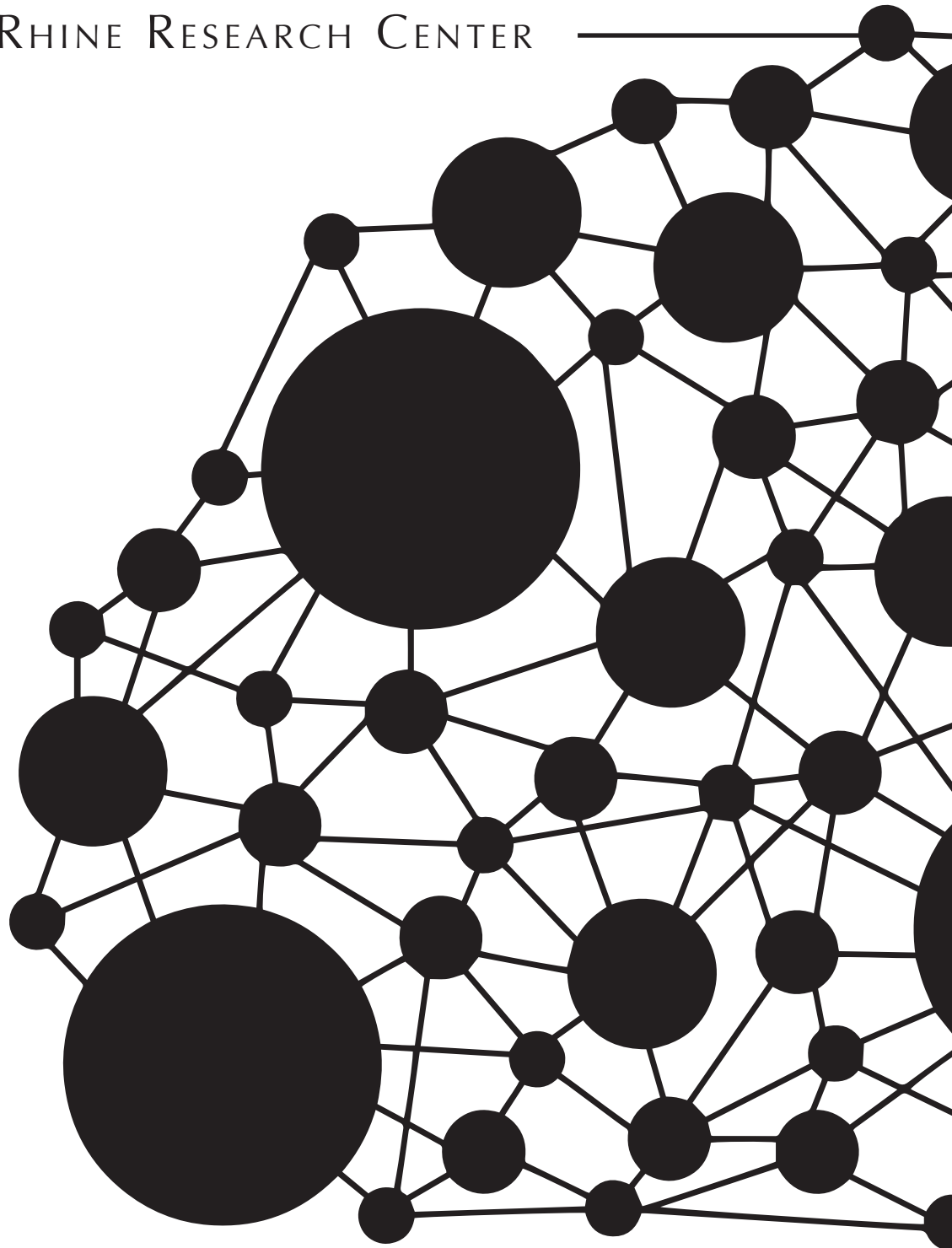
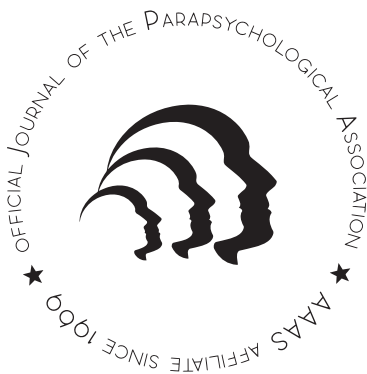


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We thank the following for their translations of the abstracts: *Eberhard Bauer* (German), *Etzel Cardeña* (Spanish), and *Renaud Evrard* (French).

Editorial: ψυχή Is a Woman

Etzel Cardeña

The title of this editorial begins with the word *psyché*, an etymology for both *psychology* and *parapsychology*. The Greek word has been translated as *mind* or *soul*, as well as being the name of one of the characters in the myth of Cupid and Psyche in *The Metamorphoses* (also known as *The Golden Ass*), the Classical Roman novel of Apuleius, written in the second century AD. With some poetic license, the story of Psyche's travails partly resembles that of parapsychology. The beautiful (para) Psyche is prohibited by her divine husband Cupid (or *Eros*) to see him (he only visits at night and their interactions occur in darkness). When, despite his injunction, Psyche decides, candle in hand, to venture into the mystery while Cupid is asleep and look at his face, she discovers a beautiful young man but lets some candle wax fall on his shoulder (or a much more sensitive part in some accounts). She is summarily exiled and is tasked by the mother of Cupid, Venus, to complete four impossible tasks before she is accepted again. Her tasks are not that dissimilar from those facing parapsychology, which dared to investigate a forbidden realm to then be mostly exiled from mainstream science. Currently the field faces apparently insurmountable hurdles, being expected by some to produce far more convincing evidence than is required for other topics while simultaneously being impeded by lack of funding, recognition, and dogmatic journal editors (e.g., Cardeña, 2015).

To propose a simile is not the purpose of this editorial, though, but to highlight the at-times neglected contribution by women to the field (Alvarado, 1989). I encouraged Carlos Alvarado to write a profile of the extraordinary Eleanor Sidgwick, a foundational figure of psychical research and mathematical analyst for early endeavors of the field, besides being a Principal at Newham College in Cambridge. Some years ago, I found out about her in an authoritative treatise on the founders of psychical research (Gauld, 1968), and read some of her very lucid papers and monographs (e.g., Sidgwick, 1885; 1915). Her clear and sharp reasoning weighed very carefully the evidence for and against various phenomena before arriving to a position, a strategy that I wish many more pro- and anti-psi authors would follow. Yet, most accounts of the early days of psychical research emphasize the contributions of men such as her husband, Henry Sidgwick, Myers, James, and a few others while, and this will come as no surprise to many of my readers, neglecting or undervaluing the contributions by women.

Similarly, the attention paid to the landmark research on spontaneous events by Louisa E. Rhine (e.g., Rhine, L. E., 1977) pales in comparison to that paid to the experimental program of her husband Joseph Banks Rhine. And the list should be extended. Has the field fully acknowledged the essential early contributions of the "subjects" Mrs. Piper, Mrs. Leonard, and other women mediums/shamans? And where would the field be without the decades-long funding and organizational contribution of Eileen Garrett and her daughter and granddaughter through the Parapsychology Foundation? To these

should be added the extraordinary contributions on the psychology of psi by Gertrude Schmeidler (e.g., Schmeidler, 1958) and Rhea White (White, 1977), as well as leading analyses and research in our days by Julie Beischel (e.g., Beischel & Schwartz, 2007), Emily Williams Kelly (e.g., 2007), Antonia Mills (e.g., 2014), Julia Mossbridge (e.g., Mossbridge, Tressoldi, & Utts, 2012), Marilyn Schlitz (e.g., Schlitz, Wiseman, Watt, & Radin, 2006), Jessica Utts (e.g., 2015), Caroline Watt (e.g., 2016), and Nancy Zingrone (e.g., 1988), among others. Some of them participated in a Parapsychology Foundation meeting in 1991 dedicated to “Women in Parapsychology,” which also included two contributions by a dear friend, the one-of-a-kind anthropologist Ruth-Inge Heinze (Coly & White, 1994). Currently 40% of the Associate Editors of the *JP* are women, and this editorial is a small token of gratitude and acknowledgement to all women, past and present, in the field.

Besides the historical piece on Eleanor Sidgwick (and expect more historical reviews in future issues of the *JP*), this issue also contains the Abstracts from the 61st Annual Convention of the Parapsychological Association, including reports of successful psi studies and analyses (obrigado and gracias to Everton de Oliveira Maraldi and Ramsés D’León Macías for sending the abstracts), an empirical paper by John Palmer on dissociation and psi (a link that is very complex but deserves further attention), and a research note by Ian Tierney, Caroline Watt, and Anna Flores proposing that a shortening of perceived time may predict psi performance. Stanley Krippner provides an obituary for the researcher Michael Persinger, who contributed studies on geomagnetic variables and psi performance, and is probably most famous for his “God’s helmet,” although the effects he reported seem to be at least partly due to experimenter effects (Granqvist et al., 2005). Rex Stanford, commissioned by the previous editor, offers a very thorough discussion of a book proposing a transcendent nature of the mind, and this issue also includes shorter reviews by Erlendur Haraldsson, Gerhard Mayer, and Tom Ruffles on books dealing with ostensible reincarnation, coincidences, and outstanding Icelandic and Polish mediums. The issue closes with a letter by Nancy Zingrone and Carlos Alvarado underlining the importance of knowing the history of the field. Enjoy!

Newsflash: As we were going to press we received the Call for Papers for the 62nd Annual Convention of the Parapsychological Association in Paris, France (July 4-6th 2019), with an earlier-than-usual deadline of February 4th. See: https://parapsych.org/articles/0/464/2019_pa_convention_call_for_papers.aspx

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In Memoriam: Michael Persinger

Stanley Krippner

Saybrook University

It is with great sadness that I read of Professor Michael Persinger's (1945-2018) passing. The Laurentian University obituary noted that "Dr. Persinger... was a very engaged member of the Laurentian community. He graduated from the University of Wisconsin, Madison in 1967. He received his M. A. in Physiological Psychology from the University of Tennessee in 1969, and his Ph.D. from the University of Manitoba in 1971. Dr. Persinger began his academic career at Laurentian in 1971 as an Assistant Professor in the Department of Psychology. In 1975, he was promoted to Associate Professor, and became Full Professor in 1980. In 1983, Dr. Persinger founded the Behavioural Neuroscience program at Laurentian, bringing together psychology, chemistry, and biology. He remained Coordinator of the Behavioural Neuroscience program until his passing. Dr. Persinger was globally renowned for his scholarly contributions, having published hundreds of peer-reviewed academic journal articles across several different fields of study... He was a regular contributor to national and international television and radio programs, speaking on topics such as unusual experiences, climate change, religious experiences and many more."



This official obituary does not go into depth regarding Michael's contributions to parapsychology. I first discovered this interest when I was lecturing at a university in South Dakota and my host told me that a friend of his was engaged in parapsychological research. I had never heard of him but asked for contact information so that we could communicate. Michael wrote me a long response and told me about his analysis of spontaneous cases. He found a link between low geomagnetic activity and putative ESP, and a link between high geomagnetic activity and PK.

Michael asked for my data and I sent him the "hits" and "misses" for the first night for every participant in our Maimonides dream/ESP studies. The analysis demonstrated a significant connection, especially for what we called "high hits" (high ratings on correspondences between dream reports and pictorial targets) and low geomagnetic activity (Persinger & Krippner, 1989; Krippner & Persinger, 1996).

When our team observed the psychic claimant Amyr Amiden in Brazil, I devised a 5-point scale that a group used to evaluate each of the spontaneous PK phenomena that occurred in our week with

him. These data were correlated with geomagnetic readings obtained with the only geomagnetometer in Brasilia, transported to an isolated field to ensure accuracy. This time there was a significant relation between high scores on the scale and high geomagnetic activity.

Michael did not attend very many conventions because of family obligations as well as his incredible output of published research. The *American Psychologist* identified him as one of the most widely published of its members. Michael had an inquisitive mind, an original approach to research, and a lively sense of humor. He once told me, "You refused to call yourself a neuroscientist, but that is your basic identity." The compliment was undeserved but, I assume, was made in jest. Michael enjoyed hearing accounts of UFO sightings, mediumistic readings, and phone calls from the dead, although he typically sought naturalistic explanations such as electric storms, cortical anomalies, and the environmental ambiance. His work was not without criticism and controversy, even among parapsychologists. But there was nobody quite like him. Who else could have evoked quasi-mystical experiences with an apparatus he jokingly referred to as "The God Helmet"?

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Abstracts of the 61st Annual Convention of the Parapsychological Association

Full Papers

Parapsychology Needs a Theoretical Program

Peter A. Bancel

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Demanding exceptional evidence for exceptional claims is not just a scientific position, it is also a social one. To put it prosaically, it addresses the worry that I might waste my time on a wild goose chase, as well as the concern that my field and I may lose credibility in the eyes of other scientists and the public. The demand is a gatekeeper for vetting which extraordinary claims (or effects or data) should receive attention within a discipline. But how does a discipline or sub-field become accepted in the first place? This too melds scientific and social issues. Parapsychology has long had one foot in and one foot out of the house of Science. Why is this, and is it normal? Obviously, the interplay of theory and experiment plays a big role in establishing any field. But it is worth a look at how this happens. Among many examples, two stories from contemporary physics are instructive: quasicrystals and quantum foundations. Both are cases where an anomaly developed into an accepted discipline when theory and experiment converged. In the first case, experiment led the way and in the second it was theory. These stories convey very different timelines, but the same unsurprising message: a discipline is unlikely to flourish without both theoretical and evidential groundings. Parapsychology should, therefore, redouble its theoretical efforts, for theory is the weak leg. We have some start points but see mostly fog. How to proceed? Ironically, a good bet will be to mine recent results from quantum foundations research, for there questions of causality, consciousness, and impossible effects that mirror many aspects of parapsychological science have been treated with great rigor and may be importable to psi theory. Several examples will be sketched to illustrate possibilities.

Simulating Questionable Research Practices

Peter A. Bancel

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Meta-analysis provides evidence for psi effects across a number of well-established protocols. However, the drawbacks of meta-analyses, which are well-known, can weaken the evidence, particularly for researchers in other disciplines who are not familiar with the parapsychological literature. Moreover, recent scrutiny on the variety and frequency of questionable research practices (QRPs) – methodological problems such as publication bias or unplanned analyses that can lead to spurious effects – has called into question meta-analytical evidence across many fields. A paper by Bierman, Spottiswoode, and Bijl used Monte Carlo simulations to test whether meta-analytic databases can be explained by QRPs alone. The paper is novel in that it attempts to simulate a comprehensive set of QRPs operating simultaneously. This work presents a reformulation of the problem that improves the power of QRP tests and speeds up simulation times by a factor of roughly 1,000. The method was applied to the ganzfeld database. In addition, it is shown how understanding QRPs can lead to insights about other databases, namely those of micro-PK RNG and Global Consciousness Project (GCP) experiments. Specifically, I show that a broad set of QRPs fails to account for the ganzfeld data, even if these are used in maximal combination and are adopted by researchers at frequencies approaching 100%. Applied to the micro-PK data, I argue that, although the data are not amenable to full simulation, the QRP analysis suggests that the heterogeneity cannot be explained by maximal QRPs. If substantiated, this conjecture would supply new support for a micro-PK psi effect and help clarify some of the confusion about this complex database. The GCP is fully pre-registered and hence generally immune to QRPs. QRP analysis is consistent with this framing. I indicate how the GCP, while exhibiting a real psi effect, does not provide evidence for its Global Consciousness hypothesis.

Ability of Alleged Mediums to Assess Mortality from Facial Photographs¹

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Studies of characteristics of the human face indicate that it contains a wealth of information about health status. Most studies have involved objective measurement of facial features as correlated with historical health information, but some individuals (labeled psychics or mediums) also claim to be adept at intuitively gauging mortality based solely upon a quick glance at a person's photograph. To test this claim, we invited 12 such individuals to see if they could tell if a person was presently alive or dead based solely on a brief examination of his or her photograph. All photos used in the experiment were

¹ This research was supported by the Bial Foundation (grants 234/12 and 188/16).

transformed into a uniform gray scale and counterbalanced across eight categories: gender, age, gaze direction, glasses, head position, smile, hair color, and image resolution. Participants examined 404 photographs displayed on a computer monitor, one at a time, with each shown for a maximum of 8 seconds. Half of the individuals in the photos were deceased and half were alive at the time the experiment was conducted. Participants were asked to indicate if they thought the person in a photo was alive or deceased by pressing an appropriate button. Overall mean accuracy on this task was 53.6%, where 50% was expected by chance, $p = 0.005$, and significant accuracy was independently obtained in 5 of the 12 participants. We also collected 32-channel electrocortical recordings and observed a robust difference in the early event-related potential at 100 ms post-stimulus onset between images of deceased individuals who were correctly vs. incorrectly classified. Then, to see if machine learning techniques could classify the photographs as good as or better than humans, both random forest and logistic regression machine learning approaches were used. Both classifiers failed to achieve accuracy above chance level. These results suggest that some individuals can intuitively assess mortality based on some as-yet-unknown features of the face. In this report, we also outline a follow up experiment where we asked participants to classify the cause of death from photos of now-deceased individuals. Preliminary results on this experiment will be presented at the Parapsychological Convention.

Development of a Predictive Anticipatory Activity (PAA) Software: A First Step towards a Medium-Term Goal

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There has been enough research in the last two decades to report significant effects regarding predictive anticipatory activity (PAA), the unconscious ability to predict future events as measured by physiological responses. Although there have been advances in the design, methodology, and analysis of data, replication of this phenomenon still seems to be insufficient to confirm many of the findings concerning which type of stimuli, physiological response, or even task duration are best. Furthermore, the cost of the psychophysiology equipment, the high level of statistical analysis, and the skepticism of professional researchers in other areas decrease the probability that replications will ensue. Therefore, we have set a medium-term goal to develop PAA software with high-quality design compatible with low-cost psychophysiological equipment and allowing testing with a wide range of stimuli. The first step in the development of this tool was to replicate a double-masked free-running PAA experiment using visual stimuli with 39 participants, in more than 1600 trials, and measuring the skin conductance level and heart rate with inexpensive psychophysiological hardware and an exercise device. We divided the visual stimuli into different categories for independent analysis and comparison: the Calm category, formed by 40 images of landscapes and objects was used as control group. The Excitatory category contained both an Erotic category, formed by 10 images, and a Fearful one, formed by 5 images in the Animal-Injury category and 5 images in the Death-Danger category. The results showed evident differences prior to the shown stimuli, both in the skin conductance level and in the heart rate. In the

former, a significant difference between Calm and Death-Danger trials was found in all participants, $z = 1.73$, $p = 0.0418$, one-tailed; in the latter, there was a significant difference between Calm and Erotic trials in males, $z = -1.98$, $p = 0.0239$, one-tailed, between Calm and Fearful trials in females, $z = 1.69$, $p = 0.045$, one-tailed, and between Calm and Animal-Injury trials in females, $z = 1.77$, $p = 0.038$, one-tailed. Differences in gender were found, suggesting that fear negatively affects PAA in females while it aids the physiological prediction in males. The general difference between Calm and Excitatory images in all participants, both in skin conductance, $z = 1.45$, $p = 0.07$, one-tailed, and heart rate, $z = 0.58$, $p = 0.28$, one-tailed, were non-significant. This may be due to differences in gender perception and stimuli valence. Arousal and valence, as the orienting and defense response, seem to play fundamental roles on physiological activity in the anticipation of any event, as also happens in normal physiology studies when the stimuli are presented. Overall, we have independently replicated previous research and suggested new characteristics of the phenomena regarding the influence of the stimulus's valence, while implying that the use of inexpensive psychophysiological hardware may be used to test the body's ability to scan the future.

Where Mind Connects With Matter: Replicating the Correlation Matrix Method²

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This article describes two experimental replications of a new methodological paradigm called Correlation Matrix Method (CMM) (Lucadou, 1987). The CMM was developed by Walter von Lucadou in an effort to offer a solution to the problems of replicability and decline effect in parapsychological experiments, namely in psychokinesis. Supported by von Lucadou's Model of Pragmatic Information (MPI) and von Lucadou, Wallach, and Romer's (2007) Generalized Quantum Theory (GQT), the model correlates physical variables produced by a Markov-chain random number generator with psychological variables measured from the participant before the PK task. The method claims that correlations produced between psychological and physical variables during the experimental session considered as statistically significant should be interpreted as non-local entanglement correlations. The two experiments used the same method and were pre-registered at the KPU registry at the University of Edinburgh. Each experimental session took on average 20 to 30 minutes. Forty-four participants completed 213 sessions in the first experiment, and 105 participants contributed 200 sessions in the second. In both experiments, participants were asked to influence solely by intention a physical target shown on a computer screen that was controlled by the output of the random number generator. The psychological data was derived from button presses to reflect the subject's intention as well to fulfill the CMM requirement of organizational closure, that is to maintain the participant interacting with the physical process so the experimental session is a whole structure. As a control analysis, a permutation method was used to generate new correlations using data from each experimental variable separately. The permutation also cleanses

² We are grateful to the Bial Foundation for supporting this research with grant number 117/16.

dependencies between the existing variables. A non-parametric correlation coefficient, Spearman's rho, was calculated across all subjects and sessions to calculate the number of significant correlations at $p < 0.05$. In both experiments, the number of significant correlations produced between participant influence and the values produced by random number generator were significantly more than the ones produced by the control method. Limitations in the method and in the analyses are discussed, and suggestions for further experiments are explored.

Associative Remote Viewing Dream Project

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The present double-masked study is the first of its kind to utilize dreaming as a precognitive tool within an Associative Remote Viewing (ARV) protocol. A cohesive group of experienced remote viewers who varied in experience with intentional ESP precognitive dreaming practices took part in a year-long study that included 56 trials in which they attempted to have precognitive dreams that would enable them to produce descriptions and sketches that would match a photograph they would be shown at a future time. Five out of 7 remote viewers turned dreamers were able to consistently produce dreams at will. Their transcripts were utilized for the purpose of making predictions and wagers on outcomes of sporting events. If we focus on 17 hits out of 28 predictions, a binomial test has $p = .07$ one-tailed for the proportion of hits. Although this is not significantly better than chance, overall monetary winnings increased by 400 percent. Further, one individual dreamer had a 72 percent correct hit rate based on 13 hits and 4 misses with 20 passes while another had 16 hits and 9 misses. We discuss variables such as raising the threshold of Confidence Ranking Scores (at a CR score of 6) for dream based ARV predictions as opposed to traditional ARV predictions involving conscious application of psi based strategies; establishing spreads between CR scores for individual vs. aggregate dreamers; and other issues related to intentional ESP dreaming outside of a laboratory setting. Examples of sketches from ARV Dream Transcripts are also included.

An Exploration of the Effects of Mood and Emotion on a Real-World Working Computer System and Network Environment³

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A customized computer system and network were designed to induce anxiety in a sample of computer operators while the network was monitored for errors. The study participants were asked to complete a series of simple computer tasks in a limited amount of time. Rewards were provided to increase motivation and potentially induce anxiety. Half of the participants were part of an experimental group where the software was designed to obstruct their ability to complete the tasks by presenting barriers to their progress. A control group completed the exact same tasks, but they were unencumbered in their sessions. Both groups self-rated their anxiety during the tasks before and after the sessions. During the sessions, unknown to the participants, a computer network continuously sent messages between two computers. The network was not associated with the software or tasks being performed, but the network was monitored for errors. Additional sessions were run when no computer operator was present as an additional no-operator control condition. Hypothesis 1 predicted that sessions with participants present would produce more errors than sessions with no operators, but results were not significant ($p = 0.35$). Hypothesis 2 predicted that participants who experienced higher anxiety would produce more errors in the network than those with lower anxiety. The initial experimental and control groups did not correspond to the predicted grouping of anxious and non-anxious participants. The full sample was examined to produce two groups based on the actually reported anxiety of the participants. The two groups that were created as a result of this regrouping, anxious and non-anxious, were examined for difference in the number of errors recorded during their sessions. The group that reported higher anxiety produced more errors in the unmonitored network than the group that reported lower anxiety ($p = 0.04$, $d = 0.45$, power = 0.61). These results indicate that anxious computer operators may affect network communication, and there may be other electronic effects as a result of their emotions. Though these results are exploratory and preliminary, their strength indicates that businesses and organizations that use computer operators or provide technical support should be encouraged to consider the working environment and mood of their computer operators to avoid unintentional effects that could disrupt the network traffic and operation of computer systems.

³ This study was supported by a grant from the BIAL Foundation and preregistered with the Koestler Parapsychology Unit's Registry for Parapsychology Experiments.

Assessing the Underlying Spirituality: Development and Initial Validation of an Implicit Measure of Paranormal or New Age Beliefs⁴

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Paranormal beliefs comprise an essential element of New Age spirituality. There has been growing interest in the investigation of the personality characteristics and cognitive processes implicated in the endorsement of such beliefs. However, little is known about the *implicit* mechanisms involved in their formation and maintenance, including associations that underlie conscious awareness. There is also a scarcity of research with respondents from outside English-speaking countries, which could be of importance to determine the role of cultural factors in explicit and implicit endorsement of paranormal conceptions. The main purpose of this investigation was to develop and test an implicit measure of paranormal beliefs for use in Brazil, based on the widely used Implicit Association Test. In addition, the study also investigated whether paranormal beliefs are malleable to change in the face of counter-arguments and whether the correlation with belief change (if valid) would remain significant for the implicit measure as well. The results attest to the validity of our Spiritual Belief Single-Target Implicit Association Test. As expected, the implicit measure correlated significantly with a self-report scale of paranormal beliefs ($r = 0.45, p < 0.001$) and was unrelated to social desirability ($p > 0.05$). Additionally, the test was able to discriminate paranormal believers from disbelievers. Both explicit ($r = 0.27, p < 0.001$) and implicit ($r = 0.14, p < 0.001$) paranormal beliefs correlated significantly with a measure of the malleability of beliefs in the face of counter-arguments. The paper concludes with a discussion of the cross-cultural implications of the present findings and their relevance to other fields of research besides parapsychology, such as the psychology of religion and spirituality.

Cross-Cultural Research on Anomalous Experiences: Theoretical Issues and Methodological Challenges⁵

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A number of studies have contributed to unveil the cognitive and personality correlates of anomalous experiences (AEs), but a less discussed topic has been cultural differences. The description of AEs as possibly non-ordinary or exceptional in terms of the Western scientific thinking raises questions as to whether this description can be considered cross-culturally valid. To most researchers in this area, the premise that anomalous experiences rest upon widely distributed, universal properties is usually accept-

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⁵ This research was sponsored by grant #2015/05255-2 from FAPESP (São Paulo Research Foundation).

ed without sufficient evidence in its favor. Little research attention has yet been given to non-English speaking countries in the investigation of AEs, and few systematic comparisons between cultures have been carried out. In much the same way as other branches of psychology, anomalistic psychology has relied mainly on WEIRD (Western, Educated, Industrialized, Rich, and Democratic) samples to explore individual differences in reports of AEs. The main purpose of this essay is to critically review the scientific literature on anomalous experiences in the light of cross-cultural research. An outline of the theoretical issues arising from cultural and cross-cultural investigations of AEs are presented, and the methodological challenges and shortcomings in this research area are discussed, followed by specific suggestions to help overcome them. To pursue these objectives, we review some of the main findings obtained so far on the psychosocial and cultural aspects of AEs, as well as the existing gaps in our knowledge about the cultural underpinnings of these experiences. We begin by highlighting the complex interrelation between anomalous experiences and beliefs, and the difficulties in isolating the experience from its interpretations. Two prominent models in this regard are evaluated, the cultural source hypothesis and the experiential source hypothesis. The existing evidence in favor of these two models is surveyed, from prevalence studies of AEs in different countries to the psychological and neurophysiological correlates of these experiences across cultures. The implications of cross-cultural research on AEs to mental health issues are outlined. We conclude with a summary of the studies reviewed and proposals for future research.

The Authority Strikes Back: Considerations about the Allegedly Fraudulent “Chopper” Poltergeist Case

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In poltergeist cases that gain public attention, confessions often acquire the function of supporting socially dominant worldviews and restoring a threatened social order. It is part of phase four of von Lucadou's (1995) model of the dynamics and temporal development of RSPK cases, the so-called “suppression phase.” This last phase denotes the reaction of society as it strives to restore normality, “the order” that has been jeopardized by the alleged paranormal phenomena. In von Lucadou's considerations, the (potential) existence of “real” psi phenomena plays a crucial role and their elusiveness should be explained, whereas the concept of rational gravity by McLuhan (2010) is primarily directed to claims of such phenomena and/or judgments of such claims, irrespective of whether they actually, or possibly, occurred or not. Rational gravity means a pullback to normality by backward rationalizing. The so-called “Chopper” case (1981/1982) presented here could aptly illustrate the suppression phase of von Lucadou's model if it were actually an RSPK case. However, the social suppressive reaction was so strong that the case is only remembered as a case of fraud even in the parapsychological community. This feature makes it worth taking a closer look at it, its circumstances and development, and is a perfect illustration of the process of rational gravity. The case, investigated by Hans Bender and an assistant, brings together many characteristic problems associated with the investigation of RSPK cases: (a) the

involvement of various kinds of mass media with their specific interests; (b) the increasing competition between different actors (reporters, German Federal Post Office, criminal investigation department, justice); (c) contradictory indications and testimonies; (d) serious scientists as debunkers; and (e) an increasingly confusing mélange of motifs, confessions, explanations, and various psychological mechanisms. In addition to the epistemological interest in the phenomenology of the case and the therapeutic and medical obligation to help, the scientific investigators are confronted with further claims and demands of responsibility from other actors: the post office attempted to eliminate external or internal disturbances of the telecommunication system and restore proper functioning. The police attempted to identify troublemakers who threatened public policy. For the mass media, the case was a valuable source for the production of attractive and promotional content. The “Chopper” case was hyped up as “the most sensational ‘poltergeist’ case of the last decades” and reached an audience across national borders. The pressure on all parties was enormous because large sums of money were involved. After bringing in the trans-regional police, a quick success was needed. Even during the ten days before its operation, the pressure on the people directly involved had increased enormously. They had to prove and/or maintain their own credibility. This led to unusual behavior and finally to the detection of fraud. The two main protagonists were extensively interrogated and made partial confessions. That was enough for the prosecution to declare the case as solved. After a long legal aftermath, the culprits were sentenced and fined, despite several contradictory statements, relativizations, and revocations of the confessions as well as unknown and hardly understandable motives. With regard to the “Chopper” case, the rational gravity had almost acquired the strength of a black hole. No more doubts existed publicly, despite the long and highly complex process with which the case had developed. From a retrospective point of view, taking all the publicly available and confidential information into account, the case would be more plausibly interpreted as a “mixed case.” Many characteristic ingredients can be found with regard to the psychodynamics involved, as well as the typical development of the case. However, the extremely high level of public attention produced by the mass media caused an extremely strong and harsh reaction from the authorities in order to restore the disturbed “order of reality.” Their outrage was enormous, as well as the derisive laughter in the media. The latter were the big winners of the game—as was the public who was given good entertainment and a change from everyday life for a while. All other parties lost: the individuals concerned, whose former life was actually destroyed, the officials, and the parapsychologists.

Preliminary Analysis of Sorrat Experiment Data: Testing Hypotheses from the PK Literature

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Psychical researchers offer a standard procedure for investigating group psychokinesis (PK). Participants put their hands on a table and, with sufficient preparation, perceive that the table moves on its own, sometimes levitating. Batcheldor (1984, 1994), Owens and Sparrow (1976), Richards (1982), Hansen (2001), and Lucadou (1995, 2015) specified variables affecting group PK. These factors include

special types of belief, artifacts, ambiguity, fraud, acclimation to anomalous experience, track record of experience, focus on special narrative, rapport, and quantum processes. These formulations allow sociological theories with testable hypotheses. Within this paradigm, PK is defined as an event regarded as paranormal by observers rather than a phenomenon verified as authentic. Hypotheses were evaluated using research data from the Society for Research on Rapport and Telekinesis (SORRAT), a group founded by John G. Neihardt in 1961. He assigned his graduate student, John Thomas Richards, to take notes during all experimental sessions. The group reported rapping sounds, paranormal table movements, table levitations, poltergeist phenomena, anomalous lights, and earthquake effects. Although the PK events experienced by sitter groups proved unstable, participant accounts reveal core features, implying a unifying process. All groups accepted the idea that special characteristics facilitated PK, that most people originally lacked these characteristics, but that the characteristics could be acquired through group interaction (Batchelder, 1994, p. 103; Owens and Sparrow, 1976; Richards, 1982). Participants came to believe that most inexperienced people inhibited PK, while a small percentage facilitated it. A sociological study tested hypotheses derived from these formulations. SORRAT notes, recorded by J. T. Richards, described over 800 experiment sessions occurring between 1966 and 2007. The notes provided session date, time, participant names, phenomena experienced, positions of witnesses, flight path of levitating objects, attempts to photograph levitating objects, reaction of levitating objects to photography, participants in trance, reactions of participants during the session, and evaluations of experimental outcomes. The records indicated that, during the documentation periods, SORRAT groups consisted of between 1-25 participants with 320 different people attending sessions. Richards evaluated each experiment regarding the degree that it achieved specified goals. Within the experiment records, 742 sessions had the goal of *levitation success*, based on height and duration of the levitation, degree that participants removed their hands, time the object remained in the air, and whether an attempt was made to photograph the levitating object. Each session was assigned one of three outcomes: failure, partial success, success. Richards' notes were specific enough to allow content analysis and statistical evaluation of correlations between major variables: group size, participant names, number of sessions attended by each participant, and levitation success. Although *levitation successes* should not be regarded as proof of PK, the variable distinguished sessions with high emotional impact from those with low, a factor affecting belief. Two sociological theories were devised: (1) An *interaction theory* argued that group participation facilitates the psychological processes allowing PK. (2) A *facilitation-suppression theory* argued that some people facilitate PK while most people suppress it. Preliminary analyses focused on three exploratory hypotheses: (1) Facilitation-suppression hypothesis: Size of experimental group should be inversely correlated with levitation success since the average person inhibits PK and larger groups are more likely to contain such people. (2) Facilitation-suppression hypothesis: Individual probabilities of levitation success should not be distributed normally but be skewed, since only a small percentage of people facilitate PK. (3) Interaction hypothesis: People attending many group PK sessions should have higher levitation success rates than those with limited experience since the former have greater exposure to factors conducive to PK. The study plan includes exploratory investigations regarding experiment location and time-period, composition of successful PK groups, and changes in individual success rates over time. Analysis of the three hypotheses resulted in rejecting the null hypotheses (treating all sessions as independent and using chi-square and Pearson correlations). Future analyses will entail multilevel analyses to account for nesting of individuals, location, and time periods.

The results supported the facilitation-suppression and interaction theories. Exploratory investigations revealed that: (1) probability for levitation success varied among experimental locations, (2) probability of levitation success declined during the second and third SORRAT time periods but increased during the final era, and (3) individual success rates tended to decline, rather than increase, with participation. This result coincides with predictions derived from Lucadou's theory and reduced faith in the *interaction theory*. Alternative theories involving special people, special groups, and ritual healing, will be discussed.

The Relation between Premonitions in Dreams and Perceived Luckiness, Style of Cognition, and Absorption

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The major aim of this study was to examine the proportion of people in Argentina who claim to have had more than one premonition in a dream and to explore comparisons between them and those who report one or fewer premonitions in terms of cognitive and perceptual variables. Out of 265 questionnaires, 234 (88%) were completed. Along with demographic information, the questionnaire contained information regarding premonition experiences, beliefs about luck, locus of control, cognitive style, and absorption. Participants were classified as either *Experients* (i.e., > 1) or *Controls* (i.e., 1 or < 1). Comparisons between the two groups revealed that *Experients* were less intuitive compared to *Controls* but scored higher on absorption. However, there was no difference between the two groups in terms of belief regarding luck and locus of control. Absorption model proposes that recall dreamers who are prone to vivid and unusual experiences during the day, such as fantasy and daydreaming, will tend to have vivid and memorable dream content and more analytical (cognitive thinking style) and thus will be more likely to remember their precognitive dreams.

Tricking the Trickster: Detecting Hidden Structure in Data from an 18-Year Online Psi Experiment

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From August 2000 through October 2017, two online psi experiments based on a five-target, forced-choice protocol collected over 100 million trials from an estimated 200,000 individuals around the world. The direct hit rate combined across both experiments was consistent with a null effect, where $p_0 = 0.20$, $p_1 = 0.19996 \pm 0.00004$, $z = -0.94$, $p = 0.35$. A planned secondary analysis, designed to detect a subtle but predicted pattern in the data, resulted in a significant deviation, where $p_0 = 0.32$, $p_1 = 0.32051$

± 0.00005 , $z = 10.6$, $p < 10^{-25}$. Control tests found no evidence that this small magnitude but highly significant positive deviation was due to optional stopping, response biases, target sequence dependencies, learning of subtle cues, or other potential artifacts.

Experimenter Effect and Replication in Psi Research II: A Global Initiative⁶

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The replication issue in parapsychology was studied through the examination of experimenter and participant belief in psi and their impact on the outcome of a psi task. This series of three formal experiments employed a standardized psi protocol developed by Daryl Bem that has been the focus of several replication attempts and that allows for a systematic collection of data under well-controlled conditions. A session lasted no more than 30 minutes per participant. The replication protocol tested the retroactive priming aspect of experiment 4 of Bem (2011) by examining reaction time for congruent or incongruent pairing of words and pictures. Study 1 examined expectancies of 32 experimenters and 512 volunteers. Study 2 manipulated priming for expectancies by showing either randomized pro or anti-psi statements (for participants) and videos (for 32 experimenters), to assess the experimenters' and/or participants expectation using questionnaires with 640 participants. In this article, we report the results of Study 2. All analyses were pre-registered. Although a trend in the expected direction was observed when comparing congruent image-word pairs versus incongruent ones for all participants combined, it does not reach the 0.05 (two-tailed) significance threshold. No effect of participants' expectancy was observed.

Modeling Supernatural Belief: Cognition and Personality

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This study set out to create and test a new model of supernatural belief based on cognition and personality, which would potentially test two theories of belief in the paranormal: The Cognitive Deficits Hypothesis and the Psychodynamics Functions Hypothesis. This was accomplished by conducting

⁶ This research was sponsored by a grant from the Bial Foundation.

two studies. Study one had 179 participants. A MANOVA followed by a discriminant function analysis revealed one profile relating to cognition: *reflective thinkers*. The profile was more likely to contain *skeptics* and *believers*, and least likely to contain *paranormal believers*. Study two had 152 participants and looked at personality alongside cognition. A MANOVA followed by a discriminant function analysis revealed *sensitive and abstract thinkers* and *reflective metacognitive dogmatists* profiles. The sensitive and abstract thinkers were least likely to contain skeptics and religious believers and most likely to contain believers and paranormal believers. The reflective metacognitive dogmatists were most likely to contain religious believers and believers and least likely to contain paranormal believers. Following this analysis, structural equation modeling was used to test seven different models of personality, cognition, and belief. Studies one and two found that the skeptics and religious believers have remarkably similar profiles, indicating that the religious beliefs themselves may have been cognitively ring-fenced off in some way. The paranormal believers, however, were not reflective thinkers and were not metacognitively active, indicating that they were not aware that they were not thinking critically or analytically. The structural equation model showed that schizotypy was the main predictor of belief. The relation between belief and cognition was more complex; it was dependent on what type of belief was active. Paranormal belief required a more intuitive thinking style to be present, whereas religious belief could withstand a reflective mindset. This study provides a unique contribution to the existing literature by establishing a model that combines cognition, personality, and belief.

Saddam Hussein Remote Viewing Experiment: A Historical Contribution to Applied Remote Viewing

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This paper describes an applied remote viewing experiment carried out on November 3, 2003, with the explicit purpose of locating and describing the circumstances and conditions of Iraqi deposed president Saddam Hussein, who was then in hiding. Over a month after the remote viewing data were collected, analyzed, and operational hypotheses were developed, on December 13, 2003, Saddam Hussein was discovered by American forces assisted by Iraqi nationals. The paper describes the protocol used by 47 remote viewers, the development of operational hypotheses, and an assessment based on American government-sourced information as to the accuracy of the remote viewing session data.

Water, Wine, and the Sacred: An Anthropological View of Substances Altered By Intentioned Awareness, Including Objective and Aesthetic Effects

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This paper discusses the ancient anthropological linkage of water and wine with sacred rituals after these substances have been the focus of nonlocal perturbation. The changes produced can be both physical as well as subjective aesthetic reactions arising when individuals have a sensorial interaction with the treated substances. In making this argument the paper presents and discusses research done by others and the author, including reporting the results of a 12 part series of experiments in which groups of seven people tasted wine from one 750ml bottle that had been decanted into two identical 375ml carafes. The histories of the carafes were the same except that one, before the tasting, had been the focus of intentioned awareness by meditators, while the other was a control. Twelve sessions were conducted, 11 resulted in a majority preferring the treated wine, and one resulted in a tie. Using an exact binomial test, $p = .0005$. With 95% confidence we can say that the probability that a majority would prefer the treated wine is at least 0.76. The conclusion discusses the implications of the totality of this research.

A Qualitative Exploration of Skeptics' Experiences of the Exceptional

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Disbelief in paranormal phenomena has been relatively under-researched in the literature. This study explored how disbelievers in paranormal phenomena described exceptional experiences (ExE) that occurred in the context of a laboratory experiment that was designed to encourage them. Thirty-three strong disbelievers (as identified by scoring 2 or more SD below the mean on Tobacyk's Paranormal Belief Scale) participated in a laboratory study designed to encourage exceptional experiences (ExE). As part of another study, participants took part in three 30-minute sessions in which they were asked to relax in a reclining chair while wearing earplugs and an eye mask. On two out of three of these occasions they wore a sham head device, designed to resemble a device that has previously been associated with exceptional experiences (ExE). The participants were encouraged to verbalize any experiences they noticed and answered 4 semi-structured interview questions following the 30-minute session. Any verbalizations and interviews were transcribed and these transcriptions were retained for the analysis if they included a description of an ExE (cf. Belz & Fach, 2012). Verbal descriptions of ExE were analyzed using inductive Thematic Analysis (TA). Four themes were identified: actor-observer separation, intensified thoughts and perception-like imagery, attributions of normality, control, and emo-

tional responses. These themes indicate that a subset of skeptics is prone to ExE that included thoughts that were vivid and perceptual-like (external) sometimes akin to synesthesia. When ExE occurred, they were described in a manner that emphasized a position of logic (controlled thinking) over irrationality (automatic, emotional and uncontrolled thinking) and there was frequently an attempt to label and attribute experiences to experimental context, state of consciousness, and the workings of the brain and body with a view to normalizing them. This study complements and extends the existing literature on the psychology of exceptional experiences and the psychology of paranormal disbelief.

A Couple of Radical Ideas for Advancing On the Problem of Consciousness

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In this paper, I suggest a two-pronged proposal for advancing on the problem of consciousness. That is, in addition to an ontological explanation of consciousness, we must also confirm such a theory using objective data. On the ontological front, I propose wave function psychism; the entity behind the quantum wave function is the base of conscious experience. My proposal results from combining three strands within philosophy of mind and physics: 1) Russellian monism, 2) the ontology of the quantum wave function, and 3) Tonini's Integrated Information Theory. I argue that my proposal is able to address many of the problems that face panpsychism. But perhaps most important, this framework is consistent with anomalous data known as psi. Thus my proposal is consistent with extant anomalous data on consciousness.

Research Briefs

The Associative Remote Viewing Re-Judging Project

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Associative Remote Viewing is one of the only methods studied formally in parapsychology that has demonstrated successful application of psi in predicting future events leading to the acquisition of money. In order to understand what worked when predictions resulted in hits and what went wrong when predictions resulted in misses, a large sample size of completed ARV trials for 86 events (sports or financial events) consisting of 220 free response remote viewing transcripts were analyzed. Six independent judges (single new judges) and two teams of two judges operating under masked conditions

repeated the judging, scoring, and predicting while keeping all other variables stable. These new scores/predictions were then compared to the original scores and predictions as well as to each other. Our premise is that if we find that specific single new judge(s) or a team of two judge's predictions matched the actual outcome more consistently than the others, this will illuminate which judging methods (single vs. team of judges) and scoring methods (7 point scale vs. 3 point scale) can be considered more effective if the end goal is having higher hit rates and fewer misses. As of March 1, our experimental phase has been completed and we are now in the analysis phase with results projected to be determined by early June 2018.

Remote Viewing of Concealed Target Pictures under Light and Dark Conditions

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The belief that darkness plays a facilitating role in putative remote viewing and other psi-related phenomena is well established in esoteric and traditional beliefs (Grim, 1983; Hallowell, 1942; Lyon, 2012). However, the role of darkness in RV success is unclear beyond these esoteric explanations. This study explored the differential effect of darkness/light on purported remote viewing ability alongside the effect of time and their potential interaction. From an initial sample of 20, 7 remote viewing claimants contributed a total of 19 sessions each (nine light/ten dark) that utilized randomized target selection, free-response descriptions, and ratings by both participants and an independent judge. Although the usable data gave the edge to the dark condition performance, the difference was not significant. Due to participant attrition, the usable data were too few to provide a definitive answer to this question. On the whole, participants who left the study early reported they did not find the target pictures “engaging,” “interesting,” or “emotionally involving.” This led to exploratory post-hoc analyses concerning the numinosity of target images, to determine if this characteristic was associated with success. For the numinosity ratings of target images, a mean difference of 11.24, 95% CI [.12, 22.3] was shown as significant, with the target images of participant hit sessions containing higher numinosity ratings than unsuccessful miss' sessions, $t(11.47) = 2.22, p = .048$, with a large effect size, $d = 1.02, [.01, 1.99]$. No significant difference was found for target's numinosity ratings between independent judge hit and miss sessions. These findings suggest a response bias with participants inclined to select a more numinous target regardless of if it is the target or decoy, which should be investigated by future studies. To our knowledge this analysis is the first attempt to directly evaluate the degree of target numinosity's effect on attempted remote viewing success. The findings may have implications for the use of participant judgment in future remote viewing research. Furthermore, because there are several advantages to what parapsychologists refer to as free response targets as opposed to forced choice targets (Honorton, 1975), this finding may have implications for the selection of target material in future parapsychological research.

Exploring the Correlates and Nature of Subjective Apparitional Experiences

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Several studies have explored the correlates of exceptional experiences (ExE). However, more research is needed on the correlates and nature of different types of experiences. In addition, recent trends in psychology and parapsychology include the application of qualitative methods to various types of ExE. This project explores the psychology of subjective apparitional experiences in an online survey investigation using Qualtrics. A series of questions asking about subjective apparitional experiences (employing neutral language) was developed from existing measures. These questions were presented with response options that allow for different forms of appraisal such that the tendency to experience can be explored separately from the tendency to consider the experiences as paranormal (following Irwin, Drinkwater, and Dagnall, 2013). Additional questions asked about ways of experiencing and the nature of the experiences. A battery of individual difference measures included evaluations of creativity; synesthetic experiences; locus of control, the revised Transliminality scale; Hartmann's BQ a measure of hyperaesthesia and two measures of body awareness (interoception and a measure of somatic focus). We plan to analyze data using quantitative and qualitative methods. A difference test will explore whether synesthetes differ from non-synesthetes on subjective apparitional experiences; correlates of subjective apparitional experiences will be explored, and open-ended questions will be analyzed qualitatively (using grounded theory). The survey will also identify participants who will be invited to participate in a psychomanteum study at a future date.

Investigating an Unconscious Physiological Precognitive Response to Schematic Face Stimuli Using EEG

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This study is intended as a replication of a series of experiments designed and performed by Jacob Jolij (Jolij, 2015), and is being pre-registered with the Koestler Parapsychology unit as a confirmatory experiment. The Jolij team had been performing unrelated research on electroencephalographic (EEG) responses to a schematic face randomly embedded in static noise and analyzing its post-stimulus response. However, they were aware that other laboratories have reported findings in which physiological effects before random events could predict these events above chance levels, with the pre-stimulus events mimicking the post-stimulus events with a weaker effect size (Mossbridge, Tressoldi, & Utts, 2012). A later meta-analysis of experiments conducted after those covered by the first meta-analysis

showed a larger effect size (Duggan & Tressoldi, 2018). Due to the research conducted by Mossbridge, Tressoldi, & Utts (2012), the Jolij team analyzed their pre-stimulus data for any precognitive effects and found a significant effect. This experiment aims to replicate Jolij's results. We hypothesize that the pre-stimulus EEG response will be predictive of the random stimulus at a significant level. We also hypothesize that the directionality of the pre-stimulus effect will be the same for the post-stimulus effect, but with much smaller pre-stimulus effect size.

Precognitive Priming of Compound Remote Associates: Using an Implicit Creative Insight Task to Elicit Precognition

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Precognition encapsulates the notion that something that occurs in the future can influence cognition and/or behavior now. However, a number of recent research projects examining this aspect of parapsychology have failed to produce consistent results. Nevertheless, some light may be shed on these disparate findings by a recent meta-analysis that suggests that what may be considered fast/implicit type tasks are more able to elicit such effects as compared to slower/explicit type tasks. Given this, and the proposed link between individuals' creativity and their ability to exhibit psi type behaviors, the current study attempted to elicit a precognitive effect using a creative insight task. The task involved presenting participants with three words and requiring them to come up with a fourth related word. This compound remote associates task is a standard test for creative insight. Here, the added twist was that participants were primed with the correct answer to half the items *after* they had completed the task. The prediction was that participants would be more accurate when completing the compound remote associates for which they were later primed compared to those not primed. The results showed no evidence of a precognitive priming effect. The lack of such an effect could parsimoniously be interpreted as suggesting that precognition is impossible, but there remain many positive effects reported in the literature. Hence, a plausible alternative explanation offered suggests that the context (lab-based vs. online) of the research may influence the outcome.

Testing Precognition and Altered State Of Consciousness with Selected Participants in the Ganzfeld: A Pre-Registered Study

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This study was the first to contribute to a registration-based prospective meta-analysis of ganzfeld ESP studies (Watt, 2017a, b; Watt & Kennedy, 2016, 2017). We sought to maximize the anticipated psi

effect size by selecting participants on one or more of the following criteria: self-reported creativity; prior psi experience or belief; and practice of a mental discipline. Psi targets and decoys were short video clips randomly selected with replacement from a pool of 200. We employed an automated precognition design for simplicity and security, and to add to the small database of precognitive ganzfeld studies. The experiment was designed by CW and the computer program was written by AT. As well as predicting overall significant precognition task scoring, we tested the assumption that the ganzfeld method elicits a psi-conducive altered state of consciousness. Based on Cardeña and Marcusson-Clavertz's (2017) findings, we predicted that higher target similarity ratings (measured by session z-score) would be associated with greater evidence of ASC during the session, measured using the Phenomenology of Consciousness Inventory and a time estimation task. Procedure. Three experimenters (ED, AP, & HR) each conducted 20 trials. The participant reclined wearing a red eye-shield so that they perceived a uniform red visual field. Headphones first played a 9-minute progressive relaxation exercise, then played white noise for 25 minutes. The participant reported their impressions aloud and these were recorded. After the impression period ended, the participant estimated the time duration of the session. The experimenter then reviewed the participant's impressions and, when ready for the judging phase, the experiment program was progressed to randomly select one target pool. The participant rated each target for similarity to their mentation on a 1-100 scale (where 1 = no correspondence). After the participant's ratings were submitted, the PCI was completed. Finally, when the participant was ready to view the conclusion of the session, the program was advanced and the precognitive target clip was randomly selected and played to the participant for feedback. Results. Twenty-two direct hits were obtained out of 60 trials, corresponding to a statistically significant 37% hit-rate. Therefore, our hypothesis that the randomly selected future target would be identified to a greater than chance degree was supported. Contrary to prediction, no significant relation was found between measures of ASC and session Z-scores. We conclude that further ganzfeld ESP research is justified because previous extensive research with the ganzfeld has identified moderator variables that can optimize effect-size and because the method maps on to common features of spontaneously reported paranormal experiences.

Workshop

Parapsychology and Transpersonal Psychology in Dialogue

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Parapsychology and transpersonal psychology were founded independently and have evolved separately as two distinct movements, although there is considerable overlap in both their content and in the interests of a number of scholars who are active in both areas. Harris Friedman, co-president of

the Association of Transpersonal Psychology, and Dean Radin, President of the Parapsychological Association, engaged in an informal discussion on the salient commonalities and differences between the two movements. Their focus was on exploring ways that the two could be brought into better alignment, such as including more transpersonal approaches within parapsychological studies and vice versa. Harris delineates the two areas by separating between extraordinary events and extraordinary experiences, seeing the former as more characteristic of parapsychology, and the latter as more characteristic of transpersonal psychology. Harris also sees them as typically using different methods, viewing parapsychology as tending toward more use of quantitative approaches vested in mainstream positivistic science, and transpersonal psychology as tending toward more use of qualitative approaches vested in a human-science paradigm. However, Dean thinks the main difference between the two is that parapsychology has focused on experimental methods (largely quantitative, but also qualitative) designed to explore the nature of both extraordinary events and experiences, with a goal of determining if they are what they seem to be and, if so, how they work. Dean also sees transpersonal psychology as largely focused on the psychological implications of these experiences for human mental (and possibly physical) health, and for what such experiences imply about human potential. Stanley Krippner, whose seminal work straddles across other areas, chaired the workshop, introducing and serving as a discussant for Harris and Dean's views, as well as presenting his own views on the relation between parapsychology and transpersonal psychology.

Posters

Parapsychology and Buddhism: Interviews with Vipassana Meditation Practitioners across India

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The analysis of Pali canon's understanding of psychic powers was one of the main phases in my PhD research at the University of Northampton. The main categories and themes found in the Pali canon analysis led me to understand psi better. The analysis showed the theme of merits or good deeds as the base for developing psychic powers. Based on that analysis further evidence gathering was important with living examples of meditation practitioners. I chose to interview 15 Vipassana meditators from India around the Aurangabad, Mumbai, Nashik, and Pune region of Maharashtra state. These interviewees are serious Vipassana meditation practitioners as taught by S. N. Goenka under the tradition of Sayagi U Ba Khin. This research is being done on the Theravada Buddhist scriptures known as the Pali canon. The follower of this tradition is strictly expected to observe the five precepts and meditation routine of at least two hours daily sittings. The participants involved in the interviews had at least 5 years of experience in this tradition. The aim of this paper is to emphasize the relevance of the theme of merits

as found previously in a Pali canon analysis that can come into operation in psi experiments, along with the notion of Act of Truth.

Are People Conscious of Scopaesthesia? Do the Number of Starers and the Introduction of Acoustathesia Affect Hit Rates? A Pilot Investigation

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Scopaesthesia is derived from the Greek words for thinking and knowing. In this paper, the phenomenon of Acoustathesia, the idea that individuals are able to sense when they are being spoken about has been introduced, with the hope that this may increase the hit rates produced by participants. The influence behind the idea of including the phenomenon of Acoustathesia is based on research produced by Friday and Luke (2017). In this research the idea of being able to sense when someone is staring at you has been linked with the idea that it is possible to feel when you are being spoken about. The hypothesis behind the current study is that when an individual is being spoken about and stared at they are able to sense this more strongly and pick this up more often. It has also been hypothesized that individuals with higher levels of Paranormal Belief will produce higher and more accurate hit rates. The ability to feel when someone is staring at you and to feel when you are being spoken about has been said to be evolutionarily advantageous, which would suggest that in the past survival may have been dependent on these abilities. The results of this study showed a marginal positive correlation between the mood of the participants after the study and the hit rates scored by participants and their confidence that they would contribute to the study. This means that the higher the mood of the participant the higher their hit rates and the higher the confidence of the participant, the higher their hit rate. Paranormal belief levels did not significantly correlate with the hit rates produced by the participants nor did a stare condition. There was no significant difference between the hit rates scored by the participants within the group stare or single stare condition when compared to chance.

Anomalous Experiences Reported by Nurses: Second Study Examining Personality, Perceptual and Cognitive Factors

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The aim of this study was to determine the degree of occurrence of certain unusual perceptual experiences in hospital settings often related by nurses. Three hundred forty-four nurses were recruited from 36 hospitals and health centers in Buenos Aires, grouped as 235 experiencers and 109 non-experiencers. The most common experiences were sense of presence and/or apparitions, hearing noises,

voices or dialogues, crying or complaining, intuitions, and extrasensory experiences. In relation to the experiences of their patients, the most frequently reported were near-death experiences, religious interventions, and many anomalous experiences in relation to children. The rationale of the present study was to confirm the early findings for work stress and absorption with a bigger sample of nurses (Parra & Gimenez Amarilla, 2017) and measuring schizotypy proneness and empathy skills. The initial finding that nurses who report anomalous experiences tend to score higher on work stress was not confirmed. However, experiencers scored higher on a depersonalization factor when compared with non-experiencers. Nurses reporting these experiences tended to score higher on absorption and proneness to schizotypy, mainly “positive” schizotypy (unusual experiences). Also, they reported higher cognitive empathy and emotional comprehension than non-experiencers.

Invited Addresses

Parapsychology as an Essential Component of an Expanded Science of Mind: Promises and Challenges

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In the course of more than half a century of work in the psychology of consciousness and the development of transpersonal psychology, I have become convinced that practical deployment of psi abilities could be used to enrich a science of the mind *per se*, that we could, as it were, put the psyche back in psychology. It will be difficult, but I want to share this vision. Early historical attempts to develop this as a kind of mental chemistry foundered on six major, unaddressed issues, viz. ignoring individual differences, experimenter bias, culture boundedness, insufficient training in introspection, a dominance of materialism that marked introspection as inherently inferior, and a rigid assumption of the inherent privacy of mental events. The development of practical, intelligence-gathering applications of psi, remote viewing in the last few decades indicates that, if remote viewing were applied to multi-observer psi observations of others' mental processes, the inherent privacy of mental events could be greatly reduced and a more probabilistically useful, if not exact, understanding of mind could be developed, including normally “unconscious” processes. Building on the accomplishments of methodologically sophisticated technical parapsychology, a deeper psychology and transpersonal psychology could allow relatively scientific refinement of experiences and knowledge quite important to people, but now generally isolated behind barriers of rigid beliefs and prejudice.

The Software of Consciousness: Intriguing Lessons and Lingering Puzzles on the Far Side of the Stargate

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The advent of global computer communications and social networking presents the parapsychology community with novel opportunities for wide-ranging experimentation. These opportunities come at several levels: (1) Research can be extended in real or differed time around the world, eliminating many financial and logistical concerns with travel and coordination. (2) A higher degree of accuracy in data capture is assured through the network's precise text and time recording. (3) Spontaneous phenomena having to do with coincidences, distance perception, and non-locality can be unobtrusively captured and studied well beyond the anecdotal quality of current practice. And (4) much greater security and protection against fraud can be provided, verified, and audited. The parapsychology community has begun taking advantage of the new tools in a number of ongoing studies where current practices can be extended. Beyond these practical considerations of the technology, however, new phenomena have also been frequently noted by software teams coding the systems and they could be profitably reviewed. They date back to the very early days of the Arpanet (circa 1975) when we developed the first social network for use by research and industrial organizations. Our observations forced us to think of the software systems we were building as tools that superseded both space and time. Since remote viewing presented itself as a convenient experimental scheme, we applied it to a series of innovative tests whose results could be re-purposed today in the current environment of widely-available, inexpensive social networks. To that end, we review here the experimental setup that was used to assess the impact of the technology on the perceptions and behavior of our users of the new software; we note the phenomena we observed during these tests; and we project these observations into proposals for the exploitation of social networking as a convenient new instrument for the study of social and individual consciousness.

Eleanor M. Sidgwick (1845-1936)¹

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Abstract: Eleanor M. Sidgwick was an important figure in the early history of the Society for Psychical Research (SPR). In later years she became known for her critiques of the evidence for physical mediumship, and for her systematic studies of apparitions of the dead and hauntings, premonitions, clairvoyance, and the mediumship of Leonora E. Piper. Sidgwick also made significant contributions to the study of spontaneous and experimental telepathy, the cross-correspondences, and book tests, or attempts to get spirit communicators to obtain veridical information from the content of a book.

Keywords: history of parapsychology, Sidgwick, mediumship, telepathy

Mrs. Eleanor Mildred Sidgwick was one of the most productive psychical researchers of the early Society for Psychical Research (SPR). Born Eleanor Mildred Balfour in 1845, at East Lothian, Scotland, she was part of the influential Balfour family. In addition to the psychical research work, which is the topic of this note, Sidgwick participated in some of the physical experiments of her brother-in-law, John William Strutt, 3rd Baron Rayleigh (e.g., Rayleigh & Sidgwick, 1883), and was very active in the university education of women. She was Treasurer, Vice-Principal, and Principal at Newnham College. Sidgwick married Henry Sidgwick in 1876, and shared with him deep interests in women's education and in psychical research (for biographical information see Johnson, 1936; E. Sidgwick, 1938).

Sidgwick was involved in psychical investigations before the SPR was founded. Together with some close associates, among them Edmund Gurney, Walter Leaf, Frederic W. H. Myers, and Henry Sidgwick, she had séances with several physical mediums during the 1870s (Sidgwick, 1886b). They included Annie Fairlamb, Anna Eva Fay, Kate Fox (then Mrs. Jencken), Mary Rosina Showers, and Catherine Wood. But the results of the séances were not in favor of the genuineness of the phenomena and led the group to a general feeling of skepticism. Sidgwick commented at the end of the article:



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I feel bound ... to admit that by far the larger part of the testimony put forward as affording solid ground for a belief in them, which I have been able to examine, is of such a nature as to justify the contempt with which scientific men generally regard it; and though it is to be regretted, it is hardly under the circumstances to be wondered at, that this contempt is hastily extended to the whole of the testimony. If what I have written should contribute, in however small a degree, to the improvement of the evidence on this subject in the future, I shall feel that it has not been written in vain. (Sidgwick, 1886b, p. 74)

In her article about Spiritualism for the ninth edition of the *Encyclopaedia Britannica* Sidgwick returned to the issue of physical mediumship and stated that most witnesses “do not seem to have duly appreciated the possibilities of conjuring, nor to have taken sufficient precautions to exclude it” (Sidgwick, 1887, p. 406). She continued to present such views in evaluations of the phenomena of spirit photography and the performances of William Eglinton (Sidgwick, 1886a, 1891b), the cause of several internal SPR controversies (Gauld, 1968).

In later years Sidgwick continued her critiques of physical mediumship, representing the skepticism of many about physical phenomena, and upholding accurate reporting of the conditions of research. Two examples are her reviews of the investigations of Enrico Morselli with Eusapia Palladino (Sidgwick, 1909) and of W. J. Crawford with the Goligher Circle (Sidgwick, 1917). Some of her book reviews also represented a conservative and sometimes necessary reminder of ambiguities in popular publications (Sidgwick, 1918).

Many of Sidgwick’s main contributions showed what Alan Gauld (2018) has referred to as her “gift for subduing and organizing large quantities of refractory material.” An initial one was her evaluation of about 370 cases the SPR had collected about apparitions of the dead, some of which were haunting cases (Sidgwick, 1885). This included: 1) an examination of conventional explanations; 2) an evaluation of patterns in the cases or lack of them; and 3) a consideration of ideas to explain genuine apparitions. The amount of detail and critical analysis presented by Sidgwick had no precedent in the previous literature examining apparitions of the dead, a situation similar to her later analyses of cases of premonitions (Sidgwick, 1888) and clairvoyance (Sidgwick, 1891a).

She also showed great care in her analyses of published cases of “phantasms of the living” (Sidgwick, 1923), and of the psychological aspects of Mrs. Leonora E. Piper’s mediumship (Sidgwick, 1915). Some of the chapters of this study were about spirit controls and their relation to the medium; language, memories, and association of ideas by the spirit controls; aspects of various spirit communicators (such as difficulties in communicating and symbolic statements), and relations between Piper’s different states of consciousness. She concluded about the medium’s trance that it was “probably a state of self-induced hypnosis in which her hypnotic self personates different characters either consciously and deliberately, or unconsciously” (Sidgwick, 1915, p. 330), but with telepathically acquired information. Sidgwick discussed mental mediumship in several other papers, including papers about cross-correspondences, book tests, and specific séances (see, respectively, Sidgwick, 1910, 1921, and Sidgwick & Piddington, 1909).

Furthermore, Sidgwick studied telepathy. She conducted experiments (Sidgwick & Johnson, 1892; Sidgwick, Professor [H.], Sidgwick, Mrs. H., & Smith, G. A. (1889), some of which she presented at the

1892 International Congress of Experimental Psychology held in London (Sidgwick, 1892). In an overview of the topic, published in the *Dictionary of Philosophy and Psychology*, Sidgwick concluded that “there is a body of evidence of various kinds for the existence of telepathy which cannot be ignored; but, as yet, little has been done towards discovering the nature of the process or the conditions under which it occurs” (Sidgwick, 1902, p. 672).

Sidgwick herself contributed to knowledge about the process in an analyses of its “hindrances and complications” as shown in percipient’s impressions (Sidgwick, 1924). She wrote:

I have now, I think, sufficiently shown that there are obstacles or at any rate difficulties in the way of telepathic transmission which easily may, and in fact often do, interfere with the process, and prevent a “message” being received as the sender intended. Apart from difficulties on the agent’s own side, and even when a message has apparently safely reached some part of the percipient’s mind, it may fail to pass successfully from that to the normal waking consciousness. And this not only because the impression is sometimes too feeble to prevail, but because as transmitted to the normal consciousness the latter may fail to interpret it. And the difficulties may be aggravated by differences in the results, according as different modes of externalisation—different methods of transferring the subliminal impression to the normal consciousness—are used, and even by deliberate invention in the subliminal mind. (Sidgwick, 1924, p. 68)

In a lecture about the history of the SPR (Sidgwick, 1932), Sidgwick stated that she thought there was “occasional communication of an evidential kind between the living and the dead” (p. 26). The paper was read in absentia by her brother Lord Balfour, who said to the audience at the end of his presentation that Sidgwick had authorized him to state to the audience that she believed in survival and in spirit communication.

Alice Johnson (1936) commented on the great amount of work that Sidgwick had done for the SPR behind the scenes. This included very time consuming editorial work related to the Society’ Journal and Proceedings, as well as working with the committee behind the Census of Hallucinations (Sidgwick, Johnson, Myers, Podmore, & Sidgwick, 1894). According to Johnson the massive report of this work “was written chiefly by Mrs. Sidgwick, who also worked out the statistical calculations” (Johnson, 1936, p. 67).

I suspect there are different evaluations of the value of Sidgwick’s work. Believers in the physical phenomena she criticized felt she had a hypercritical stance. An example was the article the medium William Eglinton wrote about Sidgwick, to which was appended a great number of letters from other individuals defending the medium or reporting having witnessed events that could not be accounted for by fraud (Eglinton, 1886). Others, such as Edith Lyttelton, praised her “eagle eye for any possible inaccuracy or faulty corroboration” (Lyttelton, 1936, p. 21). There is no question that Sidgwick had an immense capacity for analyzing large quantities of data to find weaknesses and patterns in reports of psychic phenomena, and that her behind the scenes organizational work was very important for the development of the SPR.

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Eleanor M. Sidgwick (1845-1936)

Eleanor M. Sidgwick était une figure importante de l'histoire de la Société de recherche psychique (SPR). Au cours des dernières années, elle a été reconnue pour ses critiques des preuves de la médiumnité physique, pour son étude systématique des apparitions des défunts et des hantises, des prémonitions, de la clairvoyance et de la médiumnité de Leonora E. Piper. Sidgwick a également fait des contributions importantes à l'étude de la télépathie spontanée et expérimentale, aux correspondances croisées et aux tests de livre (ces essais pour obtenir des informations véridiques de la part d'un esprit à partir du contenu d'un livre).

Eleanor M. Sidgwick (1845-1936)

Eleanor M. Sidgwick war eine wichtige Figur in der frühen Geschichte der Society for Psychical Research (SPR). In späteren Jahren wurde sie bekannt als Kritikerin des Beweismaterials in Bezug auf den physikalischen Mediumismus sowie für ihre systematischen Untersuchungen der Erscheinungen Verstorbener und Spukerscheinungen, von Vorahnungen, des Hellsehens und der Medialität von Leonora E. Piper. Von Sidgwick stammen auch wichtige Beiträge zur Untersuchung spontaner und experimenteller Telepathie, zu Kreuz-Korrespondenzen und Büchertests oder zu Versuchen, mit Hilfe jenseitiger Kommunikatoren zutreffende Informationen über den Inhalt eines Buches zu erhalten.

Eleanor M. Sidgwick (1845-1936)

Eleanor M. Sidgwick fue una figura importante en la historia temprana de la Society for Psychical Research (SPR). En los últimos años se hizo conocida por sus críticas de la evidencia de la mediumnidad física, y por sus estudios sistemáticos de apariciones de muertos y "casas encantadas," premoniciones, clarividencia, y la mediumnidad de Leonora E. Piper. Sidgwick también contribuyó significativamente al estudio de la telepatía espontánea y experimental, las correspondencias cruzadas, y la prueba de libros, que se refería a que los comunicadores espirituales pudieran obtener información verídica del contenido de un libro.

Training Anomalous Cognition in a Motor Task with Subliminal Auditory Feedback

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Abstract: On each of 60 trials, 5 participants (Ps), selected based on high state and trait dissociation scores in a previous motor automatism experiment, explored with a computer pen a 16x16 inch grid affixed to a computer writing tablet, stopping to register a response to a randomly selected target square. The grid is conceptually divided into 16 squares (4 in each of 4 quadrants). The dependent variable was the average of 2 z-scores representing square and quadrant hits. Ps attended 2 1-run baseline sessions and 2 1-run test sessions. In between, they completed 15–20 1-run training sessions with subliminal auditory feedback. The feedback stimulus was the spoken word(s) “good” (quadrant hit) or “good good” (square hit) superimposed on brownian (similar to pink) noise. 1 of the 5 Ps significantly confirmed the hypothesis of higher scoring on test than baseline runs. There was significant or suggestive evidence of anomalous cognition in the baseline and/or test results of 4 Ps and the 5 difference scores showed significant between-subjects variability. There was no evidence of learning in the training sessions. According to the underlying theory, conditions for learning were not met because Ps did not successfully blank the mind and were overly attentive to the feedback sounds.

Keywords: anomalous cognition, motor automatism, subliminal, feedback, dissociation

The general hypothesis tested in the overall research program is that psi is facilitated by dissociated states of consciousness and that the most dissociated form of psi expression is motor automatism, such as automatic writing and dowsing, where conscious cognitive processing is minimized. Motor automatisms are similar if not identical to what nowadays is referred to as psychomotor behavior, but I will continue to use the parapsychological term.

The first experiment to test for anomalous cognition (AC) using motor automatisms was by Brugmans (1922), who had a special participant (P) point to a square with a letter-number on a grid while blindfolded, with the hope that he would point to the randomly selected target for the trial. Highly significant results were obtained, but the randomization method was poor. One of the card-guessing methods used in J. B. Rhine’s famous card-guessing experiments was “screen-touch matching,” in which P pointed to one of five “key cards” representing the five Zener cards symbols. This technique was used in the prominent and controversial Pratt-Woodruff experiment (Pratt & Woodruff, 1939). In the 1990s,

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Palmer conducted a series of six experiments in which the responses were eye fixations on one of a matrix of symbols flashed on a screen for 150 ms (Palmer, 1992, 1993, 1994, 1995, 1996; Palmer & Johnson, 1991). The AC task or individual trials were preceded by subliminal presentations of pictures or words that were usually emotionally evocative. Results were inconsistent and significance spotty, but when overall significant results were obtained in the experimental subliminal conditions, they were either tight variance or AC-missing.

The most relevant studies to the current endeavor were modeled on the Ouija board. Sargent (1977) reported two experimental series in which Ps played with a Ouija board mostly unaware that an ESP test was involved. The characters on the board had been arbitrarily divided into odd and even categories, the response sequences being compared to random sequences of binary digits. In both series, below-chance scoring was obtained when the sequences of characters from the board were “structured” (e.g., meaningful words) and above chance when they were unstructured, with the difference being statistically significant each time.

Palmer (2011) appealed to the Ouija board concept in a free-response AC experiment, substituting an “alphabet board” to avoid the occult connotations of the Ouija board. Ps were instructed to repeatedly move a pointer randomly around the board until they felt the impulse to stop on a particular letter, at which time they were to record the letter on a notepad. The targets were one-syllable homographs divided into 20 sets of 5 each. At the end of the session, Ps were asked to rate the correspondence of each homograph in the target set to the letters they stopped on, as well as any imagery they experienced during the session. The differences between the ratings of the target and control words were converted to z-scores. The main finding was highly significant hitting among a subgroup of Ps who claimed they felt their hand being moved by an outside force 1–40% of the time during the session.

In a follow-up experiment (Palmer, 2017), 80 volunteers completed the Dissociative Processes Scale (DPS). A Ouija board analog was created consisting of a 16-square grid divided into 4 quadrants with each square in the quadrant numbered 1 to 4. P could get a hit on square ($P = 1/16$), quadrant ($P = 1/4$), or number ($P = 1/4$). For each of 36 trials one square was randomly selected as the target. Ps indicated their response by stopping on a square for 1 s. To lay the groundwork for dissociation, the task was preceded by a progressive relaxation exercise, followed by suggestions for dissociation and success in the task. The independent variables in the 2 x 2 factorial design were the hand used to move the pen and one of two methods to facilitate dissociation by getting the conscious mind “out of the way” during the task — (a) keep the eyes closed and blank the conscious mind; (b) distract the conscious mind by reading quotations on a computer screen. There was significant AC hitting for location (square or quadrant hit) in a combined 3 of the 4 conditions. In these conditions, location hits were significantly higher among Ps who reported that they experienced their hand being moved by an outside force sometime during the task (state dissociation) and high scores on a subscale of the DPS (Detachment).

The purpose of the present experiment was to select promising participants from Palmer (2017) and give them long-term training using the motor-automatism technique applied in that study. The motivation to follow up with a training study had to do with my belief that in order to demonstrate the reality of psi to

the mainstream scientific community, it is necessary to increase the strength, and especially, the reliability of psi in the laboratory. It seems to me that the best way to do this is through training of psi ability, and such attempts should be made, even if they are “long-shots,” as this experiment arguably was.

Previous psychological studies of automatic writing (esp. Solomons & Stein, 1896) suggest that much more practice is needed to master motor automatism techniques than I could provide in the practice period of Palmer (2017). In addition to practice, I thought immediate feedback of hits might improve scoring through the well-documented principle of operant conditioning. The few published feedback-training studies of AC have yielded mixed results, and it is often not clear if superior post-training performance was due to learning (Braud & Wood, 1977; Honorton, 1970, 1971, Jackson, Franzoi, & Schmeidler, 1977; McCallam & Honorton, 1973, Tart, 1976; Tart, Palmer, & Redington, 1979).

As Tart (1976) pointed out, the operant conditioning approach presupposes that there are genuine psi hits to be reinforced. This is why I selected the participants for the present study based on their results in Palmer (2017). Because the 36-trial forced-choice task was too short to be a reliable indicator of psi ability, I chose P's capacity to enter what according to my theory is a psi-conductive (i.e., dissociative) state during such a task. I made three additional assumptions about the conditioning process. First, psi would be operative on only a small percentage of the trials, as evidenced by the performance of the best forced-choice test subjects in the history of parapsychology. Second, activation of the psi process is accompanied by an internal cue or sensation that the participant can identify, at least unconsciously. The feedback informs the participant that the internal cue is associated with hitting. Third, once the cue has been identified with hitting, the participant will “learn” to wait for it before stopping the pen. As multiple presentations of the cue are necessary for the participant to associate it with hitting, the experiment had to include a large number of trials, which had to be spaced over multiple sessions to avoid fatigue.

I used subliminal auditory feedback for two reasons. First, supraliminal feedback would interfere with the blanking of the mind that I consider to be necessary for detection of the internal cue. Second, that subliminal feedback interacts with mental activity at the unconscious level fits the main assumption underlying my research, namely, that AC is a function of the unconscious mind, which performs this function best when it is dissociated from and unhindered by the conscious mind.

Most of the evidence for effects of subliminal stimulation or priming on cognition or behavior has involved visual stimuli (Bornstein, 1989, Bornstein & D'Agostino, 1992), and Bem (2011) used apparently subliminal primes to significantly influence picture preferences retroactively in an AC adaptation of the mere exposure effect. Although auditory subliminal stimulation has been used much less frequently than visual, evidence of its effectiveness has been demonstrated in several studies (Kouider, de Gardelle, Deheune, Dupoux, & Pallier, 2010; Mitchell, 1993; Sloan, 1996; Zenhausern, 1974). A study reporting the effects of subliminal auditory messages on autonomic nervous system (ANS) measures (Borgeat & Goulet, 1983) is especially relevant because ANS activity has been shown to mediate psi in numerous AC presentiment experiments (e.g., Bierman & Radin, 1997).

The specific aim of the present study was to test the following hypothesis pre-registered with the Koestler Parapsychology Unit Registry: Scoring on the AC task by the 5 Ps, both individually and collectively, will be significantly higher after training than before training.

I did not make a hypothesis about the effect of subliminal feedback on AC scores because I did not think there was a sufficient empirical or theoretical basis to justify a formal hypothesis (see Palmer, 2016). This aspect of the study was purely exploratory.

Method

Participants and Experimenter

Five participants (Ps) were drawn from a pool of 80 persons who completed the author's previous successful motor automatism experiment (Palmer, 2017) and were considered most likely to succeed in the training based on the following criteria: (a) availability for testing over a continuous period of at least 2 months; (b) comfort with computers; (c) a positive response to the outside force (state dissociation) question in Palmer (2017); (d) a score of 20 or above on the DPS Detachment subscale.² Persons who met these criteria were prioritized according to (a) the percentage of time they felt their hand was being guided by an outside force; and then (b) the extremity of their deviation score from mean chance expectation on the AC test.

I (John Palmer) was the sole experimenter in the study. A male, I was 71–72 years old during data collection. I consider my personal “belief” in psi as a paranormal phenomenon to be 4 on a 1–5 scale, 5 being most positive.

Test Protocol

Relaxation exercise and instructions. Immediately prior to the AC task, Ps listened to a taped progressive relaxation exercise adapted from Jacobson (1938/1974). This was followed by suggestions for success at blanking the mind and getting a high score on the task, as well as a brief review of the task procedure:

After you have placed the pen in the proper location, close your eyes, blank your mind, and begin moving the pen around the grid. Be sure to explore the entire grid, which means that you will occasionally bump into the barrier, which is quite okay. Your ESP will let you know where and when to stop the pen for a moment, which is how you will make your ESP response. Where the pen is pointing at this time will correspond to the location of the target for the trial. After the brief pause, resume moving the pen around the board until your ESP correctly tells your hand where to stop for the next trial. Keep your eyes closed and your mind blank throughout the ESP task.

The exercise was recorded with my voice and saved as a .wav file. The file was played through external speakers in the baseline and test sessions and through the computer's speaker in the training sessions.

Apparatus and target selection. The AC task was completed on the same type of computer writing tablet used in Palmer (2017). The target area was again a 16” square grid conceptually divided into 16 1” squares classified further as 4 quadrants of 4 squares each, but there were no number targets. As the theme

² I used the DPS in the previous study because it is geared to nonclinical populations and has items that reflect the kind of dissociation I was interested in (Palmer, 2017). Data on the psychometrics of the scale (e.g., reliability, norms) can be found in Watson (2003).

was described as map dowsing, the target area was covered by a landscape photo map of barren terrain with no vegetation, and there were no visible lines representing the squares. Strips of balsa wood $\frac{1}{4}$ " high were pasted along the outer edges of the target area to keep the pen inside the area during the task.

For each trial, one square was randomly selected as the target using the thoroughly validated algorithm of Marsaglia and Zaman (1987). Participants could get a square hit ($P = 1/16$) and a quadrant hit ($P = \frac{1}{4}$). The dependent variable consisted of z-scores representing an unweighted average of square and quadrant hits.³ They are referred to as "location zs."

Response procedure. The eyes-closed method was used throughout because it is simpler than the quotations method and the two methods worked equally well in generating location hits in Palmer (2017). All Ps performed the task with their dominant (right) hand. For each trial, Ps began by exploring the target area with the computer pen ("dowsing rod") lightly touching the surface. When ready to make a response, Ps stopped the pen for 1 s. This response was registered by the tablet and sent to the computer for storage. Ps then resumed moving the pen for the next trial. During the task, Ps attempted to blank the mind with eyes closed, not looking at the writing tablet. An audible tone signified the end of the task.

Pre-Training (Baseline) Sessions

The two baseline sessions took place at the Rhine Research Center (RRC). They were on separate days but no more than 1 week apart. At the beginning of the first session, Ps read and signed the consent form, which included a description of what they would be doing in the experiment. They then completed two baseline runs, each consisting of 60 trials. The first run was preceded by however many practice or "warmup" trials Ps needed to convince me, based on my observing them, that the mechanics of the procedure were overlearned and automatic. This never took more than about 20 "trials," in part because they had learned the technique through their participation in Palmer (2017). When Ps were listening to the relaxation exercise and doing the AC task, I was in an adjacent room with the door closed, reading. Ps knocked on the door when the AC task was complete, at which time I gave them a rating scale containing the outside force questions, as well as questions about their expectations for a high AC score, success in blanking the mind and level of relaxation, followed by an open-ended question about anything distinctive about the session that they considered worth mentioning. After they completed the scale, I interviewed them about their answers. They were not given feedback of their AC scores at the end of the session.

Preparation for Training Sessions

Creation of feedback stimulus files. The protocol specified that I create 3 feedback files: (a) the spoken word "good" at 25 db, (b) the spoken words "good good" at 25 dB, and (c) no sound, each superimposed on 40 db noise. The files were created using Audacity software. After setting my computer's speaker volume and Windows Media Player volume to their maximum settings, I first created a 1.5 s baseline "brownian noise" file from the Audacity "Generate" menu. This sounds more like the pink noise used in psi ganzfeld experiments than Audacity's pink noise. On playback, I found that the output of this file registered at 60 db on

³ This decision was made to allow for the fact that some Ps, or the same Ps at different times, might have either a broad focus (which would favor quadrant hits) or a sharp focus (which would favor square hits). Combining the two types in a single score gives P some credit whichever kind of resolution is in place for the trial.

my decibel meter, which I held near to my ear while I was seated in front of my computer. Using the “Amplify” option on the “Effect” menu, I progressively reduced the volume of this file until my meter recorded 40 db. This corresponded to a reduction of 30 db on the Audacity “playback level” scale compared to the baseline file. As my db meter is not sensitive below 40 db, and getting a meter with that capability would have been prohibitively expensive, I used an extrapolation procedure to determine the proper volume for the word stimuli: I first created a “good” baseline file by speaking the word “good” into the computer at moderate loudness. When I played back the file, my meter showed about 62 db. I then created a series of additional files, lowering the Audacity playback level the same amount each time and noting the meter reading when I played the file back. As expected from theory, the reduction in the meter reading was about the same each time, meaning that the relationship between the two measures was linear. A 10 db reduction in the playback level corresponded to about a 7 db reduction in the meter reading. Given that the playback reduction from the baseline file that produced a 40 db meter reading was 35 db, I could deduce that reducing the playback to 55 db from baseline would correspond to a meter reading of 25 db.

To create the “good” subliminal feedback file, I mixed the 25 db “good” baseline file and the 40 db brownian noise file. To create the “good good” subliminal feedback file, I first inserted a repeat of the “good” sound into the baseline “good” file, immediately after its location in that file. I then repeated the extrapolation and mixing procedures I had used to create the “good” feedback file. To create the “silence” feedback file, I mixed a “silent” file from the Audacity menu with the 40 db brownian noise file.

Uploading of files. The plan was for Ps to bring the laptop computer they plan to use for the training sessions (at home) to the second baseline session. After the AC run, a folder containing the files needed for the training runs was to be uploaded onto this computer. A calibration and a threshold test (described below) were then to be performed on this computer.

This plan was followed successfully for Ps 1, 4, and 5. I was unable to upload the software onto P2’s computer. Thus, I uploaded the software onto my laptop computer. As this was the computer on which I had created the feedback stimulus files, I was confident that the calibration test would succeed on the first try, and it did. I then had her perform the threshold test with my computer. P3 owned only a desktop computer so I went to his home to conduct the preparation procedures. However, I was unable to get the programs to run properly on his system. Thus as with P2, I uploaded the software, calibrated the sound stimulus, and performed the threshold test on my laptop computer.

Calibration of subliminal feedback stimuli. To calibrate the feedback stimuli, I or P set the speaker volume settings on the host computer to their maximum level. I then played the baseline brownian noise file to be sure that the output measured 60 db on my decibel meter. I then played the noise file on Ps computer and had P lower the volume until the meter reading was 60 db. P was instructed to never change that volume setting until the training ended.

Threshold test. The purpose of the threshold test was to assure that Ps could not hear the feedback words over the brownian noise. Ps took the test seated in front of the host computer. The “good good” stimulus and the silent stimulus were presented in random order across 20 trials. After each trial, Ps were asked indicate with a mouse click whether they heard anything other than the noise, yes or no. I considered a passing grade to be a score from 7 to 13 (max $z = \pm 1.34$). All Ps met this criterion.

Training Sessions

Testing locations and schedule. P1, P4, and P5 completed their training sessions at home. I made this decision because I wanted to avoid Ps having to make 20 additional trips to the laboratory, and given the computer control of the study I could find no security risk. In any event, the crucial results as far as demonstrating psi is concerned were obtained at the RRC. Because P2 and P3 were to use my computer, and also because their home environments were not ideal with respect to ambient noise and comfort, I conducted these sessions at my apartment, which is very quiet, especially after I turned off humming appliances. I felt that my apartment would provide an ambience more similar to the P's home than would the RRC.

The original intention was to have each P complete 20 1-run (60 trial) training sessions at mutually convenient times, with an option of additional runs if I noticed signs of learning in the 20. However, due to a memory lapse I gave the first four Ps only 15 runs. There was never more than one session per day.

Procedure. The procedure for the AC task was the same as for the baseline sessions, except that immediately following each quadrant hit Ps were presented subliminally with the "good" stimulus and after each square hit with the "good good" stimulus. After each miss Ps were presented with the silence stimulus. An encrypted file containing the AC results was stored on the host computer and a corresponding nonencrypted file was automatically sent to my email account. I did not access the file with the AC results until the session was over. Ps tested at home phoned me after completing each run and I presented the rating scale to them using a semistructured interview format. The items were the same as in the scale used for the baseline sessions, with questions added about possible distractions during the session and whether they could tell whether they got a hit from the sound of the feedback stimulus. For Ps tested at my apartment, during the test I was in a room several feet down the hall with the door closed. Ps called me after the AC task was completed and I then conducted the rating scale interview face-to-face.

Post-Training (Test) Sessions

After training was completed, Ps came to the RRC for two more sessions during each of which they complete one AC run. The sessions were on adjacent or nearly adjacent days. The procedure was identical to that for the baseline sessions, which means no auditory feedback stimuli were provided.

Debriefing. At the end of the second test session I debriefed each P, explaining details about the protocol that I had not explained before (there was never any deception), summarized their AC results, and give them an opportunity to ask any questions they had about the study. I showed them the AC results of the run they had just completed and offered to send them by email the results of all the runs they had completed during the experiment. All 5 Ps took me up on the offer. Finally, I paid them their \$450 fee and had them fill out and sign the receipt. The protocol was approved by the Rhine Research Center's Institutional Review Board.

Results

Because location z-scores are not conventional z-scores, their statistical significance cannot be assessed by z-test. Thus, a Monte Carlo procedure was employed. Null distributions of location z-scores

were created using a program I wrote in the R programming language, with random target and response sequences generated using the “Marsaglia-Multicarry” option in R. I generated distributions for a 60-trial test, a 120-trial test (corresponding to the N s for the combined baseline and combined test runs), and the difference between two 120-trial tests.

The analysis was beset by what the error messages said was the occasional insertion of unwanted hidden characters in the distribution of generated scores when I attempted to convert it to a vector for processing. Through trial and error I discovered that the problem could usually be resolved by the insertion of carriage returns at the compromised locations, but this fix was quite tedious and not always effective. The larger the N , the greater the number of intrusions, and the greater the tedium and risk of failure. I found that for $N = 4,000$ the mean of the scores was close enough to the expected mean of 0 (.0005 to .007) to be satisfactory. With $N = 1,600$, the deviations from 0 were an order of magnitude higher and some were negative. This evidence of convergence reassured me that my assumption that the theoretical mean is 0 was correct.

I calculated the p -value of each observed location z by finding where that value, or the next most extreme value, fell in the distribution after the z s had been ranked in ascending order using the “sort” command in R. In case of ties, the location closest to the midpoint of the distribution was chosen. Thus, if the observed *negative* z was at location 50, its p -value was $50/4000$, or .0125. Likewise, if the observed *positive* z fell at location 3950, the p -value was $(4000-3950)/4000$, again .0125. Because I wanted the equivalent of a two-tailed test with alpha at .05, I performed the corresponding procedure on the opposite-signed value and added the two probabilities.

Individual Participants

Participant 1. P1 was a 38 year old female at the time of her participation. One of her professions is acting. Shortly after completing testing she gave a benefit performance for the RRC in which she did an improvisation of a character in which there was a profound change in her tone of voice. When I asked her, she said she enters a kind of trance state during these performances. She practices meditation and occasionally engages in automatic writing.

In Palmer (2017), her score on DPS Detachment was 29. She was in the eyes-closed/left-hand condition. She claimed that she felt her hand was moved by an outside force 81-100% of the time during the AC task. Her location z -score was +1.57.

Her 15 training sessions covered a period of 24 days with an average interval between sessions of 1.6 days and the longest interval being 3 days.

She reported that during the training runs she experienced excess “water” in her eyes and mouth that she had never experienced before. She also frequently experienced a blue light.

She estimated her hand to be moved by an outside force 61-100% of the time (counting baseline, test, and training runs). Her mean location z -score in the two pooled baseline runs was 0.40. Her mean location z -score in the pooled test runs was a significantly negative -2.58 ($p = .045$). The difference between baseline and test approaches significance ($z = -2.98$, $p = .082$).

Participant 2. P2 was a 49 year old female at the time of her participation. She has practiced meditation in the past. She occasionally used the Ouija board as a child and occasionally practiced automatic writing as an adult but stopped about 10 years ago.

In Palmer (2017), her score on DPS Detachment was 25. She was in the quotations/left-hand condition, which produced overall psi-missing in that study. She claimed that she felt her hand was moved by an outside force 41-60% of the time during the AC task. Her location z-score was -0.32.

Her 15 training sessions covered a period of 46 days with an average interval between sessions of 3.2 days and the longest interval being 9 days. The long intervals occurred early in the training.

The feedback stimulus seemed louder to her on some trials than others, especially on those that she independently expected to be a hit. During the task she would have a visual image of the pointer moving over a grid divided into quadrants. The grid was colored, mostly blue, but sometimes purple, light green or white, and the color sometimes varied depending on whether she expected a hit. Unlike the other Ps, she did not consistently expect a positive AC score and she was the only one to occasionally predict a below-chance score.

She was the only P to answer the outside force question “no” (on 6 runs) and on the other runs she estimated her hand being moved by an outside force 1-60% of the time. Her mean location z-score in the two pooled baseline runs was a nonsignificantly negative -1.63. Her mean location z-score in the pooled test runs was a nonsignificantly positive 1.35. The difference between baseline and test approaches significance ($z = 2.98, p = .082$).

Participant 3. P3 was a 76 year old male at the time of his participation. He practices meditation and hatha yoga.

In Palmer (2017), his score on DPS Detachment was 21. He was in the quotations/right-hand condition. He claimed that he felt his hand was moved by an outside force 61-80% of the time during the AC task. His location z-score was 0.66.

His 15 training sessions covered a period of 30 days with an average interval between sessions of 2.1 days and the longest interval being 5 days.

He experienced his hand being moved by an outside force 61-80% of the time in the baseline runs and 81-100% of the time in all subsequent runs. As he described his experience during training, I came to the conclusion that he experienced being guided more by an “inside force” than by an “outside force.” He consistently expected to score “strongly above chance.” He often commented that he was in an exceptionally deep altered state during the task and he consistently gave the highest rating (10) on the altered state question.

His mean location z-score in the two pooled baseline runs was a nonsignificantly positive 0.25. His mean location z-score in the pooled test runs was a nonsignificantly positive 0.82. The difference between baseline and test is not significant.

Participant 4. P4 was a 71 year old female at the time of her participation. She does relaxation exercises regularly.

In Palmer (2017), her score on DPS Detachment was 22. She was in the quotations/left-hand condition, which produced overall below chance AC scores in the previous experiment. She claimed that she felt her hand was moved by an outside force 41-60% of the time during the AC task. Her location z-score was -1.98.

Her 15 training sessions covered a period of 41 days with an average interval between sessions of 2.9 days and the longest interval being 6 days.

She consistently heard a hiss superimposed on the feedback stimulus on about 80% of the trials, which she interpreted as an indication that her response was a hit.

She experienced her hand being moved by an outside force 41-100% of the time. Her mean location z-score in the two pooled baseline runs was 2.49, which is significant in the positive direction ($p = .041$). Her mean location z-score in the two pooled test runs was a nonsignificantly positive 1.08. The difference between baseline and test is not significant. The high baseline score was due to 29 quadrant hits in the first baseline run, a 48% hit rate where MCE is 25%. This result is highly significant ($p = .00008$). In contrast, she obtained only four square hits on the run, close to the MCE of 3.75. There was nothing unusual or distinctive about her rating scale responses compared to her other runs in the experiment.

Participant 5. P5 was a 74 year old female at the time of her participation. She used the Ouija board and occasionally practiced automatic writing many years ago.

In Palmer (2017), her score on DPS Detachment was 20. She was in the eyes-closed/right-hand condition, the condition most similar to the present experiment. Her location z-score was -0.04.

Her 20 training sessions covered a period of 36 days with an average interval between sessions of 1.8 days and the longest interval being 4 days.

In many of the sessions, starting in the first baseline session, she saw a blue or lavender haze as if through her eyelids. Occasionally this began during the relaxation exercise. At the end of session 15 she mentioned that occasionally during this and past training sessions she had experienced small squares or dots within the larger square. I assumed she was thinking of the previous experiment in which the 16 squares were visible as such on the grid. This led me to review the scoring procedure for the experiment, especially the distinction between square and quadrant hits. Then during the debrief at the end of the study, she mentioned that she had not understood the scoring procedure until I had explained it this second time, and the explanation had made her self-conscious in subsequent sessions.

During training, she noticed that on some trials the feedback stimulus sometimes had a high pitch. After a few sessions this was replaced by hearing one or two breaks in the hiss of the feedback stimulus. I was concerned that she was detecting “good” or “good-good”, so after Session 5 I had her do two additional 20-trial threshold tests at her home. I had put the test on her computer during the second baseline session. The first test was the standard one with 10 silent stimuli and 10 “good-good” stimuli

presented in random order. She was told to respond “yes” if she heard one or more breaks in the hiss. She responded yes 9 times and 12 of her detections were correct. I then emailed her another file labeled “good-good” and asked her to replace the old file with the new one. The new file was actually a “silent” file but I did not tell her that, only that it differed from the original in a way I didn’t specify. Thus, all 20 trials were silent trials. She made 10 yes responses and only 7 of the detections were correct. I then explained what I had done and told her that I now had evidence that the breaks she was hearing most likely were her imagination and should not be interpreted as a valid indicator of whether she had gotten a hit.

She experienced her hand being moved by an outside force 61-100% of the time. Her mean location z-score in the two pooled baseline runs was -2.19, which is close to significant in the negative direction ($p = .069$). Her mean location z-score in the two pooled test runs was 2.30, which is close to significant in the positive direction ($p = .055$). The difference between baseline and test is significant ($z = 4.49, p = .011$).

Group Results and Hypothesis Tests

The experimental hypothesis was that the participants, collectively and individually, will score significantly differently ($p < .05$, two-tailed) on the post-training test trials than on the pre-training baseline trials. The hypothesis was significantly supported for one participant (P5), although the baseline-test difference approached significance for two others (P1 and P2).

The plan was to test the hypothesis for the group by a paired t test. This test implicitly assumes that the direction of scoring is consistent across Ps, which it clearly was not: for two of the five Ps, including one of those for whom the pre to post difference was suggestively significant (P1), scoring was more positive in the baseline runs. Thus, not surprisingly, the t test produced chance results. The mean baseline z was -0.14 ($SD = 1.86$) and the mean test z was +0.59 ($SD = 1.86$ also), $t(4) = 0.53$, ns. To determine if the unusually high variability across the five results was significant, a post hoc variance test was performed on the location z difference scores using the formula $\chi^2 = (n-1) s^2/\sigma^2$, where n is the number of observations (5), s^2 is the empirical variance of the five scores (9.38), and σ^2 is the population variance, estimated from the Monte Carlo analysis of the distribution of null location z difference scores. The analysis demonstrated statistically significant variability: $\chi^2(4) = 12.59, p = .013$. The results are summarized in Table 1.

Table 1

Location z Results

	Baseline		Test		Difference		Training	
	z	p	z	p	z	p	M(SD)	r^{sa}
P1	+0.40	ns	-2.58	.045	-2.98	.082	+0.13 (1.23)	-.02
P2	-1.63	ns	+1.35	ns	+2.98	.082	-0.10 (1.15)	-.34
P3	+0.25	ns	+0.82	ns	+0.57	ns	+0.23 (1.14)	+.20
P4	+2.49	.041	+1.08	ns	-1.41	ns	+0.06 (0.68)	-.34
P5	-2.19	.069	+2.30	.055	+4.49	.011	-0.15 (1.51)	+.12

^a Spearman correlations between trial numbers and location z-scores. All are nonsignificant.

There was little variability in rating scale responses either between or within Ps. Except as noted

above, Ps consistently gave a relaxation rating of 7 or 8, with a smattering of 9s, on the 10-point scale, and they consistently expected to score “somewhat [or] strongly above chance” on the AC task.

However, a learning interpretation of the difference scores requires evidence of improvement of scores across the training runs in the direction of the test session means. As can be seen from Figure 1, there was no evidence of such improvement in the training sessions for any of the Ps. None of these trends approached significance, although four of the five were in the direction expected by the learning hypothesis (for what little that is worth). Finally, the learning hypothesis would lead one to expect that the baseline z would be close to 0 and noticeably closer to 0 than the test z. That was the case only for P1.

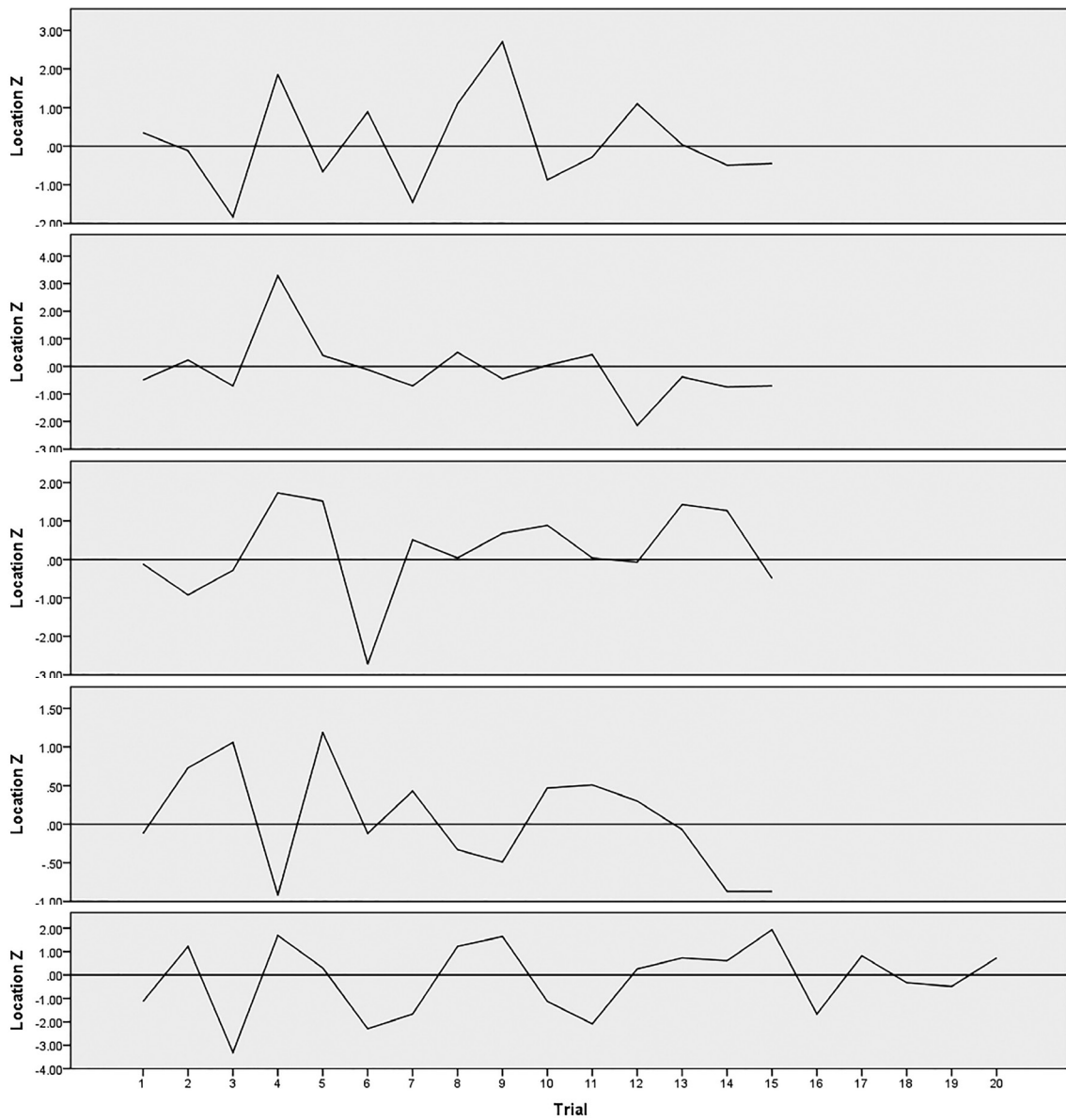


Figure 1. Scoring trends across training sessions.

Discussion

There were a considerable number of statistically significant or suggestive results in the study, including (a) 7 of the 15 effects in the crucial Table 1 describing the scores of individual Ps in the baseline and test sessions, (b) the demonstration of significant overall variability in these sessions across Ps, and (c) a highly significant ($p = .00008$) single run score with a hit rate almost double the MCE of .25, all this despite low statistical power. These outcomes with Ps selected on the basis of dissociative tendencies encourage further research on the dissociation-ESP relationship demonstrated in Palmer (2017), especially with dissociative individuals.

However, no such success appeared during the training sessions. This lack of improvement in the training sessions indicates that whatever genuine AC occurred was not due to learning, and there was no evidence of learning in any of the Ps' data. I had always considered the learning component of the study to be "high-risk/high-reward." As I state in the introduction section, I believe strongly that it is important for parapsychology that we find ways to increase the strength and especially the reliability of psi performance in the laboratory, and feedback training has always seemed to me the best way to achieve this objective. On the other hand, there is little evidence in the literature that such efforts are likely to succeed, although most studies have been marked by too few trials for success to realistically be expected.

I tried to strike a balance between having enough trials to demonstrate an effect and allow for learning on the one hand, and introducing burnout and a decline effect on the other. Although the experiment employed more training trials than previous endeavors, it still was probably not enough. Participants must not only detect the subtle internal cues associated with operation of the psi process (assuming such cues exist—a big "if") but they must also filter out the false feedback they get from chance hits. This is not easy to do. There was a mildly encouraging trend in P4's sessions, but if it was real one would expect it to show up in her test runs, which was not the case. Most importantly, there was no evidence of learning in runs 16–20 of P5, the one P whose baseline and test scores confirmed the original hypothesis.

A major purpose of the altered state induction and the request to blank the mind was to make it easier for Ps to detect internal cues by eliminating the mental noise from their thoughts and imagery as well as from bodily sensations. This effort obviously was not successful. These involuntary intrusions were particularly unfortunate because the great majority were task relevant and often used by Ps to assess or worry about how they were doing. I had hoped to avoid this problem by making the feedback subliminal, but the feedback was distracting in very much the same way that I believe supraliminal feedback is distracting and why I am biased against its use despite its potential information value. In a similar vein, the lack of learning could be partly attributable to Ps paying too much conscious attention to the feedback stimuli, rendering them ineffective. This problem might be mitigated in a future study by using the quotations rather than the eyes-closed procedure to facilitate dissociation. Because P's attention would be on the quotations, it would not be on the feedback stimuli. The bottom line is that my meta-hypothesis that a pure motor task should be especially psi-conducive was not tested in this experiment, because a condition for testing it was not met. Finally, all significant results in the study should be considered tentative unless they are successfully replicated on the first try or there is a significant meta-analysis of multiple attempts.

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Entraîner la Cognition Anomale dans une Tâche Motrice avec un Feedback Auditif Subliminal

Lors de chacun des 60 essais, 5 participants (Ps) sélectionnés sur la base de scores de dissociation état et trait élevés dans une précédente expérimentation d'automatisme moteur ont exploré avec un stylet une grille de 16x16 pouces fixée sur une tablette numérique, s'arrêtant pour engistrer une réaction à un carré cible sélectionné aléatoirement. La grille est conceptuellement divisée en 16 carrés (4 dans chacun des 4 quadrants). La variable dépendante était la moyenne de 2 scores z représentant les succès dans les carrés et les quadrants. Les Ps ont assisté à 2 sessions d'un essai de base et 2 sessions d'un test. Dans l'intervalle, ils ont effectué 15 à 20 sessions d'entraînement avec un feedback auditif subliminal. Le stimulus de feedback était le ou les mots énoncés "bien" (succès dans le quadrant) ou "bien bien" (succès dans le carré) surimposé sur un bruit brownien (similaire à un bruit blanc). L'un des 5 Ps a significativement confirmé l'hypothèse d'un meilleur score dans la session test que dans la ligne de base. Il y a eu des effets significatifs ou suggestifs de cognition anomale pour les résultats de base ou en test de 4 Ps ; et les 5 scores de différence ont montré une variabilité significative entre sujets. Il n'y a eu aucune preuve d'apprentissage dans les sessions d'entraînement. Selon la théorie sous-jacente, les conditions d'un apprentissage n'étaient pas rencontrées car les Ps n'ont pas suffisamment fait le vide dans leur esprit et étaient trop attentifs aux sons de feedback.

Zum Training anomaler Kognition bei einer motorischen Aufgabe mit sublimalem auditivem Feedback

In insgesamt 60 Versuchsdurchgängen erkundeten 5 Teilnehmer (Vpn), die in einem früheren Experiment zu motorischen Automatismen aufgrund ihrer hohen State- und Trait-Dissoziationswerte ausgewählt worden waren, mit einem Computerstift ein 16x16 Inch grosses Gitter, das mit einem Computerschreibtablet verbunden war, wobei sie innehielten, sobald sie bei einem zufällig ausgewählten Zielquadrat eine Reaktion erhielten. Das Gitter bestand aus 16 Quadraten (jeweils 4 in 4 Quadranten). Die abhängige Variable bildete der Durchschnitt von 2 z-Werten aus Quadrat- und Quadrantentreffern. Die Vpn absolvierten 2 Einzeldurchgänge an Baselinesitzungen und 2 Einzeldurchgängen an Testsitzungen. Dazwischen absolvierten sie 15-20 Einzeldurchgänge als Trainingssitzungen mit sublimalem auditivem Feedback. Der Feedbackstimulus bestand aus dem gesprochenen Wort gut (Quadrantentreffer) oder den Worten gut gut (Quadratreffer) und wurde mit einem Brownschen Rauschen (vergleichbar mit dem rosa Rauschen) überlagert. Bei 1 von 5 Vpn konnte die Hypothese einer höheren Trefferleistung in den Testdurchläufen verglichen mit den Baselinedurchgängen signifikant bestätigt werden. Es ergab sich eine signifikante oder suggestive Evidenz für anomale Kognition in den Baseline- und/oder den Testresultaten bei 4 Vpn, und die 5 Differenzscores unterschieden sich signifikant innerhalb der Vpn-Variabilität. In den Trainingssitzungen ergab sich kein Hinweis auf einen Lernerfolg. Die Lernbedingungen gemäß zugrundeliegender Theorie wurden nicht erreicht, da die Vpn nicht in der Lage waren, ihren Geist leer zu machen und dem Feedbackrauschen zu viel Aufmerksamkeit schenkten.

Entrenamiento de Cognición Anómala en una Tarea Motora con Retroalimentación Auditiva Subliminal

En cada uno de 60 ensayos, 5 participantes, seleccionados en base a puntuaciones altas en estado y rasgo de disociación en un experimento anterior de automatismo motor, exploraron con un bolígrafo de computadora una cuadrícula de 16x16 pulgadas fijada a una tableta de escritura de computadora. Tenían que detenerse para registrar una respuesta en un cuadrado objetivo seleccionado al azar. La cuadrícula se dividió conceptualmente en 16 cuadrados (4 en cada uno de los 4 cuadrantes). La variable dependiente fue el promedio de 2 puntuaciones z que representaban los aciertos en cuadrados y cuadrantes. Los participantes asistieron a 2 sesiones de línea base de 1 ensayo y 2 sesiones de prueba de 1 ensayo. Entre tanto, completaron 15-20 sesiones de entrenamiento de 1 ensayo con retroalimentación auditiva subliminal. Los estímulos de retroalimentación fueron la(s) palabra(s) habladas "bien" (cuadrante) o "bien bien" (cuadrado) superpuestas a ruido browniano (similar al rosa). Uno de las 5 participantes confirmó significativamente la hipótesis de una puntuación más alta en la prueba que en la línea base. Hubo evidencia significativa o sugestiva de cognición anómala en la línea de base y/o resultados de las pruebas de 4 participantes y las 5 puntuaciones de diferencia mostraron una variabilidad significativa entre sujetos. No hubo evidencia de aprendizaje en las sesiones de entrenamiento. De acuerdo con la teoría subyacente, las condiciones para el aprendizaje no se cumplieron porque los participantes no lograron borrar la mente y estuvieron demasiado atento a los sonidos de retroalimentación.

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

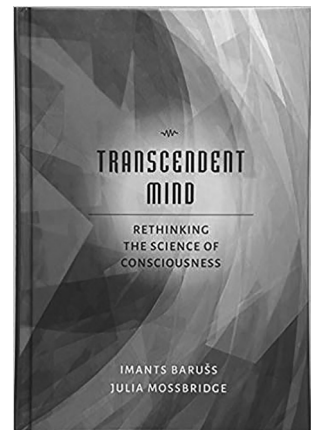
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Abstract: This review reflects at length on a landmark publication by the American Psychological Association, a volume whose argumentation, bolstered by reviews of empirical evidence from parapsychology and other disciplines, brings into question the 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 electrophysiological 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 Mar czy chess-playing case (pp. 93-94) and throw in some additional details that appear to relate to memory of Mar czy 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.

Measuring Organizational Closure in the MPI/GQT/CMM Context¹

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Abstract: Organizational closure (OC) is a theoretical state within Walter von Lucadou's Correlation Matrix Method (CMM) for producing evidence of mind/matter interactions or *psi*. In practical terms, OC is affected by attentional, emotional/ affective, contextual, and motivational elements in the experimental system. Because maximal OC is considered desirable in obtaining non-local entanglement correlations, a user-friendly measurement of OC in the CMM experiments would be useful. Based on studies that have found a positive association between the participants' under-estimation of session duration and *psi* task performance, the method suggested here involves the participants' retrospective estimate of duration. This research note concludes with advice on how this measure may be implemented.

Keywords: Correlation Matrix Method, Organizational Closure, time contraction

Organizational closure (OC) has been a consistent element of Walter von Lucadou's Model of Pragmatic Information (MPI) for many years (Lucadou, 1996). More recently, with Harald Walach, Hartmann Römer, and others, he has discussed the relevance of this term within a Generalized Quantum Theory (GQT) interpretation of mind-matter or psychokinesis experiments (Lucadou, Römer, & Walach, 2007) and its relevance to the Correlation Matrix Method (CMM: Lucadou, 2015; Flores, Tierney, & Watt, 2018). In this method, maximal OC is considered necessary, in addition to complementary measurements or observations, to produce the conditions that allow the occurrence of non-local entanglement correlations. These occur within the matrices derived from psychological and physical variables in psychokinesis experiments. Consequently, a user-friendly measurement of OC in the CMM experiments would be useful. This research note proposes that a single measure, retrospective estimate of duration, accurately reflects the elements/processes within a CMM experiment that are specific and autonomous to that trial and, in the sense that "one cannot step in the same river twice," measures the degree of OC assessed from within.

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Defining and Maximizing Organizational Closure

In the terms defined by Maturana and Varela (1992) “an organization denotes those relations that must exist among the components of the system for it to be a member of a specific class” (p.43). The class of experiments that the CMM seeks to investigate is one that promotes non-local entanglement correlations.

The term *organizational closure* is associated specifically with Varela and describes an organization of processes such that: “(1) the processes are related as a network, so that they recursively depend on each other in the generation and realization of the processes themselves, and (2) they constitute the system as a unity recognizable in the space (domain) in which the processes exist” (Varela, 1979, p.55). Varela believed that organizational closure is closely related to autonomy and that “every autonomous system is operationally closed” (Varela, 1979, p. 58; Bourguine & Varela, 1992). An autonomous process can be defined as: “undertaken or carried on without outside control” (Merriam-Webster, Definition 1b).

What does Varela’s somewhat abstract definition mean in practical terms for experimental parapsychologists? Below, we highlight four elements of the experimental system implicated in OC and that may be manipulated to maximize OC.

Attentional: Primarily this relates to the participants’ engagement with the experiment, the degree to which they are absorbed in the experience. We suggest that retrospective estimate of duration adequately measures this element. However attention is affected by, and is entwined with, the next two elements.

Emotional/affective: This subdivides into those elements specific to the participant and the experimenter separately, and those that result from their interaction. That these affective elements are very strongly entwined with the first, attentional, one has been explored by many researchers (see Glicksohn, 2001, for a review). Primarily they involve concepts of arousal as reflected in excitement, anxiety, anticipation, and competitiveness, in the experimenter but primarily in the participant.

The anecdotal observation on successful psi-inducing environments make reference to the degree to which the experimenter can produce a warm, welcoming, honest and trusting ambience that involves a little light-hearted humor and competition. Importantly these elements, in combination with the next, reduce the distraction that would result from apprehension and doubt (Kennedy & Taddonio, 1976), thus maximizing OC.

Contextual: This includes the participant/experimenter interaction over the entirety of the participant’s involvement, as well as the physical environment. It includes the important and necessary elements of novelty (Lucadou, 1996) and privacy, as well as the way instructions are given to the participant, reflecting the emotional/affective elements above. Although there is some anecdotal evidence that the presence of a sympathetic experimenter may be helpful, the reassurance that the participant and the OC environment is not being observed from outside the system (either directly or by a camera so that there are potentially multiple observers) is probably crucial to OC (Batcheldor, 1984). Whether this is caused by a factor that is theoretically limiting, resulting in decline effects or non-replicability, such as

the NT axiom (Lucadou, Römer, & Walach, 2007; Walach, Lucadou, & Römer, 2014), or some other constraint, remains to be elucidated by experiment. Lastly, although it is not clear what the ideal physical dimensions of the experimental space should be, it is likely to be one that engenders a sense of security, privacy and lack of distraction, thus facilitating attention on the task, increasing OC.

Motivational: This element pertains both to the participant and the experimenter, and is less entwined with the other three elements. To some extent the participants' motivation are revealed in their willingness to become involved with the experiment in the first place (unless significant rewards for participation are on offer). Although motivation might include the participant's belief in the possibility of succeeding at the task, the literature on this is so confused, and the term so loaded, as to suggest that it should be avoided in a questionnaire. Again, there may be a case for asking whether the experience matched expectations but without knowing what they were the answers might be misleading.

In the background of this element is the motivation of the experimenter. The specter of experimenter effect has to be considered – then possibly ignored for the present, until the CMM approach is proven beyond doubt. Many of these elements of OC are familiar to experimental parapsychologists as psi-conducive practices and issues and discussed in depth in a series of invited papers collated by Delanoy (1997).

Time Contraction

The elements of attention and arousal also feature in Glicksohn's (2001) polemical review of the literature. Defending his widely quoted cognitive-timer model, Glicksohn has summarized some of his conclusions:

I have suggested a refinement of the cognitive-timer model for apparent duration, showing how the number of subjective time units and their size covary.... The first conclusion is regarding the strong interplay among attention, arousal, and time perception that is at the base of the cognitive-timer model. Second, a conclusion can be drawn concerning the notion of a single pool of attention that can be deployed in an internally oriented or externally oriented manner, with a necessary trade-off between the two. Third, a hyperbolic relationship between the number of subjective time units and their size is suggested (p.17)... Assuming a common pool of attention, there is a trade-off between externally oriented and internally oriented attention. *The more absorbed the subject becomes in his or her subjective experience (due to a predisposition for high absorption and/or via an experimental technique such as introspection or concentrative meditation), the slower time appears to be.*"(p.9) (Emphasis added)

The result for absorbed participants is that their retrospective estimate of session duration is an underestimate compared to the objective duration of the relevant period measured by the experimenter. This tendency to underestimate may be labeled *time contraction*. The relevance of this to the CMM procedure can be obtained by reverse engineering from the results of past findings in the parapsychological literature. Schmeidler (1982) reported that self-proclaimed psychics hardly differ

from comparable control groups except for one trait, the time contraction trait. Bierman (1988), in reporting a Ganzfeld experiment noted: “the scoring rate of 77% (MCE=25%) for the subjects who showed time contraction is larger than any known scoring rate of a group selected on the basis of a single measure” (p.8). This finding has also been reported by Palmer, Bogart, Jones, and Tart (1977) who used a different measure and also found a positive relation between time-contraction and positive psi scoring.

From the perspective of CMM methodology, the evidence in the parapsychological literature for positive correlations between time-contraction and successful attempts to produce psi suggests that retrospective estimate of duration may function as a measure of OC.

Measurement of OC: Specifics

In the CMM environment, disruptive or jarring events that might reduce OC should be minimized. A single measure, associated with successful psi performance in the literature, and requiring a minimum of time to implement, using information already available to the experimenter, could be a useful measure of OC. The retrospective estimate of duration by the participant involves a question posed by the experimenter immediately after the end of the participant’s session: “Without looking at your watch, please estimate the duration of this experimental session in minutes from the point I said ‘please start when you are ready’ to the present.” The participant’s answer is recorded, and then compared with the recorded duration to derive a proportional measure of accuracy. Estimates shorter than the recorded time indicate higher OC (absorption) and estimates longer than recorded time indicate low OC characterized by boredom, distraction, lack of motivation, etc. This measure should only be used once with each participant without informing the participant before the session that the question will be asked. Participant awareness before the session is completed that the question may be asked would change the participant’s behavior from one of retrospective estimation at the end of the session to one based on possible continuous awareness/ monitoring of time passing during the session. That, as a distractor from the intention task, is likely to be detrimental to OC. Although the utility of this measure will be proven or otherwise by its use in research using the CMM methodology, the present evidence from both the parapsychological and the time estimation literatures suggest retrospective estimate of duration may function as a useful measure of organizational closure.

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Mesurer la clôture organisationnelle dans le contexte du MPI/GQT/CMM

La clôture organisationnelle (OC) est un état théorique de la méthode de corrélation des matrices (CMM) de Walter von Lucadou pour produire des preuves des interactions entre esprit et matière ou *psi*. En termes pratiques, l'OC est affectée par les éléments attentionnels, émotionnels/affectifs, contextuels et motivationnels du système expérimental. Puisqu'une OCE maximale est désirable pour obtenir des corrélations d'intrication non-locales, un outil de mesure accessible de l'OC dans les expérimentations CMM serait utile. En se basant sur des études qui avaient trouvé une association positive entre sous-estimation par les participants de la durée de la session et performance à une tâche *psi*, la méthode suggérée ici implique l'estimation rétrospective de la durée par les participants. Cette note de recherche conclut sur des conseils sur la façon d'implémenter cette mesure.

Zur Messung der Organisierten Geschlossenheit im Kontext von MPI/VQT/KMM

Die Organisierte Geschlossenheit (OG) ist ein theoretisches Konzept innerhalb der von Walter von Lucadou entwickelten Korrelations-Matrix-Methode (KMM), mit deren Hilfe sich psychophysische Wechselwirkungen ('*psi*') nachweisen lassen. In praktischer Hinsicht wird die OG durch aufmerksamkeitsbedingte, emotional/affektive, kontextuelle und motivationale Elemente im experimentellen Auf-

bau beeinflusst. Da zum Zustandekommen nichtlokaler Verschränkungskorrelationen eine größtmögliche OG als wünschenswert angesehen wird, empfiehlt sich eine benutzerfreundliche Messung der OG bei Experimenten mit der KMM. Unter Rückgriff auf Studien, die bei Versuchsteilnehmern einen positiven Zusammenhang zwischen der Unterschätzung der Sitzungsdauer und dem Psi-Treffererfolg festgestellt hatten, verwendet die hier vorgeschlagene Methode die retrospektive Schätzung der Dauer. Die Forschungsnotiz mündet in einen Vorschlag, wie diese Messung berücksichtigt werden kann.

Medición del Cierre Organizativo en el Contexto MPI/GQT/CMM

El cierre organizativo (OC) es un estado teórico dentro del Método de Matriz de Correlaciones (CMM) de Walter von Lucadou para producir evidencia de interacciones mente/materia o *psi*. En términos prácticos, el OC se ve afectado por elementos atencionales, emocionales/afectivos, contextuales, y motivacionales del sistema experimental. Debido a que el OC máximo es deseable para obtener correlaciones de entrelazamiento no locales, sería útil tener una medida de OC fácil de usar en experimentos de CMM. Basado en estudios que han encontrado una asociación positiva entre la subestimación de la duración de la sesión y el desempeño en la tarea *psi* de los participantes, el método sugerido aquí es la estimación retrospectiva del tiempo de los participantes. Esta nota de investigación concluye con consejos sobre cómo se puede implementar esta medida.

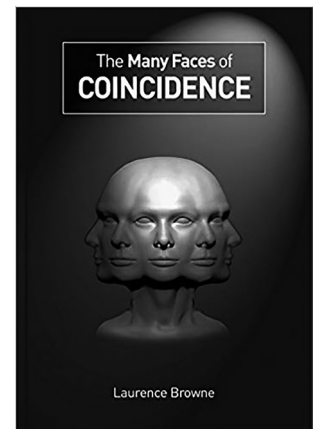
A Walk through the Manifold World of Coincidences

A Review of
The Many Faces of Coincidence, by Laurence Browne

Exeter, UK: Imprint Academics, 2017. Pp. 202. \$ 29.90. , ISBN 13: 978-1-84540-915-9

by Gerhard Mayer¹

Synchronicity has become a vogue expression used in many ways to denote “occurrences.” In a similar way, the term coincidence is often used in a reductionist context to eliminate meaning from seemingly related occurrences. Both terms have a contextual relation and are often used in a barely reflected way. Thus, it is a meritorious undertaking to examine the use of these terms more closely and summarize the findings to an interested readership. *The Many Faces of Coincidence* is the published version of the dissertation of philosopher and historian of science Laurence Browne on coincidences and synchronicity. It is a well-written work that provides a theoretical framework for the classification of different forms of coincidences. The book is divided into six chapters and they contained several surprises and interesting new insights.



In the first chapter, Browne deals with the introduction of the concept of *synchronicity* by Carl Gustav Jung, its genesis, and Jung’s predecessors and main influences. These include Arthur Schopenhauer with his *transcendent will*, Gottfried Leibniz and his *Monadology*, and Paul Kammerer (1919) and his *law of seriality*, as well as theologian and sinologist Richard Wilhelm (Wilhelm & Baynes, 1967) who introduced Jung familiar to the I Ching. Jung’s idea of a synchronistic principle arose within the context of this relationship with Wilhelm. However, Jung elaborated the concept of synchronicity further in the context of the fruitful exchange with physicist and Nobel physics laureate Wolfgang Pauli at the end of the 1940s and during the 1950s (Lindorff, 2004). Nevertheless, the two scholars did not always share the same opinion. For example, Pauli criticized parts of Jung’s conception of this kind of coincidences as logically inconsistent.

Jung was fascinated by specific characteristics of quantum physics that deviated from classical physics, and by the findings of experimental parapsychology, which he regarded as substantiating his model of acausal connections. Pauli recognized its potential as a possible explanation of synchronistic events, but he held the opinion that Jung did not differentiate in an appropriate manner between

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quantum-physical characteristics of nature and the findings of experimental parapsychology. He also considered the term *synchronicity* inapt because the relevant events would not necessarily have to happen exactly at the same point in time (i. e., be synchronous). However, although Jung was open to Pauli's suggestions and criticism, he kept the term, arguing that in synchronistic events the *timeless* sphere of archetypes would be connected to occurrences in the *timely* sphere, and therefore two synchronistic occurrences would not need to be synchronized in conventional time. The inclusion of the archetypal sphere into the characterization of synchronistic events makes Pauli's criticism of Jung's attempt to include the parapsychological experiments by Rhine understandable insofar as Pauli could not detect any meaningful occurrences based on archetypes in boring laboratory experiments consisting of countless trials of card guessing or die rolls. This is only one example of several conceptual ambiguities in Jung's thinking presented in Browne's book.

The important elements of the "composition of synchronicity," the title of the first chapter, include the idea that there are two types of synchronicity, namely a general principle of synchronicity as acausal connecting principle, and a narrower category of synchronistic events as specific manifestations of a general acausal order. Furthermore, there are spontaneously occurring and also induced synchronistic events – the latter occur in divinatory practices such as the I Ching, or in some magical practices. In general, synchronistic events are rather rare, and they are accompanied by an affective involvement of the people concerned.

In order to develop a classification of coincidence events, Browne first presents a variety of stunning case examples that, due to their improbability, rarity, or connection with meaning, provide reasons for musing about the nature of coincidence. His examples include extremely unlikely distributions of cards in card games and the apparently independent occurrence of inventions. As mentioned earlier, the term "coincidence" is often used to support a reductionistic or physicalistic worldview. The reign, or canonization, of probability began in the mid of the 19th century and it has lost none of its efficacy until now. The author quotes the ironic statement of noted astronomer Camille Flammarion: "The little god Chance sometimes produces extraordinary results" (p. 36). This quote refers to attempts to explain manifold coincidences on the ground that extremely rare events are indeed possible, and that even coincidences that appear to manifest outside of an evident causality chain can still be explained without resorting to meaning. However, the worldview-based reference then becomes a matter of faith, and the act of "deification" of coincidence referred to by Flammarion seems to be justified.

This argument is amplified in chapter three, *Cosmic Coincidences*, in which Browne leaves the framework of human experience and directs his analyses towards coincidences in the cosmic realm. With regard to astronomy, he describes the absolute "precision of the universe" (title of a subchapter) that, for instance, is necessary for the development of life forms of such complexity as those we find on earth; he points to the importance of Jupiter in our solar system which, together with Saturn, keeps away asteroids and other objects from earth like a cosmic vacuum cleaner, as well as the fact that the Earth is located exactly in the so-called Goldilock zone of our planet system, exactly in the habitable zone of the right distance with regard to the respective mass ratios. Furthermore, Browne emphasizes the importance of the fine-structure constant and other physical constants for the structure of the cosmos and our life form.

The author addresses astronomical coincidences because astronomy deals with completely different dimensions of chance probabilities than in the earthly sphere, for instance when discussing the possibility that a person hits the jackpot twice within a short time period. He mentions the number of possible universes as calculated by Stephen Hawking (1942–2018) and his collaborator Leonard Mlodinow – it includes the chance for the spontaneous and accidental occurrence of a universe suitable for life forms, which is supposed to be $1:10^{500}$ (Hawking & Mlodinow, 2010, pp. 118–119). These scientists are not the only ones who operate with chance probabilities of such a dimension that render the concept of chance absurd if applied to human conditions.

With chapter four, entitled *Enigma Variations*, the author enters the field of quantum physics. This is suggested only by the developmental history of the concept of synchronicity. Browne primarily discusses three important aspects: the phenomenon of entanglement and, closely related, the influence of the observer or experimenter on the measured or observed system, as well as the “suspension” of the everyday experience of time as a one-directional arrow of time. In the *delayed-choice* experiments of the double-slit experiments, the motion of the photon to move to one or to both slits, i.e. whether it should behave as a particle or as a wave, is decided by chance only *after* the firing of the photon by the experimenter. That means that the history of the process is not solidified before its observation. Browne cites physicist Pascual Jordan, who stated in this regard: “Observations not only *disturb* what is to be measured, they *produce* it” (p. 107). Browne’s argumentation suggests that quantum physics ultimately leads to the concept of *unus mundus*, i.e. a plane of reality in which time, space, and causality are suspended.

Chapter six, *Exploring the Tao*, is dedicated to this topic. As mentioned earlier, the I Ching and Chinese philosophy played an important role in the development of the concept of synchronicity, and with the ultimately untranslatable expression of *Tao* Jung found an equivalent to the term *unus mundus*. This chapter is an eye-opener and it provides, in addition to a helpful introduction to the understandings of *Tao*, interesting information about, for instance, the proximity of Heidegger to Taoism and his contact with Chinese scholar Paul Hsiao with whom he exchanged ideas (Hsiao, 1990). In this context, Browne mentions a well-known anecdote in Freiburg: A duck warned inhabitants of Freiburg by behaving very unusually that an air-raid was about to occur in 1944 during World War II and it thus saved the life of several people. Hsiao used this story to point at differences between European and Chinese thinking. Browne states: “[T]he European would ask why the duck was so animated at that particular time and then perhaps even look for parapsychological answers, while for the Chinese ‘the duck does not need any paranormal powers: everything is connected to everything else and in each moment there is concealed the entire past and also the open future.’ ” (p. 163) This example highlights once again why Pauli was right in warning Jung about his inappropriate mixing of parapsychological concepts and the synchronicity principle based on *unus mundus*.

Browne’s walk through the manifold world of coincidences, which excludes neither the smallest nor the biggest dimension, neither science nor philosophy, shows how useful and reasonable is a precise differentiation. He developed four categories for the classification and explanation of coincidence phenomena: random chance explanations, conventional causal explanations, paranormal causal explanations, and synchronicity explanations.

However, with regard to coincidences at an astronomical level, Browne considered the terms “conventional” and “paranormal” inappropriate, and he replaced them with “natural” and “supernatural.” These four categories are not mutually exclusive, a coincidental event can be regarded as the result of different categories, or explained plausibly with different explanations. In the afterword, Browne states that his model of categories is too sparse and that combinations and sub-categories should be developed. However, these are tasks for further analyses. Browne lays a valuable and valid foundation even if one comes to other assessments than the author regarding some questions of detail, for instance in the context of examples of coincidences at the astronomical level. In any case, Browne’s book sharpens awareness concerning the problem of “coincidences”, and differentiates between possible types of coincidence. One can only wish that the book finds many readers.

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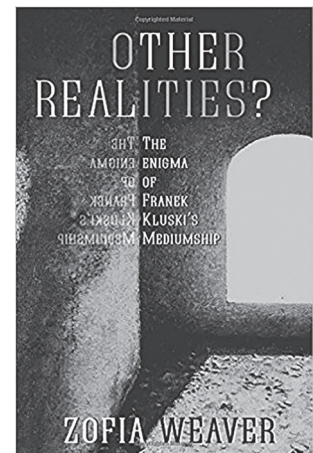
Extraordinary Physical Phenomena in Poland

A Review of
Other Realities? The Enigma of Franek Kluski's Mediumship, by Zofia Weaver.

Hove, UK: White Crow Books, 2015. Pp. xxi + 152pp. \$16.99 (paperback) ISBN 978-1-910121-39-9

by Erlendur Haraldsson¹

The Polish medium Franek Kluski was, in terms of versatility and strength of his phenomena, one of the greatest mediums of all time, comparable to D. D. Home and Indridi Indridason. Franek Kluski was a pseudonym and his real name was Teofil Modrzejewski (1873-1943). He lived a varied and successful life, was a banker (on the board of one of the largest banks in Poland), writer, poet, and journalist. Extensive records exist in Polish of Kluski's mediumship, particularly by Norbert Okolowicz (1926). In English, however, apart from the writings of Gustave Geley, little has been available. *Other Realities* by Zofia Weaver radically changes that. She gives a fascinating account of his psychic life as well as his successful worldly activities and his background in Poland. Zofia Weaver is a native Polish speaker and is highly knowledgeable of the psychic literature in Polish and in English as a former editor of the *Journal and Proceedings of the Society for Psychical Research*.



Already in his childhood Kluski had frequent paranormal experiences of various kinds. He was subject to presentiments, had visions of events at a distance, and perceived “phantoms,” which to him appeared to be living. “He found these phantoms perfectly natural and neither feared them nor found them strange. He talked to them familiarly and found them welcoming and friendly” (p. 11). From his early life he seems to have had easy and frequent experiences of phantoms or apparitions of the departed. In Geley's *L'Ectoplasmie et la Clairvoyance* (1924) Kluski's childhood experiences are described. Apparently throughout his life Kluski would see around him phantoms/apparitions of deceased friends and relatives, as well as animals. This would take place during the day, in the open, and at night in bed. Kluski's mediumship started when he was 45. His séances were generally of a rather private nature, like a home circle, but he was ready to submit himself to serious scientific examination. Best known are the studies by Gustave Geley and Charles Richet at the Institut Métapsychique International in Paris where strict controls were imposed. There the famous paraffin wax moulds of hands were produced.

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In *Other Realities* Weaver gives a vivid description of Kluski's phenomena as they are described in Polish sources, particularly by Okolowicz, who gave very detailed and minute descriptions that remind this reviewer of the protocols that the Experimental Society in Reykjavik kept of Indridi Indridason's séances (Haraldsson, & Gissurarson, 2015). As in the case of Indridi, lights of various colors and sizes would appear at the sittings. They moved around, appeared, and disappeared. It was common that chairs and other furniture moved without anyone present touching them. Knockings, as if with fingers or hands, were common, also outside of the séance context. Whispers and even singing were often heard, again remindful of Indridi, although of a much weaker nature with Kluski. Sitters often experienced breezes and touches, and apports brought from other rooms into the apartment were also reported.

There were some interesting differences between these two remarkable physical mediums. Levitations were common with Indridi but rare with Kluski, but sometimes happened to Kluski's sitters at séances. Kluski had no spirit controls whereas Indridi did. Direct voices were common with Indridi but rare with Kluski. Odors of various kinds were often smelled, unpleasant (e. g., of rotting flesh or dirty clothes) as well as pleasant fragrances similar to those coming from flowers or incense. On one occasion a sitter asked for rose oil and a few minutes later he and those close to him were sprinkled with rose oil that they could smell for a long time. Rose oil was not kept anywhere in the apartment.

Animal apparitions were frequent with Kluski, who was often seen and photographed with a bird, like a large hawk, on his head and shoulders. Okolowicz also described observations of human forms, materialized from light nebulae-like in Indridi's mediumship. Something like a pillar of light would form and in it a human form would appear. Generally these apparitions/phantoms were of a natural size, but sometimes they were partial or undersized. Okolowicz reports that 84 persons confirmed recognizing 88 phantoms of deceased persons known to them. Automatic writing was common with Kluski and with handwritings that differed from his own. Kluski also possessed more "ordinary" psychokinetic powers. He was able to influence compass needles and galvanometers, and turn electric lights on and off.

The subject index in the book is short and rather meager, a more extensive index would have increased the value of this important work. It contains some small photographs but it would have been interesting to see some more as much photographic work was done with Kluski. Zofia Weaver's *Other Realities* is a great contribution to the existing English literature on physical mediumship. Kluski was without any doubt one of the greatest mediums of all time and *Other Realities* is a fascinating book to read and highly informative.

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A Clearly Presented Account of Iceland's Most Famous Medium

A Review of
Indridi Indridason: The Icelandic Physical Medium,
by Erlendur Haraldsson and Loftur R. Gissurarson.

Hove, UK: White Crow Books, 2015. Pp. xvii + 271. £12.99. (paperback). ISBN 978-1-910121-50-4

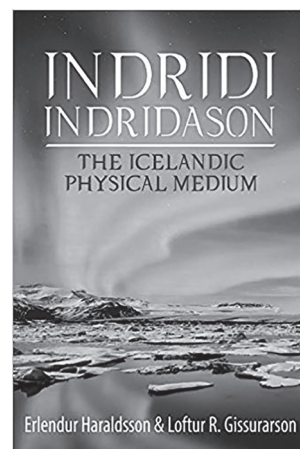
by Tom Ruffles¹

The achievements of Icelandic medium Indridi Indridason (1883-1912) deserve to be better known than they are. His mediumship was extensively studied during his lifetime, but most of the records are in Icelandic and have consequently been neglected by psychical researchers. In this landmark biography Erlendur Haraldsson and Loftur R. Gissurarson bring him to the attention of a wider audience. They produced a study of Indridi, published as an issue of the Society for Psychical Research's *Proceedings* (Haraldsson & Gissurarson, 1989), but since then further records of his séances have come to light, and their treatment of him here has been much expanded.

The authors have had access to a wide range of information: a number of first-hand accounts of sittings, and two minute books compiled by the Experimental Society in Reykjavik, a body dedicated to documenting Indridi's mediumship. These books detail a number of his séances and had been considered lost, but they were rediscovered in about the year 2000. These sources, extracts from many of which Erlendur and Loftur have translated from the Icelandic, supply detailed accounts of Indridi's séances. Although other minute books are still missing, the surviving papers are a valuable source for understanding the scope of his mediumship.

Indridi, a farmer's son, came to Reykjavik around the end of 1904 as a printer's apprentice. Einar Hjorleifson Kvaran (1859-1938), a writer, had read F. W. H. Myers' *Human Personality and its Survival of Bodily Death* (Myers, 1903) the year after its publication, and started a circle to examine mediumship. In early 1905 Indridi became its medium, his abilities having manifested during sessions at the home of a relative of his, and the circle was formalized as the Experimental Society, marking the foundation of psychical research in Iceland. As a member of this group Indridi achieved a high profile in Iceland and was intensively investigated.

Unfortunately Indridi's mediumistic career was short, spanning a period of less than five years, from



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1905 to 1909. During that time the Experimental Society made observations and carefully recorded the phenomena. Its members were affluent enough to be able to erect dedicated premises and pay Indridi a salary. There were often large numbers of sitters at the séances, at times as many as a hundred. They were separated from Indridi and “watchmen,” who sat close to him and were responsible for ensuring he did not cheat, by a mesh screen that was firmly fixed to walls, ceiling and floor and accessed by a slit.

Indridi produced a wide range of phenomena, analyzed in detail by Erlendur and Loftur, including raps, lights, breezes, movement of objects – sometimes violently – smells, playing of musical instruments, and automatic and direct writing. Even more dramatically, there were materializations of figures and body parts, even a figure described as somewhere between a calf and a horse which occurred outside the séance room but in Indridi’s presence; the dematerialization of Indridi’s arm; levitation of the medium; sitters’ sensations of being touched and even kissed; and voices, including male and female voices singing a duet.

There were also mental phenomena, including information provided by spirit communicators. Notable in this category was “Emil Jensen” accurately describing a fire in Copenhagen in 1905 Indridi could not have had knowledge of by normal means, which the authors analyze in detail and compare to Emanuel Swedenborg’s vision of the 1759 conflagration in Stockholm (Haraldsson, 2011). Intriguingly, an Emil Jensen had existed, and lived a couple of houses away from the site of the Copenhagen fire. He died in 1898. By any standard, Indridi’s was an impressive repertoire. During the winter of 1907-8 he was even the focus of what appeared to be a poltergeist outbreak.

If Indridi’s mediumship was fraudulent, it was a sophisticated performance, yet he was not well educated and sources of information that might have been available to assist him in deception were limited. Unfortunately, although some phenomena occurred outside the séance room, most sittings took place in darkness, which weakens their value, despite the medium having been subjected to thorough searches and the introduction of luminous tape in late 1908. This initiative was carried out by Gudmundur Hannesson (1866-1946), a scientist who investigated Indridi during the winter of 1908-9, ensuring that conditions were as rigorous as possible (Hannesson, 1924). His verdict was positive, even though he could not explain what he had witnessed. Sadly, Indridi’s mediumship was cut short by a severe attack of typhoid fever in 1909, after which he gave no further sittings, and he died in 1912, aged 28, from tuberculosis.

After an analysis of Indridi’s own phenomena there is a comparison with those of D. D. Home, Rudi Schneider, and Einer Nielsen which helps to contextualize Indridi, and indicates just how significant his mediumship was. Further detail is provided in appendices: a chronological breakdown of the phenomena, with comments by the authors; a summary of the precautions taken by Gudmundur which shows just how painstaking they were; further comparisons with the mediumship of D. D. Home; and an annotated list of the large cast of séance participants, communicators and other significant individuals who figured in Indridi’s mediumship, compiled by Walter Meyer zu Erpen and Haraldsson.

In their clearly laid out account, the authors ably track the progress of Indridi’s mediumship without unduly pushing a particular view, though they are sympathetic to the claim that it was genuine. Although they note that records were not always comprehensive and often leave questions unanswered,

in their summing up they discuss aspects that lend support to the genuineness of Indridi's mediumship, such as the communication of facts unknown to him that were later verified, correspondences between characteristics of communicators and the individuals they purported to be when alive (individuals Indridi had never met), and skills that Indridi did not possess, such as xenoglossy and the ability to sing the male and female parts of a duet. They conclude that there is "strong evidence for paranormal physical phenomena... [pointing towards] human survival of bodily death" (p. 228).

This is a case that tests one's boggle threshold, yet the alternatives are collusion or credulity of enormous proportions by the participants in Indridi's séances. From the available evidence, those who sat with Indridi were intelligent and aware of the need for strict supervision, taking appropriate action to guard against fraud, and not gullible. A chapter on contemporary criticism of Indridi shows that much of it was based on prejudice rather than being a balanced assessment.

Unless the missing minute books surface, shedding further light on Indridi, this is the most authoritative overview of his mediumship we are likely to have. The authors have supplied a valuable contribution both to our understanding of this outstanding medium in particular, and to the literature of physical mediumship generally. As Carlos Alvarado suggests in his introduction, in its clarity it stands as a model for rescuing other past mediums from obscurity.

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Forgetting the Past

To the Editor:

In a recent editorial in the *JP* Etzel Cardeña (2017) referred to the “ignorance or disregard [some show] . . . of earlier and very pertinent research and literature, as if somehow the topic had not been studied until the authors decided to focus their attention on it” (p. 4). This situation, also discussed by others (e.g., Alvarado, 2014; Braude, 2012), may produce incomplete views based on lack of historical continuity that, in turn, cause misconceptions, as well as rediscoveries or reformulations of previous findings and ideas.

Cardeña (2017) states that this neglect of the relevant literature “evidences an inadequate literature review and a failure to do one’s homework, but also arrogance in the assumption that what was done previously is not worth reading, as if somehow we are now more knowledgeable and/or brighter than people in the past” (p. 4). But this problem also suggests we have forgotten the functions of literature reviews in science (something that may vary across some disciplines). This includes the exploration of relevant theoretical ideas and assistance in the development of hypotheses and the selection of research methodology. Furthermore, knowledge of the previous literature is essential to assess our findings in relation to previous knowledge in the field.

We would also argue (e.g., Zingrone, 2006), that some persons in the field, particularly those coming from other areas, have a low level of basic literacy in the parapsychological literature. This is easy to understand due to the marginal status of parapsychology in academia and, consequently, the lack of formal educational programs.

It is not hard to find papers in the literature whose authors do not cite relevant literature. One example is the paper by Vasilescu and Vasilescu (2001), in which they presented an experimental study of precognition, but did not cite a single laboratory study of the phenomenon.

Similarly, there was no mention of previous relevant discussions in an interesting study on the introspective experiences of mediums (Rock, Beischel, & Schwartz, 2008). Another author mistakenly assumed that ESP from the living as an explanations of mediumship started in the publications of the early Society for Psychical Research (Sudduth, 2009).

It may be argued that such omissions are trivial and not worth so much criticism. We disagree, pointing out that if we believe that science is to some extent a cumulative enterprise, we need to have more continuity in our writings, continuity that may bring progress or at least increase the possibility of new developments. We also disagree with those who want to minimize the issue arguing that this is a common problem in science and that there are explanations for such citation myopias, such as per-

ceived lack of relevance or the existence of biases against citing older references (among them, thinking that just because something is old it is invalid and not relevant today). The issue should not be brushed aside on these terms; instead we need, to recognize the problem and then to try to solve it.

Trying to go beyond only criticism, we have tried to improve the situation by focusing on the dissemination of information about the field. This includes, for example, bibliographies (e.g., Alvarado, 2002, Zingrone, 2006), and the organization of the Parapsychology Research and Education series of free, open online courses (also know as ParaMOOC) (Zingrone, 2016). Others have contributed significantly to these efforts as well as to the compilation of comprehensive collections of essays that survey the field's history, research, and theory on the global stage (e.g., Cardeña, Palmer, & Marcusson-Clavertz, 2015; May & Marwaha, 2015).

In addition to the constant growth of literature in all topics—a somewhat less daunting prospect in parapsychological literature than in mainstream science—a key problem here is the belief that trying to know as much as we can about the past literature relevant to our topics of concern is not important to our future success. Authors are the first ones who need to be concerned about this, but they can and need to be assisted by the critical eye of editors and referees. After all, papers published without relevant literature reviews diminish both the reputation of the journal and the usefulness of its content to present and future researchers. For this reason we are very pleased to see Cardeña's (2017) statement: "As one of my editorship policies, I demand that papers contain adequate literature reviews of relevant works, no matter how old" (p. 104). We are not arguing that every paper needs a long review going back to antiquity, drawing in historical sources for every aspect of the topic. In fact, some reviews are too general or unfocused, full of references not of direct relevance to the topic at hand. But a good review is important, as we have argued, because it provides context, builds consensus, and deepens the meaningfulness of our research.

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We extend our gratitude to the following reviewers for Volume 82 of the *Journal of Parapsychology*:

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