical and empirical literature reflecting a very different perspective than the survival/spiritistic possibilities emphasized by Baruš and Mossbridge. In general, readers learn relatively little, in my view, about contemporary scientific knowledge of altered states of consciousness, although altered states center much of the discussion. The authors, though, had different objectives in mind. A scholarly edited volume (Cardeña, Lynn, & Krippner, 2014) might interest readers wishing more coverage of scientific studies of altered states.

A concern about readership reception of the basic message is that considerable in this book seems dished out without, perhaps, sufficient consideration of whence many psychologically trained readers might start their journey relative to transcendental mind. The experimental psi research and quantum theory, however convincing their presentation and discussion of their possible implications, might not have prepared many readers for the mind-boggling (and arguably inadequately evidenced) stories that awaited them. The transcendent mind message might usefully have been handled more gently. People often take time to grow into new ideas.

The authors' very thoughtful discourse seems accessible for cognitively motivated novices, whatever the topic of discussion. Their strongly affirmed metaphysically idealist stance posits consciousness as underlying all there is, including the brain. Whether this metaphysical investment will resonate with the ideas and pragmatic concerns of clinicians is unclear. Alternatively, perhaps practicing psychologists would welcome the suggestion that a deeper view of reality—possibly via transcendent mind—might support personal transformation, embracing a vision in which humans treat each other more kindly and likewise the environment that sustains life of all kinds, promoting a deeper, more life-sustaining view of the world.

Certainly Baruš and Mossbridge's message directly affronts the brain-is-supreme message that nowadays seems shouted to the public via the media and that increasingly dominates the psycholog-

ical literature. That this book will awaken its readers to the possibility that a metaphysically materialist perspective is seriously problematic seems a fond hope of its authors, whose passion, enthusiasm, and hard work in exposition and explication are evident throughout. Their discourse is conceptually engaging, rewardingly provocative, and, thanks to American Psychological Association publication, is accessibly placed before a potential readership with the background and intellect needed to understand and potentially learn from it that there arguably are reasonable grounds for questioning the unquestionable.

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Voyage Méditatif dans les confins de l'Esprit : À propos de *Transcendent Mind*

Cette recension se veut une longue réflexion sur une publication marquante de l'American Psychological Association, un ouvrage dont l'argumentation, renforcée par le passage en revue des preuves empiriques de la parapsychologie et d'autres disciplines, vient questionner la perspective que la plupart des psychologues ne remettent a priori plus en cause, selon laquelle le fonctionnement mental, conscience incluse, est seulement la conséquence d'interactions avec le monde physique, ce que les auteurs du livre appellent le « matérialisme ». En exposant leur pensée très divergente dans l'arène du débat public, les auteurs épousent hardiment la perspective métaphysiquement idéaliste selon laquelle le monde est essentiellement basé sur le mental ou la conscience. Les idées qu'ils discutent – probablement étrangères à de nombreux psychologues – semblent au reviewer, en général, prudentes, clairement articulées et profondément réfléchies. La valeur éducative élevée de l'ouvrage est renforcée par l'utilisation, par les auteurs, d'interprétations alternatives des observations. Le reviewer se sent obligé de remarquer, toutefois, des exemples spécifiques où le propos généralement académique se relâche par occasion, tels que la restitution imprécise des travaux d'autres chercheurs, des affirmations empiriquement injustifiées, et l'incapacité à identifier certains chercheurs dont les travaux sont discutés. On peut espérer que cet ouvrage engagé conceptuellement et provocateur à souhait puissent aider à éveiller les psychologues et les autres personnes intéressées par l'obtention de bases raisonnables pour soulever les questions sur ce qui est apparemment inquestionnable.

Erwägungen zu einer Reise in Reichweiten des Geistes Ein Besprechungsaufsatz von *Transcendent Mind*

Diese Besprechung stellt ausführliche Überlegungen zu einer bahnbrechenden Veröffentlichung der American Psychological Association an, einem Band, dessen Argumentation, gestützt mit empirischen Befunden aus Parapsychologie und anderen Disziplinen, die Ansicht in Frage stellt, die wahrscheinlich unter vielen Psychologen unhinterfragt kursiert, nämlich dass psychisches Funktionieren, Bewusstsein eingeschlossen, lediglich das Resultat physikalischer Wechselwirkungen darstellt, was die Buchautoren als "Materialismus" bezeichnen. Indem sie eine völlig andere Denkweise öffentlich zur Debatte stellen, vertreten die Autoren unerschrocken die metaphysisch-idealistische Anschauung, dass die Welt in ihrem Wesen mental ist oder auf Bewusstsein basiert. Ihre Begründung—die wahrscheinlich vielen Psychologen äußerst befremdlich erscheinen mag—erscheint dem Rezensenten durchgehend sorgfältig durchdacht, klar formuliert und sehr reflektiert. Der hohe Bildungswert des Bandes wird verstärkt durch das Anführen alternativer Deutungen für Beobachtungen seitens der Verfasser. Allerdings fühlte sich der Rezensent verpflichtet, darauf hinzuweisen, dass an einigen Stellen die im allgemeinen lobenswerte Gelehrsamkeit Schwächen zeigt, zum Beispiel in unpräziser Wiedergabe anderer Arbeiten, im Aufstellen empirisch fragwürdiger Behauptungen, oder in Bezug auf das Unterlassen der namentlichen Nennung von Forschern, auf deren Arbeiten Bezug genommen wird. Es bleibt zu hoffen, dass dieser konzeptuell einnehmende und bereichernd provokative Band Psychologen und andere interessierte Gruppen dahingehend aufrütteln kann, dass es vernünftige Gründe dafür gibt, scheinbar Selbstverständliches neu zu hinterfragen.

Reflexiones sobre un Viaje a las Fronteras de la Mente: Una Crítica/Ensayo de *Transcendent Mind*

Esta extensa crítica reflexiona sobre una publicación señera de la American Psychological Association, un volumen cuya argumentación, reforzada por revisiones de la evidencia empírica de la parapsicología y otras disciplinas, cuestiona la visión, presumiblemente incuestionable entre muchos psicólogos, de que el funcionamiento mental, incluida la consciencia, es únicamente una consecuencia de las interacciones del mundo físico, lo que los autores del libro denominan "materialismo." Colocando su pensamiento muy divergente en la arena del debate público, los autores defienden audazmente la visión metafísicamente idealista de que el mundo es esencialmente mental o basado en la consciencia. Su discusión, tal vez muy ajena a muchos psicólogos, le parece a este revisor generalmente cuidadosa, claramente articulada, y profundamente reflexiva. Aumenta el alto valor educativo del volumen el que los autores usualmente mencionen interpretaciones alternativas de las observaciones. Sin embargo, el revisor se sintió obligado a observar en ocasiones casos específicos de limitaciones en este trabajo generalmente loables tales como menciones caducas, interpretación inexacta del trabajo de otros, afirmaciones empíricamente injustificadas, y falta de identificación de ciertos investigadores cuyo trabajo se discutió. Puede esperarse que este volumen, provocativamente estimulante y gratamente provocativo, ayude a despertar a los psicólogos y otras partes interesadas a que existen motivos razonables para comenzar a plantear preguntas sobre lo que parece ser incuestionable.