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EDITORIAL

'Fringe Science'—A Tautology, Not Pariah

James Houran
editor@scientificexploration.org
https://orcid.org/0000-0003-1725-582X

Henry H. Bauer
hhbauer@vt.edu
www.henryhbauer@homestead.com

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This joint Editorial is uncustomary but motivated by the authors' shared concern about the problem of *scientism*, i.e., the excessive belief in the power of scientific knowledge or techniques (Bauer, 2014; Gasparatou, 2017; Pigliucci, 2018) or what some authors have described as the arrogance of scientific authority (Butler, 2015). On this issue, Frank (2021) noted that

> The most important reason [scientism] is a mistake is because it is confused about what it's defending. Without doubt, science is unique, powerful, and wonderful. It should be celebrated, and it needs to be protected. Scientism, on the other hand, is just metaphysics, and there are lots and lots of metaphysical beliefs. (para. 7)

We further think that scientism involves rigidity about what research topics are branded 'acceptable' vs. 'heretical.' The implication here being that some issues are offensive to orthodox sensibilities because they presumably (a) have no value in generating *new* scientific knowledge, or (b) undermine confidence in the evidence for *current* scientific thought.

To clarify, orthodoxy is simply the majority view of present-day professional experts or what scientific institutions assert; it is not guaranteed to be faithful to Nature's reality. In criticizing anything contrary to mainstream thinking, the belief is implicitly conveyed that the currently held majority view in science is always to be trusted and used as the basis for important actions. Explicitly, of course, even the most fervent science groupies will admit that the scientific process is not infallible. But as everything unorthodox is denigrated and faulted, it is subliminally asserted that the reigning scientific views can always be trusted; thus, a conviction of certainty is expressed even when actual certainty is lacking (Bauer, 2014) and with apparently an overt deniability that this is being done deliberately.

Those seduced by scientism certainly mirror passionate advocates with uninformed or unexamined beliefs about mysterious phenomena (Irwin et al., 2017). The 'true believer' vs. 'ardent skeptic' dichotomy, thus, is contrived, if not patently false. To our way of thinking, every measured researcher is inherently part *believer* (i.e., has confidence in the relevance of research results) and part *skeptic* (i.e., adopts procedures and controls to reduce errors and bias in inferences). The most maverick investigators also seem to exhibit high levels of curiosity and humility in their pursuit of knowledge, especially about their own blindspots in research. This latter characteristic—*intellectual humility*—comprises a budding movement in academia and reflects the simple recognition that the things you believe in might, in fact, be wrong (Bąk et al., 2022; Fetterman et al., 2019; Pennycook & Rand, 2019; Porter & Schumann, 2018; Rohrer et al., 2018). As such, this essay addresses three questions that came to us when we pondered the scientific community's historical quest to balance conviction and humility in the light of discovery.

Are 'Fringe Topics' Truly Heretical in Mainstream Science?

The key issue is not why everyday people "believe weird things" as Shermer (1997) put it, because scientists likewise have convictions about many bizarre sounding and scientifically unresolved concepts including the Big Bang, dark energy, multiverse theory, and quantum gravity and entanglement. A more cogent question might be "What is the merit of studying weird things?" Here we mean unusual or unexplained observations that cynics variously describe as being fanciful to delusional (e.g., Carroll, 2003; Novella, 2018; Shermer, 2002) but are nonetheless popular within lay and technical sources on unexplained phenomena. Before delving into the potential benefits of researching such anomalies or aberrations, we should first address whether the academic community actually thinks there *is* any merit to be had.

For a preliminary answer, we devised a 'Five-Minute Search' quasi-scoping exercise to gauge mainstream science's engagement with unsolved mysteries in the public's awareness and imagination. Scoping reviews are commonly used to examine the extent, range, and nature of research activity in a topic area and to determine the value and potential scope and cost of undertaking a full systematic review (Pham et al. 2014). Accordingly, we searched the broad scholarly literature via Google Scholar, PubMed, Scopus, ResearchGate, and Academia.edu for 'recent and accessible' peer-reviewed articles that matched 76 keywords across nine groups of popular anomalies (cf. Table 1).

We confined the search to articles that (a) preferably were published within the last *five* years (2017—2022) but were (b) not more than between *six* and *ten* years old (2012—2016); and (c) appeared in *mainstream* journals versus niche periodicals catering to anomalists (e.g., *Journal of Parapsychology*, *Cryptozoology*, or *Journal of UFO Studies*). To measure the ease of accessibility of the literature, we also searched for only 5 minutes per each keyword. This time limit seems arbitrary and restrictive, but one researcher of online consumer behavior noted that ". . . a reasonable benchmark for average session duration is between 2 and 3 minutes. A good average session duration, then, might be anything above 3 minutes. In fact, 55% of the marketers we surveyed reported an average session duration greater than 3 minutes, and 27% reported average session durations greater than 4 minutes" (Albright, 2021, para. 25–26).

This exercise produced some sobering outcomes that undercut our expectations. Table 1 shows that out of the 76 'fringe' topics: (a) Only 3 (i.e., 4%) were *not* found in mainstream sources; (b) 12 (or 16%) were represented in studies published more than a decade ago; (c) 19 (or 25%) were published within the last 6 to 10 years; and (d) 42 (or 55%) were covered by studies within the last 5 years. This suggests that anomalies characterized as 'pseudoscientific, conspiratorial, or junk science,' in some circles are actually well represented in the recent, peer-reviewed literature. This finding softens some of the suspicions about heretical topics that we held earlier in this Editorial. That is, we found no evidence that mainstream science has ignored or dismissed out of hand these lines of study. It seems therefore that the phenomena listed in Table 1 are plainly not 'off limits, irrelevant, misguided, silly, or taboo.' Rather, academia seems to agree that controversial or hot-button topics can and should be studied or contextualized scientifically. But accusations that such anomalies can be 'strange, amusing, or dangerous' (cf. Carroll, 2003) are fair and appropriate, as their mere presence or connotation ostensibly challenges some of the orthodoxy. Moreover, the skeptical literature clearly shows that debunkers regard it as dangerous, even an existential threat, when the contemporary, mainstream scientific consensus is not fully accepted as true for all practical purposes. Such 'pseudo-skeptics' are, in fact, merely acolytes of scientism (Truzzi, 1987).

How Do Scientists Deal with 'Fringe' Observations?

Our cursory findings do not imply that *all* journal editors, reviewers, or authors are open-minded to fringe areas. Sadly, like many of our *Journal* authors, we too have experienced irrational responses or feedback when submitting papers to some mainstream periodicals. But our exercise indicates that these topics are not systematically disliked or shunned. It seems to us that the real targets of ire or scorn in mainstream academia are the 'unorthodox' interpretations or conclusions about anomalies proposed by some authors. This is to say that academic authorities typically resist such claims. True enough, published research about an anomaly is neither always synonymous with its confirmation nor an endorsement of a particular interpretation.

Hence, Table 1 also indicates how many of the cited studies reached 'favorable, unfavorable, or neutral conclusions' about the scientific validity of the subject under scrutiny. For ease, an independent party rated the articles so that the trends would not reflect our personal biases. Of those topics with corresponding references (n = 73), the rater noted that 46 (63%) of the studies drew neutral conclusions, 17 (23%) seemed favorable, and 10 (14%) were clearly unfavorable. The scoping exercise revealed that a large variety of fringe topics appear in the mainstream literature, but these latter results suggest that the respective authors' interpretations or conclusions are mixed albeit certainly skew toward open-mindedness or agnosticism.

TABLE 1. Illustrative Studies of 'Fringe' Topics Published in Mainstream Academic Journals

General Topic	Conclusion **Pro, Con,** or **Neutral**	Reference
Parapsychology—Spontaneous Cases		
Apparitions / visions	Neutral	Castelnovo et al. (2015)
Haunted houses	Neutral	Dagnall et al. (2020)
Macro-psychokinesis	Neutral	Wiseman & Morris (1995)
Near-death experiences	Neutral	Moore & Greyson (2017)
Out-of-body experiences (OBEs)	Pro	Smith & Messier (2014)
Precognitive dreams	Con	Valášek et al. (2014)
Reincarnation / past life memories	Neutral	Moraes et al. (2021)
Parapsychology—Experimental		
Mental mediumship	Pro	Sarraf et al. (2021)
Physical mediumship	Neutral	Wiseman et al. (2010)
Precognition / predictive anticipatory activity	Pro	Mossbridge & Radin (2018)
Telepathy	Con	Rouder et al. (2013)
(Entity) Encounter Experiences		
After-death communications	Pro	Woollacott et al. (2021)
Alien abduction experiences	Neutral	Forrest (2008)
Electronic voice phenomena	Neutral	Williams et al. (2021)
Entity encounters and DMT	Neutral	Davis et al. (2020)
Fairy encounters	Neutral	Young (2018)
Instrumental transcommunication	Pro	Laszlo (2008)
Mirror- and eye-gazing experiences	Pro	Caputo et al. (2021)
"Old Hag" attacks—sleep paralysis	Neutral	Jalal & Ramachandran (2017)
Sensed presences	Neutral	Barnby & Bell (2017)
Cryptozoology		
Dragons	Neutral	Cheetham (2014)
El Chupacabra		---
Jersey Devil	Neutral	Regal (2015)
Loch Ness monster	Neutral	Moir (2015)
Mutagens	Neutral	Anderson (2021)
Sasquatch	Con	Sykes et al. (2014)
Sea serpents	Con	France (2018)
Unicorns	Neutral	Kosintsev et al. (2019)
Ufology		
Anomalous implants	Con	Perrotta (2020)
Belief in UFOs	Neutral	Escolà-Gascón et al. (2021)
Cattle / animal mutilations	Neutral	Goleman (2011)
Implications of extraterrestrial life	Pro	Andresen & Chon Torres (2022)
Missing (or altered) time experiences	Neutral	Stanghellini et al. (2016)
Physical traces of UFOs		---
Techno-signatures	Neutral	Mannings et al. (2021)
Unaccounted for pregnancies		---

TABLE 1 (*continued*)

Biomedical & Bioenergy Phenomena		
Acupuncture	Neutral	Ji et al. (2020)
Color effects on human functioning	Neutral	Elliot (2015)
Kirlian photography	Neutral	Rastogi et al. (2021)
Music effects on human functioning	Neutral	Manikandan & Akshaya (2021)
Reiki (therapeutic touch)	Neutral	Thrane et al. (2017)
Spontaneous human combustion	Con	Koljonen & Kluger (2012)
Spontaneous Remissions	Neutral	Radha & Lopus (2021)
Superhuman physical abilities	Neutral	Kozhevnikov et al. (2013)
Anthropology, Ethnography, & History		
"Antikythera mechanism" (ancient Greece)	Neutral	Freeth et al. (2006)
Bermuda Triangle	Neutral	Neilsen (2000)
Crop circles	Neutral	Northcote (2006)
Dracula mythology	Neutral	Akeroyd (2009)
"Jack the Ripper" serial murders	Neutral	Louhelainen & Miller (2020)
Kennedy assassination	Con	Linsker et al. (2005)
King Arthur legend	Neutral	Breeze (2015)
Lost Continent of Atlantis	Neutral	Rapisarda (2019)
Pope Joan	Neutral	Noble (2013)
Shakespeare authorship question	Neutral	Leigh et al. (2019)
Shroud of Turin	Neutral	Casabianca et al. (2019)
Stonehenge monument	Neutral	Cox et al. (2020)
Vampirism	Neutral	Browning (2015)
Werewolf mythology	Neutral	de Blécourt (2007)
Physics, Cosmology, & Nature of Reality		
Ball lightning	Pro	Keul (2021)
Cold fusion	Pro	Freire & de Andrade (2021)
Observer-based reality	Pro	Proietti et al. (2019)
Simulation hypothesis	Pro	Bostrom & Kulczycki (2011)
Teleportation	Pro	Langenfeld et al. (2021)
Time travel	Pro	Tobar & Costa (2020)
"Warp drives" (faster-than-light travel)	Pro	Lentz (2021)
Religious or Occult Phenomena		
Astrology	Con	Helgertz & Scott (2020)
Curses or hexes	Neutral	Waters (2020)
Demonic possession	Con	Perrotta (2019)
Exorcism	Neutral	Giordan & Possamai (2016)
Marian apparitions	Pro	Krebs & Laycock (2017)
"Miracle of the Sun" at Fatima	Con	Wirowski (2012)
Power of prayer	Pro	Simão et al. (2016)
Stigmata	Neutral	Kechichian et al. (2018)
Voodoo	Neutral	McGee (2012)
Witchcraft	Neutral	Conti (2019)
Zombiism	Pro	Nugent et al. (2018)

As for believers, an initial curiosity about any mystery is surely a natural characteristic of humans. The desire to find an answer likely predisposes these individuals toward accepting positive evidence perhaps too readily. But why should anyone be passionately determined that no one else should take mystery-pursuits seriously? Here some skeptics echo the Velikovsky Affair, whereby people purporting to speak for 'science' declared Velikovsky wrong while also admitting they did not read his book (Bauer, 1984). But this pessimism is too broad of a stroke to characterize all or even most researchers. The reality is that the broad scientific community seems quite comfortable, at least in some contexts, confronting unusual or disruptive information. There are even formal names for some of these observations or data—i.e., *outliers* and *fringeliers*—although these concepts have important similarities and differences.

In simplest terms, an outlier is a data point that differs significantly from other observations. Osborne and Overbay (2004, p. 1) nicely summarized some nuances about its meaning or relevance:

> Although definitions vary, an outlier is generally considered to be a data point that is far outside the norm for a variable or population (e.g., Jarrell, 1994; Rasmussen, 1988; Stevens, 1984). Hawkins (1980) described an outlier as an observation that "deviates so much from other observations as to arouse suspicions that it was generated by a different mechanism" (p. 1). Outliers have also been defined as values that are "dubious in the eyes of the researcher" (Dixon, 1950, p. 488) and contaminants (Wainer, 1976).

Understand that outliers are inherently different from *noise*. An outlier is part of the data, but noise is a random error that could involve mislabeled, mistaken, or even missing information in a dataset. Wainer (1976) also introduced the related idea of the fringelier. This term denotes "unusual events which occur more often than seldom" (p. 286). These points lie near three standard deviations from the mean and hence may have a disproportionately strong influence on parameter estimates yet are not as obvious or easily identified as ordinary outliers due to their relative proximity to the distribution center.

And then sometimes we have completely new and potentially disruptive observations that can spark paradigm shifts in scientific thinking (Kuhn, 1962/1996). We liken these types of anomalies to a 'Nolan Ryan fast ball'—high, hard, and you did not swing because you did not see it coming. It is also worth noting that such discoveries certainly help to promote intellectual humility. In the end, though, scientists seemingly deal with 'fringe' or 'anomalous' looking information like any other data point, i.e., by using repeated or iterative testing to determine whether unusual, unexpected, or unexplained observations are due to *error* ('noise'), *aberration* (e.g., 'outlier or fringelier'), or an *a-ha* ('breakthrough').

How Can Science Best Learn from Fringe Topics?

This question has the most straightforward answer. Consistent with the above, Wuestman et al. (2020, table 1) explained how scientific breakthroughs stem either from questions or observations. For example, *charge-type* discoveries are driven by a question, be it a new or known question, and are in line with existing literature. This first category addresses "known unknowns" (Logan, 2009) and might describe most studies and their conclusions. But then we have two other categories that are observation-based versus question-based. *Chance-type* discoveries are driven by new observations or evidence that could agree with existing literature or not. *Challenge-type* discoveries are driven by new or existing evidence that bucks the existing literature.

The discovery of a new explanation for certain 'facts' (i.e., valid and replicable observations) is most critical for challenge-type discoveries, not the uncovering of the facts per se. So, studying the nature and meaning of anomalies directly relates to quality control in scientific model-building and theory-formation. That is, outliers, fringeliers, and other unexpected or non-standard observations are especially valuable because they can indicate crucial errors with accepted data, analysis, or interpretation (a chance- or challenge-type discovery). This view of 'anomalies as object lessons' nicely parallels the approach of modern technology firms and their mantra of 'fail fast' and a striving to 'break things' to learn information as quickly and intelligently as possible (for a discussion, see Draper, 2017). But noted physicist John Archibald Wheeler (1911–2008) should be recognized as possibly the first to voice this basic insight with his recommendation that "In any field, find the strange thing and explore it."

SOME CLOSING THOUGHTS

The term *fringe* (or *edge*) *science* is undeniably a tautology because the process of knowledge accumulation and scientific discovery—by definition—is always on the boundary of current understanding and thus on the brink of the unknown. Although all of science is ultimately fringe, this does not imply that all topics are automatically appropriate for the *Journal*. Our periodical targets questions, and especially observations, that are "ignored or studied inad-

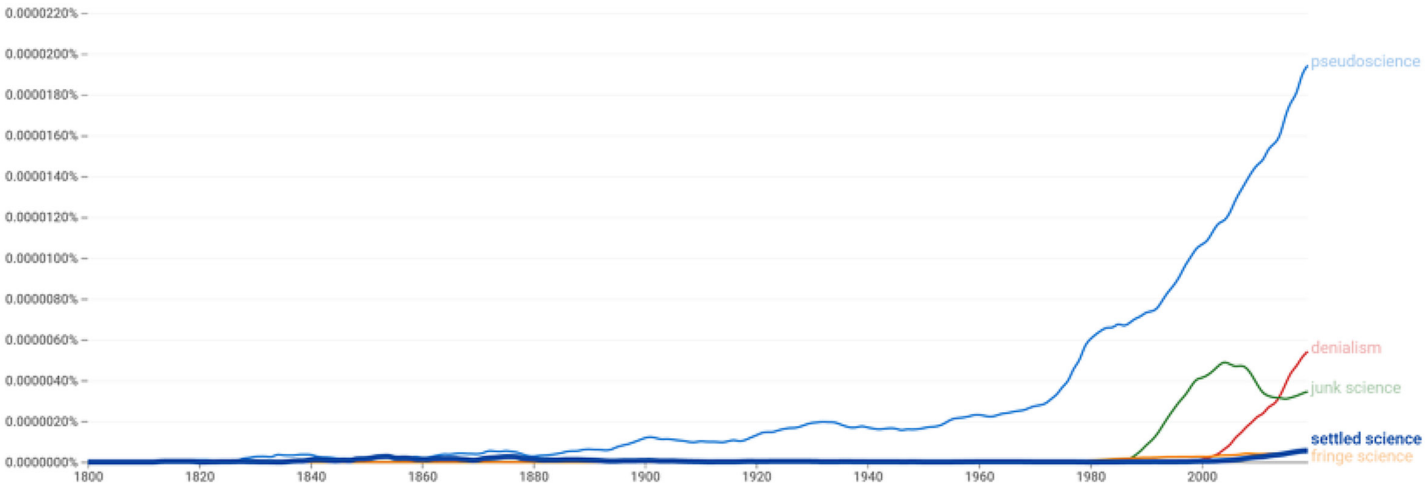

Figure 1. Google Books Ngram Viewer Results for scientism-type terms (1880–2019, English). Note: Analysis conducted July 3, 2022.

equately within mainstream science." Thus, its authors and readers represent a community of students and scientists in the doorway of potentially chance- or challenge- type discoveries. For this reason, we personally prefer the all-inclusive term *frontier science* (and frontier scientists) to describe the interests and activities of the Society for Scientific Exploration (SSE).

Contrariwise, we wonder how often pejorative phrases such as 'pseudoscience' or 'junk science' are used by those with low intellectual humility to ignore fundamental questions of truth and falsehood. For instance, Figure 1 shows a Google Ngram of the frequency of usage of such terms in English books. This graph is not specific to adherents of scientism, but it does arguably reflect an increased influence of scientism on society. After all, the central question is whether particular research activities characterized in negative ways are properly science or not. Pseudoscience originally referred to the reasonable concern about claims of using scientific methods when these were not actually used. Rigorous frontier science instead involves applying the scientific method appropriate to the topic and maintaining clarity about any biases that prevent or support a particular interpretation of the results. It also includes creating applications that can further inform us about the underlying mechanisms of a frontier science topic.

We therefore encourage frontier scientists not to focus on short-term efforts to convince myopic debunkers or disinterested mainstream researchers about the respectability and value of studying various kinds of anomalies. Any corresponding results would be akin to wisdom falling on deaf ears. Likewise, we agree with Braude's (1998, 2020) concerns over attempts to rename or rebrand frontier science topics as more 'acceptable' subjects versus plainly declaring what they are. This tactic is unnecessary. Our cursory review indeed shows that mainstream academia knowingly confronts frontier topics, although individual authors still hotly dispute their nature or meaning. But this longer-term system of peer review and debate to verify observations and conclusions is how science is supposed to work; taking the necessary time to distinguish true discoveries from false ones.

Published findings on frontier science topics are well-positioned to engage and inform the one audience that conceivably matters most, i.e., the assemblage of future researchers who will be guided by the cumulative and evolving empirical literature. Our collective energies are thus better spent celebrating and 'owning' our unique and valuable place in the scientific arena. To be sure, we deem anomalistics and frontier science as something more than a field of study; it is actually a practiced philosophy that balances verifiability in science with vigorous intellectual humility toward chance- and challenge-type discoveries. In this spirit, we modestly propose that another term and associated ideology is the real pariah and threat to scientific progress—namely, statements of *settled science*. This oxymoronic phrase never seems to be used to advance inquiry and understanding, but rather only as a weak argument to shut it down.

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RESEARCH ARTICLE

Empirical Analysis of the Hugh Gray 'Nessie' Photograph

Roland Watson

shimei123@yahoo.co.uk

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HIGHLIGHTS

Detailed inspection of the best-quality version of an early photograph of the 'Loch Ness Monster' does not verify the presence of a swimming dog or other familiar object. This picture thus remains an intriguing piece of evidence that seemingly supports a biological mystery at this famed location in Scotland.

ABSTRACT

The vintage Hugh Gray photograph is the first ever to presumably show a 'Loch Ness Monster.' Many monster believers have regarded it as genuine proof, whereas others have insisted that it reveals something other than a Nessie and may even be a hoax. Based on systematic comparisons of the photograph's subject matter to control images, an empirical case is made that the picture contains an authentic anomaly, its current criticisms are more speculative and not properly evidence-based, and the corresponding interpretations offered as alternatives are inferior. The present analysis does not conclusively identify any species in the photograph, but it underscores that any proposed explanation (whether unorthodox or conventional) for an esoteric phenomenon must be subjected to hypothesis-testing to ensure its viability and validity when applied to a specific case.

KEYWORDS

Empiricism, hypothesis-testing, Loch Ness Monster, Nessie, photographic analysis, skepticism

INTRODUCTION

The first photograph reputedly of Scotland's famous 'Loch Ness Monster' (affectionately termed 'Nessie' in mainstream culture) was taken on 12th November 1933 around noon by a local man by the name of Hugh Gray. The term Loch Ness Monster is emphasized, because the author believes the photograph to be genuine and part of the evidence portfolio (e.g., Bauer, 1986, 2002a,b). Figure 1 shows the picture that generally circulates.

The *Scottish Daily Record* took his picture and Mr. Gray gave the following account to them, having been interviewed by Hugh Mackenzie (the future Provost of Inverness), Peter Munro representing Hugh Gray's employers at the British Aluminium Company, and a *Daily Record* staff member:

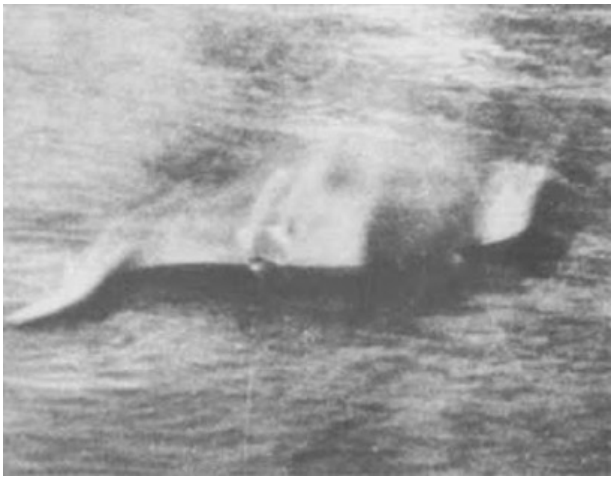


Figure 1. Hugh Gray's famous photograph of the 'Loch Ness Monster' (Whyte, 1957).

> Four Sundays ago, after church I went for my usual walk near where the river enters the Loch. The Loch was like a mill pond and the sun shining brightly. An object of considerable dimensions rose out of the water not very far from where I was. I immediately got my camera ready and snapped the object which was two or three feet above the surface of the water. I did not see any head, for what I took to be the front parts were under the water, but there was considerable movement from what seemed to be the tail, the part furthest from me. The object only appeared for a few minutes then sank out of sight. (Mackenzie et al., 1933)

The tenor of the account suggests some throwing up of spray and water as portions of the body beat about the waters and hence caused some opacity around that region of the picture. Mackenzie described Gray as a man highly respected by his fellow workmen, employers, and locals. Likewise, the *Daily Record* had the negative examined by four experts who deemed it to be untampered. It caused a stir but was rejected by zoologists and faded along with general Nessie-lore as the world entered war six years later. Twenty-two years on in May 1995, Loch Ness researcher Constance Whyte visited Hugh Gray who was sticking to his story and still had vivid memories of that day, plus he also recounted five other times he claimed to have seen the monster over those decades (Whyte, 1957, p. 77).

The best-known researcher Tim Dinsdale (1961) also recounted how he visited Gray in April 1960 and described him as "a most courteous individual" (p. 88) as Gray took him to the spot of the sighting. He spoke with "complete conviction" about that day and maintained the accuracy of his account (Dinsdale, 1961, p. 88). He also added some detail of his other sightings that partly consisted of rapidly moving bow waves with no visible cause. What remains of the photograph today is uncertain. A few prints have been extant over the years, but the negative appears to be lost forever. In this Internet age, one original print scanned from a book tends to win the day and becomes the prevalent picture.

EARLY COMMENTARY AND CRITICISMS

Zoologists of the time summarily dismissed the photograph and suggested other explanations such as a log, wreckage, or a whale, which are all reactions that do not surprise anyone familiar with the phenomenon. The popular wildlife author Maurice Burton (1961, p. 78) suggested that Gray had seen a gas-buoyed tree trunk in the water and even formulated a picture (Figure 2) to simulate how such an object could produce the image on the photograph.

Figure 2. Maurice Burton's (1961) depiction of the 'floating tree trunk' hypothesis for H. Gray's photograph.

Logs are often invoked as explanations for Loch Ness Monster reports, and some are indeed responsible for deceiving inexperienced observers. The light grey colour of the object is inconsistent with the darker colour of tree trunks and then there is the problem of uncritically proposing that tree debris can assume almost any shape one wishes to achieve. This is too simplistic an approach. Leaving aside Mr. Gray's un-log–like testimony, the smooth appearance of the object also dictates against the rougher texture of tree debris, and it has to be noted that this explanation has not really found favour among critics of the photograph in the ensuing decades.

Likewise, the gas-propelled tree trunk proposal suffers from the sketch in Figure 2 being hard to relate to the original photograph. Once again, the sketch is rendered to fit the theory with little consideration as to whether such an unusual trunk could achieve these effects. It has since been determined that the oligotrophic nature of Loch Ness makes the production of decomposition gas an unlikely event (Shine & Martin, 1988, p. 167). The whale theory has more mileage in that some features do look whale-like such as the large body, but others such as the long tail or neck are not consistent with a whale.

Another author, Ronald Binns (1983), had other ideas and indulged in some innuendo when he asserted that Gray was a "leg-puller" and so implied that he had hoaxed the picture (p. 96). How he came to that conclusion was rather circuitous. First, he claimed where the picture was taken should have had more foliage visible. He does not state why he assumed the tree growth had not significantly changed in the intervening 40 to 50 years. In fact, a one-inch ordnance survey map of the area drawn up between 1921–1930 depicts the site as devoid of heavy growth at that time (OS One Inch "Popular" edition Sheet 37). Look for the white circle in the "F" of "Foyers". A comparison Google satellite image from 2012 shows the increase in tree growth since the time of Gray's photograph (Figure 3).

A further reference by Binns (1983) to an "A. Gray" from the 30 May 1933 issue of the *Inverness Courier* is also presented as evidence. This Gray was reportedly contriving to use hooks, fish bait, and a barrel to capture the monster at

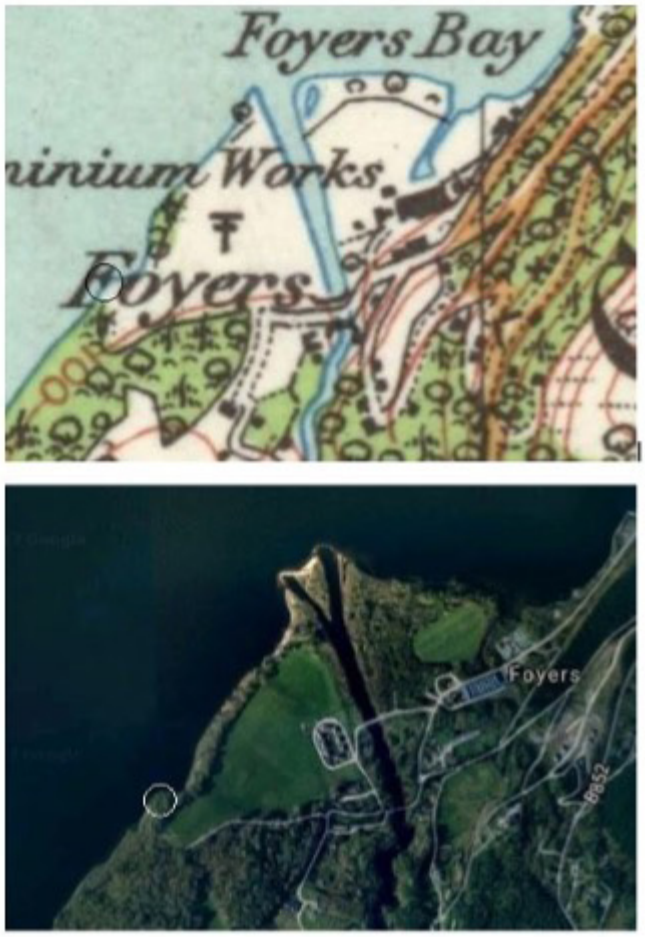


Figure 3. Evidence suggesting a lack of foliage in the area and time period of H. Gray's photograph. Contemporaneous survey map (top) compared to modern Google satellite image (bottom).

Foyers. Binns speculated that he may be the same Mr. Gray and hence a bit of a prankster. However, apart from being a Mr. A. Gray instead of a Mr. H. Gray, the matter can be laid to rest here. For some reason, Ronald Binns failed to mention a key fact from the article that Mr. A. Gray was stated as being a bus driver, whereas our Mr. H. Gray was a fitter at the Foyers Aluminium Works (*Daily Record*, 1933). In fact, it is likely that the A. Gray in question was Hugh Gray's brother, Alexander Gray, who tragically died in a drowning incident at Loch Ness in the 1940s (*Dundee Courier*, 1949).

Another researcher, Steuart Campbell, quotes Dinsdale as suggesting the photo looks retouched and mentions the popular theory today that the photograph shows nothing more than a dog swimming toward the camera with a stick in its mouth (Campbell, 1996, p. 36). Admittedly, Dinsdale is ambivalent about the picture and seems uncertain as to what it shows. As a result, he commits neither way to it and simply moves on. However, it is doubtful that Dinsdale regarded Gray as an outright faker given his previous comments about him. The matter of retouching will be addressed later.

In terms of analysis, monster researcher Ted Holiday was the most enthusiastic supporter and regarded the picture as a major piece of evidence to support his idea that the monster was a giant invertebrate. In fact, he conducts a close examination of the picture which to him reveals evidence of some warts, a slime sheet, neck segmentations, and two appendages (Holiday, 1968, Plate 8). However, the clarity of the photograph is not sufficient to be that conclusive, and this is partly because the creature was throwing up spray at the time, as well as some overexposure being present on the film.

Overexposure due to defects in the camera or film as opposed to water motion can be partly determined by observing the lighter smudged areas away from the object and comparing them to suspected water features in and around the object which carry more definition and structure—this idea will be addressed below. Nevertheless, the detail on the film is superior to most Loch Ness Monster pictures and has provoked various explanations. The most recent being that it is a swan, which we will also examine in a later section. But first we can empirically test the prevailing idea that the picture merely shows a swimming dog.

Scrutinizing the 'Swimming Dog' Interpretation

The fact that many people perceive a Labrador dog in Hugh Gray's photograph is not compelling evidence, as such reports could represent pareidolia effects. This refers to the meaningful interpretation of an ambiguous stimulus, usually visual, so that one sees an object, pattern, or meaning where none exists. This misperception or illusion certainly occurs in 'anomalous or paranormal' contexts (Brugger, 2001; Drinkwater et al., 2020; Williams et al., 2021), but everyday examples include perceived images of faces or familiar objects in cloud formations or seeing faces in inanimate objects (e.g., Wang et al., 2022). The concept further extends to allegedly 'hidden messages' in recorded music (e.g., Thorne & Himelstein, 1984) and hearing voices (mainly indistinct) or music in random noise (e.g., Alvarez Perez et al., 2017).

Refer back to Figure 1 to test whether you see a dog swimming toward you. Following from the above, there are three reasons why the 'swimming dog' hypothesis should be discounted. Firstly, and by way of experiment, the author sourced a good photo of a dog swimming in the same posture (see Figure 4) that could be layered with Gray's photograph using Microsoft Windows software.

The four-step comparative process is simple—(a) layer the control dog picture over the Hugh Gray image, (b) resize the Gray picture until it is the same size as the control

Figure 4. Control image of a swimming dog for layered analysis with H. Gray photograph. Courtesy: 123RF Limited under Free License.

picture, and (c) draw in circles to fix where the right eye and nose on both pictures are to align them (Figures 5 and 6):

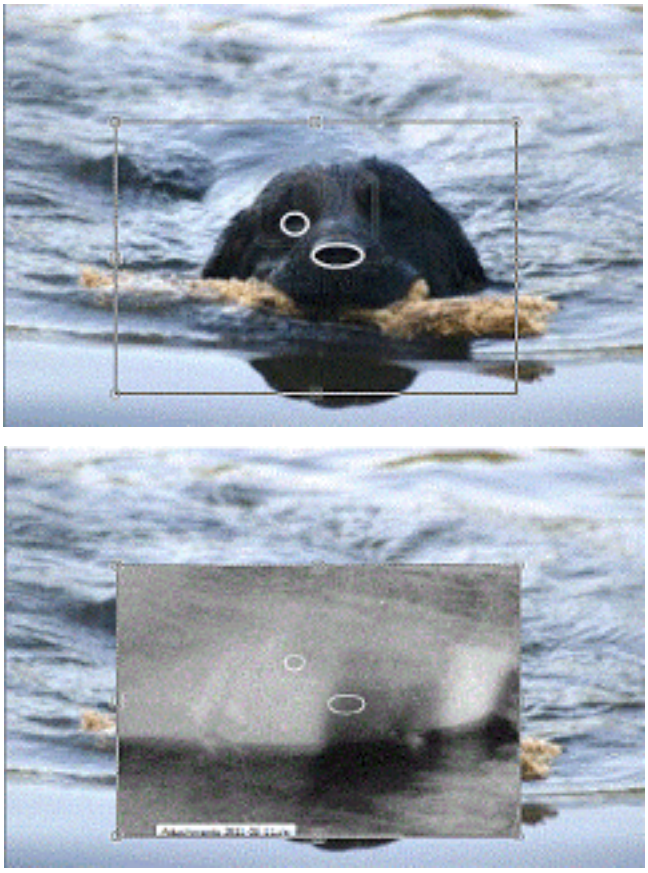


Figure 5. Placement of the nose and right eye of the control dog image with the H. Gray image.

And finally, (d) use the opacity slider on the software to vary the transparency of the control dog image to compare and contrast the key areas (Figure 6):

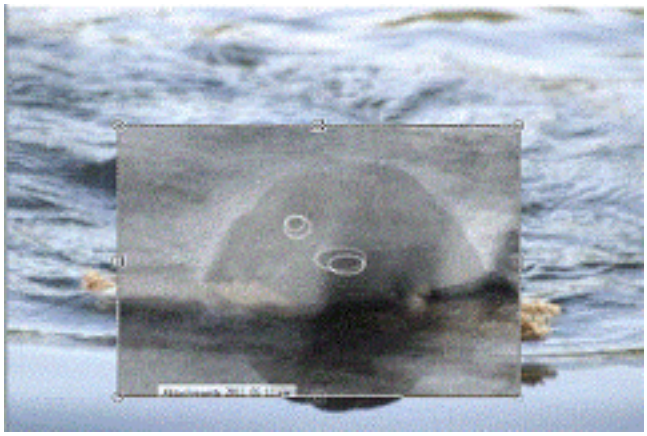


Figure 6. The control dog image layered on the H. Gray photograph for feature comparison.

What is the conclusion? The Hugh Gray 'dog' appears to be missing half of its face on the right. There is no recognisable eye or ear to fill in the complete picture. There is a splash to the right where the ear seemingly should be. The other problem is that there appears to be nothing recognizable as a stick. There is a very sharp shadow line where the creature meets the water that does not compare well with the control 'dog/stick' picture. The other problem is that the 'snout' in the Gray photograph is more elongated. Note that the real dog has his muzzle raised and spread out to accommodate the stick. In fact, a dog will tend to raise its muzzle above the water to aid breathing. The supposed 'dog' in the Gray picture appears to have its mouth too close to the water. The final observation comparing the two layered images is the distinct water line of the object, which is far too clear-cut for what is expected of a dog swimming.

The second and most important argument against a dog is that the popularly circulated version of the Gray photograph is *not* the original. In true media fashion of the time, the image was retouched to make it more legible for their readers. This was a common practice at that time where certain features were added, removed, or emphasized for publication. This may even have involved damage to the negative as scalpels were used to achieve certain effects. It was basically the photoshop of its day and was ill-advisedly used on a genre of photograph with which they were not familiar. Contemporary researcher Rupert Gould said it was retouched in his 1934 book and this was likewise reiterated in Peter Costello's (1975, p. 42) book where he lays the blame with the *Daily Telegraph* for touching it up to emphasize the waterline. This was likely the retouching that Dinsdale referred to (see earlier in this article).

How this was achieved exactly is unknown but increasing the contrast of the image also looks to have been

part of the process with the resulting over-emphasizing effect on the 'dog' image. It is best in these cases to get the most original image, and luckily, another print came into the hands of Maurice Burton in the 1960s which was made from glass lantern slides in 1933 for an E. Heron-Allen (Burton, 1969, p. 191). Importantly, these contact positives were made from the original negative and represent the best untouched picture of what Hugh Gray witnessed that day. It is this picture that was used in the layering experiment above (see Figure 6). Compare this with the retouched version (Figure 1) and readers might begin to appreciate the problem at hand. Unfortunately for most people, the visual cortex having conditioned itself to see a 'dog', it may be difficult to unsee it.

The final conclusive argument is the general structure of the picture. Ask yourself one key question: "*Where is the rest of the dog?*" Look again at the control dog picture in Figure 4 and note the bow wave and its back causing turbulence at the rear of the photograph. Now compare it to the Gray photograph in Figure 7. There appears to be absolutely nothing behind the supposed dog head. That is because there is arguably no dog body, and hence there is no dog head. To get a clearer vista, Figure 8 shows the Heron-Allen picture in its most uncropped form. Note the continuity of the wave patterns suggestive of no forward motion by the object in the picture.

Now it may be objected that this is a double exposure of a dog, but this claim does not wash either. The *Daily Record* had the negative examined by Mr. M. Howard of Kodak and Mr. C. Clarke of the *Kodak Magazine* as a safeguard, and both stated that there was no tampering of the negative (*Daily Record*, 1933). In the unlikely event that these experts failed to spot a double exposure one would still expect the rest of the double exposure to disrupt the clean wave patterns that are visible.

The conclusion is that there is no compelling evidence to suggest that Hugh Gray photographed a swimming dog. One could experiment with more swimming dog images, but the presence and implication of the superior Heron-Allen image renders this unnecessary. Nevertheless, the problem of pareidolia effects has the potential to compound. On his own Loch Ness website, researcher Tony Harmsworth explains the dog theory to readers by producing two photographs (Harmsworth, n.d.). The first is the retouched image from the *Daily Telegraph* and the second is his further touched-up version, which for experimental purposes emphasizes some dog features for people to clearly discern this 'dog.' Harmsworth also decided that Hugh Gray owned a golden Labrador dog despite no evidence to support any such claim.

Harmsworth's altered image obviously should be disregarded but given the propensity for copying and pasting on the Internet, it will undoubtedly migrate under the false pretense of being the original photograph. In fact, it already has been characterized as such on at least one web-

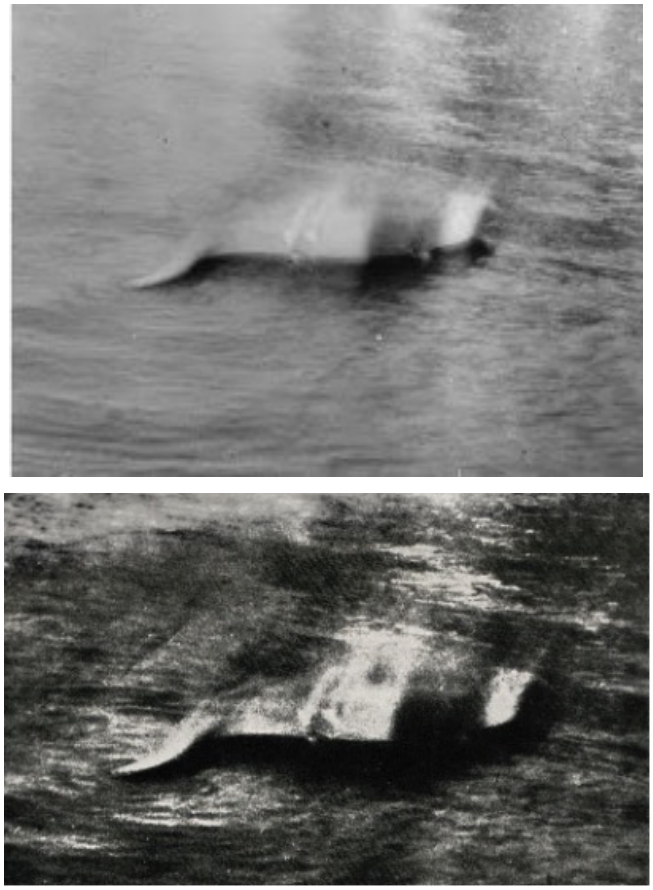


Figure 7. Heron-Allen's highest quality copy of H. Gray's photograph (top, courtesy: Fortean Picture Library) compared with the commonly circulated version of lower quality (bottom, rescanned from Whyte, 1957).

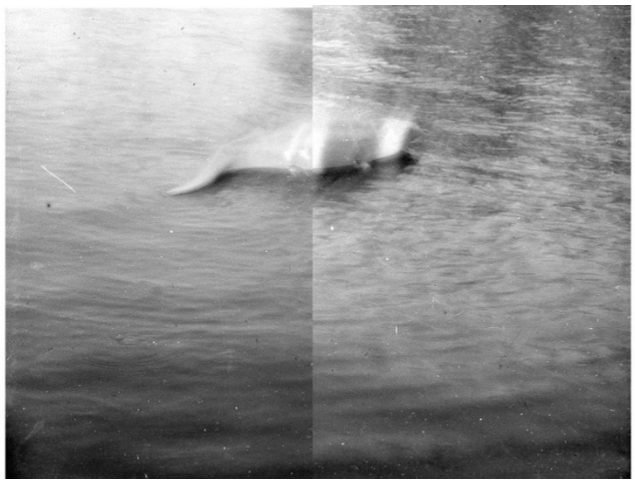


Figure 8. Uncropped version of the Heron-Allen 1933 copy of H. Gray's photograph.

site. This points out the problem with properly critiquing alternative theories today when poor quality images are used, and it is no surprise that the 'swimming dog' interpretation has persisted unchallenged for so long.

Other Interpretations and Considerations

If some interpretations overreached in extracting a dog from the photograph, then one must question whether ardent monster believer Ted Holiday equally overreached in seeing "slime sheets and warts." As previously mentioned, Holiday advocated the idea that the creature was a huge invertebrate related to worms or slugs and proposed it was a giant relative of the extinct *Tullimonstrum Gregarium*. Quite probably Holiday had a better resolution picture to magnify given that silver-based film has higher 'dots per inch' (DPI) than modern digital cameras. However, it is evident that he was using the inferior image. This is how he interpreted the image (see Figure 9, Holiday, 1968, Plate 8):

1. Neck with head submerged.
2. Neck segmentations.
3. Anterior hump.
4. One of several wart-like vesicles.
5. Anterior parapodium.
6. Sheet of slime.
7. Posterior hump.
8. Posterior parapodium.
9. A wave.

Examining these interpretations in the light of the superior Heron-Allen image, it is not certain that (1) is a neck though it does appear to slip under the water. Likewise, with (2), (4), and (6). However, the light patch marked as (6) and the wave at (9) do look like lighter patches over or on the surface of the creature. I say this rather than defects on the film (such as overexposure) because the two patches create corresponding lighter reflections on the water line below in the Heron-Allen image. The two small light "*balls*" above (5) which were erroneously taken to form the "*dog's ear*" also look like interesting features, possibly water cascades? They can be more clearly seen in the Heron-Allen image above.

Holiday's parapodia (or appendages) are certainly there, but the overall shape of the animal that Holiday draws is not correct in my opinion. Thanks to this better photograph, we can see that the outline of the creature extends beyond Holiday's wave at (9) to the right. In fact, the wave is not all it seems. The 'wave' appears to be rising and then curling down to fall, but this is an illusion, as it is more likely a water spray plus something else.

Zooming in to display that part of the creature, there is ostensibly a stubby, conical-like morphology present that can be traced partly into the spray. There is also a suggestion of something like 'drips' falling from this feature and

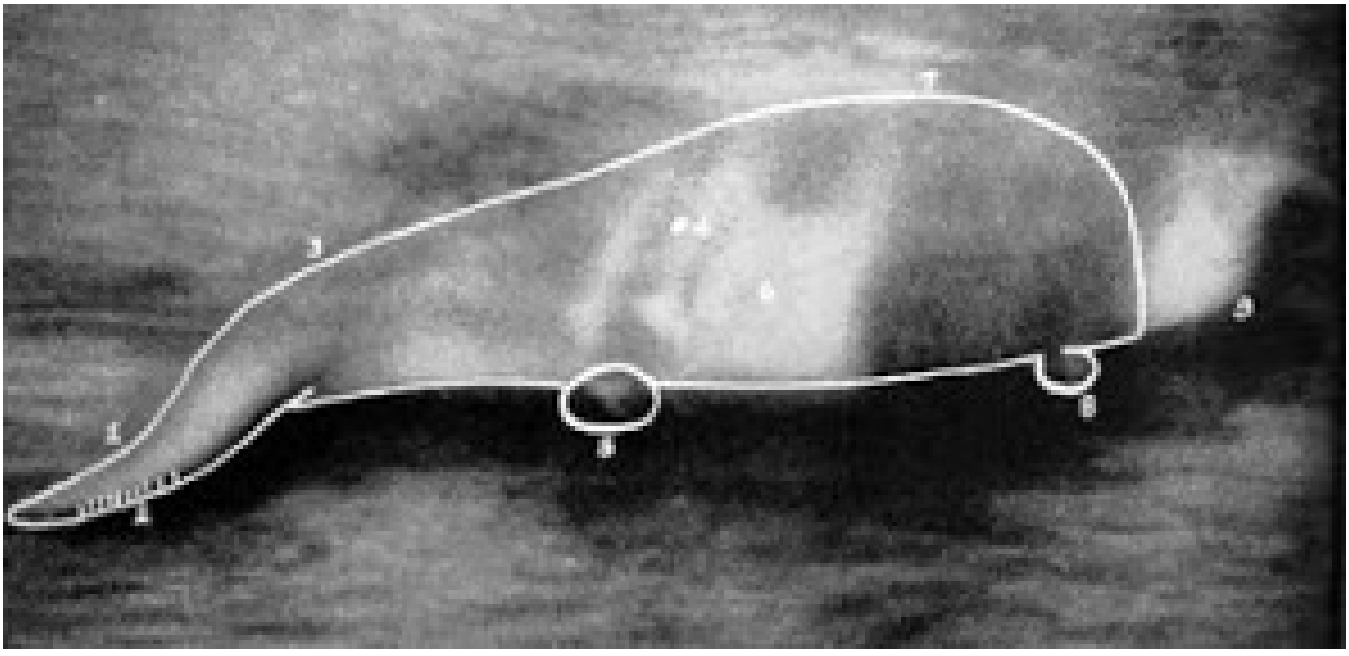


Figure 9. Holiday's (1968) interpretation of the H. Gray photograph as showing a 'huge invertebrate.'

creating their own little concentric ripples below. To confirm its solidity, Figure 10 shows that this conical feature casts a conical reflection on the water below.

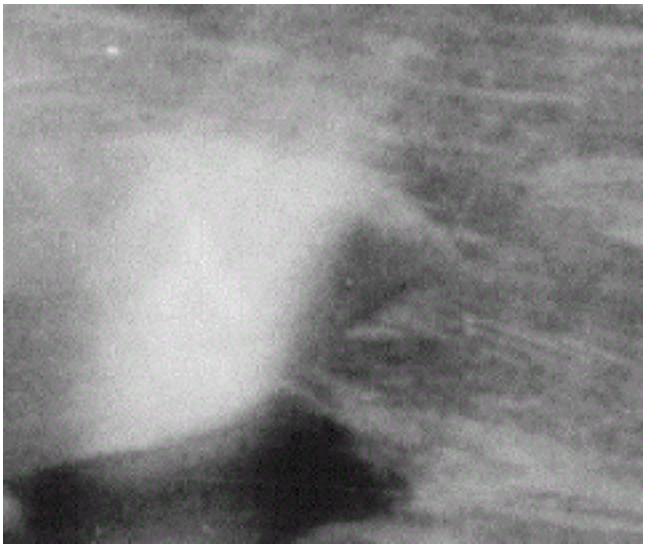


Figure 10. Apparent conical feature and associated shadow of the object in H. Gray's photograph.

The annotated image in Figure 11 highlights the features visible. The dark interior of the mouth and what may be a tongue can be seen with the suggestion that the head is partly turned to the camera. An eye is seen above the leftmost part of the mouth. The distorted reflection below shows the outline of the head in the water.

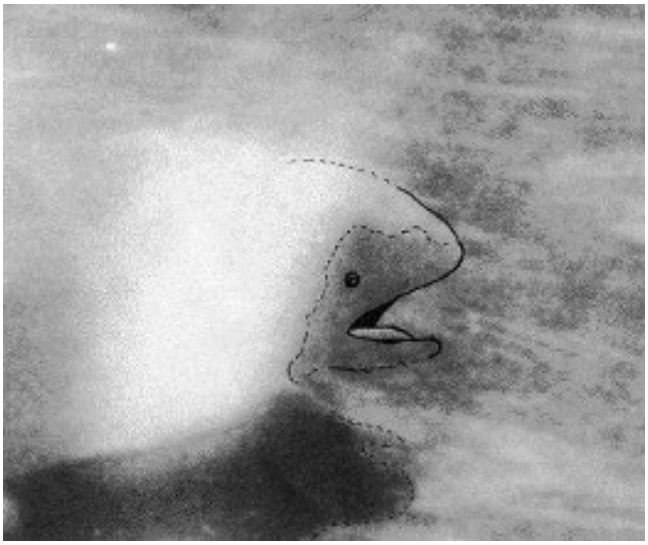


Figure 11. Annotations to emphasize features apparently visible regarding the object in H. Gray's photograph.

In my opinion it produces a reflection on the water below compared to the darker body reflections, as well as its clear demarcation at the waterline. Note also the reflection is at a different angle to the spray as one would expect. It is into this water formation that the head disappears, and it is difficult to deduce anything about a potential neck from that point onward, though clearly it cannot be of a great length given the proximity of the body. The position of the presumed eye suggests a more fish-like than cetacean appearance, since whales and dolphins have eyes beyond the end of their mouths and not above it.

What this object depicts certainly resembles some type of face with an eye and open mouth. This interpretation is less likely to be pareidolia, as it is a clearer feature than the incomplete 'dog,' and it casts a reflection on the waters. But it may be argued that it could still be a solid object exhibiting pareidolia effects. In that context, Ted Holiday classed it as a 'wave,' but he was using an inferior image, and it is difficult to see how a splash would achieve this pareidolia effect. As was stated with the floating log explanation, one has to be careful in indiscriminately invoking explanations without good cause. Not every proposed explanation is conducive to pareidolia. What kind of situation would provide the necessary fertile ground to produce such a deceptive image? In the case of the 'dog,' it was a chaotic combination of editorial retouching, poor contrast, water movement, features on the object, and some overexposure.

Practically all the theories proposed in the last six decades to explain the general picture make no provision for pareidolia in the specific 'head' portion of the image. It was an observation they could not make with the inferior photograph that was extant. We await further critical feedback on the matter as explanations such as logs, dogs, and swans were not concocted with this in mind. As for the interpretation itself, there is no compelling reason against proceeding with the assumption this is indeed the head or face of an animal, known or unknown. One could argue that no precedence from other researchers for seeing such a head casts doubt upon it. However, such an interpretation was only possible using the superior Heron-Allen image. It seems that this image only came into the public domain in 1986 when it appeared in two books. Bauer (1986, p. 14) explained in one of these books that a glass lantern slide was made for an E. Heron-Allen in 1933 and which came into the possession of Maurice Burton in the mid-1960s. Sadly, these better images were not made available for another twenty years when Steuart Campbell negotiated their release to the Fortean Picture Library. By then, almost all of the classic books on the creature had been written and next to no books were published for the next twenty years that could have offered such an interpretation.

But the 'head or face' presumption is no novel interpretation of the photograph, as the old news clipping in Figure 12 shows (*Courier Herald*, 1933).

LOCH NESS MONSTER

BLURRED PHOTOGRAPH

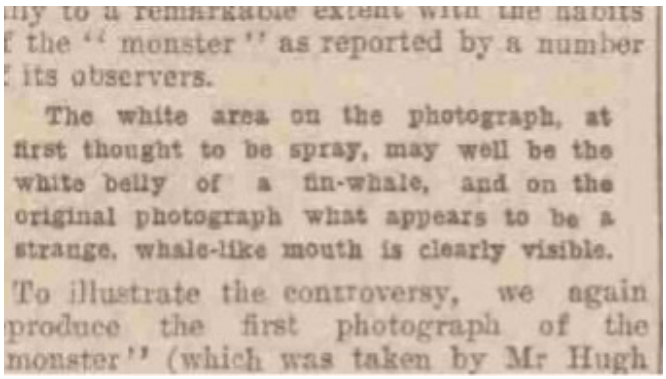
LONDON, December 6.

The "Sketch" publishes a snapshot which is described as that of the unidentified monster reported to have been seen in Loch Ness. The snapshot is a blurred impression of an animal with a body like a whale, a head like a seal, and an elongated tail. Scientists say that the negative is genuine, and has not been tampered with.

Mr. Norman, the chief of the piscatology department of the Natural History Museum, London, says he is not sceptical about the fact that some sort of animal exists in the loch, though the eyesight plays extraordinary tricks, and the alleged animal may merely be wreckage.

Figure 12. *Courier Herald* (1933) clipping noting that the object in H. Gray's photograph had a seal-like "head."

This is repeated in another newspaper of the time which suggests a whale's head is visible (Figure 13; *Aberdeen Press and Journal*, 1933).



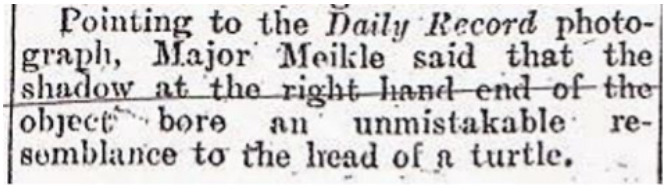
...ly to a remarkable extent with the habits of the "monster" as reported by a number of its observers.

The white area on the photograph, at first thought to be spray, may well be the white belly of a fin-whale, and on the original photograph what appears to be a strange, whale-like mouth is clearly visible.

To illustrate the controversy, we again produce the first photograph of the monster" (which was taken by Mr Hugh

Figure 13. *Aberdeen Press and Journal* (1933) clipping that described the H. Gray photograph apparently showing a "whale-like mouth".

Moreover, the *Daily Record* ran some readers' opinions, and one suggested that the object bore a remarkable resemblance to a turtle (Figure 14; *Daily Record*, 1933).



Pointing to the *Daily Record* photograph, Major Meikle said that the shadow at the right-hand end of the object bore an unmistakable resemblance to the head of a turtle.

Figure 14. *Daily Record* (1933) reference to a turtle-like head on the object in the H. Gray photograph.

The photograph made international headlines and naturally people speculated about what it showed. Curiously, no one mentioned Labrador dogs with sticks in their mouth, and we ascribe that to having a superior reproduction of the photograph at hand. The first article mentioned an animal with the "body like a whale, a head like a seal, and an elongated tail" (*Daily Record*, 1933). This description, from a source shortly after the picture was taken, implies the perception of a head and thus corroborates the 'face with an eye and open mouth' interpretation mentioned earlier. The only difference is that this vintage description assumes it to be seal-like rather than our fish-like speculation. To that we should add the proposed whale and turtle heads. The present author is unsure what animal head it represents, but a head certainly seems to be present.

One later critic of the picture who may mention this head was Maurice Burton. As stated at the beginning, he had suggested a tree trunk as an explanation. But when he came into possession of the superior Heron-Allen prints, he altered his explanation to that of an otter rolling in the water. Now, he does not explicitly state that he perceived a head to the far-right of the image, and so it is partly speculative whether he conjectured an otter's head was sufficient to explain what he saw. To me, it is clear that the head of an otter bears little resemblance to the head in the photograph.

But it is pointed out that Hugh Gray said he saw no head in his original account. How are these reconciled? Reviewing his testimony at the top of the page, it seems he was mistaken when he says he saw no head because "the front parts were underwater." In other words, he took the elongated feature on the left to be the long neck as per previous eyewitness testimony. Once he decided that, what was on the right he then assumed to be the tail. The "considerable motion" he describes on that side plus the water sprayed around further obscured his sight. To quote Constance Whyte (1957), who later interviewed him:

> He was looking down from a height of 30 feet or so when suddenly there was a great upheaval of the water followed by a terrific commotion about a hundred yards out, and about 40 feet of a thick rounded back and a powerful tail came in sight but the head was submerged. Contrary to reports which appeared at the time, Mr. Gray never saw the head. The creature lashed about furiously and was so enveloped in spray that further details could not be distinguished. He took five snaps before the object disappeared. Because there was so much splashing and also because he was busy with the camera, Mr. Gray did not have an opportunity to observe the creature closely. (p. 2)

So, can Hugh Gray be described as a reliable observer in this instance? The answer is *yes* and *no*. Reading his account to Whyte, it is revealed that the spray thrown up by the creature meant "further details could not be distinguished." It is also stated that his observations were hindered because "he was busy with the camera." This is no surprise because he was occupied with taking five photographs with a rudimentary 1930s box camera (only one image came out in development). He spent more time looking through the viewfinder than looking at the creature.

Note that this was a man who lived many years at the loch and regularly went for walks along the shoreline. It should be a given that he was familiar with the different moods of the loch and its regular inhabitants, and thus be less prone to self-deception than a tourist. However, once he initially spotted the object, assessed it was a creature of extraordinary appearance, his experience became no better than that of any other average person in zoology. His attention was torn between camera and naked eye. It is no surprise then that his normal attention to detail was compromised. That does not negate the fact that he saw a large creature in the waters, only that further details were wanting. What was revealed in the photograph compensated for that.

Whyte tells us that she re-examined the photo with Hugh Gray. Unfortunately, this was more than likely to be the inferior image published in her book which totally obscures all features to the right. Critics will often point out that humans are imperfect recording machines and that images recorded on film will often help resolve matters. In this case, the present author quite agrees with them. No dorsal fin is visible, though this is not really an issue for fish such as the eel. Thinking of an eel in this context immediately suggests Roy Mackal's (1976, p. 140) thick-bodied eel interpretation of the creature. Putting this together gives a speculative outline of the creature's body as shown in Figure 15.

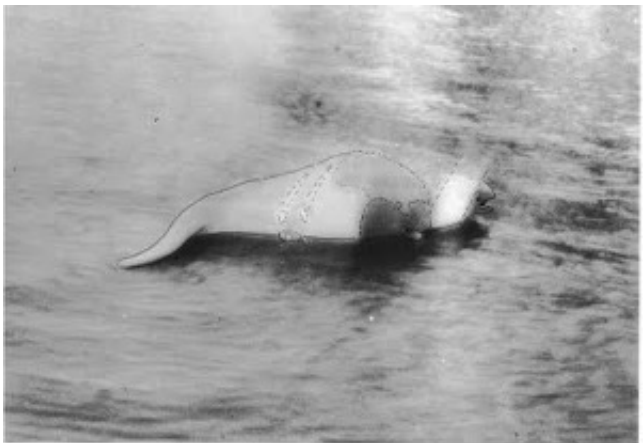


Figure 15. Speculative outline of the object in H. Gray's photograph, as based on cumulative descriptions.

The appendages are marked as per Holiday (1968), and the present author has further indicated two possible water cascades perhaps thrown over from the other side by other appendages. Several areas of shading are observed, but it is uncertain whether they are part of the creature's skin. The splash is again noted to the right which obscures the creature's form before we see the opened-mouth head. How the hump curves into the water is denoted by dotted lines, as again the water spray makes its curvature into the water unclear. Notice how the dark reflection in the water clearly denotes a raised hump structure that descends toward the spray where the reflection lightens between the hump and head. There is also a lighter area of reflection between the tail and mid hump suggesting this is also an area of spray.

The creature is unusually high above the waterline, and it is unknown how it is being propelled upward as there is little evidence of flipper commotion in the waters around it. It is exactly like Hugh Gray said, i.e., it rose out of the water and sunk back down again. In fact, this is not uncommon to Nessie sightings and has led to suggestions that the creature has some form of internal buoyancy. Of course, all aquatic creatures need some form of buoyancy else they would sink to the bottom. Some achieve it through motion of appendages and other by internal volumes of gas or liquid less dense than water. This volume is regulated to cause them to rise or sink. Whether this is being achieved by flippers or other means cannot be ascertained from the photograph.

Some Objections Answered

It could be objected that if Mr. Gray was where he indicated at the stated date and time, then the reflection is in the wrong place. The sun would be roughly to his left and hence the reflection should be more to the right on the image. It should be pointed out that the question does concern a reflection rather than a shadow. The peaty waters of Loch Ness ensure a reflective surface for objects on it. The shadow would be behind the creature and out of view. Naturally this raises the question of not so much Hugh Gray's position on the shore but rather his orientation with respect to the sun and the creature. We can be confident where he stood and where the sun was, but the location of the creature is less certain.

The use of the NOAA Solar Calculator (https://gml.noaa.gov/grad/solcalc) reveals more of the position of the sun on that bright November day (see Figure 16). There are three lines: (a) the direction of sunrise (right), (b) the direction at the time of the sighting (center), and finally (c) at sunset (left).

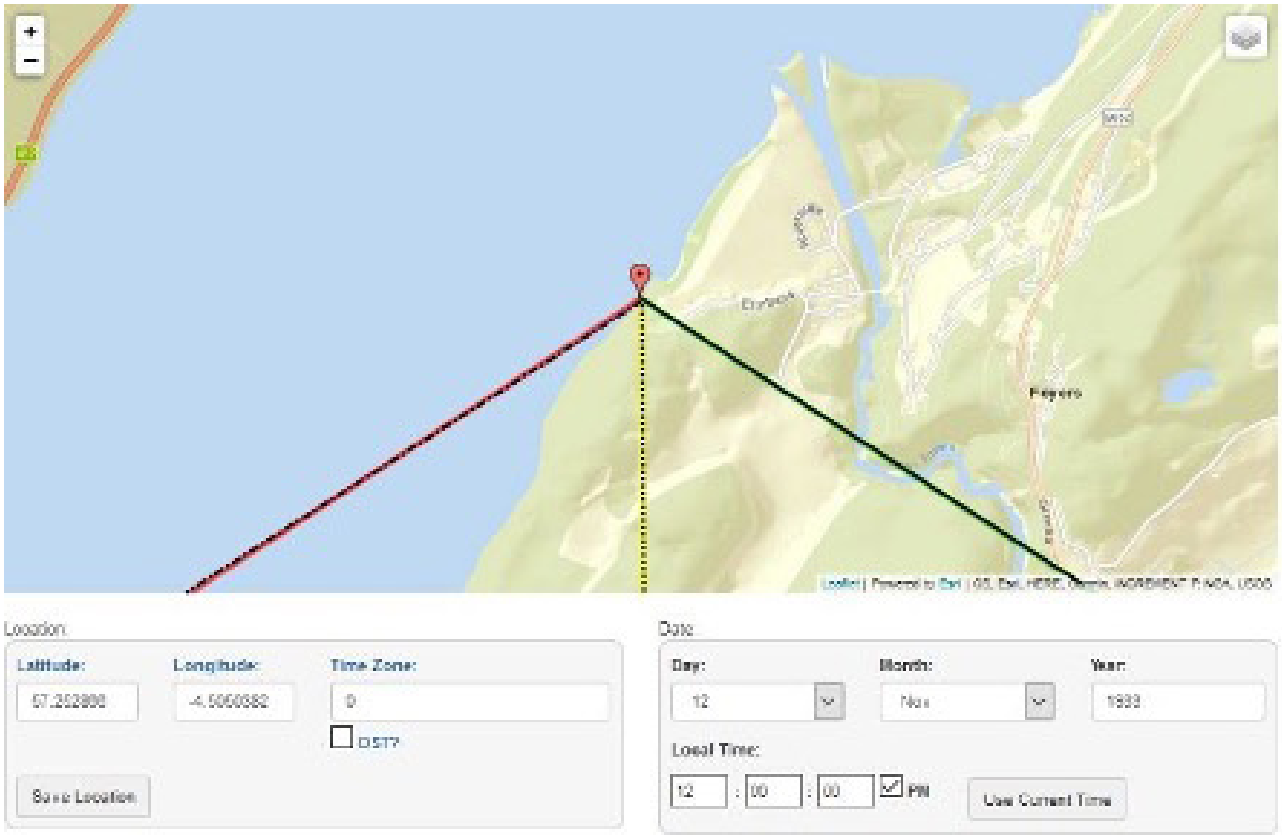


Figure 16. Position of the sun at the time and place of H. Gray's sighting at Loch Ness.

The geographical position is well-known given the accounts in Whyte (1957, p. 2) and Holiday (1968, p. 26). The azimuth of the sun is given as 179° and the elevation as 15°. Based on the shape of the reflection, it can be inferred that Hugh Gray photographed the creature with the sun behind him, so he and the creature were positioned somewhere along the direction of the dotted line in Figure 16. However, one should not assume that the reflection is a perfect representation of the creature's dimensions, but it can help to make some deductions. Firstly, reflections lengthen and shorten according to the sun's position. At a solar elevation of 15° this gives us the rough diagram in Figure 17.

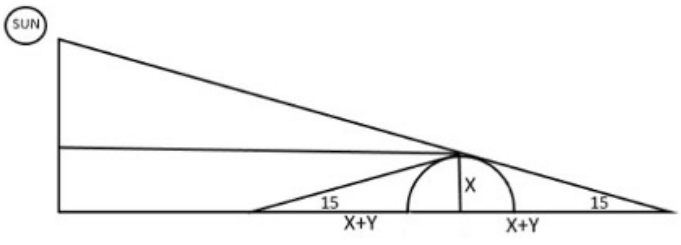


Figure 17. Calculating the position of the object in H. Gray's photograph relative to the sun.

Here, ***x*** is the height of the creature above the water and ***y*** is the length of the reflection. The angle at the apex is our 15°. The one assumption made is that the creature formed a roughly semicircular shape out of the water when viewed laterally. The analysis can be done on this and the ratio ***y:x*** is 2.7:1. That is, the reflection **y** is 2.7 times longer than the height of the animal out of the water. But you then look at the photograph and it is evident that the reflection is not 2.7 times longer than the apparent shape of the creature. This is due to the angle at which the observer viewed the object. Imagine the observer was directly over the creature. In this case, the witness would see the entire reflection length at 2.7 times the height of the creature. At the opposite extreme, if the witness was at the same eye level as the animal, no reflection would be seen. So, at this range from 90° to 0° was an angle at which the observer viewed the creature, and which would proportionately present a foreshortened reflection.

Now from what the author can ascertain from Holiday (1968) and Whyte's (1957) information, Hugh Gray estimated that he saw the creature from about 100 yards and was about 50 feet above it. If this was accurate, Figure 18 yields the resulting approximate diagram (in meters).

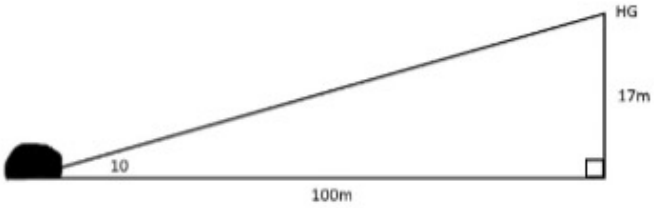


Figure 18. Hugh Gray's approximated viewing position based on published information.

This gives us an angle of incidence of about 10° as a first estimate. We then divide this by 90° and then multiply it by the ratio of 2.7:1, and the apparent reflection ratio is now only about 0.3:1 of the height of the creature. Looking at the photograph of the suggested height of the creature and the extent of the reflection, we can see that this estimate is not quite there. This implies that Hugh Gray must have mis-estimated something. However, another calculation of the observer angle can be deduced from the photograph itself. In the original Heron-Allen image there is a circular ripple (Figure 19) that appears in the bottom left of the photograph and is shown here along with a superimposed ellipse and axes.

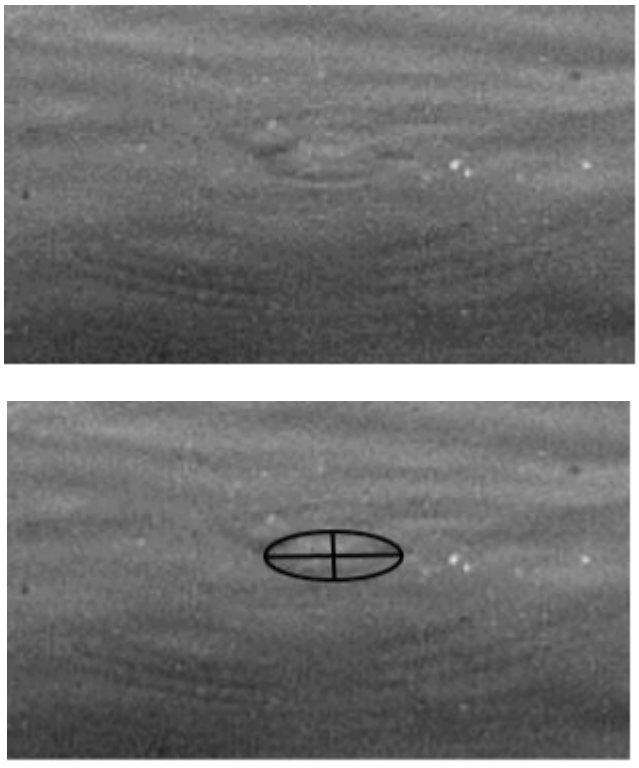


Figure 19. Circular ripple shown in H. Gray photograph (top) with ellipse and axes superimposed (bottom).

Applying basic mathematics to the axes suggests that Hugh Gray's observation angle was up to 28° and not the author's previous estimate of 10°, meaning that the creature was no less than 32 meters from the observer. The adjusted apparent reflection size becomes 0.84:1 instead of 0.30:1, which is more in keeping with the photograph. Since the creature is farther away in the picture than the elliptical ripple, however, 50 meters looks to be a closer estimate of the actual distance.

Another objection is that Gray's picture would have included some far shoreline. The problem with this argument is two-fold. Firstly, we do not have the complete negative and what has passed down to us is likely an enlargement of a cropped area of the negative. One prime example from that time is the famous 'Surgeon's Photograph.' which was cropped and enlarged to show only the object of interest. Once the original uncropped print was found decades later, the remote shoreline became visible. Even to this day, media outlets crop pictures to zoom in on the 'juicy bits.' Thus, talk of shoreline on the original is open to debate. Secondly, when the present author visited the site of the Gray photograph (Figure 20), it was simple to photograph a spot 200-meters away looking in that general direction and which did not include any shoreline. To be fair, however, digital cameras have different parameters from Gray's box camera.

Figure 20. Modern-day site of the H. Gray photograph Courtesy of the author.

Finally, the cropped version of the photograph picture has led some to claim that it was not even taken at Loch Ness, which obviously plants the seed of doubt (Binns, 1983, p. 99). This is a common reply when there is lack of known foreground and background objects in a picture. This is more psychological than forensic, as it places an unwarranted burden of proof on those who accept the tes-

timony of Hugh Gray. Rather, it should equally be the case of asking for a reason why it should not have been taken at Loch Ness. After all, cannot pictures be equally faked at Loch Ness than anywhere else? Perhaps the uncropped picture will turn up one day, until then there is no reason to doubt it was taken at the loch.

The 'Swan' Interpretation

A newer speculation asserts that Gray's photograph shows a swan (Naish, 2016). The proposer was somewhat contradictory about the image in that at one point he spoke of the "sheer ambiguity" of the picture yet later stated that it is "almost certainly" a swan. However, this latest hypothesis has several problems not unlike the 'swimming dog' interpretation. Observe the two images in Figure 21 that were published in defense of the swan re-interpretation. The top photograph is the inferior version of the Gray photograph, whereas the bottom picture is an idealized drawing of a swan in the 'Hugh Gray' position. We would be invited to recognize the apparently common features, i.e., the long neck, the body, the white-feathered tip at the posterior, and the partially submerged rear leg coinciding with one of the bulbous objects on the waterline.

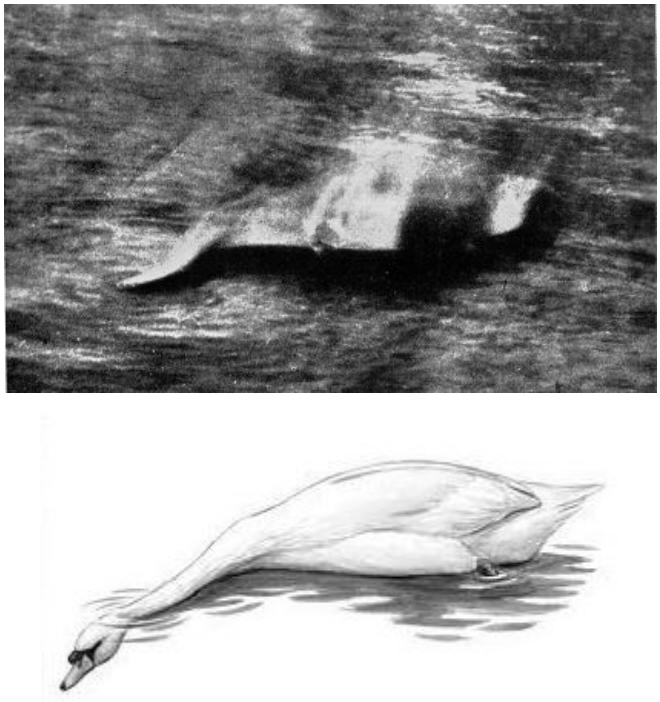


Figure 21. The Hugh Gray photograph (top) artistically re-imagined as depicting a diving swan (bottom).

When one studies this pair of images, several critical questions came to mind. The first was, "*Why was this version of the Gray photograph used?*" It is poor quality, whereas the Heron-Allen image is far better. Using this superior version of the picture, we can discern some features that undermine Naish's (2016) swan interpretation. Zooming to the far right-side, features become decidedly un-swan like. In particular, the observer is immediately confronted with the fish-like head that was previously discussed. Also, there is the area resolved as a spray of water heading up vertically which is difficult to reconcile with a swan in this proposed posture. Likewise, the partially submerged rear leg coinciding with one of the bulbous objects on the waterline fails to account for the second bulbous object farther to the left. Nevertheless, could the pointed tip of a swan produce a pareidolia effect, looking like a fish head? The photograph below of a real swan dipping into the water (Figure 22) suggests the tail tip bears no resemblance to an illusory fish-like head or for that matter the Hugh Gray photo as a whole.

Figure 22. Photograph of a real diving swan (Unknown, 2022).

A further examination of the superior Heron-Allen image reveals another problem, i.e., "*Where are the feathers?*" There is nothing visible that suggests feathers or anything avian. On the contrary, the image suggests a surface that is smoother in appearance and contour with no indication of differentiation of feathered regions. My own opinion on this is that if the superior image had been used, it would have rendered the swan theory inadequate. However, the inferior—and more ambiguous—image that was used better suited their case. Darren Naish and his advisers on this matter know that this superior image exists, so its exclusion is puzzling. After all, Steuart Campbell (1996) featured it decades ago in his popular book.

A third question was, "*Why did they use a drawing of a swan instead of a real one?*" Sketches are problematic because they can be drawn to fit any pre-conceived concept. Thus, it is no wonder that the 'proposed swan' resembles

the Hugh Gray object. But software can likewise overlay the swan drawing over photographic subject as detailed earlier. The result shows that the two images do not fit (Figure 23). The problem is apparent when the neck and shoulder are aligned to fit in proportion. Hugh Gray's creature is far more extended that the hypothetical swan and no amount of resizing could get the two images to line up adequately. At least when a sketch was used earlier for the proposed fish-like head, it bore a good resemblance to the original. As it turns out, another layer of complexity is now required—it was a deliberate double exposure by Hugh Gray to produce this strange image. This argument was literally on shaky ground; however, when one Loch Ness researcher (Harmsworth, 2010, p. 83) said that the image suggested evidence of camera shake, making a perfect alignment of the two swan exposures highly unlikely and indicating that no mandatory tripod was involved in this alleged deception. However, it is unclear whether the box camera that Gray used could have accommodated a tripod. It must also be remembered that, as with the claim that the alleged 'dog' was a result of a double exposure, the same applies here as the photographic experts who examined the camera and negatives at the time saw no evidence of tampering. Obviously, the whole matter could be brought to a head if the proponents of the swan theory created a test photograph using the techniques that they accuse Hugh Gray of employing. Naturally, only resources available in 1933 could be used.

DISCUSSION

The Loch Ness Monster ostensibly posed for its first photograph in November 1933. Yet despite 88 years of scrutiny, it seems that the original 'head' interpretation has gone unnoticed virtually all that time. This is apparently due to the combination of (a) the press of that era touching-up the image, (b) the uncritical 'dog' theory holding sway for at least 20 years, and (c) various Loch Ness researchers of note leading other paths of interpretation or just ignoring the picture. Indeed, empirical analysis does not support the frequently touted conventional explanations for the Hugh Gray photograph, namely, that it shows a swimming dog or a diving swan. In fact, detailed and comprehensive study instead reveals that the documented object more likely has a conical shape and associated features that suggest a head of a living animal. The appearance is arguably consistent with an eel or perhaps even a turtle, which have been both discussed as potential candidates for the Loch Ness Monster (see, e.g., Bauer, 2020; University of Otago, 2019). Of course, the photograph might also depict exactly what the photographer claimed to have seen, i.e., an anomalous creature of considerable size.

Figure 23. The diving swan drawing layered on the H. Gray photograph for feature comparison.

The controversy over hugh Gray's photograph illustrates both the *substantive* and *sociopolitical* difficulties of investigating topics on the margins of science (see Bauer, 2001, pp. 77–79). Substantive difficulties include the evanescent nature of important material, e.g., original negatives or prints of photographs are lost and important actors and witnesses may no longer be available. The sociopolitical difficulties include typically a highly polarized audience, i.e., confirmed believers versus insistent disbelievers who improperly call themselves 'skeptics.' There is a notable lack of engagement between the two groups, let alone any constructive adversarial collaboration as seen in other areas of mainstream academia and edge science (e.g., Bateman et al., 2005; Cowan et al., 2020; Honorton & Hyman, 1986; Laythe & Houran, 2022). A form of skepticism often confidently presents untested speculations and sometimes even levies ad hominem attacks. Rather than make a sustainable case that the evidence presented by proponents is inconclusive or incorrect, such critics insist that believers are simply wrong and thereby assert certainty where there is none (Bauer, 2014).

Quite often photographs are presented as objective evidence, as opposed to the subjective accounts of percipients. However, critics are just as likely as believers to interpret things in photographs that are simply not there and merely feed confirmation bias. Attempts to reproduce classic photographs to validate theories can also be a slave of bias when resources above and beyond what would be reasonably expected of the original witnesses are employed in the search of an impressive twin image. In reality, almost any image from the past can be reproduced, rendering their use a moot point. The nature and quality of skepticism have accordingly diminished when explanations are presented as probabilities or certainties without proper due diligence on their viability or validity. Indeed, any claim should require evidence before its acceptance. The same applies to the testing of competing theories for a given anomaly, irrespective of whether those propositions are grounded in conventional thinking or tethered to edge science. This is particularly crucial when dealing with information or evidence that carries a high risk of error in reasoning. Accordingly, it is strongly recommended that skeptical commentators refrain from publishing or otherwise disseminating dogmatic explanatory statements about anomalies without (a) offering direct evidence in support, or (b) emphasizing caveats about their untested speculations. Anything less could well undermine public education in science. It also causes one to wonder if pseudo-skeptics truly believe their own rhetoric or whether it is all a matter of getting rid of troublesome photographs and therefore that troublesome creature in a distant loch.

IMPLICATIONS AND APPLICATIONS

The controversy around interpretations of the Hugh Gray photograph could be constructively leveraged to advance studies of individual differences in pareidolia, and especially how they influence bottom–up and top–down factors in perceptual processing (see e.g., Caruana & Seymour, 2021; Salge et al., 2021; Zhou & Meng, 2020). As noted earlier, confirmation biases and other confounds that promote pareidolia have been identified in believers of esoteric ideas or phenomena (Brugger, 2001; Drinkwater et al., 2020; Houran & Williams, 1998; Williams et al., 2021). However, as acolytes of 'scientism' (Truzzi, 1987), pseudo-skeptics are likewise expected to be susceptible to these same effects and pressures as demonstrated by the present research (for a discussion, see Drinkwater et al., 2019). Thus, future research using the Gray picture (or related photographs) in stimulus–response exercises might affirm that *uncritical disbelievers* have attitudes and behaviors similar to *uncritical believers*. Assessing an image such as this from 88 years ago will be different from one taken in the present time. The tools are different, and the main actors are long dead. Stated facts are harder to verify and the forensic trail has gone very cold. It is perhaps their iconic status that motivates researchers to either pull them down or keep them aloft. In that sense, the driving factors can vary from the present-day evidence and the need to avoid bias and prejudice is even greater.

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RESEARCH ARTICLE

Don't Judge a Book by Its Cover: A Case Study and Comparative Analysis of Popular vs. Academy Psychology Books

Debra Lynne Katz

debrakatz@yahoo.com

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HIGHLIGHTS

A review of selected 'pop psychology' books shows that they do not inevitably contain misreported facts or findings from the original research studies they reference. This result suggests that negative sterotypes of such lay books should be reconsidered.

ABSTRACT

Many academic psychologists hold negative and stereotypical views about popular psychology books, even though there have been few formal investigations into these materials to understand their content, construction, purposes, or orientations, or their authors' credentials. This paper explores the origins of these views within the Sociology of Scientific knowledge literature and psychological literature. Through formal case study methodology, an extensive review and comparative analysis of books with the psychological construct of "*Attention*" in their title was undertaken to determine whether the current delineations between scholarly and popular materials and those who write them are as clear-cut as they seem, or if an alternative model of the relationship between academic and popular psychology literature could be offered. A bibliography of 145 books was compiled, followed by what turned out to be an arduous and at times impossible task of sorting these books into either popular or scholarly categories. This revealed flaws in the dualistic nature of this activity that is often required of university students, instructors, and scholars alike. Six popular and six scholarly books (Table 3) revealed that while some of the popular books were less rigorous in referencing and representing experimental or original findings, they offered bibliotherapeutic benefits and were cited by others within journal articles, books, and dissertations across multiple disciplines, thus suggesting that popularization is not simply a trickling down of knowledge from the scientific arenas to the public, but that science can be informed by professionals with expertise in applied areas. Meanwhile, the six books designated as scholarly only had a collective of 14 Amazon reader reviews. This project's findings have implications for educators, researchers, librarians, and journal editors who may presently disqualify useful materials without fully understanding them, and for writers seeking to improve in their research and writing skills.

KEYWORDS

Popularization of science, popular science, attention, scholarly books, pop psychology, popular psychology, bibliotherapy, self-help books

BACKGROUND

Popular psychology books comprise a billion-dollar and ever-expanding industry. However, there exists a gulf between many academics and the rest of society in relation to the materials they read and write. Many academic psychologists view "popular" books, along with those who write them, with outright disdain. Often, they are not at all familiar with the books they are disqualifying (Campbell, 2017).

Academic psychologists make generalized statements that "how to," self-help, and self-improvement books misrepresent good science through distortion of evidence and unsubstantiated or exaggerated claims. They accuse those who write such books of duping the public for their own financial gain. Reviewers for journals may reject papers or recommend that citations referencing such sources be eliminated prior to publication, even when they are unfamiliar with the sources. Similarly, instructors may forbid students from citing books perceived to fall under these headings in class papers. One senior librarian who has worked at the University of Illinois Library of Social Sciences for close to two decades stated in an interview with the present researcher that some psychology professors discourage students from citing books at all, including those she carefully selected for them.

Morris (1977) referred to books written by nonscientist experts or experiencers as "airport" books, occasionally referencing them while minimizing their importance through the suggestion that they are like those found in airport gift shops. Rosen (1975, 1981) referred to popular psychology books as "psychobabble," and Lilienfeld et al. (2017) calls them "psycho-mythologies," blaming a naïve public for trusting "folk mythologies," "intuition," and "common sense" instead of utilizing "critical thinking" and "the scientific method." While these authors accuse popular psychology books of exemplifying "bad science," a review by the present researcher of some of these materials found such criticisms to be more related to a clash of philosophical frameworks that largely go unrecognized as such.

Such criticisms were found by the present author not to be based on a careful review of popular psychology materials themselves (Lack & Rousseau, 2020) but rather on skeptical and negative attitudes toward entire subdisciplines of psychology, including parapsychology (Gale & Null, 2019; Carol et al., 2018; Buyniski, 2018; Pinsker, 2015; Weiler, 2013), psychoanalysis, humanistic, transpersonal, and positive psychology (Lack & Rousseau, 2016; Justman, 2005). An extensive review of sources containing the keywords "popular" or "pop" psychology revealed that some of the most prolific opponents of popular psychology books have come from leaders of activist skeptical organizations (Lack, 2012), some who are influential within the American Psychological Association (APA). Some are also authors of top-selling introductory textbooks (Lilienfeld et al., 2017). This means they have the potential to wield influence among their own academic peers and the 1.6 million students who enroll in introductory courses each year (Gurung et al., 2016), who may be unaware of the author's skeptical orientation or that some of their writings are part of a larger activism campaign.

Another group of critics, such as Ganz (1993) and Justman (2005), are those with strong religious views who advocate for biblical teachings over psychotherapy. These authors are highly critical of the entire field of psychology.

Sociological Literature on Popularization of Science

A dominant theme within the *sociology of scientific knowledge* (SKS) literature is that definitions of popularization are always situated within a particular viewpoint, orientation, or philosophical framework, although they often go unacknowledged (Mannheim, 1936; Kuhn, 1962; Parsons, 1967; Merto, 1973; Latour, 1979; Gieryn, 1983, 1995; Hilgartner, 1990; Miller, 2000; Fleck et al., 2009).

Gieryn (1983) has written extensively about positivist science origins, which had as its central task "boundary work," defined as the need to demarcate itself from all other pursuits. According to Vuolanto (2015), scientists take the position of superiority within their own discipline (intradisciplinary), between different scientific disciplines (interdisciplinary), between themselves and professionals outside the fields of science, and between themselves and the public. Mellor (2003) noted that boundary work can involve disparaging another group while also benefiting from their positive traits.

O'Connor (2009) defined the "popularization of science," or "popularized science," as the discussion and conversion of the work of "elite" scientists into a simpler form. "Popular science" is a broader term referring to the discussion of a topic that would fall within the subject areas of science and technology, not necessarily referencing past works.

Whitley and Shin (1985) define the "conventional view" or "simpler view" of popularization as "the transmission of scientific knowledge from scientists to the lay public for purposes of edification, legitimation, and training" (p. 3). It is seen as a "low status activity, unrelated to research work, which scientists are often unwilling to do and for which they are ill-equipped." They write that popularization is "not viewed as part of the knowledge production and validation process but as something external to research which can be left to nonscientists, failed scientists, or ex-scientists as part of the general public relations effort of the research enterprise" (p. 3).

Hilgartner (1990) refers to this older, outdated framework as "the traditional view" or "old view" or "dominant view of popularization." He juxtaposes this with "the new theoretical framework," which recognizes that the sharing of knowledge is bi-directional, meaning information flows both from the experimental domains to the public and vice versa, with the general population being made up of scientists and critical thinkers from many disciplines, who through various materials and venues help inform science. He also argues that all knowledge is transformed as it moves from one source to another, and that the old model is unrealistic as there are far too many groups of "experts" and venues in which they operate to pre-determine who has the true right to share knowledge.

This viewpoint is supported by Bowler (2009), who declared,

> Historians and sociologists of science now recognize that popularization is not just a top-down dissemination of knowledge from the scientific elite. This means they understand that it is necessary to adjust delivery and dissemination of information to the particular audience. (p. 3)

Historical & Feminist Perspectives on Popularization in Psychological Science

The field of Sociology, and particularly the SKS literature, addresses the topic of popularization much more extensively than does the psychological literature. Danziger (1990) points out that within the first psychological laboratory run by Wundt in 1879, initially researchers and subjects were one and the same. When this was found to be impractical, they took turns serving in these roles. It was only later that the separation of roles occurred, as psychological science attempted to emulate the medical model with a strong divide between expert and patient—this was the model of the French hypnotists, who were highly educated males studying mostly women believed to be suffering from various deficiencies.

Schmidt (2018) conducted a case study of the highly controversial publication of a female journalist's book *Passages* (Sheehy, 1974). This best-selling book was the first to suggest that women's psychological development might look different from that of men, particularly at the mid-life point. The author was sued by a prominent psychologist who had been interviewed for the book and claimed he therefore deserved to receive royalties from it. At the same time, he and his colleagues openly disparaged the book, using terms such as "psychobabble" and women's "folklore," but would go on to publish their own less successful versions later on.

Schmidt suggests the male psychologists' derogatory attitudes toward popular psychology materials were a direct assault against female writers, tracing these to financially motivated interests, to beliefs that only male scientists can generate useful knowledge within a top-down, trickle-down model, and to biases against the feminist and "human potential movement," which includes concepts such as "self," "development," and "liberation" (p. 160). She makes the case that it was the subjectification of women by established male scientists that led to continued present-day misconceptions of women writers, including those who publish popular psychology materials.

Adams (2006) has found coverage of the popularization of psychology to be minimal and lacking coherent definitions of the term "popular psychology." He suggests that some of the difficulties with conceptualizing the popularization of psychology are reflective of the same difficulties with conceptualizing the field as a whole. Psychology is a newer field that developed from and continues to interface with other disciplines such as philosophy, physiology, and psychiatry. The field is divided into two disparate realms that are often at odds with each other: experimental/academic psychology and applied psychology, or "being a science and being a practice" (Woodward, 1982). This confusion is represented in much of the literature. Adams attempts to sort out these differences through offering a typography of popular psychologies. These include naïve/homespun/folk psychology, therapeutic "pop" psychology (self-help), and the popularization of (scientific) psychology. He notes that readers of popular psychology are constantly checking the material against their own personal experience. This means for the writing to be accepted, it cannot contradict what the reader already knows from their own experience, but it still needs to be shown with "sufficient novelty that the material presented cannot be taken for simple commonsense."

Bibliotherapy and "Giving Psychology Away"

In his 1969 presidential address to the American Psychological Association (APA), George Miller stated there were two directions the field could take: It could develop as a professional elite, with specialized knowledge that only the experts would have access to, or that they could "give psychology away" (p. 1066). He emphasized that it should be the aim of psychologists to follow through on their social responsibility to help people learn how to help themselves. He expressed that there would not be enough psychologists to meet the psychological needs of the people, and that therefore it was up to the psychologists to establish applications and theories and carve the way for people to serve as their own psychologists.

Since then, several methodological studies have focused on the topic of bibliotherapy, which can be defined as the assigning of self-help materials by professionals to clients who may or may not be closely supervised (Dufour, 2014). Scogin et al. (1990) found that self-help programs yielded significantly better results in comparison with no treatment. Gould and Clum (1993) examined the effectiveness of 40 self-help studies that used no-treatment, waiting-list, or placebo comparisons as control groups. The effect sizes for interventions involving self-help were almost as large as those involving therapist-assisted interventions. Kurtzweil et al. (1996) analyzed 53 published studies on the clinical efficacy of selected self-help programs; the findings indicated that these programs were more effective than no-treatment controls.

Norcross et al. (2000) conducted a review of several studies on bibliotherapy, including a survey of 2500 mental health professionals, declaring that a "massive, systemic, and yet largely silent revolution is occurring in mental health today and is gathering steam for tomorrow." He referred to this movement as "self-help efforts without professional intervention," declaring the participation of organized psychology's participation as "vital" (p. 370). He asserted, "Despite the professional proclivity to devalue self-help resources, their success is reasonably well established" (p. 371). Still, he noted that of the 2000 self-help books published each year, less than 95 percent of them undergo outcome evaluations, concluding "Popular science is not our enemy but rushing into print before or without supporting evidence is" (p. 375).

Williams (1995) asserted that culturally relevant bibliotherapy is needed for those who cannot afford other forms of therapeutic help. Schliebner (1992) noted many cultures shun the practice of sharing one's problems and seeking help from outside the family system, and therefore receiving guidance in written form may be their best source of professional help.

METHODOLOGY

Research Questions

This project focused on three main questions: 1) How should popular psychology be understood? 2) How is popular psychology characterized by academic psychological science and to what extent do these characterizations accurately and appropriately represent popular materials? 3) How might a more fair and fruitful relationship between academic and popular psychology be conceived?

These questions were addressed by means of analyzing a set of popular psychology books through adaptation of *formal case study methodology* developed by Yin (2017). Unlike many qualitative methods that don't require identification or testing of hypotheses, Yin's method calls for a more systematic approach to a case study, in which the researcher identifies their own hypotheses as well as rival ones, and then sets about testing these through a comprehensive comparative analysis. Overall, *the new view of popularization of science as defined* by Gieryn (1983, 1995), Shinn and Whitley (1985), and Hilgartner (1990) informed my own hypothesis construction, while the *dominant/old/view of popularization of science* personified by myth-busters such as Lilienfeld (Ausch, 2016; Ganz, 1993; Justman, 2005; Lilienfeld et al., 2017; Rosen, 1987, 1993) formed my rival hypotheses. For brevity's sake, all 4 hypotheses and findings will be presented in the "Results" section below.

Books on Attention

The choice was made to narrow the subject matter of these books to the psychological construct of Attention, which is a historically enduring category that has been handled in different ways over time (Hatfield, 1995; Neumann, 1971; Burnett, n.d.). It initially was addressed mostly in academic texts, but in recent years has become the central topic in a growing number of popular self-help and how-to books that seem to be enjoying increasing sales (Konnikova, 2020; Van der Stigchel, 2019), with little to explain its movement into the popular arena. Therefore, it was theorized that the way in which Attention is currently addressed in popular and in academic-oriented literature may not only help to demonstrate differences between these types of books, but might offer insights into the field of psychology's current state and what topics and methods it considers to be more legitimate than others. This theory was informed by a social constructionist approach, as described by Danziger, who examined the history of other psychological constructs such as Memory (2008) and Intelligence (1990).

The first phase of this study involved surveying the entire collection of Attention-themed books from their inception to the present, which ranged from the year 1880 to 2020 and covered multiple subtopics and interdisciplinary fields. The books had to specifically be about the construct of Attention, rather than just have the word in their titles.

This resulted in the identification of 145 book titles, which were entered into a spreadsheet along with information about their authors and publishers. Next, a collection of library guidelines was utilized to determine whether each title was popular or academic/scholarly. From this larger set, a sampling of 12 books was selected for the purpose of performing a more careful analysis of their actual content. This sampling included the eBook versions of six scholarly and six popular books. eBooks were chosen in-

stead of print due to the ease of acquiring them and the usefulness of their notetaking features.

Criteria for selection of the 12 books included that they be published within the last 20 years (but preferably more recently), that they all be available in eBook versions, and that they serve as a representative sample of the various subfields found within the literature on Attention. Initially, the plan was to try to match the subfields between scholarly/popular categories (for example, to have an ADD/ADHD book that was scholarly and one that was popular). However, all books related to this topic seemed to fall into the popular category, while all books about cognitive science seemed to fall into the scholarly category. Therefore, efforts to match subtopics between categories were abandoned.

The covers for the 12 books are presented in Figure 1. Their full citations are included in the references section.

Per Yin's formal case study method, a thematic analysis of each of the selected books was completed first. This was followed by a comparative analysis of the books for the purpose of testing both the predetermined hypothesis and rival hypothesis for each of the 4 hypotheses.

While several characteristics of their books and authors were compared, three foci were central to this examination. These included: 1) handling of factual statements in relation to references, 2) bibliotherapeutic value, and 3) how much potential the book had for helping its readers in the aspects for which it was intended to do so.

To support investigations into these aspects, a thematic analysis of Amazon reviews and Google Scholar citations was conducted. Other assessments included whether each book had stated its purpose clearly or seemed to achieve its purpose, whether a methodological or philosophical approach was defined, how well its table of contents indicated the topics of the chapters, how well chapter headings defined content contained therein, formality of language, number of chapters, extra sections beyond chapters, number of pages, whether the book contained a foreword (yes or no and by whom), how well an eBook's structure allowed for ease of movement between citation and references, and how many resources/referrals were provided for helping purposes.

Rating Scale. A simple 0–3-point rating scale was developed and utilized by the present author to tally scores for each measure per book, and ultimately per category, to determine whether scholarly vs. popular books had collectively received more scores. These ratings were meant to complement the qualitative findings and not to supersede them. The scale was used as a simplification device to reduce both numerical values and qualitative assessments

Figure 1. Twelve selected books for case study (those assigned as "popular" are in the top row).

down to one of three ratings for easy comparison (see Figure 2).

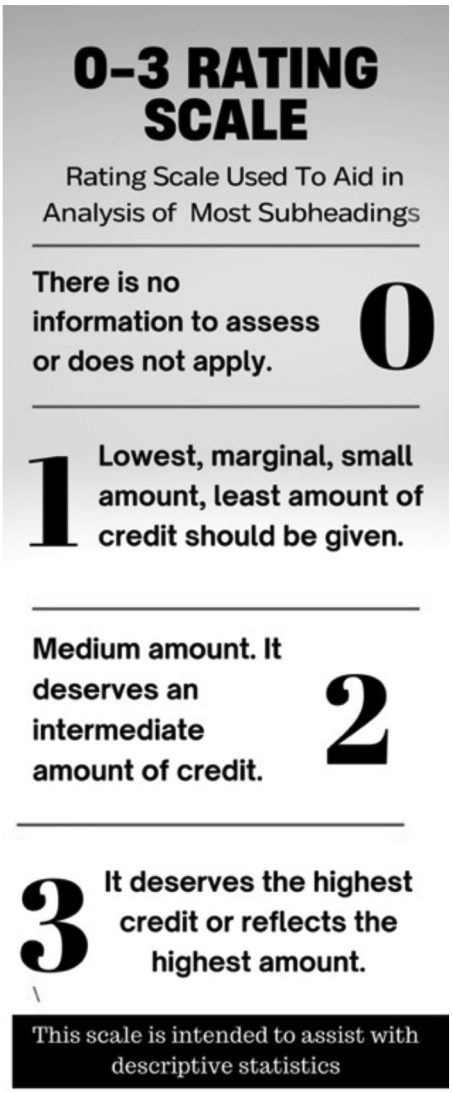

Figure 2. Rating scale used to aid in analysis of various subheadings.

Article Review

A review of factual statements that referenced other studies or experiments or historical information was vital to this project's objectives, particularly since a core criticism of popularized materials is that they do not accurately reflect the original researcher's methods or findings. This review was accomplished by selecting and evaluating a minimum of six statements or passages for each book that made an assertation of fact or truth.

Statements were selected by flipping through a book's pages until a statement asserting a fact or finding that was supported by at least one reference was found. At least six statements were selected from six different chapters within each book. While this procedure would not be considered randomized, the statements were chosen per preestablished criteria: The statement needed to be a truth or factual statement. It needed to contain a citation to at least one other written source that was not part of the book. Factual statements that contained multiple references were preferable and selected more often than those containing only a single reference. The cited sources could be anything from a website, blog post, book chapter, or journal article, provided these were obtainable. Although only three of the scholarly books solely focused on neuroscience or neurobiology, other books in both categories included a single chapter, section, or discussion addressing cognitive science. Therefore, at least one, if not more than one, reference from chapters with these words in their headings were selected for analysis in order to provide a more homogenous dataset.

For each factual statement selected, the goal was to continue to trace and study every source until the original was found. For example, if an author referenced an online blog post, this post would be located and examined. If the post then mentioned a published magazine article, this article would be examined. If the article then referenced a formal experimental write-up in a journal, the journal article would be examined. If any sources per statement in this chain were not found, the entire statement was eliminated from the overall analysis, and a new one was selected to take its place.

For every factual statement examined, factors such as number of supporting references, quality, type and strength of references, accuracy of statements, and quality of the discussion were assessed. A checklist of questions was utilized to determine the following: Were the citations properly, accurately, and fully referenced in terms of formatting considerations? How closely and accurately did the statements under evaluation portray the original material? Did the author exaggerate, dramatize, or overemphasize anything compared to the original, or state information in a neutral manner? Were the references provided within the passage to original experiments and research, or to other popularizations? If the references were to other popularizations, how well did these reflect original experiments or original research in terms of accuracy? How well did the authors integrate this material into their discussions or in presentation of their own arguments or of other viewpoints? The statements selected for analysis could stand independently on their own or within entire passages of connected statements. It was through assessment of connected statements within entire passages that it was possible to evaluate how the author presented their own positions and arguments in relation to alternative arguments.

RESULTS

Hypothesis #1

Researcher's Hypothesis	Rival Hypothesis	Findings
The division and differences between scholarly and popular books will be found to be less definitive than the rival hypothesis suggests.	Scholarly books are written by credentialed authors with advanced degrees. Popular books are written by those who are much less credentialed and are not scientists.	Researcher's hypothesis confirmed. Rival hypothesis was partially confirmed, partially rejected.

Some books were easier to distinguish and categorize than others. Categorizing 145 titles proved to be an arduous task. Most authors held at least Master's-level degrees. The most obvious indicator that a book belonged to the scholarly category was whether the publisher was an academic one affiliated with a university. Titles of books such as those that had terms associated with behavior, cognitive science, or neuroscience were the easiest to categorize, especially when paired with a university publisher. However, several publishers whose names did not include the title "university press" or "academic" were found to publish an array of both scholarly and popular materials, so even studying the publisher's website did not always prove useful with sorting efforts.

Harder to categorize were titles with the words "mindfulness" or "consciousness," or words pertaining to disorders such as "Attention deficient," "ADHD," "aphasia rehabilitation," "autism," and suffering from "life challenges" and "Attention challenges." These titles also frequently contained "your" and "how to." Easier to categorize as popular were titles that seemed to suggest how to solve a problem or get along better in life. There was a theme in some titles with the following words: overcome / accomplish / improve / capture / control / master / inspire / create / seize / build / teach / grab / manage / self-regulate.

Several of these also seemed to make reference to these actions within society, mentioning a place or a larger world, such as "in an organization," "in a noisy digital market," "in a busy world," "in an Attention economy," "in a world full of," "in a constantly connected workplace," "in a modern culture," "in the globe," "in everyday life," "in a land of." Many titles with these words did not seem to address constructs found within experimental psychology, but rather in other academic disciplines like business management, marketing, or finance. These titles included words such as business / economy / professionalism / productivity / employees / profitability / accountability / time management / visibility / marketing / persuasion. Many of these were written by people with PhDs or MBAs.

A sampling of titles that were difficult to categorize included: *The Art of Attention. A Poet's Eye* (Revell, 2007); *Rapt: Attention and the Interested Life* (Gallagher, 2010); *Now You See It: How Technology and Brain Science Will Transform Schools and Business for the 21st Century* (Davidson, 2012); *The Attention Revolution: Unlocking the Power of the Focused Mind* (Wallace, 2006); and *The Cinematic Mode of Production: Attention Economy and the Society of the Spectacle* (Beller, 2006). After several attempts at working these out over several weeks, all but 10 of the 145 titles were categorized.

Hypothesis #2

Researcher's Hypothesis	Rival Hypothesis	Findings
There will be varying levels of quality and consistency of popularized experimental findings when compared to the original experimental write-ups. This will be true in both categories.	Scholarly books will primarily be based on well-researched findings and report such findings in a methodological, organized, factual way, written in a formal manner with proper citations, while the popular books will be based on hearsay, superstition, wives' tales and will perpetuate falsehoods and myths rather than share factual information based on experimental findings.	Researcher's Hypothesis #2 was confirmed, with the first part of the Rival Hypothesis partially confirmed and the second part rejected.

How well-referenced were the sources presented in relation to a book's factual statements? Figure 3 displays the number of references provided for each book and the total for each category. The scholarly books, particularly those that focused on topics related to cognitive and neuroscience, were written for the express purpose of sharing findings about psychological science and contained the most references. One title, *The Neuropsychology of Attention* and *Orienting of Attention* (Cohen, 2013), contained more than 7900 references. The books with the next-highest numbers of references were also in the scholarly category, and were written from a philosophical perceptive (*The Attention Complex* [Rogers, 2014] and *Structuring Mind* [Watzl, 2017]). Within the popular category, books covering the history of Attention within the media or with a media focus, *The Attention Merchants* (Wu, 2017) and *Indistractable* (Nir, 2019), had substantially more references than the three books that focused on ADD/ADHD. *Driven to Distraction* (Hallowell & Ratey, 1995), written by two Harvard-trained MDs, did not even contain a reference section. The authors occasionally did mention experiments or projects to support their own statements, but the author's name might be mentioned on one page, while the partial name of the study itself was mentioned on another, and then where it was conducted was presented on yet another. They more often referred to lectures they had attended than written sources, as if they were writing from memory.

How closely did the books in each category report procedures, findings, and conclusions referenced in the original sources they cited? How well were sources integrated into discussions? (See Figures 4 and 5.) Scholarly books did appear to back up statements more frequently and consistently with references to original sources, and tended to be more reflective of the language and intentionality of the original authors. They did a better job overall of integrating the findings of references into their discussions of factually based statements. Despite the complexity of the topic of the neuroscience of attention, *Orientating of Attention* (Wright & Ward, 2018), which covers the development of experimental paradigms that study covert orienting and related theoretical issues, was found to do the most artful job of integrating earlier findings into its own discussion and presentation of the topic (see Figure 5). The book contained 850 references and introduced topics and definitions, while providing multiple examples and references, sometimes within a single paragraph, that spoke to multiple perspectives and competing theories and controversies. They were comprehensive in pointing out where prior sources complemented or contradicted each other or their own research. The authors' writing style seemed to be largely devoid of emotionality, which seemed to be accomplished through keeping the use of adjectives and adverbs to a minimum.

Conversely, popular books more frequently seemed

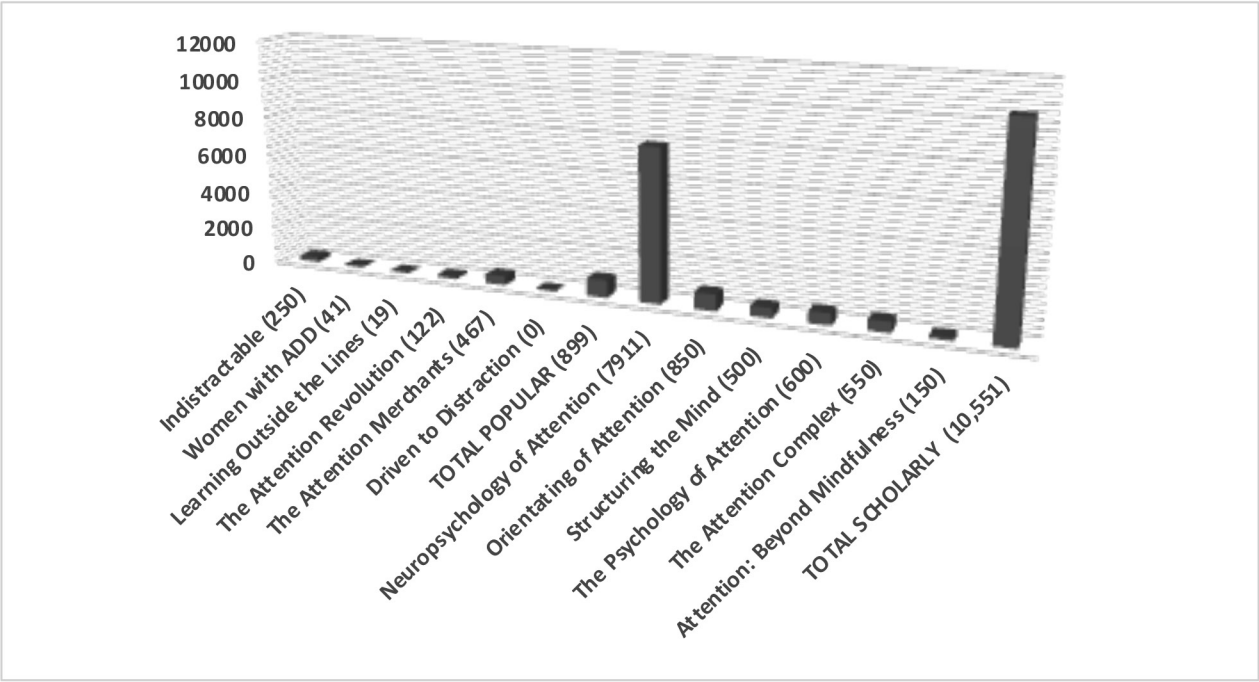

Figure 3. Number of references.

to reflect statements made by less formal, popularizing sources over the language used in the original research articles. This was particularly found to be the case in *Indistractable*, which included 250 references. The author frequently referenced both an original study and then also an online article or blog post that discussed the original research. A review of both sources found that the author had taken wording from the popularized source that was more emotional in its tone, more expressive of a strong opinion, and that used more definitive language than the original article.

It was observed that across most of the peer-reviewed journal articles examined, whether these were reporting original experiments or replications, a more conservative approach to the wording was used in discussions around their results, regardless of how successful they appeared. This was not the case with popularizing sources referenced by the six books in the popular category, or by the book authors themselves. Instead, they tended to use more emotional language and make stronger statements of veracity or truth. Still, there was no evidence to suggest that any of the 12 book authors in either category incorrectly reported results. For both categories there was a wide range of number of sources, types of sources, and in how well they were integrated into discussions. Ratings for the individual books and comparisons between the two categories are reported in Figures 4 and 5.

Scholarly books also exhibited issues with referencing. Popular books were not the only ones that exhibited issues with referencing. *The Attention Complex* contained

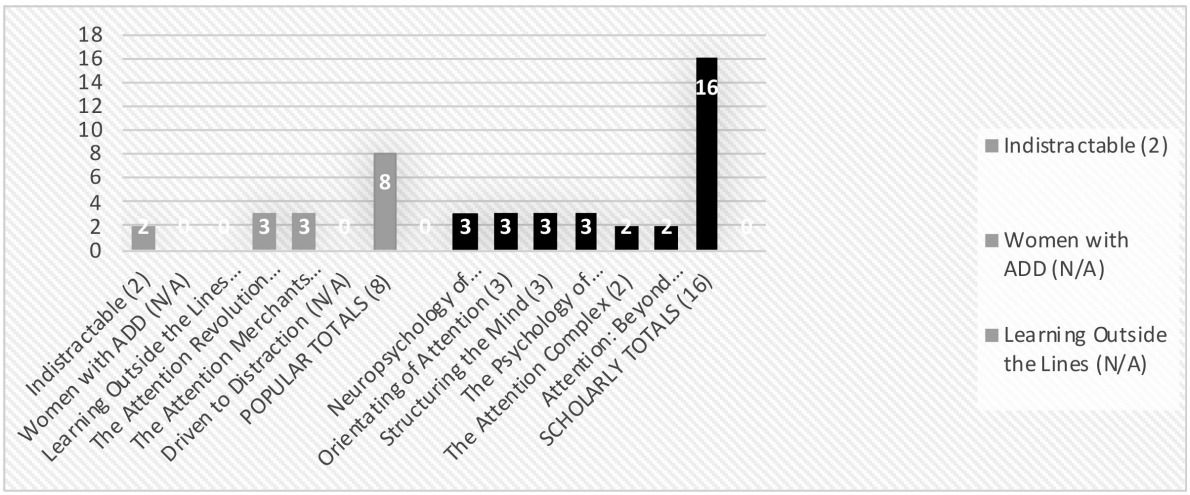

Figure 4. How accurately the book portrayed original research using a rating scale of 0–3.

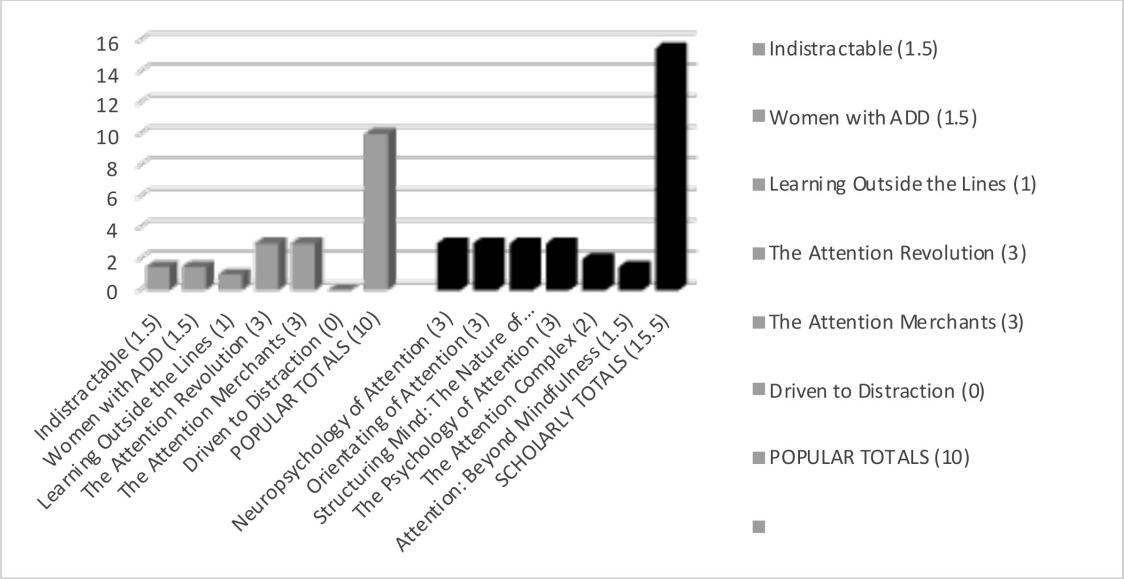

Figure 5. How well do book authors integrate references supporting factual statements into discussions?

550-plus references, but was inconsistent in its referencing. Sometimes citations were handled with finesse, in a similar manner to the authors of *Orienting to Attention*, through including multiple sources espousing different perspectives and theories within the same paragraph to support the author's own position. However, at other times Rogers stated something as factual without providing any support for it. Sometimes he included an in-text citation, but this citation was nowhere to be found within the notes or bibliography sections. There were sometimes missing page numbers. This made it difficult to examine the original sources to assess how clearly or accurately he had represented them.

Meanwhile, another book in the scholarly category, *Beyond Mindfulness* (Watson, 2017), included a section addressing neuroscience. However, rather than referencing any formal studies directly in these discussions, Watson merely interviewed a neuroscientist who seemed to give his opinion rather than referencing formal literature. She also provided several references in her bibliography that cited both popularized sources and the original source within the same entry, leading one to surmise she may have only read the popularized version without reviewing the original one, although there is no way of verifying this. An example of this is the reference: "M. Heidegger, on the way to Language (New York, 1971), p. 123, as cited in Bortoft, Appearance, p. 135." Here the full reference for Bortoft is not given, but it is mentioned in the entry above this in full form. Still, a review of her references did not reveal any discrepancies between the information she shared and the original author's statements. For this reason, I gave her a rating of 3 for how she handled her referencing of informational or factual statements.

No evidence found of popular books perpetuating "superstition," "wives' tales," "folklore," or myths. The second part of rival hypothesis 2, suggesting popular books perpetuate superstitions, wives' tales, falsehoods, or myths, was rejected. Although, as noted above, popular books did not seem to do as good a job as their scholarly counterparts in terms of handling references, most of the popular books' authors backed up many of their statements with either references to their own expertise as long-time and highly credentialed professionals in the field or to statements made by other credentialed experts and professionals. These included practitioners, researchers, authors, and instructors who shared information through formal conference talks, organizational sources like workshops, presentations, magazine and newsletter articles, blog post articles, and interviews, group interactions, or private correspondence. This was true of the three books focusing on ADD/ADHD. Little evidence was found to suggest that information being shared should be called "naïve" or homespun, or of the folk categories. The concepts of bibliotherapy were more accurate. Resources shared within these books were reflective of professional work and long-time study of the topics by the book authors—even if this study often occurred outside of a cognitive sciences laboratory or academic setting.

Driven to Distraction, written by two MDs, contained no bibliography, and the studies cited within the text appeared to be poorly handled. While one could easily question the validity or veracity of statements made considering the lack of formal referencing, especially in their discussions around medications and physiology, their discussions were clearly intended to be supported by their reputations based on professional-level experience as medical experts, as well as the research findings of their colleagues. While some might argue that professional experience is not on par with peer-reviewed articles published in formal journals (with others suggesting just the opposite), the information shared within the evaluated passages would also not be considered mere "wives' tales" or myths.

Further, some of the books that fell within the popular category, such as *The Attention Merchants*, which contained 467 notes, citations, and references, overall handled sources in a diligent manner, earning it a top rating of 3 (see Figures 4 and 5). Since this book focused on the history of Attention in the media, it utilized the most logical sources, which were not experimental science write-ups, but rather newspaper clippings, industry magazines, biographies in book and video format, and some archival materials. Sometimes the original source was used, and sometimes the only available source seemed to be used. In one instance, however, a popularized source was used instead of the original, which would have yielded a slightly more representative picture of the past than was offered.

The Attention Revolution was another book in the Popular Books category that didn't rely on sources from psychological science, yet it contained 122 references. Since the book centered on Buddhist meditation practices, most of the sources were to original Buddhist texts or translations of these texts, and to other writers, philosophers, and practitioners of Buddhist thought, tradition, and practice. There was nothing to indicate that authors relied on old wives' tales or myths or even folk psychology, even though the subject matter addressed Attention from a practice-based perspective, which involved topics related to mindfulness, meditation, and transpersonal psychology.

Authors of books in the popular category mostly had comparable qualifications to those in the scholarly category. A review of credentials of 145 authors within the master spreadsheet of Attention books, and of the 12 authors of books chosen for closer evaluation, demonstrate that a majority of all authors had at least the equivalent of

HYPOTHESIS #3

Researcher's Hypothesis	Rival Hypothesis	Findings
The distinctions between academic qualifications of researchers will be less clear than in the rival hypothesis.	Scholarly books will be written by credentialed experts and professionals, while popular books will be written by lay people without credentials and for purposes largely motivated by financial gain.	Hypothesis #3 was confirmed, while the rival hypothesis was rejected.

a master's degree, while many held a PhD or equivalent, even within the popular category. Further, those who were not academics were often long-time professionals within their domain of expertise or held dual credentials in multiple professional or academic disciplines. Within the sampling of 12 books, there was only one book, *Learning Outside the Lines* (Mooney & Cole, 2000), whose authors held only bachelor's degrees (albeit from an Ivy League school).

In terms of the rival hypothesis' assertion that authors of popular books are only writing for their own financial gain, there was only one incidence where a popular book written by a marketing professional (*Indistractable*) did seem more self-serving than the other books in either category.

Eyal Nir is a marketing professional who first wrote *Hooked: How to Build Habit Forming Products* (2013), designed to help companies get their clients addicted to their services. He then wrote *Indistractable: How to Control Your Attention and Choose Your Life* (2019) to help the public combat such influences. Nir encouraged readers visiting his website to access additional journaling and self-discovery materials. In order to do this, it was necessary to input one's email into his contact form. The materials did arrive soon after via email, but these were heavily branded with his own business logos, which ironically was quite distracting.

He devoted an entire page of the book to ask readers to do "a personal favor" for him and leave him an Amazon review. Readers responded, leaving 1407 reviews. While many reviewers seemed to simply recite his end-of-chapter "points to remember," numerous readers gave specific examples of ways they had changed their behaviors as a result of his book. One provided a diagram of the methods she had applied from his book, and their results over time. Personally, I found that applying his lessons and approaches enabled me to cut down on my own social media time by approximately 90 percent, something I've maintained even a year later.

These intrusive marketing approaches were not found in any other books. Some of the popular authors did not even mention their own websites in their books.

Differences between scholarly and popular books are found to be reflective of different epistemological and ontological approaches to knowledge. The largest differences in referencing were between books intended to present the experimental findings of attention studies as they relate to neuroscience and those expressly written for human-centered purposes, such as the three books focused on issues related to ADD/ADHD. All authors in

HYPOTHESIS #4

Researcher's Hypothesis	Rival Hypothesis	Findings
Categories of popular and academic books would not simply be related to differences in quality or presentation of information, but rather would be reflective of different epistemological and ontological approaches to knowledge, such as those reflected within the natural sciences vs. human sciences frameworks.	A top-down, uni-directional flow of information from experimental settings to the public in a watered-down fashion would be found within all books, since none of these were reporting original research findings for the first time. However, this would be much more prevalent in the popular book category.	Hypothesis #4 was confirmed. The rival hypothesis was not confirmed.

both categories stated their book's purpose in either the foreword, introductory chapter, or within multiple chapters (see Table 1). For popular books on ADD/ADHD, the authors' stated purposes indicated that they were writing the books to help readers determine whether or not they were lifelong sufferers of ADD/ADHD, and how to cope with their limitations while emphasizing the positive aspects. Included with human or person-centered books was *Indistractable*, which was designed to help readers overcome distractions posed by the media and advertising. *The Attention Revolution* was included as human-centered as it informed readers about how to deepen their personal meditation practices.

TABLE 1. Comparison of Categories on Various Measures Using a 0–3 Point Rating Scale

Measure	Popular	Scholarly
Did author discuss a methodological approach outlined?	2	11.5
Did author achieve a stated purpose?	18	18
Is the author an expert on the topic on which the author is writing?	15	12
Ease with eBooks' functionality going between text and references	12	3
Social impact and level of personal helpfulness	13	3
Formality of language	12	18

Four out of five of the popular books did include references to written sources, but they tended to cite professional sources that were also more human-focused, such as expert advice delivered face to face through conference presentations, organizational meetings, personal interviews, and personal correspondence. Additionally, even within the scholarly category, *Attention: Beyond Mindfulness* took a human-centered approach through conducting qualitative research that involved interviewing a range of experts on their phenomenological experiences of Attention in their professional and creative endeavors. Therefore, her references included citations of personal interviews and correspondences, as well as books such as biographies.

Meanwhile, the philosophers such as Rogers, who wrote *The Attention Complex* and applied Foucault's writings to address aspects of power and subjectivity as they relate to the history of Attention and development of Attention as a psychological construct, had as their focus a social sciences orientation rather than a natural sciences emphasis on experimental and laboratory science. These books did offer numerous references, but their sources were more often philosophers than experimentalists. Their tone was less formal and more emotional than the experimentalist/cognitive science writers. They also spent more time defining their approach and frameworks than any of the other types of writers.

From these three orientations—natural sciences, humanistic, and philosophical social constructionist approaches—flowed different types of content, references, discussions, terminology, and use of voice and other stylistic devices. Probably the most obvious example of this was when the authors of "outside the lines" used profanity, with the experts writing the foreword using slang words such as "yo!" While some Amazon reviewers found this to be surprising and distracting, one could not even imagine these devices being used in the books written about neuroscience.

Evidence of bidirectionality of information flow, rather than only top-down from science to the public. All titles within the popular book category were cited on Google Scholar by sources published in peer-reviewed journals. This is evidence that these books are being used to advance knowledge within scientific arenas. Per Table 2, the total number of Google citations for all popular books was 1704, while for scholarly books it was slightly higher, totaling 2054. While these numbers are already close, one should keep in mind that the popular books selected had more recent publication dates than some of the scholarly books. This means that the popular books earned more citations faster than the scholarly books, or, conversely, the scholarly books have been around longer to potentially receive more citations.

It was also clear from statements made by the authors themselves, from those who wrote the forewords to their books, and from the formal book reviewers and the numerous Amazon reviews analyzed (Table 2), that these books often were used to inform other professionals, instructors, and researchers, who utilized the information in their professional work. Sometimes these books were mentioned to professionals by their clients or students as having had a positive impact on their own behavior or on someone else close to them, and sometimes the professional recommended the book to their client and then observed a useful effect. Some of the books included in both popular and scholarly categories referred to each other's books.

For example, Drs. Holloway and Ratey, co-authors of *Driven to Distraction*, wrote the foreword for Solden's book, *Women with ADD*, asserting it had a significant impact on the field and had stimulated further research into the area

TABLE 2. Showing Number of Amazon Reviews and Google citations

Year Published, Author	Book Title	# of Amazon Reviews	# of Google Scholar Citations
2019, Nir	*Indistractable*	1407	18
2002, Solden	*Women with ADD*	406	78
2000, Mooney & Cole	*Learning Outside the Lines*	152	123
2002, Goleman & Wallace; 2006, Wallace	*The Attention Revolution*	112	459
2017, Wu	*The Attention Merchants*	263	448
1995, Hallowell & Ratey	*Driven to Distraction*	1400	578
Totals for Popular eBooks			1722
2013, Cohen	*Neuropsychology of Attention*	0	583
2018, Wright & Ward	*Orienting of Attention*	1	6
2017, Watzl	*Structuring Mind: The Nature of Attention*	1	449
2006, Styles	*The Psychology of Attention*	1	96
2014, Rogers	*The Attention Complex*	8	18
2017, Watson	*Attention: Beyond Mindfulness*	3	6
Totals for Scholarly Books		14	1522

of gender differences with this disorder. Further, it was clear from Solden's statements, along with the writers of *Learning Outside the Lines* and formal and informal Amazon reviewers, that Holloway and Ratey's original edition of *Driven to Distraction* had a similar impact on themselves as people, professionals, researchers, and the field overall.

DISCUSSION

A Third Category Exists—Replacing Either/Or with "And"

The act of designating whether a book is scholarly or popular was necessary so that a comparative analysis of such books could be carried out. This activity also enabled the researcher to experience firsthand what a student, educator, or editor might encounter when using library guides to aid in the categorizing of similar materials. Initially, it was assumed that through the use of such guides, this sorting would be an objective practice. However, it was clear that many designations were subjective, as they changed after repeated attempts and could only be carried out with the aid of further investigations into the book's publisher and the author's background.

While titles containing references to neuroscience, neurobiology, and cognitive science published by university presses were easy to categorize, the lines between popular and scholarly for all other books were blurred. At times, the sorting process proved to not only be an arduous task, but an impossible one. The library guides were only partially useful. They offered inaccurate and confusing advice, particularly in stating that authors of popular works would not have advanced degrees. Further, none mentioned how to handle a situation in which an author had an advanced degree or was actively teaching in a different discipline from the one in which they were writing.

It was found that the dichotomy between scholarly and popular books could be seen as existing on a continuum, rather than falling strictly into one category or the other. In Figure 6, I've shown where the 12 books in this study seem to fall. This graphic demonstrates that two of the scholarly books teetered on the line of what might be considered more popular, with several of the popular books falling somewhere in a middle zone.

Let Primary (Original) Source Designation Be the Guide

When looking for sources and reference materials, recommendations were to let go of the dualistic consideration of scholarly or popular and replace it with one basic question: *Is this truly the most original, reliable, and correct source to back up my statement or series of statements?* In

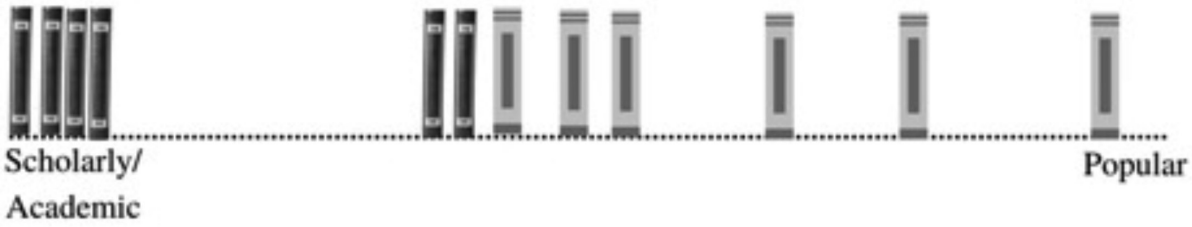

Figure 6. Distribution of 12 books on the scholarly/popular continuum.

doing this, one will sometimes find sources other than peer-reviewed journal articles, which may be frowned upon by reviewers or editors. However, as was discovered during the present study, there are a variety of situations where a writer or researcher may not be referencing an earlier study, but instead cite an original newspaper article reporting on a historical event, or credit the creator of a definition or method. One example of this came from Gay Watson's book *Attention: Beyond Mindfulness*. She shared a passage from the autobiography of Philip Glass, *Words Without Music*, where he described how he trained himself in "the habit of attention" (p. 83). This information was purely anecdotal, yet it was one of many stories that served to present a larger picture of what attention is to different people, which was part of her overall qualitative project.

Amazon reviews and Google Scholar citations suggest lack of concern for referencing. An examination of the content of book reviews from a sampling of each of the 12 books showed that reviewers often voiced appreciation for resources shared by authors, such as referrals to other practitioners or to other books, but not a single criticism related to references was found.

A tallying of Google Scholar citations revealed that lack of referencing did not stop other researchers from referencing books that were deficient in this area. As mentioned earlier, *Driven to Distraction* did not even contain a references or bibliographic section. Instead, the authors spread out mention of a single source's name, publisher, and title across multiple paragraphs, with other topics tossed in between. If this was intended as a demonstration of writers suffering from ADHD, they did an effective job, but otherwise some of their passages would likely not have been deemed acceptable within even a high school essay.

Despite this lack of referencing, according to Google Scholar, *Driven to Distraction: Recognizing and Coping with Attention Deficit Disorder* has been referenced 904 times in books, dissertations, and journals. Further investigation into the purposes and reasons for referring to this book lies beyond the scope of this project, but it would be interesting to know whether those referencing it noticed its lack of a references list. This example may illustrate the need for researchers to more closely examine the materials they cite, even when they are written by former faculty members of Harvard Psychiatric Hospital and Medical School.

Authors should not cover a topic unless they have the ability and intention to do so properly. In two of the ADD/ADHD books within the popular category, and even in one of the philosophically-oriented academic books, authors sometimes included chapters, sections, or discussions on neuroscience. However, rather than citing original studies or meta-studies, or even popularized written sources by neuroscientists, they elected to share verbal comments made by neuroscientists in private conversations or at meetings or conferences. This may have been appropriate if the scientists they were referencing had been sharing information not published elsewhere, but this did not seem to be the case. Rather, it seemed as if the authors failed to do their due diligence. In both Solden's (2002) and Hallowell and Ratey's (1995) books, it came across as if they were simply getting through an obligatory, uncomfortable discussion as quickly as possible.

An example of this comes from *Women with ADD*, under the heading "How Medications Work for ADHD." Solden wrote:

> The above complexities of attention are commonly linked to the inefficient or inconsistent transmission of information in the brain through chemical brain messengers called neurotransmitters (NTs). You can refer back to this in the pocket guide which you read earlier. But to review, neurotransmitters send information between the millions of nerve cells in the brain.

Even though she refers the reader back to "the pocket guide," which she describes in the introduction as a "section that is useful for people who are not familiar with ADHD and can also be consulted as a handy reference throughout the book" (p. 422), this section does not actually contain a single reference.

It would have been better to not even attempt inclusion of the topic of neuroscience if it was not going to be

handled diligently, as many scientists (especially neuroscientists) reading these books would just outright dismiss a book on this basis alone, when in fact the topic was not central to the book's purpose or focus.

Despite the flaws mentioned in relation to books found in both categories of books, it should be reiterated that no evidence was found suggesting they contained major inaccuracies or outright incorrect information. They all held merit in different ways, meeting their own expressed purposes.

Additional Writing Tips

It is recommended that popular writers rethink using metaphorical, flowery, or creative wording when composing their chapter headings. It was sometimes difficult to gain an understanding of the type of content contained within the chapter for the popular books.

Writers of both types of books needed to be careful about finishing one thought before starting another. Some would make a statement, introduce another thought, and then go back to the first thought, all in the same paragraph. This was confusing to a reader.

None of the six of the scholarly eBook/Kindle versions of the books were on par with the popular books in terms of technological functionality (see Table 1). The features that made for effortless movement between in-text citations, notations, and references within the popular books were largely not available for the scholarly books. While lack of modernization of functions is likely due to the age of some of the scholarly books, some of these are in their second or third editions. Despite this lack of functionality, scholarly books had much higher pricing than their popular counterparts. *Neuropsychology of Attention* cost $189.99 for the eBook and $247.08 for print (see Table 3).

TABLE 3. Pricing Differences between Popular and Scholarly Books in U.S. Dollars

Book Type	Popular	Scholarly	Difference
Kindle Mean Price	12.83	63.83	51.00
Paperback Mean Price	15.00	68.00	53.00
Hardcover Mean Price	39.65	92.80	53.15

Given that scholarly books are precisely the kind where readers would need to reference the sources, more attention needs to be paid to such functionality.

Further, scholarly books were in no way immune to errors. Even with just spot checking, Rogers's 2014 book *The Attention Complex: Media, Archeology, Method* contained several citations that could not be found in the references section. It was also missing page numbers, and some factual statements were not well-supported by references in some sections, while they were in others.

Further, some of the scholarly books even on neuroscience had occasional links to website pages (such as one that was linked to a federal government website) that were no longer working. Because of the precarious nature of webpages and links, I recommend that links to websites not be included in either print or digital versions of a book.

IMPLICATIONS AND APPLICATIONS

One of the earliest promoters of science was John Tyndall (1820–1893). Acting as superintendent at the Royal Institution in London, Tyndall was tasked with demonstrating "to lay and scientific audiences the progress of scientific knowledge" (Gieryn, 1983, p. 780). This was a daunting undertaking, given that much of Victorian English society still subscribed to the authority of the Church, believing that religion and prayer would solve their problems. Additionally, Tyndall encountered resistance from tradesmen such as mechanics, engineers, and architects, who enjoyed political power within their communities and saw themselves as having advanced their technology through hard work and life lessons. Many opposed any alignment with this new domain called "science" that sought to appropriate their achievements and assert authority or superiority over them. Likewise, as the present project suggests, many books written on Attention revealed that they, too, were written by accomplished professionals who gained knowledge through observations made while working and living within the larger laboratory called "life."

Hilgartner (1990) defines the new view of popularization as the bi-directional transfer of information from the public to the scientific domain. Support for this view was found for this in the present study by counting the number of Google Scholar citations (even by many peer-reviewed journal articles) the books in the popular category had received.

Franczak (2016) less diplomatically defines the new view of popularization as "the process whereby the dominant administrators of scientific knowledge lose their monopoly position" (p. 19). He attributes this change to technological advances, which give those who do not have "legitimate institutional or scientific authority," such as politicians, the media, business or religious leaders, new social movements, and the "determined amateur enthusiast" the power to voice alternative viewpoints. He asserts, "Many of these successfully defend themselves against labels of 'counter-knowledge' or 'pseudoscience' and seek supporters within channels not necessarily sanctioned by scientists" (p. 20). Meanwhile, he believes that many scientists seem not to have noticed that this change has occurred.

Having just survived two years of a worldwide attack by microscopic creatures invisible to all but the scientists who have the means to study and develop ways to combat them, we see that today's concept of Superman looks less like Christopher Reeves and more like Dr. Anthony Fauci, who has been dubbed "science's defender" (Ledford, 2020).

Despite our reliance on biologists, virologists, and immunologists to develop vaccines and medications and provide vital preventive information, topics such as these have become highly politicized (Bokemper, 2021), with politicians stepping in to offer their own medical advice that is not only untested but has sometimes directly opposed to the guidelines put out by their own scientific advisors and appointees. While strain between politicians and scientists has been observed during pandemics of the past (Cohn, 2003), we have never had so many individual media sources available to communicate a large barrage of inaccurate and unsubstantiated information, mixed in with some experimental findings and useful advice.

Clearly, most of the public is more inclined to wake up each morning and turn on a major or local news network on their television set—with some older folks perhaps still opening up a newspaper—rather than starting off the day with a cup of coffee and a library search to discover whether a new peer-reviewed study has been released. However, this is not to say many people in the public (outside academia) would not want to read such articles, but most would not have easy access to them.

While there has been a movement in recent years for organizations like the SSE to make their journals open access, many have yet to follow suit. For scientists who remain critical of popularizing efforts, rather than writing about errors in critical thinking (again) or protesting visiting lectures given by parapsychologists or psychics, it might be more productive to take up efforts such as helping to ensure easier access to original or quality experimental findings by the public. Meanwhile, encouraging and helping non-academics to get involved in formal scientific pursuits will ensure a more informed, enthusiastic, and participatory public overall. Engaging in projects related to citizen science that show direct practitioners of many modalities, or even educators, inventors, and business leaders, how to move to more formalized documentation and reporting of their work with clients and customers; opening up to more forms of research that include human science methods and qualitative research; joining forces with media outlets to create quality science-based programming; and even engaging with younger people where they spend much of their time these days—on social media outlets—are all manageable activities that scientists can do to promote critical thinking, the production of more quality research and writing, and science overall.

IN CONCLUSION

This study was one of the first of its kind to systematically review popular psychology books about Attention under the lens of dueling hypotheses. While Attention is a topic historically studied within the domain of psychology, it intersects with other fields such as marketing, business, advertising, art and aesthetics, philosophy, history, aviation, athletic performance, neuroscience, and Eastern religion and spirituality. While it is not possible to say whether this study's findings are transferable to books addressing topics outside the area of Attention, future projects might utilize the methodology described here to perform case studies on books from other scientific disciplines.

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RESEARCH ARTICLE

A Pre-Registered Test of a Correlational Micro-PK Effect: Efforts to Learn from a Failure to "Replicate"

Markus A. Maier*

Ludwig-Maximilians-Universität München, Germany
markus.maier@psy.lmu.de

Moritz C. Dechamps*

Ludwig-Maximilians-Universität München, Germany

*shared first authorship

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HIGHLIGHTS

We describe a failed replication of an erroneous finding in a previous psi / psychokinesis study. However, an advanced statistical analysis seemed to confirm a pattern we found in past studies. This result is consistent with the idea of unconscious experimenter psi.

ABSTRACT

Micro-psychokinesis (micro-PK) research studies the effects of observers' conscious or unconscious intentions on random outcomes derived from true random sources such as quantum random number generators (QRNGs). The micro-PK study presented here was originally planned, preregistered, and conducted to exactly replicate a correlational finding between two within-subject experimental conditions found in an original micro-PK dataset (n = 12,254) using a QRNG. However, after data collection and analyses, a data error was detected in the original to-be-replicated dataset. A reanalysis of the original correlation effect after error correction revealed strong evidence for the absence of a correlation in the original data. This study's primary goal was to test the existence of a correlational micro-PK effect in the present data as specified in the pre-registration. In addition to this replication attempt, the present study also can be considered an unsystematic case report or field study on experimenter psi (e-psi), since a strong expectation was formed initially about the occurrence of an effect that indeed was objectively absent from the original data. This study's results indicate no evidence for the existence of a correlational (and standard) micro-PK effect. In other words, the actual correlational data did not meet the experimenters' conscious expectations, and thus no consciously based effect of e-psi on micro-PK was found. However, the change in evidence for the effect across time described by the sequential Bayes factor resembled a data pattern that also was frequently reported by the experimenters in past studies. Although these data did not meet the criterion of statistical significance and a rejection of the null hypothesis failed, the marginal effects might be interpreted as weak influences based on unconscious e-psi. In addition, the trend observed matches both experimenters' general beliefs about the occurrence of e-psi in micro-PK. These findings' implications for the application of scientific methods to the study of micro-PK and psi in general are discussed.

KEYWORDS

Experimenter effects, experimenter psi (e-psi), micro-psychokinesis (micro-PK), quantum random number generator (qRNG)

INTRODUCTION

The present study was originally designed as a pre-registered experiment that aimed to replicate a statistical anomaly observed in the data of a series of three micro-psychokinesis (micro-PK) studies (Dechamps et al., 2021, Studies 1 to 3). These studies were conducted to test observer effects on quantum-based random number generators' (qRNG) outputs. Micro-PK research using qRNGs is a long-established tradition (Schmidt, 1970, 1974) and it explores human observers' abilities to mentally influence quantum-based outcomes. Numerous studies have been conducted with different variations in observers' intentions and various outcome measures, yielding an impressive amount of data. Two meta-analyses of these studies found an overall significant effect and thus evidence for observer-dependent variations in quantum randomness (Bösch et al., 2006; Radin & Nelson, 1989). Traditional micro-PK research to a greater extent applied active intention inductions by instructing participants to bias QRNG outcomes in a certain way (see, e.g., Jahn et al., 1997). A recent high-power micro-PK study using an active goal induction reported by Mossbridge and Radin (2021) found evidence for micro-PK in a first dataset that could also be replicated in a pre-registered analysis of similar data in a second dataset. These and other findings corroborate the usefulness of overt mental intentions in micro-PK.

In contrast to this approach, in the three precursor studies mentioned above, Dechamps et al. (2021) tested the impact of more implicit, unconscious processes on micro-PK outcomes using a subliminal priming technique in a within-subjects design. In the experimental condition during each trial, participants were subliminally primed with a pre-selected positive image before the qRNG chose between this same positive image or a negative counterpart. In the control condition, neutral priming was implemented. Each condition consisted of 20 trials involving the same 20 target pairs. All 40 trials were presented in randomized order. As predicted, in the first study (n = 4,092) the mean score of positive pictures based on qRNG selection exceeded chance expectation in the priming condition (M = 10.10; SD = 2.27), and Bayesian analysis revealed strong evidence for a micro-PK effect (BF_{10} = 13.35). Moreover, in accordance with their predictions, no decisive evidence for micro-PK (M = 10.03; SD = 2.26) was detected in the control condition (BF_{01} = 4.19). Later replication attempts (Studies 2 and 3) found no evidence for micro-PK in either condition (all BFs_{01} > 10; with one exception: in Study 2's control condition, the BF_{01} was > 4). The authors concluded that this data pattern obtained across the three studies could either be interpreted as a false positive in Study 1, owing to failed replications in Studies 2 and 3, or as a decline effect frequently observed in micro-PK research, corroborating the model of pragmatic information (MPI; von Lucadou, 1995, 2015, 2019; von Lucadou et al., 2007). In an additional exploratory analysis of the combined data from Studies 1 to 3 (total n = 12,254), the correlation between the mean scores of positive images obtained from the experimental and control conditions was calculated. This post hoc analysis revealed a correlation coefficient of $r(12{,}254) = .032$, $BF_{10} = 36.46$, indicating very strong evidence for a non-random correlation. Figure 1 depicts the sequential Bayes factors indicating the change of evidence of the micro-PK effects obtained in the experimental condition, in the control condition, and obtained from the correlation between both micro-PK effects.

After a visual inspection of the graphs, these authors hypothesized that the correlation effect would appear after evidence for micro-PK in the experimental condition had vanished. They concluded that as long as any standard micro-PK effects were absent, a stable correlation would be found. Thus, both authors had strong expectations that an exact replication would yield strong evidence for a correlation between micro-PK conditions. They set up a pre-registration in which they predicted the re-appearance of the above-described correlation in case a standard micro-PK effect was still absent from a newly to-be-collected dataset. Thus, the primary goal of the present study was to replicate the micro-PK findings reported by Dechamps et al. (2021, Studies 1 to 3) including a further test of the standard micro-PK effect found in the experimental condition of their original Study 1. Most central, however, the focus here was on the exact replication of the post-hoc finding described above: an a priori test of a positive correlation between the micro-PK data from both experimental conditions found across the dataset of Studies 1 to 3.

The authors conviction of the existence of such a correlational effect was based on their belief that strong Bayesian evidence has been found in the original dataset (Dechamps et al., 2021, Studies 1 to 3). This subjective expectation as documented in the pre-registration was therefore—in their view—supported by objective, although still preliminary data. The planned replication should further test and corroborate the objective nature of the correlational effect. However, the empirical background regarding the correlational effect and consequently the scientific substantiation of its appearance after closer inspection of the original data had to be revised. Before, during, and after the data collection and analyses of the study described herein, these authors did not know that the strong evidence for the correlation effect found in the data of Dechamps et al.'s (2021) Studies 1 to 3 was based on an erroneous coding of eight participants in their Study 3. The program coded the mean scores of positive images from subjects

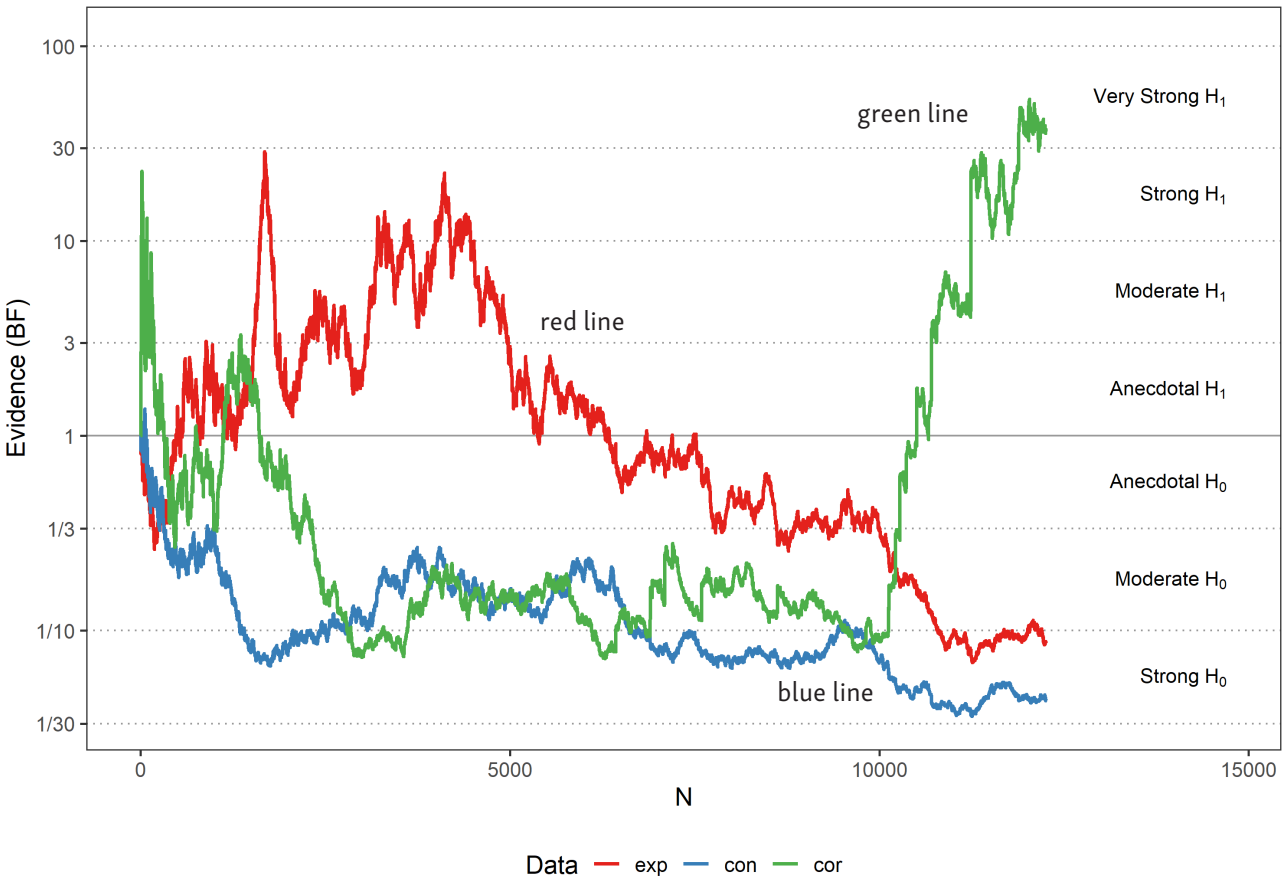

Figure 1. Erroneous sequential Bayes factors for Studies 1 to 3 from Dechamps et al. (2021) for experimental condition (red line), control condition (blue line), and correlation of both conditions' mean scores (green line).

who participated twice in a study with minus signs before the respective values. These data should have been deleted from all analyses but were mistakenly retained within the dataset. The authors detected this error after the present study's data had already been analyzed. After correcting the error and excluding the erroneous data, the original strong evidence for the correlation effect disappeared. As Figure 2 indicates, the real correlation effect in the data of Dechamps et al. (2021; Studies 1 to 3) was non-existent ($r(12,254) = -.01$), with a final $BF_{01} = 49.48$ indicating very strong evidence for a null effect. In other words, these authors' expectations and a priori hypothesis concerning the correlation effect in the present study were based on the erroneous assumption that a true effect existed. The propositions and predictions made in the pre-registration remained untouched by this new development, and the present data have been analyzed in accordance with all the specifications made in the pre-registration. However, on a theoretical level the interpretation of the present data in terms of replicability of an original effect turned into an unsystematic exploration of an experimenters' false expectations effect. Since these authors had a purely subjective expectation of the occurrence of an effect that stood in stark contrast to its true nonexistence, this new study only provided a case report or field study of potential experimenter effects in an actual micro-PK experiment. Although the unplanned nature of the investigation of the experimenter effect addressed here admittedly weakened the empirical value of this undertaking and cannot be viewed as an adequate scientific test of experimenter psi, the data can provide some preliminary and anecdotal insights into the question of which role purely subjective expectations of experimenters and data analysts might play in micro-PK (see, e.g., Rabeyron, 2020).

Experimenter effects have long been suspected of playing a significant role, particularly in micro-PK research (e.g., Kennedy & Taddonio, 1976). Varvoglis and Bancel (2015) argued that successful micro-PK investigators frequently showed outstanding micro-PK results when testing themselves. They also remarked that some experimenters' mindsets (e.g., Schmidt) exerted a biasing influence on micro-PK data obtained in their studies (see Varvoglis & Bancel, 2015, p. 278, footnote 8). Since micro-PK research using qRNGs explores observer effects on

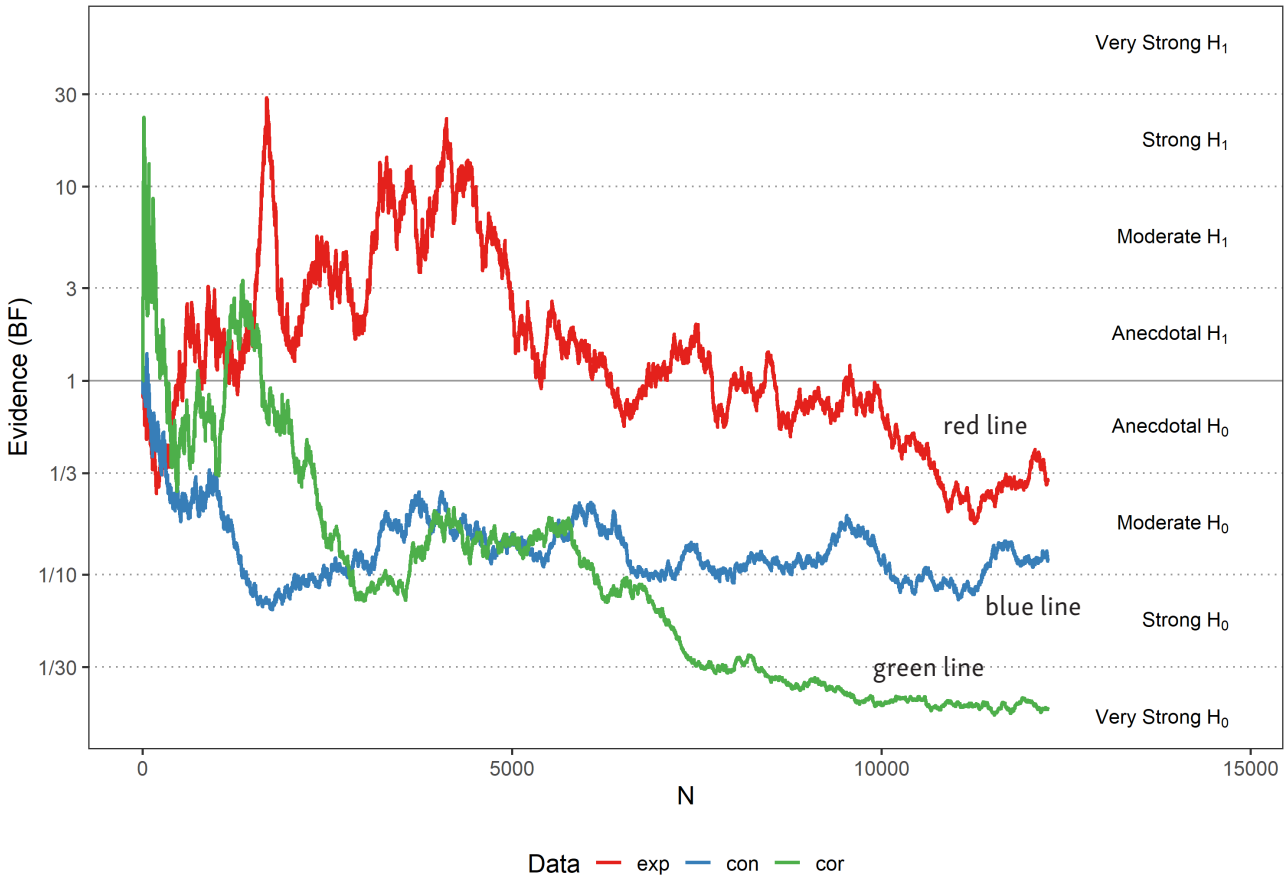

Figure 2. Actual sequential Bayes factors for Dechamps et al.'s (2021) Studies 1 to 3 for experimental condition (red line), control condition (blue line), and correlation of both conditions' mean scores (green line).

quantum-based outcomes, observation effects of this kind must not be restricted to the participants but should also be attributed to investigators and data analysts. This conjecture is central to the PMIR (psi-mediated instrumental response model (Stanford, 1977), which suggests that psi effects can arise unconsciously provided they fulfill an individual's (this includes experimenters) need. As Rabeyron (2020) has emphasized, this constitutes a major problem in research of this nature, as the scientific process typically demands that experimenters and their expectations be clearly distinguished from the phenomenon under review. In micro-PK specifically, it creates the problem that the true source of potentially successful results remains unknown. Although studies have reported anecdotal and indirect evidence of experimenter effects on micro-PK results (see Palmer, 2017), direct experimental tests of the occurrence of such effects in micro-PK remain rare, with the exception of a study conducted by Bierman (1978) that found no evidence for conscious experimenter effects in RNG data. The use of "silent" or "hidden" RNGs, which were concealed from the principal investigator during their studies, revealed significant results indicating experimenter psi (e-psi) (Berger, 1988; Honorton & Tremmel, 1979; Varvoglis, 1989; Varvoglis & McCarthy, 1986) and provided some preliminary evidence in this regard. However, additional research is required to further explore experimenter effects in micro-PK paradigms.

In sum, the present study's central goal was to test a standard micro-PK effect originally reported by Dechamps et al. (2021, Study 1) and to replicate a correlational micro-PK effect found with supposedly very strong evidence for H1 in a post-hoc analysis of the data of Dechamps et al. (2021, Studies 1 to 3). This effect consisted of a substantial correlation between the two within-subject conditions in a high-power micro-PK study involving subliminal priming. Moreover, no explicit reference to a decline effect was made. The study, including the original expectations and analytical methods, was preregistered at OSF (https://osf.io/a47g2), and data collection, reporting, and analyses followed the exact protocol outlined in this pre-registration. With regard to the empirical background concerning the original data and the interpretation of the present data, some adjustments had to be made. Since the original evidence for the correlational effect was based on faulty data

and indeed this original dataset included a true null effect with respect to the correlation, the study unintentionally changed into an anecdotal field experiment unsystematically exploring experimenters' expectations on micro-PK correlations. The experimenters expected that strong evidence for the correlation (BF_{10} > 10) would be found under the condition that the standard micro-PK effect was absent in the experimental priming condition. Given the existence of experimenters' expectations that were indeed unknowingly in contradiction to the objective data, a relevant question concerning the role of experimenters' expectations in micro-PK could be anecdotally addressed and later discussed: Would such a false but strong belief itself affect the appearance of a correlation effect?

In additional analyses that were not part of the preregistration, we explored on a post hoc basis the stability of evidence for the correlation effect tested in this study. With additional data collection, evidence for this correlation will either remain stable or will decline after the initial evidence has been documented. In the latter case, *Change of Evidence* (CoE; see "additional analysis" section below) analyses will be applied to test the observed effect changes against random fluctuations in the evidence for the effect (false positives).

METHODS

Participants

Ethical Statement

Participants consented electronically to participation in the study by pressing an 'accept' button prior to the experiment. They were also informed in general terms about the study and advised that participation was voluntary and were given a brief written explanation of the study's purpose after its completion. All data were coded, stored, and analyzed anonymously. The procedure was approved by the ethical board of the Department of Psychology at LMU Munich and at Kantar (https://www.kantar.com/de), a data collection company specializing in online surveys that conducted the data collection.

Subject Pool

The study's sample comprised German participants distributed throughout the country. Participant recruitment and data collection were organized by Kantar. Kantar distributed invitations to participate in the study to a random selection of their participant pool daily via email, aiming for a completion rate of about 100 per day.

Statistical Approach and Data Collection

We adopted a Bayesian approach in this study. Bayesian inference statistics allow for data accumulation (i.e., the addition of individuals' data until a specific stopping criterion has been met). The correlation effect was assessed using a Bayesian correlation test. The following procedural details were specified in the preregistration: A BF of 10 indicating strong evidence for either H_0 or H_1 was defined as the stopping rule. In case the stopping criterion was not reached until approximately 1,000 participants had been tested, data collection would be stopped at this point. However, if a clear trend toward BF_{10} = 10 was recognizable at n = 1,000, we planned to increase this number of participants further.

The prior for the correlation analysis $\rho \sim$ Beta (0.1) was chosen a priori. This prior was based on an estimated effect size of r = .1 and has also previously been applied in the analysis of faulty correlation data (Dechamps et al., 2021).

Regarding standard micro-PK effects, we additionally tested whether the mean scores of positive pictures would exceed chance expectation in the experimental and control conditions using two separate one-sample Bayesian *t*-tests with a one-tailed approach for the analyses performed (applying an informed prior centered around 0.05 with an *r* of 0.05, i.e., $\delta \sim$ Cauchy [0.05, 0.05]). Although we a priori predicted and tested for the existence of a micro-PK effect, especially in the experimental condition, we were actually hoping for a null finding, as reported in Dechamps et al.'s (2021) Studies 2 and 3. The absence of a standard micro-PK effect in the data was considered to be a precondition for the occurrence of a correlation effect. This assumption was stated in the preregistration of this study.

The Bayesian analyses for the correlation and the standard micro-PK effects in the experimental and control conditions were performed on an irregular basis with the respective actualized sample's mean scores (more or less weekly within the first 1,000 participants and then again with the total sample of more than 2,000 participants). We used the statistical software R's 'Bayes Factor' package for the Bayesian analyses. The data collection took place between January 2021 and February 2021.

Sample Size

Although the stopping criterion of BF_{10} = 10 for the correlation effect was reached early on during data collection at n = 24, we considered the power of this subsample too small to provide convincing evidence for the effect. We thus continued data collection until the supplementary stopping criterion of n = 1,000 participants was reached. At this stage of data collection, the actual correlation was

r = .034 and was within the range of the original result (r = .032). However, the corresponding Bayesian evidence remained inconclusive: BF_{10} = 0.42. We thus decided—in accordance with the preregistration—to collect additional data from a further 1,000 participants. We ceased data collection at this point, since the financial resources were exhausted. The total sample size was N = 2,052 (demographic data available for 1990 participants: 49.20% male, 50.55% female, 0.25% diverse; mean age = 44.22, SD_{age} = 13.84). Final Bayesian analyses were then performed with this complete set of data, as specified in the preregistration.

Materials

Experimental Program

The study was an exact replication of Dechamps et al.'s (2021) Studies 1 to 3. As in the original studies, the present study was run as an online experiment. All subjects could participate from any location using their private computers and internet access. The experiment was executed using a dedicated web server based in the university's computer center and displayed on the participants' web browsers. This was implemented using jsPsych (v 6.1.0; de Leeuw, 2015), a JavaScript library designed to run online behavioral experiments.

Stimuli

Positive and negative images were used as target stimuli and a mixture of them as prime stimuli. The target stimulus sets consisted of photographs obtained from Shutterstock, a provider of royalty-free stock images. The pictures used were pre-selected from this pool by the authors, both experts in experimental emotion induction techniques, using pictorial material. Stimulus selection was primarily based on independent valence estimations. Strongly negative and positive photographs were chosen based on the experts' ratings in case of mutual agreement. The positive target stimuli comprised 20 photographs depicting pets, peaceful landscapes, and groups of happy-looking people. Negative target stimuli encompassed 20 photographs depicting dangerous or attacking animals and other cataclysmal scenarios. The stimulus material was converted to black and white to balance out a general inequality with regard to the coloring of the positive and negative images. Both target sets were matched; that is, each positive target picture had a negative counterpart that was similar with respect to content. These pairs of target pictures represented specific subjects (e.g., a dog) with either positive (e.g., a friendly dog) or negative (e.g., an aggressive dog) valence.

From the target stimuli, two classes of priming stimuli were created. For the control (neutral priming) condition, each priming stimulus comprised an overlay of two matched target pictures. These primes were designed in such a way that the positive and negative stimuli were arranged with an equal emphasis (50/50). Therefore, the prime represented a homogenous mixture of both matched target pictures. Homogeneous mixtures of both target pictures were considered to constitute neutral primes since such arrangements were assumed to reflect the superposed existence of both affective states in the preconscious mind of the observer and would not activate any specific affective tendency above the other. Since 20 matched target pairs existed, the resulting number of corresponding priming stimuli was 20. Primes were accompanied by forward and backward masks comprising scrambled and indefinable versions of each prime. Each priming stimulus was presented three times during a given trial before the target display. The latter was randomly selected by a qRNG from the pair of targets from which the corresponding prime stimulus was created.

The experimental (positive priming) condition used the same mixtures from the matched target pairs and the same presentation modes during the trial, but following the first perfect 50/50 mixture presentation two slightly different priming images were displayed during each priming sequence within a given trial. In the second prime presentation, the positive share representing the positive target was displayed more distinctly than the negative share (60/40) and in the third prime presentation even more so (70/30). The positive image thus became more dominant during the priming sequence and was expected to be more strongly activated in the perceiver's unconscious mind. This rather unusual positive priming procedure should within a trial mimic the evolution of a classical reality and its conscious perception out of the preconscious mind of the observer under the biasing impact of an intentional goal (see Figure 3 for sample stimuli).

The assignment of experimental or control priming to a trial was performed using a pseudo-RNG (pRNG). Following the priming sequence, the quantum-based RNG (qRNG) randomly selected one of the two target images from which the priming stimuli were created in a given trial.

Generation of Quantum Randomness

During each trial after the priming sequence, a qRNG was used to determine whether the positive or negative image from the trial set was presented. To achieve this, a Quantis qRNG by ID Quantique was connected to the web server. This device generates two equally likely superposed quantum states by sending photons through a semi-conductive mirror-like prism. Upon measurement, only one of

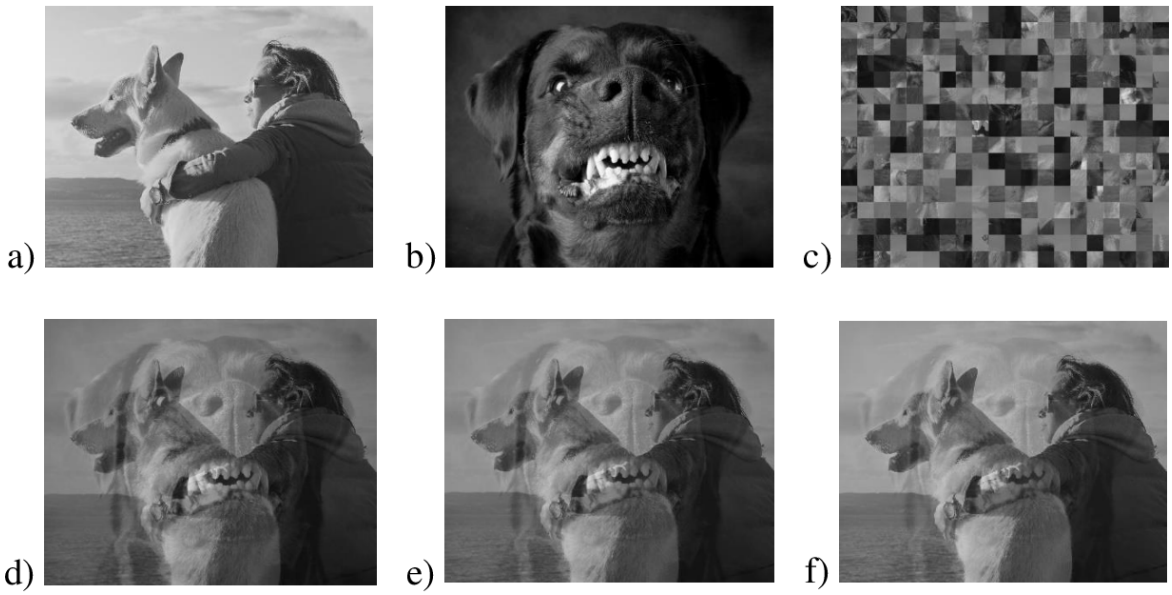

Figure 3. Sample stimuli. One trial consisted of a positive target picture (a) or a negative target picture (b), chosen by the qRNG. Scrambled-up versions of the 50/50 mixtures served as masking stimuli (c). Participants were primed with either an equal mixture of the two outcomes (d; neutral priming) or with a sequence of mixtures becoming more accentuated toward the positive target (d, e, and f; positive priming).

the two states can be observed and translated into either a 0 or a 1 bit. Using the random nature of quantum state reduction, a truly unpredictable result is generated. The qRNG consequently passed all major validation tests of randomness, such as the DIEHARD and NIST test batteries, and is regarded as one of the most effective sources of randomness (Turiel, 2007). The device was connected directly to the server via USB and generated a random bit for each trial after completion of the priming sequence and immediately before the display of the target stimulus, therefore working without a buffer. Care was also taken to ensure that each participant had received an individual bit.

Design

The study employed a within-subjects design with two conditions: an experimental condition (positive priming), in which the positive images from the respective matched target pairs served as the dominant share of the prime stimuli, and a control condition (neutral priming), in which neutral mixtures from respective matched target pairs served as prime stimuli.

Procedure

The invitation to participate in the study was issued via email by the polling company Kantar (https://www.kantar.com/north-america/about) to their pool of professional clients. Participants were advised to ensure an undisturbed environment before commencing the survey. They were asked for basic demographic information to ensure that they satisfied the inclusion criteria. They were then provided with a link that when clicked on took them to the experiment running on the university's web server. After the participants were asked to activate their browser's full-screen mode, they were shown written instructions for the task. Participants were advised that over the course of the experiment, they would repeatedly see flickering visual stimuli as well as positive and negative images and that these stimuli should be watched passively. They were reminded that they could abort the experiment at any time. Prime and image presentations began after the participants had acknowledged the instructions and had consented to participation.

Each participant viewed a total of 40 trials. For each individual, half of the 20 matched target pairs were randomly assigned to the experimental condition and the other half to the control condition using a software randomizer (pseudo-RNG) at the beginning of the experiment. Each of the 20 target pairs was used twice in this setting, resulting in a total of 40 trials. Next, the pseudo-RNG was used to individually permutate the order in which the 40 trials were presented via sampling without replacement.

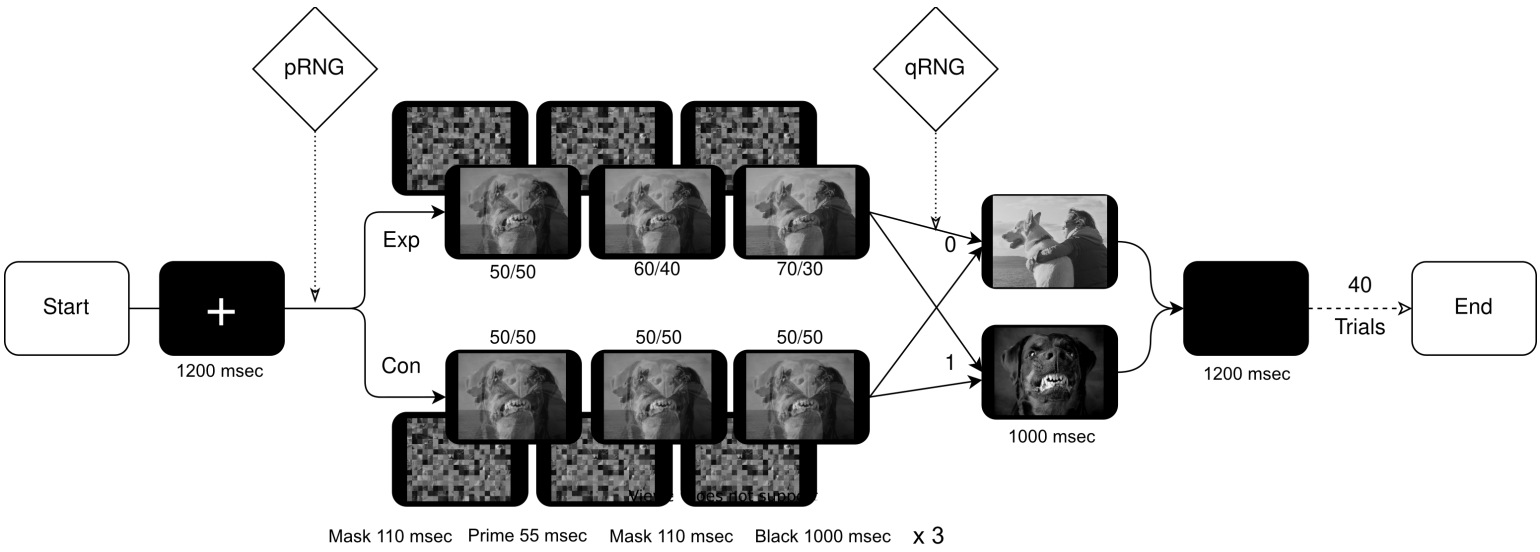

Figure 4. Schematic overview of the experimental design.

During each trial (see Figure 4), a fixation cross was first presented in the center of the screen (1200 ms) to direct the participants' attention toward this location. Next, in the priming sequence, a mixture (control condition) or various mixtures (experimental condition) of the two images that corresponded to the respective target pair of a given trial were used as prime stimuli. In the control condition, the 50/50 mixture prime stimulus was displayed three times for 55 ms each and in each instance was accompanied by a corresponding forward mask (110 ms) and a backward mask (110 ms) to ensure subliminal presentation. Each prime stimulus had a specific masking stimulus that was a scrambled version of the original. A 1000 ms-long gap showing a black screen was displayed between the three prime sequences. In the experimental condition, the presentation mode and times were the same as in the control condition, but the prime stimuli varied. The first prime presentation was a perfect mixture (50/50) of the target pair used in a given trial; the second prime presentation was a 60/40 mixture with the positive target picture being more visible; and the third prime presentation was a 70/30 mixture of this kind. In each trial, after the priming procedure was displayed, the qRNG was activated to provide an individual random bit that determined whether the positive or negative target stimulus from a given matched pair would be presented. The selected target picture was presented for 1000 ms. After this, a black inter-trial interval was presented for another 1200 ms before the next trial started. The two dependent variables consisted of the mean number of positive pictures and, therefore, the number of 0 bits generated by the qRNG in the experimental and control conditions.

RESULTS

First, evidence for the existence or non-existence of the standard micro-PK effects in both conditions were analyzed. Two separate Bayesian one-sample *t*-tests (one-tailed) were performed to test whether the mean number of positive images was higher than expected by chance in the experimental and control conditions. The expected mean score to occur by chance was 10 positive images (out of 20 possible) on average for each condition.

For the experimental condition, the Bayesian one-sample *t*-test (one-tailed) revealed a final BF_{01} = 11.00, indicating strong evidence in support of H_0. The mean score for positive pictures in this condition was M = 9.96 (SD = 2.26).

For the control condition, the Bayesian one-sample *t*-test (one-tailed) yielded a final BF_{01} = 13.95, indicating strong evidence in support of H_0. The mean score for positive images in this condition was M = 9.94 (SD = 2.26).

Figure 5 shows the sequential Bayesian analyses for both *t*-tests for each condition separately. Both sequential BFs showed a clear trend for a null effect, since the accumulated evidence increasingly supported the null hypothesis in both conditions.

Since a standard micro-PK effect was absent in both conditions, the assumed precondition required for a correlation effect to emerge was fulfilled. The Bayesian correlation (Bravais–Pearson) analysis between the mean scores of positive pictures obtained from the experimental and the control conditions yielded a correlation coefficient of $r(2,052)$ = .01 with a final BF_{01} = 8.03, indicating moderate evidence in support of H_0. Figure 6 shows the sequential Bayesian analyses for the correlation.

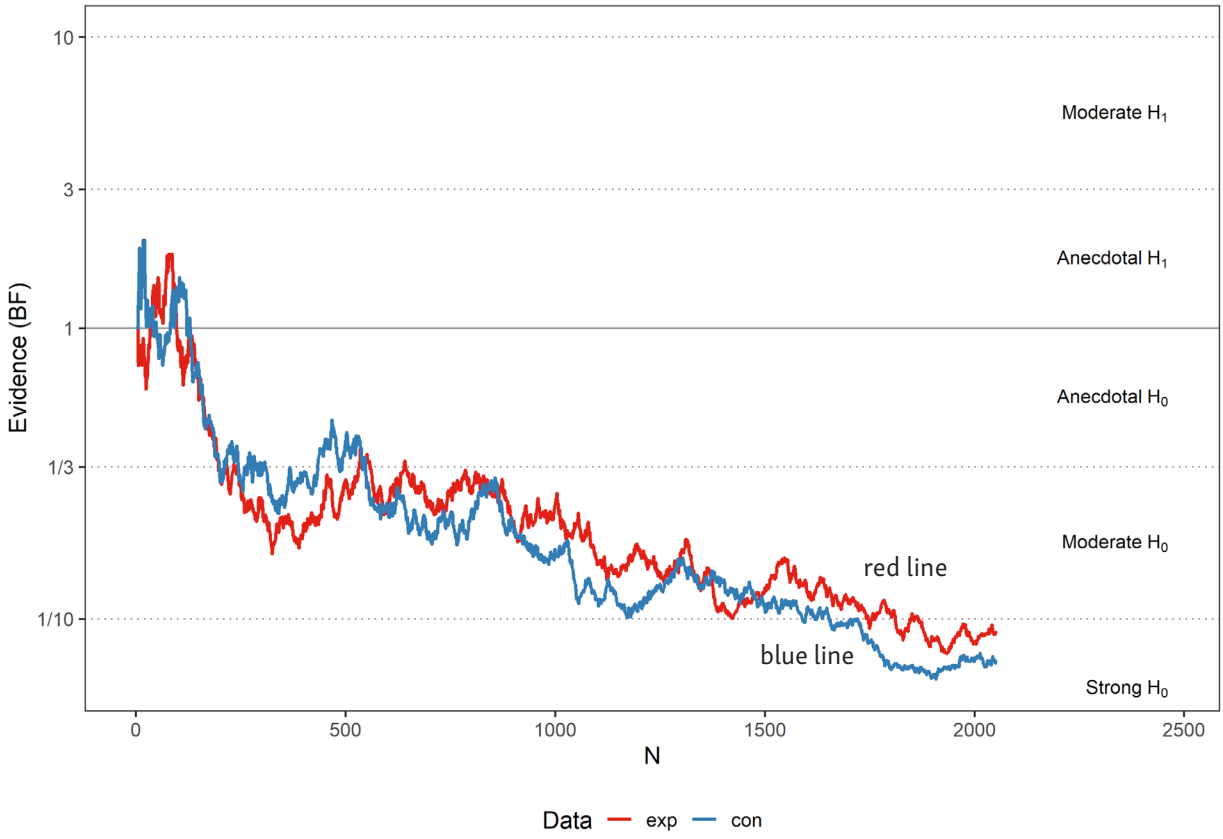

Figure 5. Sequential Bayes factors for experimental condition (red line) and control condition (blue line).

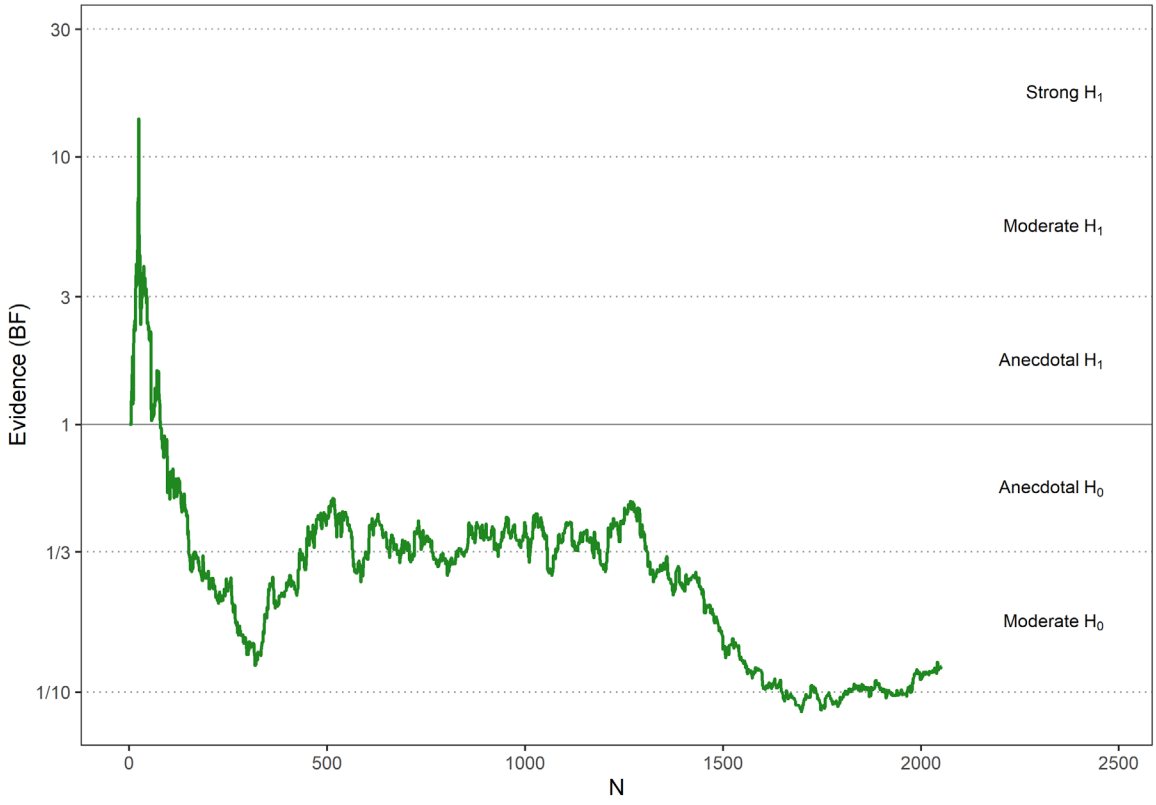

Figure 6. Sequential Bayesian correlation analysis of both conditions' mean scores.

As the graph illustrates, the stopping rule was met at n = 24 with a BF_{10} = 13.90 at this stage of the data collection process. In accordance with the preregistration, we could have ceased the data collection process at this point, on the grounds that strong evidence for the predicted effect had been found. However, we did not fully trust the results, in view of the sample size, which remained low at this phase of our investigation. We therefore increased the sample size to 1,000 participants. With this subsample, the BF still indicated moderate evidence in support of H_0, but the correlation coefficient of r = .034 appeared promising. A further 1,052 participants were recruited to closely monitor further changes in evidence for or against the effect. The correlation coefficient and its corresponding sequential BF declined further throughout this additional data collection process until the above-mentioned final BF was reached.

Additional Analyses

Although the above-mentioned stopping rule of BF_{10} > 10 was reached early on during data collection (at n = 24), these authors decided to carry on with data collection to test the robustness of the effect. At this time, they were still convinced that the effect remained stable. However, the results did not meet this expectation. The initial effect declined, finally indicating moderate evidence for a null effect.

Regarding the stability of the correlational effect across data collection, the presence of a decline in the effect was not explicitly mentioned in the preregistration and thus was not predicted. The authors tacitly assumed that this effect would hold true even if additional data were collected. However, two potential scenarios could be proposed: either strong evidence for this effect would remain stable until the data collection had concluded or, after strong evidence (BF_{10} > 10) had been reached, the effect might decline. The theoretical background for this second proposition is based on the MPI (von Lucadou, 1995, 2015, 2019; von Lucadou et al., 2007), according to which the initial indications of an anomalous effect are expected to decline during additional data collection. Such effect changes across time look identical to a data pattern that consists of an initial "false positive" later revealed as a "true negative." To distinguish a "true positive + later decline" from such a "false positive + later true negative" dataset, Dechamps and Maier (2019) developed three tests, called "change of evidence (CoE) analyses", performed on the sequential BF of the effect under investigation. These analytical techniques test the non-random nature of variations found in sequential BFs in comparison to sequential BFs obtained from simulated data (see Dechamps et al., 2021; Jakob et al., 2020). CoE analyses were not part of the preregistration but would serve as additional post hoc analyses of the correlation effect.

Thus, additional analyses were performed to test whether the change in evidence for the effect expressed by the sequential BF was based on non-random temporal variations. The CoE analyses consist of (a) an identification of the highest reach BF found within the experimental data at any time during the data collection compared with the highest BFs reached in 1,000 simulations of the data obtained from the same qRNG used in the original design (MaxBF analysis); (b) a test of the areas under the sequential BFs (energy of the curve), with BF = 1 as baseline obtained from the experimental data compared to the 1,000 simulations (BF energy analysis); and (c) a calculation of the sum of amplitudes of all frequencies obtained with fast Fourier transforms (FFTs) underlying the sequential BFs of the experimental data and the 1,000 simulations with a comparison of the sum of amplitudes obtained. These analyses test the non-random variation of the effect across time and provide a conservative test of non-random sequential BF fluctuations within such datasets.

The CoE analyses applied to the sequential BF of the correlation effect obtained in this study revealed the following results: the MaxBF analysis performed on the sequential BF of the correlation showed that the highest reached BF found in this dataset was BF_{max} = 13.90 at n = 24 and that 5.16% of the simulations had the same or a higher BF at any point within these datasets. Regarding the BF energy analysis, the sequential BF curve's energy calculated as the area between the sequential BF curve and the BF = 1 horizontal line was –1309.49 for the correlation data, which was found to be surpassed by 26.13% of the simulations' energies. The mean energy of the simulations was M = –784.82 (SD = 19604.62). For the FFT analysis, the sequential BF curves for the correlation effect and the 1,000 simulations were each Fourier-transformed with a sampling rate of 1/N. Since the transform is symmetric, only the first half is considered in the analysis, resulting in 1,026 tested frequencies for the correlation effect and for each simulation. To test the FFT results from the experimental data against chance occurrence, all 1,026 amplitudes obtained from the FFT of the experimental dataset were then added up, creating a sum score of the amplitudes obtained from all tested frequencies of this set. Similarly, the sum score of the amplitudes was computed for each of the 1,000 simulations. The amplitude sum of the experimental condition was 7.56, which was surpassed by 8.46% of the simulations' amplitude sums. The mean amplitude sum of the simulations was M_{amp} = 7.40 (SD = 164.51).

In sum, none of the CoE analyses could reject the null hypothesis at the alpha = 5% level. Thus, the variations found in the sequential BF of the correlation data were not statistically different from random variations.

DISCUSSION

The study reported herein was originally designed to replicate a standard micro-PK effect reported by Dechamps et al. (2021, Study 1 to 3) and to replicate a correlation effect allegedly found between the mean scores of positive images in two within-subjects micro-PK conditions. Based on the apparently strong evidence in favor of the correlational effect (BF_{10} = 36.46), we set up a preregistration wherein we predicted that we would again find strong evidence (BF_{10} > 10) for such a positive correlation in cases wherein any standard micro-PK effects were absent (the latter was found to be true in the present data). We also supposed—but did not explicitly preregister—that this effect would remain stable across a sufficiently large data collection process. After the data collection was completed and the final Bayesian analyses of the data computed, we realized that the BF calculated from the to-be-replicated original dataset of Dechamps et al. (2021, Studies 1 to 3) was based on erroneous data. After the defective data points had been excluded, the correlation analysis yielded strong evidence for the null hypothesis (BF_{01} = 49.48). Consequently, the a priori prediction was based solely on the experimenters' expectations, and the present study involved a field experiment anecdotally exploring experimenters' expectations on micro-PK correlations.

Although a BF_{10} > 10 in line with the prediction for the correlation effect was found in the present data at an early stage of the data collection process, further data acquisition yielded moderate evidence in favor of the null hypothesis. This renders the interpretation plausible that no experimenter effects were present in the actual study. Specifically, the authors' stability assumption concerning the robustness of the effect was not satisfied. One could thus argue that strong and robust evidence for micro-PK effects cannot be produced solely by experimenters' expectancies (see also Bierman, 1978). The long-standing debate (see Bierman, 1978; Kennedy & Taddonio, 1976; Varvoglis & Bancel, 2015) as to whether and to what degree experimenter effects are significantly instrumental in micro-PK research at this point appears inclined toward the negative. This may be true, at least for the conscious expectations of Maier and Dechamps being present in that specific design and when focusing on the actual version of a micro-PK effect. It remains unclear whether this finding can be generalized to all experimenters, to unconscious expectations, and to the entire field of micro-PK research. In addition, the study presented here was not a priori designed as a test of e-psi. This lack of a systematic, scientific testing of experimenters' expectations reduces the informative value of the data in terms of providing evidence for or against e-psi to the level of an anecdotal report only.

With regard to unconscious expectations, the findings may be more ambiguous. Maier and Dechamps ran several micro-PK studies in the past, and in the majority of these experiments they found a data pattern of initial strong evidence and later decline of the effect (Dechamps et al., 2021; Dechamps & Maier, 2019; Jakob et al., 2020). We may thus envisage an implicit conviction on their part that micro-PK effects would usually follow this pattern. The CoE analyses of the sequential BF addressed this possibility. Descriptively, the sequential BF of the correlation effect exactly mimicked such a change of evidence in the data. Bayesian analyses of the actual correlation effect reached strong evidence for the effect (BF_{10} > 10) and then declined. However, none of the CoE analyses was significant—that is, the observed variation of the effect did not differ from random variations. This ruled out the possibility that unconscious experimenter expectation effects of this kind played a significant role in producing this data pattern. Nonetheless, a statistical trend was found in two of the three analyses, with one of the two tests being very close to the 5% level. Moreover, the strength of the correlation after the analysis of 1,000 participants closely resembled the expected effect size. This cast some doubt on the assertion that experimenter effects were truly absent in this study. Nevertheless, it may be safe to state that when the CoE results of this study are compared with those of past micro-PK studies conducted by our research team (Dechamps & Maier, 2019; Jakob et al., 2020), the extraordinary evidence of non-random changes in the sequential BFs of micro-PK data obtained in their research cannot be interpreted solely as e-psi but should be attributed to the participants' level of stimulus observation at least to a substantial degree. The experimenters' positive expectations might only additionally shape the micro-PK effect, making their research more successful (see also Parker & Millar, 2014) than if it were conducted by skeptics with negative expectations. Thus, facilitation or suppression effects might occur, depending on the respective expectation (Broughton, 1979), but these might not fully explain the observed effects. Rabeyron (2020) recently argued that e-psi imposes a practical limit on the scientific exploration of psi effects, making the determination of the effects' precise nature and the location of these effects' cause impossible (see also Broughton, 1979; Palmer, 1997). Regarding the conscious and unconscious expectations described above, our empirical data support this idea to some extent but also indicate that experimenters' expectations of this nature do not themselves produce psi effects—at least in our micro-PK research. On a side note, any evidence for micro-PK effects found regardless of the nature of their origin would nevertheless demonstrate that the mind creates reality.

Finally, the potential existence of a third form of the experimenter effect should be acknowledged. While writing up this discussion section, both authors retrospectively reflected on their overall attitudes toward e-psi. Their common belief was that e-psi effects simply played the marginal role of a moderating factor with small or negligible effect size; in other words, the data—as they are—were much appreciated by these authors. This attitude (or the *Journal of Scientific Exploration*'s readers' attitudes, etc.; see Rabeyron, 2020) may have produced the effects described herein. If this were the case, individuals' meta-level beliefs would have dramatic effects in psi research. The only possible escape from this dead-end scenario would be either to establish robust cynicism toward e-psi within the scientific community to reduce the potential impact of such effects or to accept that data obtained from this research (and perhaps from all scientific research) to some extent simply reflect (universal) beliefs in how the world should work.

At the end, some limitations of this study also have to be addressed. First, the unconscious processing of the subliminal prime presentations was not tested across participants with a standard test of subliminal perception such as a signal detection task. In contrast, the subliminal prime presentations' efficiency was only superficially explored by pre-testing the presentation mode in our research group and by our experiences with previous studies (Dechamps et al., 2021, Study 1 to 3). However, it was not considered crucial when some of the participants could identify the primes occasionally. In these potentially rare cases the positive picture was consciously pre-activated in the participant's mind and might have caused a similar tendency of approaching a positive target picture reality compared to a truly subliminal processing mode. Overall, we admit that a signal detection task performed after the micro-PK task to evaluate the unconscious processing of the primes would have provided a more stringent test. On the other hand, a pre-evaluation of the primes was considered sufficient for our goals and an inefficient subliminal priming procedure might not alternatively explain the null findings since this same procedure successfully revealed strong evidence for micro-PK in the past (Dechamps et al., 2021, Study 1). Secondly, the participants' compliance with the instructions and their attentional focus on the task were not controlled. Since this micro-PK task consisted of a purely passive watching task, inattentiveness during participation might have occurred and could have deteriorated the micro-PK effect. Thirdly, the prime and target pictures used in our design were chosen based on experts' ratings of valence only (see Dechamps et al., 2021) rather than using pictures from sets with normative ratings of valence and arousal. This suboptimal stimulus material could be another potential source for the null findings caused by confounding factors that counteract the valence effect.

In sum, our replication attempt of a standard micro-PK effect reported by Dechamps et al. (2021, Study 1) provided strong evidence for a null-effect in the present data. Also, in the present data and opposed to the present authors' prediction, moderate evidence for the null hypothesis was found with regard to a correlational micro-PK effect mirroring a similar null finding in the original data (Dechamps et al. (2021, Study 1 to 3). With regard to experimenter effects affecting the correlational micro-PK data, no or only minimal evidence for e-psi was found. The anecdotal nature of this aspect of our study prohibits further speculations regarding this question. Rather, future psi research should further explore the impact of e-psi on psi data in a more systematic way, within other areas of investigation and across different research teams (see Bierman & Jolij, 2020) to assess the extent of e-psi contributions in these investigative fields.

Resources

The study's preregistration, all digitally sharable material with the exception of the stimulus material, for which the authors do not hold the rights to distribute, and the experimental data and analyses scripts, are openly accessible at https://osf.io/tbha6.

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RESEARCH ARTICLE

A Grounded Theory Update on the Roswell UFO Incident

Kevin D. Randle

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HIGHLIGHTS

Two recent lines of hard evidence do not support the latest official explanation for the mysterious debris from the Roswell UFO case. The source or nature of this 1947 crash event thus remains an elusive but important question worthy of further investigation.

ABSTRACT

Something unquestionably strange fell southeast of the tiny New Mexican town of Corona in the summer of 1947—an event that has become an iconic case in ufology and part of mainstream culture. Documentation and eyewitness testimony prove that rancher William Ware "Mack" Brazel took samples of that debris into the Roswell, New Mexico, sheriff's office, who in turn reported the situation to Roswell Army Air Force officials. Controversy around the incident has always centered on the identification of the recovered debris versus its existence. Photographs of the purported debris have suggested a terrestrial explanation, while eyewitness descriptions have supported either that explanation or something much more exotic. Attempts to decipher text from a photographed document known as the Ramey Memo have not provided definitive results that would rule out any explanations. Still, there are two areas in which empirical studies can be conducted and advancements possibly made. These involve (a) strides to clarify the operational and logistical details of "Project Mogul," which is the US military's claimed source of the debris, and (b) new efforts to read the Ramey Memo from higher-quality digital scans. A grounded theory (or deductive) examination of these two lines of empirical evidence fails to clearly support the military's latest "official explanation" and thus leaves open the extraterrestrial hypothesis for the debris. Future directions for research are therefore discussed.

KEYWORDS

Empiricism, Project Mogul, UAPs, UFOs, Roswell Incident, Ramey Memo, trace cases

Unidentified flying objects (UFOs)—now commonly termed *unidentified aerial phenomena* (UAPs)—are an enduring enigma, although mass media depictions of UFOs (Sparks et al., 1998) and government attempts to manage national narratives (Haines, 1999) have stifled serious research reports in this domain. However, Sturrock's (1994a, 1994b, 1994c) survey of the American Astronomical Society found that many of its members were surprisingly open-minded about the topic when asked to reply anonymously. This same study further found that 4.6% of respondents reported witnessing or recording UAPs, whereas, more broadly speaking, five percent of all reports are never definitively explained (for a discussion, see Kean, 2010). In fact, recent government disclosures have affirmed both the anomalous and physical nature of many UFO sightings by military pilots (Office of the Director of National Intelligence, 2021). These trends agree with independent scientific panels that concluded UFOs deserve continued and

Figure 1. Map of New Mexico with the various points of interest and locations of important events involved with the Roswell incident noted. Credit: National Atlas of the United States.

in-depth study, even if the physical evidence does not necessarily indicate violations of known natural laws or the involvement of an extraterrestrial intelligence (Greenewald, 2019; Kuettner et al., 1970; Sturrock, 1999; Sturrock et al., 1998). This latter idea is known as the extraterrestrial hypothesis (ETH), which contends that some UFO/UAPs represent spacecraft occupied by extraterrestrial life or non-human aliens, or non-occupied alien probes from other planets visiting Earth.

One of the most famous and arguably complex UFO occurrences is the 1947 "Roswell Incident" (Figure 1) involving physical debris with supposedly peculiar appearance and properties. The government has offered multiple explanations for the debris, and UFO investigators have conducted perhaps the most extensive investigation of a single case in UFO history. For those unfamiliar with the Roswell incident, Table 1 lists important investigations by both advocates and detractors of ETH interpretations of this historical event. Qualitative methods, such as witness interviews, have most often featured in these authors' inquiries. However, there is also some tangible information in the form of official government records and other documentation of undisputed provenance. This paper assesses two key pieces of empirical data relative to the US Air Force's official explanation for Roswell. Specifically, the contexts and implications of these pieces of evidence are critically reviewed and evaluated via a grounded theory approach. This refers to the construction of hypotheses or theories through the collection and analysis of data, i.e., the research does not begin with a theory, but rather the theory is the outcome of the process (e.g., Chun et al., 2019). In this way, the author aims to give readers an update on the status of this undoubtedly significant case and identify the most promising directions for future research.

CASE BACKGROUND: BASIC FACTS AND CURRENT CONTROVERSIES

Beginnings of an Iconic Case

Early in July 1947, William "Mack" Brazel (Figure 2) discovered a field filled with metallic debris that interfered with ranch management and care of livestock. Suspecting that it had something to do with the military, and believing they were responsible for cleaning up the debris, he made the four-hour trip into Roswell to speak with the Chaves County Sheriff, George Wilcox. After interrogating Brazel, Wilcox placed a call to the Roswell Army Air Field. Major

TABLE 1. Chronological List of Seminal Investigations on the Roswell Incident

Advocates	Detractors
Berlitz, C., & Moore, W. (1980). *The Roswell incident.* Grosset & Dunlap.	Carrion, J. (2010). *The Roswell Deception*. CreateSpace.
Blum, H. (1990). *Out there*. Simon & Schuster.	Clary, D. L. (2001). *Before and after Roswell*. Xlibris.
Bourdais, G. (2004). *Roswell: Enquetes, secret et desinformation*. Dilisco.	Dietrich, D. (2021). *The Roswell deception and the demystification of WW II*. Sky Books.
Bullard, T. (2010). *The myth and mystery of UFOs*. University Press of Kansas.	Frazer, K., Karr, B., & Nickell, J. (1997). *The UFO invasion*. Prometheus.
Carey, T. J., & Schmitt, D. R. (2009). *Witness to Roswell*. New Page Books.	*Jacobsen, A. (2011). *Area 51: An uncensored history of America's top Secret military base*. Orion.
Carey, T., & Schmitt, D. (2022). *Witness to Roswell: 75th anniversary edition*. Red Wheel Weiser.	Klass, P. J. (1997). *The real Roswell crash saucer coverup*. Prometheus.
*Clark, J. (2018). *The UFO encyclopedia*. Omnigraphics.	+Korff, K. (1997). *The Roswell UFO crash: What they don't want you to know*. Dell.
Corley, L. (2007). *For the sake of my country*. AuthorHouse.	Lawson, G. (2021). *Roswell: The after-action report*. Amazon Digital Services.
+Corso, P. J., & Birnes, W. J. (1997) *The day after Roswell*. Simon & Schuster.	Mantle, P. (2012). *Roswell alien autopsy: The truth behind the film that shocked the world*. Independently published.
Eberhart, G. M. (Ed.). (1991). *The Roswell report: A historical perspective*. J. Allen Hynek Center for UFO Studies.	McAndrew, J. (1997). *The Roswell report: Case closed*. Good Press.
*Edwards, F. (1966). *Flying saucers—Serious business* (pp. 41–42). Lyle Stuart.	*Peebles, C. (1995). *Watch the skies*. Smithsonian Institution Press.
Friedman, S., & Berliner, D. (1992). *Crash at Corona.* Paragon House.	Pflock, K. (1994). *Roswell in perspective*. Fund for UFO Research.
*Good, T. (1987). *Above top secret* (pp. 254–7, 261–2, 333, 407, 434, 547–8). Quill.	Pflock, K. T. (2001). *Roswell: Inconvenient facts and the will to believe*. Prometheus.
Harris, P. L., & Salla, M. (2017). *Conversations with Colonel Corso.* StarworksUSA.	Redfern, N. (2005). *Body snatchers in the desert*. Pocket Books.
*Hastings, R. (2007). *UFOs and nukes*. AuthorHouse.	Redfern, N. (2017). *The Roswell UFO conspiracy*. Lisa Hagan Books.
Leacock, C. P. (1998). *Roswell: Have you ever wondered?* Novel Writing Pub.	Saler, B., Ziegler, C., & Moore, C. (2008). *UFO crash at Roswell: Genesis of a modern myth*. Smithsonian Books.
Marcel, J., Jr., & Marcel, L. (2008). *The Roswell legacy*. RWW New Page Books.	Shawcross, T. (1997). *The Roswell file*. Bloomsbury.
McAvennie, M. (Ed.). (2004). *The Roswell dig diaries*. Pocket Books.	Weaver, R. L., & McAndrew, J. (1995). *The Roswell report: Fact vs. fiction in the New Mexico desert*. Department of the Air Force.
*Randle, K. (1989). *The UFO casebook* (pp. 5–11). Warner Books.	Weaver, R. (2020). *The Roswell report: Fact vs. fiction in the New Mexican desert*. A. J. Cornell Publications.
Randle, K., & Schmitt, D. (1991). *UFO crash at Roswell*. Avon.	
Randle, K., & Schmitt, D. (1994). *The truth about the UFO crash at Roswell*. M. Evans.	
Randle, K. (2000). *The Roswell encyclopedia*. HarperCollins.	
Randle, K. (2016). *Roswell in the 21st century*. Speaking Volumes.	
Randle, K. (2022). *Understanding Roswell*. Flying Disk Press.	
Schmitt, D. (2017). *Cover-up at Roswell: Exposing the 70-year conspiracy to suppress the truth.* Red Wheel Weiser.	
Schmitt, D. (2020). *Roswell: The ultimate cold case closed*. New Page Books.	
Spencer, L. (2017). *Alien interview*. Independently published.	

* Mentions the Roswell case in connection with other UFO sightings.

+ Discredited book based on contradictions, poor research, and other distortions.

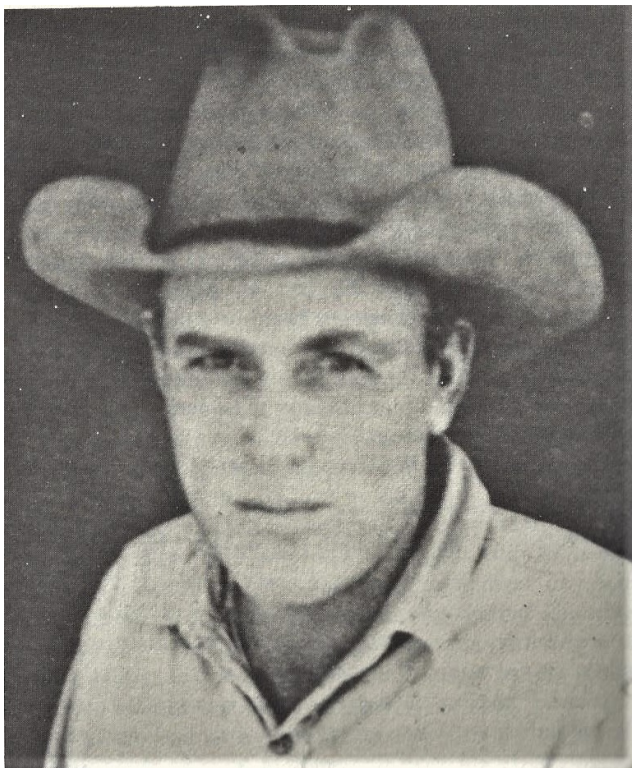

Figure 2. Mack Brazel, the rancher who found and reported the debris to Chaves County Sheriff George Wilcox.

Figure 3. Major Jesse Marcel, the Air Intelligence Officer of the 509th Bomb Group and the officer who took samples of the debris to Roswell and Fort Worth.

Jesse A. Marcel, Sr. (Figure 3), the Air Intelligence Officer at the base, responded to the call, and determined that an investigation should be undertaken. Captain Sheridan Cavitt, the officer in charge of the counterintelligence detachment in Roswell, was alerted and joined Marcel at the sheriff's office (Berlitz & Moore, 1980; Carey & Schmitt, 2009; Friedman & Berliner, 1992; Randle, 2018). Both Marcel and Cavitt had the opportunity to examine samples of the metallic debris brought in by Brazel (McGuire, 1990).

Although it was late in the day on Sunday, July 6, 1947, both men accompanied Brazel back to the ranch. Marcel said,

> We took off cross county [from the sheriff's office] behind this pickup truck this rancher had. He didn't follow any roads going out . . . so we got to his place at dusk. It was too late to do anything, so we spent the night there in that little shack and the following morning we got up and took off.

Later he would tell Linda Corley (2007) who reported it in *For the Sake of My Country*,

> I went to his house. I followed him. We left Roswell in the afternoon and got there at dusk . . . so we couldn't do anything that evening. So, we stayed at his house that night with Cavitt . . . [We] spent the night at his house. We were treated with a can of pork and beans and crackers.

The In-Field Observations

The next morning Brazel took them to the field filled with the metallic debris that Marcel would later say was about three quarters of a mile long and about 200 yards wide. Cavitt would later tell Air Force Colonel Richard Weaver the debris was spread out in a much smaller area (Weaver & McAndrew, 1994). According to Marcel, he and Cavitt collected some of the material. Marcel then sent Cavitt back to the base to brief Colonel William Blanchard, the commanding officer in Roswell. Marcel stayed on the field, filling his car with the debris. He would later tell investigators he left a great deal of it behind. Late in the afternoon, he left the field, driving back to Roswell. On his way to the base, he stopped by his house to show his wife, Vaude, and son, Jesse Jr., the strange metallic debris. Jesse Marcel, Jr., pointed to some strange symbols on one of the small beam-like members. Marcel, Jr., later said it was purple and looked like hieroglyphics (Marcel & Marcel,

2008). Marcel eventually returned to the base and briefed the commanding officer on what he had seen on the ranch.

The First Official Response

At some point on the morning of July 8, Blanchard called his public affairs officer, First Lieutenant Walter Haut, telling him to issue a press release saying that the 509th Bomb Group was in possession of a "flying saucer" (Carey & Schmitt, 2009; Randle, 2018). Haut, in various interviews with UFO investigators, said that he was unsure if Blanchard had dictated the press release to him over the telephone, if he had given Haut the information so that he could write the release himself, or if Haut had actually gone to Blanchard's office (Friedman & Berliner, 1992; Randle, 2018). Haut said that he then had either driven into town to deliver the press release to the four media outlets or he had called them and dictated it to them over the telephone. Given the timing, and the slight variations in the wording, it is most likely that Haut had called both newspapers and both radio stations rather than physically handing the press releases to the reporters and editors.

According to some newspaper reports, Marcel, who had been ordered to take samples of the debris to the Fort Worth Army Air Field in Texas, left in a B-29 at 10:30 that morning. Marcel suggested it was just after lunch. The important point here is that Marcel, on orders from Brigadier General Roger Ramey, the commanding officer of the Eighth Air Force, and the senior headquarters, left Roswell on July 8th, removing him momentarily from the story. There is a 1947 document that provides the exact times for much of this reporting. According to *The Daily Illini*, the first of the stories on the Associated Press wire appeared at 4:26 p.m. on the East Coast. That would mean that the stories went out from Albuquerque sometime prior to 2:26 p.m. (MST). This is more or less consistent with what both George Walsh of radio station KSWS and Frank Joyce from radio station KGFL remembered.

The Military's Subsequent Chain Reaction

At 4:30 p.m. (EST), there is the first "add" to the Associated Press story, which mentioned "Lt. Warren Haught [Walter Haut]," who was described as the Public Information Officer at Roswell. This new information suggested that the object had been found "last week" and that the object had been sent onto "higher headquarters," which in this case meant Fort Worth. When the aircraft carrying Marcel and three packages apparently containing wreckage landed in Fort Worth, the enlisted soldiers had been ordered to remain with the plane until a guard was posted. Marcel disembarked with the packages he had been carrying. Marcel was driven to Ramey's office, where he spoke to the General. He set the debris on Ramey's desk and then followed Ramey to what was called the map room to show Ramey exactly where the debris had been found. While Marcel was in the map room with Ramey, according to what Marcel would say later, the debris was removed from Ramey's office and a torn-up weather balloon (Figure 4) was substituted, spread out on the floor. Later Marcel would tell Linda Corley (2007) that the debris had been concealed under the brown paper that now held the remains of a badly degraded Rawin radar target.

In Fort Worth, Texas (3:30 p.m. CST, 4:30 p.m. EST), Cullen Greene, an editor at the *Fort Worth Star Telegram*, read the story as it came over the news wire. J. Bond Johnson, who worked at the newspaper in July 1947 said in an interview, "I don't know the mechanics. We'd get those alerts. The bells would ring, and it would be an attention thing. It would be an editor thing." As a reporter/photographer, he would not have worried about those "things" (Randle, 2018). Greene asked Johnson if he had his camera with him and then told him to interview Ramey at his

Figure 4. Major Jesse Marcel with the balloon and Rawin radar target wreckage in Brigadier General Ramey's office at the Fort Worth Army Air Field. Examining the photographs decades later Marcel said that this was not the material he had escorted to Fort Worth. Photograph courtesy of the University of Texas at Arlington Special Collections.

Headquarters. It was after 4:30 p.m. (CST, 5:30 EST) when Johnson arrived at the Fort Worth Army Air Field. Johnson said that it was about a twenty-minute trip from the newspaper office out to the airfield. He said that he routinely covered activities at the airfield, so when he reached the gate, he showed his press pass. He also had a Civil Air Patrol sticker on his car, which would have made it easier for him to enter the airfield. He had been told to go to Ramey's office, though he normally would visit the Public Information Officer rather than the Commanding General (Shandera & Moore, 1990).

Johnson said that when he entered the office, he was met by Ramey and Colonel Thomas DuBose, the Eighth Air Force Chief of Staff. According to Johnson,

> I posed General Ramey with this debris piled in the middle of his rather large and plush office. It seemed incongruous to have this smelly garbage piled up on the floor . . . spread out on the floor of this rather plush, big office . . . I posed General Ramey with this debris. At that time, I was briefed on the idea that it was not a flying saucer but in fact was a weather balloon that had crashed.

Photographs from General Ramey's Press Conference

Johnson took a series of photos in Ramey's office; specifically, there were two of Ramey and two of Ramey and DuBose (Figure 5). He also took two photographs of Jesse Marcel crouched near the remains of a weather balloon and holding a large fragment of a Rawin radar reflector. General Ramey saw the debris and identified it as nothing more than a mundane weather device. To reinforce this opinion, he ordered an officer from the base weather office to appear while Johnson was in the office to confirm his identification. Documentation proves that Warrant Officer (later major) Irving Newton was a weather officer at the base in 1947. Newton told the author in a letter dated July 21, 1994, that he was alone in the base weather office when he received a call ordering him to General Ramey's office. Newton said that he was the only one there, in his office, and could not leave. General Ramey then called and told him to "get your ass over here now. Use a car and if you have to, take the first one with the keys in it."

Newton wrote to me,

> I was met at the General's office by a Lt. Col. or Col. who told me that some one [sic] had found a flying saucer in New Mexico and they had it in the General's office . . . but the General suspicioned that it might be meteorological equipment or something of that nature and wanted it examined by qualified meteorological personnel. (Newton, 1991)

Newton said that when he entered the office there were several others there, including reporters. He said, ". . . when I went in . . . [there were] a couple of press people, a Major, I learned to be Major Marcel and some other folks. Someone introduced Major Marcel as the person who found this material."

However, Newton added something new to his interview when he spoke to the Air Force officers conducting a new investigation in 1994. He told the Air Force investigator,

> While I was examining the debris, Major Marcel was picking up pieces of the target sticks and trying to convince me that some notations on the sticks were alien writing. There were figures on the sticks lavender or pink in color, appeared to be weather faded markings with no rhyme or reason.

The problem is that Newton's testimony in the mid-1990s does not agree with what he had said in the past. In his interview with Bill Moore, he was asked, "But wouldn't the people at Roswell have been able to identify a balloon on their own?" Newton said, "They certainly should have. It was a regular Rawin sonde. They must have seen thousands of them." 1n 1994, he would tell Air Force investigators that "We did not use them at Fort Worth . . . These were used mostly on special projects and overseas."

An Evolving Official Narrative with Two Controversies

Then, at 7:29 p.m. EST (6:29 p.m. CST) came another new lead for the wire story. It said, "Procede [sic] Washington. Lead All Disk." This meant, simply, that the lead on the story that had been transmitted prior to this would be changed and the new lead substituted. This was interrupted with another bulletin almost immediately. It said, "Fort Worth—Roswell's celebrated 'flying disk' was rudely stripped of its glamor by a Fort Worth army airfield weather officer who late today identified the object as a 'weather balloon.'" At this point, two of the ongoing controversies were created. Johnson had photographed Ramey as he crouched near the debris. He held a document in his hand (Figure 6) in the four pictures taken of him. In one of them, the paper, though slightly crumpled, obviously contained something written. Many years later, photographic enlargements of that portion of the picture revealed some of the words. Even the most casual examination reveals enough of the wording to suggest that the

Figure 5. Brigadier General Roger Ramey and Colonel Thomas DuBose posed with the alleged wreckage of the balloon and Rawin radar target in General Ramey's office. This is the first time that the print has not been cropped and shows the full picture. Photograph courtesy of the University of Texas at Arlington Special Collections.

Figure 6. Closeup of Brigadier General Roger Ramey holding the document that has been described as "The Ramey Memo." Photograph courtesy of the University of Texas at Arlington Special Collections.

document concerns the recovery in New Mexico. The document came to be labeled "The Ramey Memo." It has since been scrutinized by a variety of methods, and controversy still rages about the entire contents of the memo. If it can be read, then the truth about the Roswell retrieval might be discovered.

The other issue is the identity of the wreckage. It is clear from the photographs, as well as the testimony of those present in 1947, and from Jesse Marcel, that the debris in the photographs are the remnants of a weather balloon and a Rawin target. The balloon envelope, though blackened, is in the picture. The Rawin radar target is badly degraded but can be identified as well. The questions raised at the time and later echoed by UFO researchers was (a) Why there had been such a Herculean effort to retrieve the remnants of a common balloon, and (b) Why there was no one in Roswell who could identify it for what it apparently was? During the Air Force search for records in the mid-1990s, an answer was offered. This was no ordinary weather balloon, but part of the top-secret "Project Mogul," designed to spy on the Soviet Union (Weaver & McAndrew, 1994). Given the top-secret nature of the project, it was necessary to collect all the remnants and important to divert attention from the ultimate purpose.

THE PRESENT PAPER

Sturrock et al.'s (1998) scientific panel review of UFO evidence recommended that "studies should concentrate on cases that include as much independent physical evidence as possible" (p. 184). To that end, the remainder of this paper reviews two core controversies in the Roswell Incident as outlined above. The preceding background demonstrates that Roswell has involved no less than three "official explanations," with the government's latest solution being that the UFO debris was weather balloon material from the top-secret Project Mogul activity (see Figure 7). The Part 1: Review section scrutinizes this claim based on both previously known and new documentation. As is discussed below, the cumulative evidence plainly contradicts this proposed resolution. This raises the question of the actual identity of the debris if it was not from Project Mogul. Part 2: Review explores this issue by assessing new photographic evidence (with accepted provenance) from General Ramey's press conference. This update and synthesis of important empirical data subsequently forms the basis of a grounded theory for the debris, as well as recommendations for future research.

PART 1: REVIEW OF THE "PROJECT MOGUL" CONTROVERSY

In February 1994, the Secretary of the Air Force, Sheila Widnall, responding to a Government Accounting Office plan to "ascertain the facts regarding the reported crash of an UFO in 1949 (sic) [1947] at Roswell, New Mexico," tasked the Administrative Assistant to the Secretary of the Air Force (SAF/AA) to lead the Air Force search. Colonel Richard Weaver and First Lieutenant James McAndrew were the officers in charge of the effort. They determined that this was one area in which there is sufficient documentation to draw the preferred conclusions. The Air Force officers then began looking for a terrestrial explanation for what fell at Roswell. Civilian UFO researchers had spent decades attempting to find such a solution. All parties had been able to rule out aircraft accidents, rockets, or missiles from White Sands Proving Ground (later the White Sands Missile Range), and normal weather balloons. However, some civilian researchers, including Robert Todd and Karl Pflock,

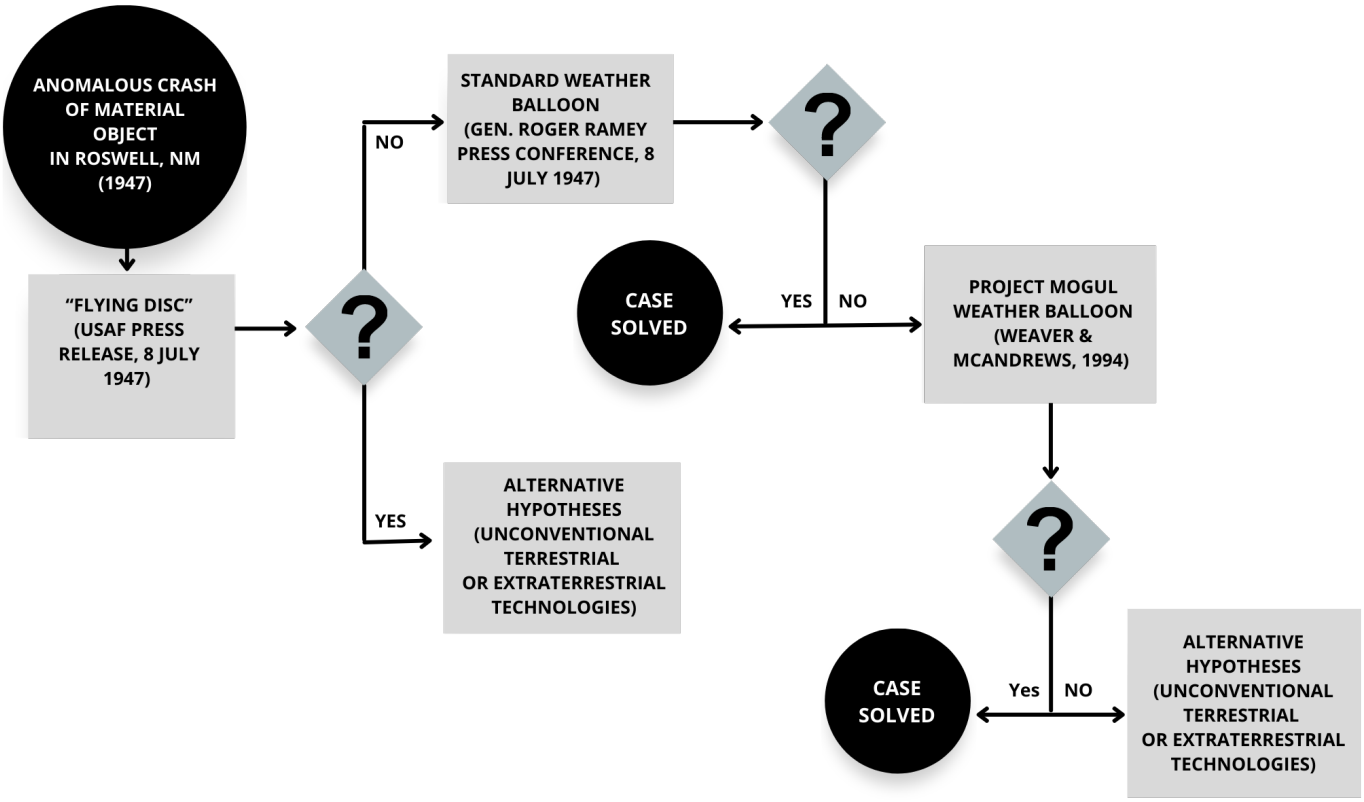

Figure 7. Flow diagram of competing explanations for the Roswell UFO debris (courtesy of Beth M. Houran).

pointed to a special project that had operated out of Alamogordo Army Air Field in June and July of 1947 (Pflock, 2010, pp. 144–165). They believed that one of these flights might account for the debris found by Mack Brazel.

Although now known in the Roswell literature as Project Mogul, in June 1947 scientists were conducting unclassified experiments to create a constant level balloon under the guidance of New York University. The theory was that an acoustical level existed in the upper atmosphere that would conduct sound waves a long distance. If a balloon could be placed in that acoustical level for an extended period of time, it might be possible to detect the Soviets' detonation of an atomic device from a great distance. Spying on the Soviet Union was the ultimate, and highly classified, purpose of the experiments in New Mexico. Called Project Mogul at the highest levels, those working in New Mexico called it the New York University high-altitude balloon experiments (Pflock, 2010; Randle, 2018; Weaver & McAndrew, 1994).

All flights of the Mogul arrays, or as Charles Moore, a team member, called them, the New York University balloon project balloon flights, were accounted for in the records, with few exceptions. The first few flights, through Flight No. 3, took place on the East Coast before they moved their experiments to New Mexico and are irrelevant to this discussion. Flight No. 4 was the first scheduled flight in New Mexico, planned to launch early on the morning of June 3, 1947. According to the diary kept by Dr. Albert Crary, the project leader, it was cancelled because of cloudy weather. Another attempt was made the following morning. Crary's entry for this attempt said:

> June 4 Wed
> Out to Tularosa Range and fired charges between 00 and 06 this am. No balloon flight again on account of clouds. Flew regular sonobuoy mike up in cluster of balloons and had good luck on receiver on ground but poor on plane. Out with Thompson pm. Shot charges 1800 to 2400.

Charles Moore, attempting to interpret what this meant, later wrote, "Crary's diary entries for June 4 are puzzling because they are contradictory." Moore suggested that Crary had copied his field notes into the diary later and that the events of early June, including the critical entry on June 4, had been copied in one sitting, which Moore believed might account for the seeming contradictions. Moore wrote:

> One interpretation of the June 4 entry is that the launch scheduled for making airborne measurements on Crary's surface explosions after midnight was canceled because of clouds but, after the sky cleared around dawn, the cluster of already-inflated balloons was released, later than planned. The initial cancellation and later launch were recorded sequentially, as they occurred, in his field notes which he later transcribed into his permanent diary, without elaboration.

Moore suggested that they had just arrived in Alamogordo and would not, at that time, have "improvised." He said that after they had rigged an entire array, "I think . . . we would have launched the full-scale cluster, complete with the targets for tracking by the Watson Lab radar." However, documentation uncovered by the Air Force investigation, including an illustration of Flight No. 5, showed no Rawin targets were used, either on the full array flights or the cluster of balloon flights launched for additional experimentation when full array flights were cancelled. As Moore told the author during a discussion in his home in Socorro, New Mexico, in 1991, they couldn't put the helium back in the bottles (see also Weaver & McAndrew, 1994). Moore added:

> I have a memory of J. R. Smith watching the June 4th cluster through a theodolite on a clear, sunny morning and that Capt. [Larry] Dyvad reported that the Watson Lab radar had lost the targets while Smith had them in view. It is also my recollection that the cluster of balloons was tracked to about 75 miles from Alamogordo by the crew in the B-17. As I remember this flight, the B-17 crew terminated the chase, while the balloons were still airborne (and J. R. was still watching them), in the vicinity of Capitan Peak, Arabela, and Bluewater, New Mexico. I, as an Easterner, had never heard of these exotically-named places but their names have forever been stuck in my memory. This flight provided the only connection that I have ever had with these places. From the note in Crary's diary, the reason for termination of the chase was due to the poor reception of the telemetered information by the receiver aboard the plane. We never recovered this flight and, because the sonobuoy, the flight gear, and the balloons were all expendable equipment, we had no further concern about them but began preparations for the next flight.

Although Moore claimed that "this flight provided the only connection that I have ever had with these places," this is simply untrue. Flight No. 17 from September 9, 1947, flew along the same trajectory and passed over the same exotically named landmarks Moore associated with the alleged June 4 project flight. In addition, they lost tracking of it in the exact same vicinity of Capitan Peak that Moore said happened for Flight No. 4. It is quite possible that Moore's 50-year-old memory of "No. 4's" flight path is really a badly distorted recollection of the real Flight No. 17 three months later.

Years later, according to Moore, he heard about the debris found by Brazel and thought it was a good description of the debris that would have been produced by one of their balloon trains. He thought that as the train dragged along the ground, kept partially aloft by the few balloons that had not yet burst, it would have shed debris, creating the mess that Brazel described. He said, "It is possible that Brazel found some of the wreckage from the NYU Flight No. 4." However, the train being dragged by still-inflated balloons to create such a debris field implies the rigging holding everything together to be still there. Yet Brazel, when interviewed by the *Roswell Daily Record* the evening of July 8, 1947, indicated he found no balloon rigging of any kind. A real Mogul constant-altitude flight of this period would have left hundreds of yards of rigging mixed in with the other crash debris it held together in flight. This discrepancy was noted by Lt. James McAndrew in his interview with Moore, but then the issue was dropped when Moore could not come up with an explanation. The other flights, from Flight No. 5 until the first week in July, were accounted for in the history of the balloon project. No other flight disappeared in this fashion in the relevant time frame. The records are quite clear on that. If Flight No. 4 does not account for the debris found by Brazel, then Project Mogul is *not* the answer.

PART 1: CONCLUSIONS AND CONSIDERATIONS

To fully consider whether Flight No. 4 could have been the source of the retrieved debris, we need to delve into various details of the operations of balloon projects, and Mogul, specifically. Balloon flights at the time were cancelled for a variety of reasons, including high winds and cloud cover at the time of the launch. While it does not seem that clouds would affect balloons, CAA (FAA) requirements require cancellation in those conditions. As the balloons ascended and descended, and given the length of some of the arrays, these could be a hazard to aerial navigation because they would be invisible in the clouds. This is described in *Technical Report No. 1, Balloon Group, Constant Level Balloon Project*, dated April 1, 1948 (covering the period from November 1, 1946, to January 1, 1948), which said

that one of the requirements was that the weather be relatively clear so that the balloons could be seen.

That same report also stated, "Notices to airmen [NOTAMs] are to be issued if the balloon is descending within designated regions of dense air traffic." This establishes a requirement for NOTAMs, both for the launch, as the arrays climbed through the civil air space, and then another for the arrays as they descended. Once at altitude, which would have been somewhere above 50,000 feet, or far above the levels where civilian aircraft operated in 1947, they would no longer be a hazard. Flight No. 5, launched on June 5, eventually came down in the vicinity of the Roswell Army Air Field, which suggests a NOTAM would have been required, and everyone in flight operations at the base would then have been aware of these long arrays, if they had not encountered information about them earlier.

Another strong indication that a NOTAM *should* have been issued comes from Flight No. 5's tracking data. This shows it passed only 4–5 miles south of the Roswell base as it was descending. In addition, it lingered less than a dozen miles south and west of Roswell air space for over an hour during its slow stratospheric backward drift while a B-17 chase plane circled underneath. The B-17 followed it all the way to its crash site, marked as only 16–17 miles due east of the base. It is difficult to believe that air controllers, plane spotters, or security guards could all have failed to notice the 400-foot–long balloon train and chase plane. In fact, Mogul flight summaries indicate it was tracked optically through theodolite from Alamogordo for 90% of its flight clear to Roswell. It was only lost from sight during its descent phase, when it fell below the horizon formed by the Sacramento Mountains east of Alamogordo. It is not likely that it could be optically tracked for almost 100 miles from Alamogordo yet remain invisible only a few miles away from Roswell base. Moreover, the B-17 should have been in contact with flight control to explain their presence in Roswell air space.

While all this might suggest that those in Roswell would have been aware of the long balloon arrays, Moore made it even clearer in various interviews. He told the present author that he and one or two others had driven to Roswell to ask for assistance in tracking their balloon arrays. He has written that he had been at the base after retrieving debris from Flight No. 5, and that he had "an interview by the Officer of the Day to whom I showed the *recovered equipment* [emphasis added] from Flight 5." This means, of course, that Moore and his colleagues would have explained what they were doing in Alamogordo and what the arrays would have looked like for those in Roswell. This is crucial because if the personnel at Roswell were aware of the Mogul balloon arrays, they should have also been able to identify Mogul as the source of the material recovered and brought to the base a month or so later. There is another fact that shows there was nothing unusual about these arrays. Crary's diary for Sunday, June 8, indicated that "Rancher Sid West found balloon train south of High Rolls in mountains. Contacted him and made arrangements to recover equipment Monday. Got all recordings of balloon flights . . ." No heavy secrecy was involved, and West appears to have known that what he found was not extraordinary debris.

Concerning the secrecy of the project, Moore has claimed that "at that time, the term MOGUL was not known" to those outside the project, even to the New York University balloon crew. The implication is that the project was so secret and compartmentalized that information about it was not widely known. The problem is that this is entirely false. Crary, in his diary, mentions the name Mogul more than once. As one example, in an entry from April 7, 1947, Crary wrote "Talked to [Major W. D.] Pritchard re 3rd car for tomorrow. Gave him memo of progress report for MOGUL project to date . . ." Furthermore, regarding those far removed from Mogul operations, a report from Wright Field on August 25, 1947, classified only "Confidential," concerned a suspected hoax crash disc from Illinois sent to them by the FBI for analysis. The term "Project Mogul" was explicitly used, saying that the object had nothing to do with it. Another FBI memo a month later, referencing the Wright Field report, uses the term "Operation Mogul" four times, even though this memo also had a low classification. What was secret was the goal of the project, not that there was such a high-altitude balloon project.

It is also important to reiterate that, as explained above, the photographs from General Ramey's press conference clearly show the remains of a neoprene balloon, and the very degraded pieces of a Rawin radar reflector made of aluminum foil and balsa wood sticks. There is nothing in the photographs to suggest that the material was exposed to the high desert for over a month, nor was there any obvious dirt clinging to it. There was also no evidence of the strings or other items used to construct Mogul arrays, or the presence of other materials besides a single balloon and a single target. These points are interesting but do not prove that the debris was unrelated to Project Mogul—though they certainly suggest that. However, the photographs lack enough information to conclusively identify a Mogul balloon, and some testimony indicates that this debris was not part of a Mogul array. Indeed, there is no evidence that Rawin radar reflectors were used in those first flights in New Mexico. Moore himself supplied an illustration for Flight No. 5, dated June 5, 1947 (again, the repurposed Flight No. 4). There are no radar reflectors on this flight. There is no mention of radar tracking until Flight No. 8, launched on July 3. An illustration for Flight No. 2,

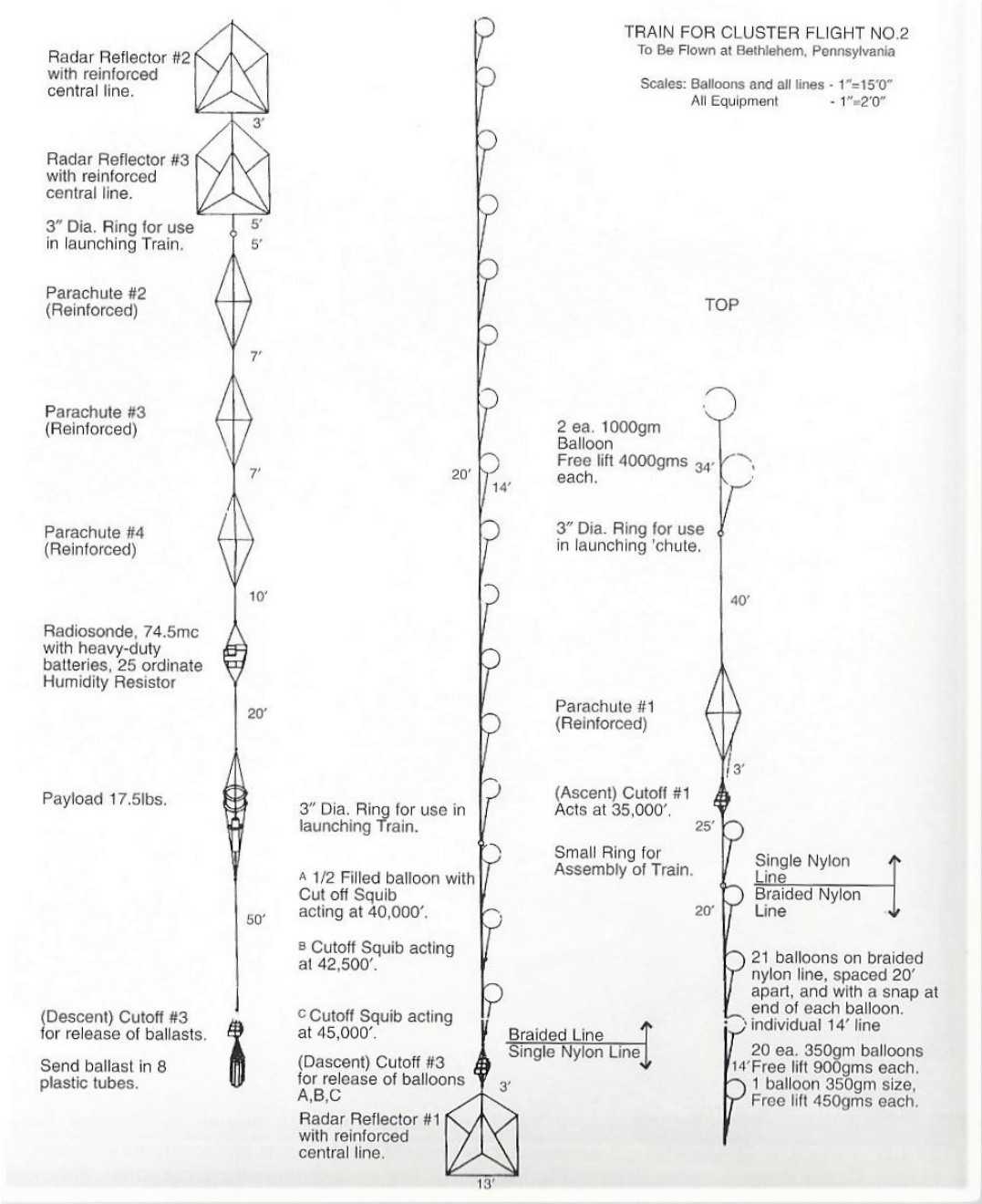

Figure 8. Illustration of the composition of Flight No. 2 that was launched from Bethlehem, Pennsylvania, before the project moved to New Mexico. This array, which included three Rawin radar targets, was more than 600 feet long and was compared to the Eiffel Tower and the Washington Monument to provide scale. Credit to the United States Air Force and New York University.

which provided no data, did contain radar reflectors (see Figure 8), but again, there is no evidence they were used in New Mexico until later.

Most critically, the overwhelming evidence points to Flight No. 4 as being cancelled, which immediately rules it out as a candidate for the source of the debris. Interestingly, in the final report on NYU's balloon activities there is a tabulation of all the flights. Both Flight No. 4 and Flight No. 9 are missing. This tabulation also notes regarding Flight No. 5, "First successful flight carrying a heavy load." Multiple official Air Force and other histories also state that a June 5 flight (i.e., No. 5) was the first AAF research balloon in New Mexico (see Figure 9). None mention a balloon flight the previous day. This would suggest that the cluster of balloons launched the day before was not a full Mogul array. Moore, however, with no documentation to support the conclusion, wrote, "I think that Flight No. 4 used our best equipment and probably performed about as well

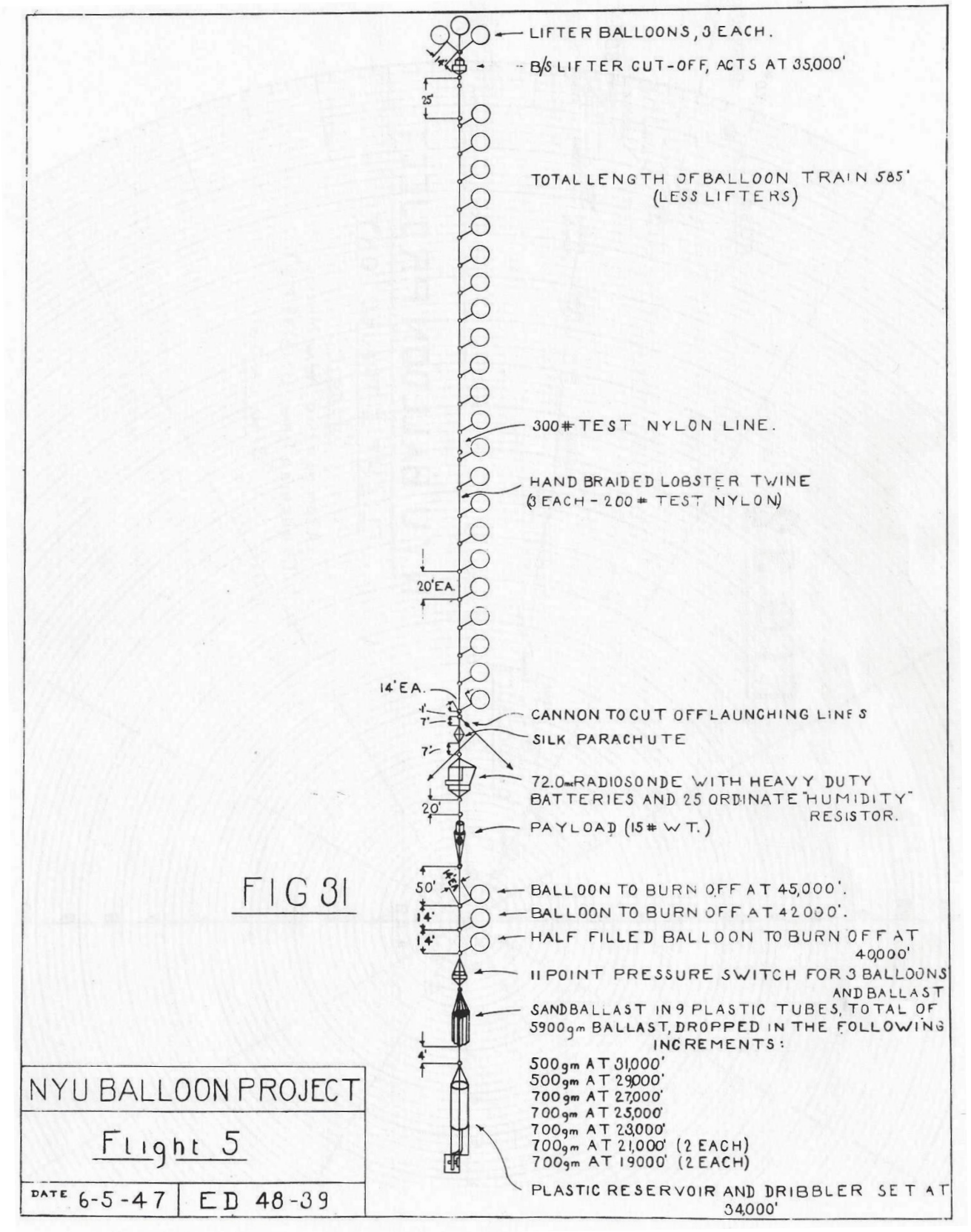

Figure 9. Flight No. 4, which had been cancelled, was made up like Flight No. 5, shown in this illustration and listed in the records as the first successful flight in New Mexico. Flight No. 5 was two-thirds the length of the arrays launched on the East Coast and contained no Rawin radar targets until sometime after July 8, according to the available records. Credit to the United States Air Force and New York University.

as or better than Flight No. 5." The logical question to be asked is if Flight No. 4 performed as well as or better than Flight No. 5, then why was it not listed in the tabulation or in official histories? It would have been the first successful flight, unless, of course, it wasn't a full Mogul array.

Given the time it took to build the full array and prepare it for launch, it would not have been possible to build a new array for Flight No. 5 and launch it that day. Crary's diary is clear on the point. Flight No. 4 was delayed by weather. Flight No. 5 was, in fact, Flight No. 4, using equipment stripped off of the cancelled No. 4 and reattached to a new balloon cluster redesignated and launched on June 5 (much like is indicated happened for No. 3, using equipment stripped from the canceled No. 2). Flight No. 5 was recovered, as Moore noted and records document. In contrast, there is no documented record of a Mogul balloon recovery of a flight from the previous day.

Many of the participants in Project Mogul, both those working on the New York University project, and in the military, were contacted and provided statements about their work. Dr. Crary's field notes and diaries provided on-the-scene documentation and later reports were filed with the proper authorities. Access to that documentation has been available since the publication of the Air Force report on the balloon activities. Other documents related to various other balloon projects, including those by the Navy, also have been examined for additional information. Given the work done by many UFO researchers, including Robert Todd, Karl Pflock, Robert Sheaffer, Don Schmitt, and Tom Carey, it is unlikely that today any new knowledge will be gained about Project Mogul and the activities surrounding it. There is a possibility that somewhere in the various archives and government agencies there could be additional information about balloon operations in the 1940s. However, the best estimate is that all such documentation has been released into the public arena.

PART 2: REVIEW OF THE "RAMEY MEMO" CONTROVERSY

Early Deciphering Attempts

In 1980, Brad Sparks (Pflock, 2010, p. 209; Randle, 2016, p. 293) obtained an enlarged copy of the photograph taken of General Ramey, in the attempt to read the document he held. Sparks was able to read the word "BALLOONS." Five years later, in 1985, after careful examination of the photograph, Sparks perceived additional words that are now "unanimously or almost unanimously agreed-upon" including "weather balloons," "Fort Worth, Tex.," and "disc." Barry Greenwood made an independent but unsuccessful attempt to read the memo in the mid-1980s. In 1991, Don Schmitt sent a copy of the photograph to former NASA scientist Richard Haines and asked if he could interpret anything on the paper. Using a microscope to scan the photograph, he could reportedly see vague words but not discern individual letters. Haines thought that a better-quality enlargement might reveal more of the message.

An official attempt to read the message was made by the Air Force during their investigation into the alleged Roswell UFO crash ordered by then Secretary of the Air Force, Shelia Widnall. According to the Air Force:

> Additionally, the researchers obtained from the Archives of the University of Texas–Arlington (UTA), a set of original (i.e., first-generation) prints of the photographs taken at the time by the *Fort Worth Star-Telegram*, that depicted Ramey and Marcel with the wreckage. A close review of these photos (and a set of first-generation negatives also subsequently obtained from UTA) revealed several interesting observations . . . In an attempt to read this text to determine if it could shed any further light on locating documents relating to this matter, the photo was sent to a national-level organization for digitizing and subsequent photo interpretation and analysis. This organization was also asked to scrutinize the digitized photos for any indication of flowered tape (or "hieroglyphics," depending on the point of view) that were reputed to be visible to some of the persons who observed the wreckage prior to it getting to Fort Worth. This organization reported on July 20, 1994, that even after digitizing, the photos were of insufficient quality to visualize either of the details sought for analysis . . . (Weaver & McAndrew, 1995, p. 29)

Other Interpretations of the Ramey Memo

That was where the matter rested until 1998, when J. Bond Johnson, who had taken six of the seven existing photographs in General Ramey's office, decided to investigate further. Johnson put together a team to inspect the photographs that included Ron Regehr, a space and satellite engineer. Using a huge enlargement of the photograph, a computer, and a variety of software and camera equipment, they were able to read more of the message. There were, quite naturally, gaps in what they could see, and they noted that the message had been typed in all capital letters. Their interpretation of the message was:

> AS THE . . . 4 HRS THE VICTIMS OF THE . . . AT FORT WORTH, TEX . . . THE "CRASH" STORY. . . FOR 0984 ACKNOWLEDGES . . . EMERGENCY

POWERS ARE NEEDED SITE TWO SW OF MAGDALENA, NMEX . . . SAFE TALK . . . FOR MEANING OF STORY AND MISSION . . . WEATHER BALLOONS SENT ON THE . . . AND LAND . . . rOVER CREWS . . . [SIGNED] . . . TEMPLE.

Others began to request copies from the original negatives held at Special Collections at the University of Texas at Arlington Library. They brought their expertise to bear on the message in Ramey's hand. To the satisfaction of many, they could also see letters and images as suggested first by Sparks and then by Johnson and his team. The problem was that many of those doing the work were not seeing the same things as had been published or that others were seeing.

Neil Morris, a technician who was employed at the University of Manchester in England, began to work on the message as part of the team created by Johnson. He broke down the message line by line so that it would be easy to follow his interpretation. He used capital letters to represent the parts of the message of which he was sure, lowercase letters to represent his best guess of some letters, an asterisk to denote a letter he couldn't decipher, and a dash where there was little more than a smudge on the message.

Morris' interpretation of the message was:

(1)----------***ARY WERE ----------
AS
(2)----------fxs 4 rsev1 VICTIMS OF THE WR
eck and CONVAY ON TO
(3)---------*** AT FORT WORTH, Txe.
(4)----------***S** smi Ths *ELSE* *****
unus-d**e T&E A3ea96 L******
(5)---------SO ught CRASHE s pOw*** ***
N***** SITEOne IS envery *****
(6)---------***D* bAsE ToLd ***a* for
we**ous BY STORY are 8*****
(7)--------lly thry even PUT FOR BY WEATHER BALLOONS n*d** were
(8)----------**** **la** l***enver*****
(9)
(10)
Temple

It was not close to an exact match for what Johnson had released earlier and deviated in several places. In this new version, while the word "victims" remains, as does the Fort Worth, Texas, nearly everything else is different. One of the major points in the Johnson version was the wording that suggested, "Emergency Powers are needed Site Two SW of Magdalena, Nmex."

John Kirby, a researcher who worked for a huge company in the computer field, also looked at the message. Using his expertise and equipment, he was unable to see much of anything. He did agree that the third line read, "At Fort Worth, Tex." The second line, which many consider the critical line, said, ". . . are the remains of the material you commanded we fly." In still a different version, David Rudiak (Kirby, 1999; Randle, 2016, p. 296) suggested he saw very little of what others had seen. According to him, and using the same mix of capitals for what he was sure of and lower case for what he suspected, he reported that the message read:

(1) ---------- officer
(2) -----(jul)y 4th the VictIMs of tHE weECK you fOrWArdEd TO The
(3) -------EaM At FORT WORTH, TEX.
(4) -------5 pM THE "DISC" they will ship [swap?] FOR A3 8th Arrived.
(5) ----or 58t(h) bom(be)r sq(?) Assit [Assess] offices? AT ROSwe(ll) AS for
(6) ---54th SAID MIStaken--------[meaning? Weather? Balloon?] of [is] story And said
(7) news [clip, chat, dirt] out is OF WEATHER BALLOONS which were
(8)----- Add[And, Ask] land d---------[dirt cover?] crews.
(9)
(10)
rAMEy

Schmitt, Carey, and Don Burleson came up with their own interpretations of the memo. For instance, Burleson (2000a; Randle, 2000) wrote, "A number of attempts have been made to read the Ramey letter. Quite frankly, most of these attempts are amateurish, and even some ufologists have concluded that there is nothing in the Ramey image that advances the case for the Roswell incident. They are MISTAKEN" (p. 15). Burleson stated that he had spent a year working on deciphering the Ramey memo. He claimed that he had the advantage of being the director of a computer lab with a background in cryptanalysis. Burleson stated that he had been using several excellent computer image enhancement software packages, "including LUCIS, the most advanced software used today in such fields as microscopy" (Burleson, 2000b, p. 8).

Burleson wrote, "here is my reading, so far . . . [indeterminate parts of words are indicated by hyphens, and missing words are indicated by parentheses.] A few spots are a bit tentative, but essentially the letter reads:

(1) RE-CO-OPERATION WITH ROSWELL DISK 074 MJ-
(2) -AT THE () () THE VICTIMS OF THE WREKCD YOU FORWARDED TO THE
(3) TEAM AT FORT WORTH
(4) () ON THE "DISK" MUST HAVE SENT LOS ALAMOS ADVANCED ()
(5) URGENT. POWERS ARE NEEDED SITE TWO AT CARLSBAD, NMEX.
(6) () SAFE TALK NEWSPAPER MEANING OF STORY AND
(7) ONLY SHOW () () BY WEATHER BALLOONS () WAVE () ()
(8) L-DENVER CREWS
(9)
(10) TEMPLE (Randle, 2000)

It should be noted that the Ramey memo is not an encrypted message, but a plain-text message that is obscured by the distance to and the angle of the camera, among other factors. In another effort, Stan Friedman contacted Rob Belyea, the owner of ProLab, to examine high-resolution scans made of the negative. Friedman paid someone to take the negative from the Special Collections and have a computer lab make the scans. Belyea said that he could not spend hours examining the message but could rule out or confirm the interpretations made by others by using his software to decide on character count and combinations of letters. Belyea stated that he could not see "Magdalena" in the text as others had claimed. He did say, "They're pulling off all sorts of [readings], but they're making some of it up."

There is an additional problem, only partially addressed in the study of the text. If this was a military message sent to a military installation, there should have been some military jargon in it. The attempts at reading it have failed to account for any military jargon. The closest is Rudiak's (Randle, 2000, p. 303) attempt to place military unit designations into the message. Rudiak noted that what he thought was "5 PM" made no sense because the military would have used the twenty-four-hour clock that would have said, "1700 Hrs." rather than "5 PM." Taken altogether, there is no consensus on what the message says, the best way to review it, or how to resolve the discrepancies. One researcher said that it had to be assumed that the message had something to do with the Roswell case, but there really is no reason to make that assumption. The message could be about almost anything, and the words and images being seen might reflect what the researcher wants to see rather than what is actually there. Analysis suggested that the word "victims" as it appears in the message is the critical word. The problem is that those studying the message do not see "victims" as universal. One researcher said that he thought that the critical word was "remains." Russ Estes (Randle, 2000, p. 303) noted that it seemed to be a mix of upper- and lower-case letters, with the viewers perceiving what they expected to see. To Estes, the first letter looked more like a "P" than a "V." He stated that there seemed to be a lower case "I" in the word, and that the last letter looked more like an italic "5" than it did an "S."

PART 2: CONTEXT EFFECTS IN INTERPRETATIONS OF THE RAMEY MEMO

The Houran–Randle Experiment

Given the preceding observations, Houran and Randle (2002a) were interested in researching the variables that guided interpretations of what was an obviously ambiguous stimulus. The Ramey memo is ambiguous, and it seems clear that the bias of the researchers has crept into their interpretations of the memo. If the document could be more easily interpreted, then this would be a simple task with a consensus regarding its contents, but as demonstrated, even those who have spent months and years in their research do not agree on their interpretations. Houran devised an experiment to test this hypothesis. There were three related studies in which three groups of self-selected participants were asked to decipher the Ramey Memo. The participants were randomly assigned to one of three possible scenarios (or experimental conditions): (a) that the memo dealt with the Roswell UFO crash; (b) that it dealt with the testing of an atomic bomb; or (c) they were "blinded" and not told anything about the contents. The expectation was that each condition would elicit significant differences in the participants' interpretations. They were also interested to see if there was significant agreement in the identity of words in the same locations, regardless of the suggested condition.

Lastly, the study also considered the psychological variables of prior knowledge or interest in UFOs generally and the Roswell case specifically, and the perceptual-personality trait of "intolerance of ambiguity." This variable involves the need for categorization and certainty that often leads to premature closure, and it has been shown to influence UFO-related perceptions (e.g., Houran, 1997; Randle, 1999). However, the roles of expectancy-suggestion and cognitive set are only two examples of potentially many motivating factors. Dewan (2011) found, for instance, that witnesses and proponents are often influenced by

> . . . the ubiquitous presence of UFO and alien imagery in American popular culture; broad-based public mistrust in the scientific establishment; the usefulness of the phenomenon in modern

> "technospiritual" reconciliations; the occasional presence of a seemingly core experience comprised of near universal characteristics, and the influence of UFO-centric cognitive models in the perceptions, interpretations, and reconsiderations of said experiences. (p. viii)

It is therefore not surprising that participants in the Houran–Randle study who believed that the memo concerned the Roswell crash studied the document for an average of twenty minutes. Those who had been told the memo was about an atomic bomb averaged sixteen minutes, whereas those who were experimentally-blinded spent fourteen minutes. Some words were perceived across all three test conditions, including "Fort Worth TEX," "Story," and "Balloons." Interestingly, those told that the memo was about atomic testing reported seeing "Glasses," "Morning," "Flash," "Atomic," "Laboratory," and "Land." Those who were given no information only saw "Fort Worth, TEX," "Flew," "Story," and "Balloons." The number of words deciphered was further related to the participants' ages, level of intolerance of ambiguity, and their prior exposure to the UFO field and Roswell case. Despite this clear influence of psychological priming on interpretations of the memo, Houran and Randle (2002a) commented that:

> The surprisingly high agreement between our participants and previous investigators on specific words in identical locations in the Ramey memo suggests that some of the document is indeed legible, even without computer enhancement. However, the meaning or context of those words remains ambiguous because the degree of interpretation of the document is strongly influenced by suggestion effects and the interpreter's cognitive style. We are inclined to believe that such effects have also tainted the previous studies on the memo using sophisticated software because there appears to be weak interpreter reliability among the earlier analysts. (p. 60)

There is a final complication with the Ramey memo. Johnson several told several investigators that he had handed the message to Ramey. That confuses the source of the document that Ramey is holding, suggesting that Johnson brought the document into the office. Johnson said that he had received it at the newspaper office, which suggests that it was one of the teletype messages that had been sent to the newspaper over the news wire that said debris was being sent to Fort Worth from Roswell. If the document Ramey is holding was provided as a prop as Johnson suggested, then it could relate to the Roswell crash but would be from a civilian source. It would do nothing to confirm or discredit an ETH view of the event. However, when Johnson was challenged on this point, he then changed his mind and said that he had not brought the teletype message into Ramey's office.

Houran and Randle—Criticisms and Follow-up

Roswell researchers had mixed reactions to Houran and Randle's (2002a) study. Illustratively, some apparent supporters of the study uncritically accepted its conclusions (e.g., Printy, 2003/2014) or speculated that the presence and glimpse of the Ramey Memo was a disinformation exercise against the Soviet Union (Westwood, 2002). This latter idea is interesting, but Randle and Houran (2002) ultimately disfavored it for various practical reasons. On the other hand, passionate advocates of the Ramey Memo strongly criticized the Houran–Randle study on conceptual and statistical grounds (see, e.g., Rudiak, 2003a, 2003b). For example, David Rudiak argued that the research participants did not spend enough time reviewing the material or were inadequately informed of the context to make a "proper" interpretation of the document's context. In support of Houran–Randle's basic experimental design to test context effects, it has been applied with similar results to examine claims of reputed writing on the Shroud of Turin (Jordan et al., 2015).

It should be noted in fairness that Rudiak did spot a few statistical typos in the Houran–Randle paper that did not alter their previous results or conclusions, but these did underscore that mistakes in reporting are always possible (Houran & Randle, 2002b, 2003). But this criticism equally applies to Rudiak's own work on the memo, which Printy (2003/2014) and others have characterized as being plagued with confirmation biases (for a discussion of this confound, see Nickerson, 1998). Still, ancillary analysis on the Houran–Randle data affirmed the influence of priming effects by showing that: (a) More interesting contexts motivated participants to spend more time trying to read the memo; (b) More interesting contexts produced more perceptions of specific words; and (c) The number of interpreted words exclusive to each experimental condition consistently exceeded the number of words that were commonly perceived across the different conditions (Houran & Randle, 2003).

It is disappointing that critics of the Houran–Randle study did not conduct any direct or conceptual replications to address their claimed weaknesses in the research design. Moreover, Houran and Randle (2002a) offered several suggestions in support of new studies of the memo:

> First, to be methodologically consistent we rec-

> ommend that standardized computer enhancement be used on the best raw data that we have using comparable software programs. Analysis should be conducted by at least three independent and blind laboratories that specialized in the area of reading and transcribing archival documents. Their only motivation should be payment for providing professional and objective reports. The laboratories could be provided all available scans of the document . . . With this triangulation approach, we can reasonably estimate the inter-rater reliability (and hence validity) of the resulting interpretations (i.e., do the laboratories show statistically significant agreement on specific words in precise locations in the text). (p. 60)

There are certainly challenges with these recommendations, such as securing ample funding, gaining the cooperation of suitable analysts or laboratories, and controlling for priming effects as discussed above. To these ends, Houran (2005) published the results of an unsung project along these lines that was financially supported by the Fund for UFO Research. In a direct follow-up to Houran and Randle (2002a), Houran had the highest-quality memo scans at the time independently evaluated by three qualified laboratories. The idea was for expert analysts to apply different methodologies for the attempted deblurring, restoring, and interpreting of the Ramey Memo photograph. Table 2 summarizes the main findings from this triangulated effort, which included the work from the laboratory that conducted image analyses of the Shroud of Turin (Marion, 1998). The test centers separately reached the same conclusion, i.e., their best techniques only slightly improved aspects of the image but the memo remained illegible because it was severely blurred and corrupted by film grain (or speckle) noise. That said, Andre Marion stated that it was best to obtain new scans under pre-specified conditions.

The Greenwood Approach

Barry Greenwood (2009, p. 13) made additional attempts to read part of the memo. He argued that it more closely matched documents transmitted over news wires than it did military teletype communications. He did note that Johnson had said that he had brought the document into Ramey's office and handed it to him, which would mean that it was a news wire teletype rather than a military one. Johnson, however, soon retracted the claim. Greenwood noted that some of the phrases in which there was general agreement were also common to news reports published on July 8, 1947. The phrase, "AT FORT WORTH, TEX," appeared in newspapers just that way. The *Nevada State Journal* on July 9, 1947, reported ". . . the commanding general of the 8th air force at Fort Worth, Tex."

To Greenwood this seemed to be additional evidence that Ramey was holding a copy of the newspaper teletype, whether handed to him by Johnson or someone else, rather than a classified message that had been delivered to his

TABLE 2. Summary of Independent Image Analyses of the Ramey Memo (Houran, 2005)

Ph.D.-Level Researcher/ Laboratory	Findings/ Commentary
Nikolas P. Galatsanos *Computer Science Department* *University of Ioannina, Greece*	**Method**: Blind deconvolution algorithms slightly improved the digital images, but not the restored images. **Conclusion**: No clearly interpretable text.
André Marion *Institut d'optique théorique et appliquée* *Centre Universitaire d'Orsay* *Orsay, France*	**Method:** Fast Fourier transform and inverse FFT, subtracted to enhance signal to noise, non-linear lookup table. **Conclusion:** No attempt to read the text. Noise remains problematic as the noise frequencies are on the same order as the text.
Hong Yan *Dept. of Computer Engineering and Information Technology* *City University of Hong Kong* *Hong Kong*	**Method**: Homomorphic filtering, wavelet-based algorithms, deblurring, and Gaussian shape PDF. **Conclusion**: No clearly interpretable text.

office from the military communications center. Greenwood argued that the phrasing in the memo was important. Nearly all military teletype messages of the era did not use punctuation marks but rather wrote them out as "CMA" (comma) and PD (period). He wrote, "The most significant difference is that while newspapers used civilian time formats (AM, PM), the military used 'Zulu,' or universal 24-hour time for their endings."

In 2009 Greenwood began another examination of the Ramey Memo. Once again, he was able to see "AT FORT WORTH, TEX." In the next line, he saw the term, "The 'DISC,'" which also agreed with the consensus. It was in the next line down that he made the important change. He noticed that the letters "GHT" seemed to stand out. Most of those attempting to read the memo interpreted this to be the end of "SOUGHT." Greenwood wrote:

> Having previously read clips in between pondering the photo [Ramey memo], I went back and flipped through it again. There was a press clip from the San Mateo CA *Times* of July 8th. Late edition papers for the 8th had carried the breaking Roswell debris news. Reading down the clip I saw this: "Lt. Warren Haught, public information officer at Roswell said . . ." And the quote continued to his press release. "HAUGHT" stood out like a sore thumb. It was a six-letter word with a "GHT" ending in an article related to Roswell . . . In the Ramey document [Greenwood's name for the memo], we don't see the word "Warren" clearly in the text. But . . . I've determined that the area before "HAUGHT" is a six-letter word and, based on the use of the word, "HAUGHT" in the press coverage, "WARREN" is the most likely fit in that area.

Greenwood's interpretation was not well-received by others who attempted to read the memo. His suggestion that it was a newspaper teletype was rejected by other researchers such as Brad Sparks. Sparks noted that a review of military messages from the era showed that, contrary to Greenwood, the use of periods and commas rather than abbreviations for them were sometimes found in military teletype messages.

Sparks speculated that the memo might be a "general to general" message, which is sometimes referred to as a "back channel." These would be more informal than official communications between commands and were often signed with the originating officer's name rather than the normal date/time group. The argument made by Sparks was that the memo was not a civilian teletype message brought to Ramey by *Star-Telegram* reporter Bond Johnson, but was, in fact, a military memo that referred to the events that had transpired outside of Roswell. All this demonstrates just how convoluted the attempts to read the memo have become.

PART 2: MOST RECENT ANALYSES

A New Approach

Research on the Ramey memo stagnated for a number of years. The scans used for attempted readings had not been redone as the technology improved, and no one had examined the negative in that time. Martin Dreyer, a New Zealand UFO researcher interested in the Ramey Memo, approached a number of experts in photographic enhancement to ask for advice. It was recommended to:

> . . . inspect and re-image the original film negative using a mix of modern analog & digital recording techniques using a digital biological microscope; high-resolution recording film and micro & macro lenses onto a modern digital camera sensor. I sought advice from Mr. J. Morelock in Memphis [TN] USA for his earlier pioneering research work & experience in the development of color microfilm.
>
> There [University of Texas at Arlington], with the assistance of Library Staff and under strict conditions of access and handling of the original film & print materials, work as described commenced on the 21st of April 2015. (Randle, 2016)

The aim of a direct inspection and re-recording of the negative was to:

- Establish physical condition of the negative/s
- Establish definition, resolution, and clarity of target
- Provide a viewing environment for direct reading of text
- Distinguish film base + Fog versus image density/s
- Define silver particles forming individual character-forms
- Identify silver particles (bleed) not forming individual character forms (font letters)—(to be sculpted away from character forms to enhance readability)
- Identify recurring characters among lines of text (aid to readability)
- Identify any "recurring flaws" or mechanical "signatures" among fonts (aid to integrity and readability)
- Determine which details are candidates for enhancement

The dual purpose for re-inspecting the negative was to estimate the extent or existence of sufficient information in the original to warrant further analysis, and if possible, to develop a methodology seeking to apply proven imaging practices to render better images of the text. In February 2020, this work was commissioned by a television production company. Gene Cooper of GIGAmarco, a California company, scanned the Ramey Memo negative using specially designed equipment. Over a period of several hours over two days, a series of photographs were made by stripping away the various levels on the negative, and then rebuilding it one level at the time. The rationale was that the noise that had been introduced to the negative by decades of handling could be eliminated and thus produce a clearer picture of the writing on the memo. To be sure, visual inspection of film negatives revealed signs of normal and robust handling in the form of (minor) chemical stains, dust particles, and scratches consistent with the age and handling to which these negatives had been subjected. What follows in the next section is an edited version of Schollum's (2015) report of the work involving the re-scanning (Figure 10) and analysis of the negatives.

EDITED VERSION OF SCHOLLUM (2015) REPORT

Observations and Conditions of the Photographic Negative

The densities of the emulsion layers appeared well 'fixed' and readable with no significant damage or degradation of the area of text (memo) which is the focus of the examination. Observable damage to the negatives is consistent both with their age and use prior to being preserved by the Special Collections Library. In particular, the time pressures and techniques of newspaper photographers often required less than optimal processing and drying times before being printed to meet short publication deadlines. Damage consistent with this practice was present. Exposure levels of the film recorded by flash were adequate and no subject or camera shake evident. The camera was well focused on critical parts of the scene and the 'memo' within the focus zone set by the photographer and diaphragm.

With these negatives in relatively good condition, well exposed, processed, and professionally preserved, the problem of whether the text can be read is one mainly of scale. The height and width of any font relative to the size and distribution of the silver halides on the film is the main determinant of whether individual letter forms can be identified and contribute to a full or partial reading of the memo. For purposes of illustration, the digital file dimensions for the full frame 4x5 negative are 3663.05 by 2743.05 pixels. In comparison, the message length is a mere 148.5 pixels wide. To image the memo in isolation, a Nikon SMZ1500 biological microscope ably operated by a talented graduate student at Arlington's School of Engineering was used to view and digitally record aspects of the 'memo' negative.

The negatives were then examined and recorded using the Special Collections digital microfiche system. David Rudiak with assistance of Library staff took a series of image recordings with bracketed exposures and raw and enhanced copies of these files. The original negatives were imaged using a Canon digital camera with both a macro and micro lens in Canon's proprietary format. I then recorded negatives using a Canon film camera with both a macro and micro lens onto ultra-high resolution Kodak recording film. Films have been sent to Wellington, New Zealand, for processing using Kodak proprietary software.

Method and Results

Images from microscope, microfiche, and digital camera were processed into groups of RAW and enhanced files. High Dynamic Range photography has been used to harness the range of tones present with negatives and in particular the Ramey memo. The products of HDR imaging have been processed into working files in the form of image stacks where the interaction among pixels among layers has been influenced variously to: (a) Reduce the visual interference of film grain within the emulsion impinging on the character forms (fonts); (b) Separate out the tones of the paper base from the fonts used in the memo to suppress background interference; and (c) To isolate and ('lift') tonal values of the fonts away from the background in order to render character forms more clearly. The end result provides an image with a resolution of over 65,000 pixels per inch (less than 1 micron resolution) and reveals the individual silver halide crystals (film grains) contained in the negative that make up the photograph. Each grain is roughly 3–10 microns in size. The resulting files provided a range of image states ranging from low contrast grey tones to contrasty separated tones for interpretive evaluation (note that pixel destructive approaches using curves or levels has [sic] not been used). Direct examination of the negative rather than viewing positive gen-

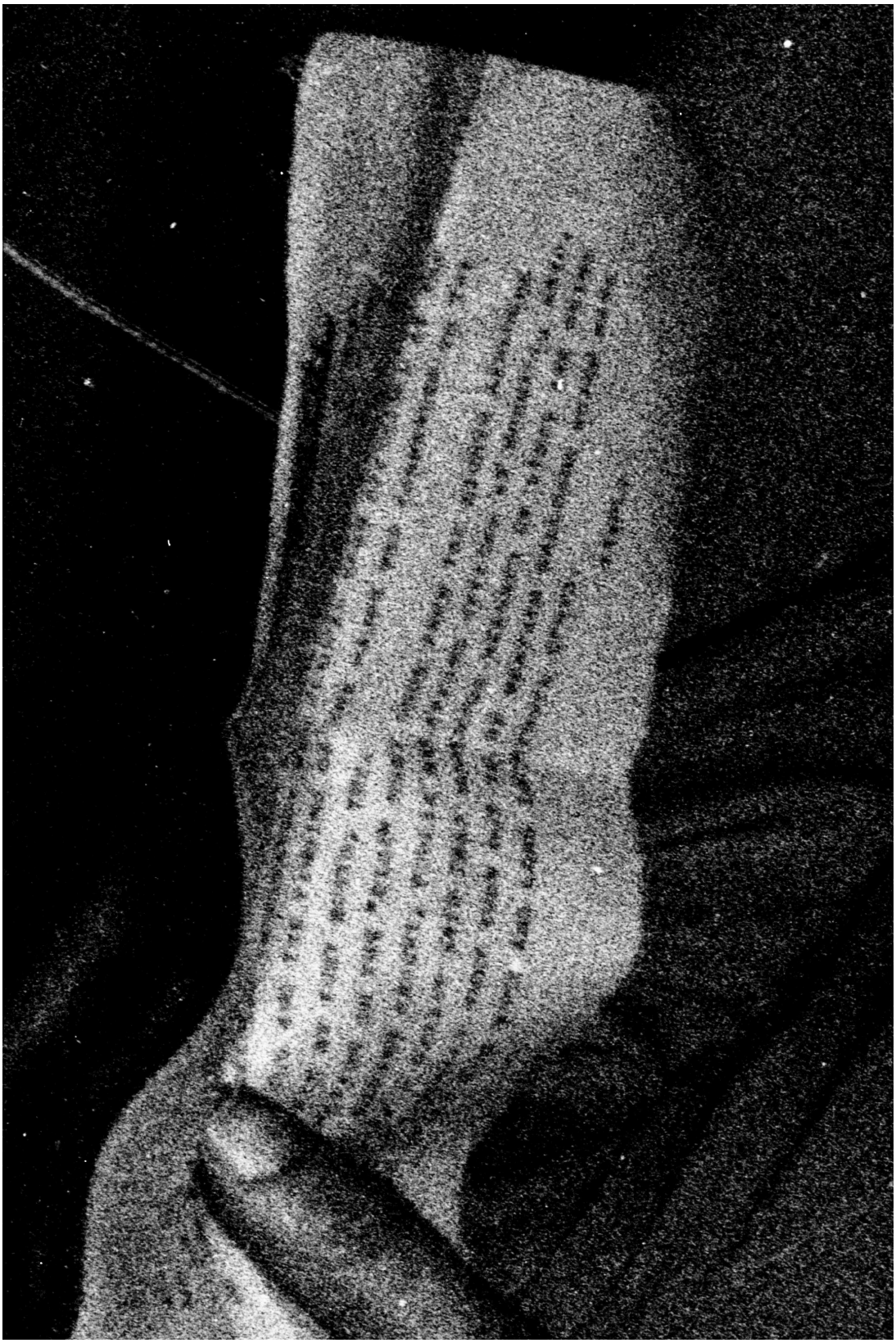

Figure 10. A high-resolution scan of "The Ramey Memo" obtained during the latest attempt to resolve the text on the document.

erations has allowed a clearer picture of the grain structure. No further increase in visual readability can be achieved. Any additional interpretation of the target message will more than likely be left to the application of adequate search algorithms to differentiate between the type fonts and message background.

Major Take-Aways

Although the process is ongoing, and new technology was applied to clarify the text, the results were disappointing. The image seemed slightly clearer, but the difference was insufficient to make any definitive statement about the memo's contents, including whether it was of military or civilian nature, or if it referenced an ETH-related event in New Mexico. The memo itself is not legible with any reasonable degree of certainty, so we are left with various interpretations of the memo that possibly suggest something extraordinary, but without the proof that it was. At this point, all that can be said is that the testing will continue with the hope that improvements to the imaging software will advance to the point where the memo can be deciphered reliably and validly. However, it must be noted that those who have conducted the latest scans think that physically scanning the negative—regardless of the quality of the equipment and the innovations in the technology—still will not settle the issue. The next step is thus more likely to be the use of artificial intelligence (AI) or machine learning (ML) methods to discriminate between the remaining noise that conceals the lettering and the actual letters, assuming that sufficient data for this approach can be obtained. As explained by Cooper (2022), next steps could include the following methodology:

1. Photograph with the same lens and film a set of known words/letters at the same distance, lighting, etc., and in a variety of suspected fonts and type styles. This would create a set of content for an examiner to use or for an AI program to use to build a baseline set of knowledge.

2. Then using that known set of data, we can use AI to read the letter/words on the memo.

3. As an example, if in one set of baseline content we know what an "A" looks like with the same lens, distance, angle, film, etc., then we can compare that to the Ramey memo and use it to crack the code, so to speak. While AI could help, it may not be entirely necessary for this particular method; this on its own might do the trick.

4. You could then take it one step further and feed the AI a set of words and vocabulary that we suspect may be in the memo, such as "Fort Worth," etc. This, however, could bias the results toward a particular outcome.

PART 2: CONCLUSIONS AND CONSIDERATIONS

At this point, given the quality of the scans that have been made and the expertise of those conducting the research, there is no more that can be done. Additional scans arguably will not bring a resolution, and the interpretations of various researchers have reached an impasse. The problem is that the film negative simply did not record the memo text data in a fashion that allows it to be understood. AI and ML techniques could potentially remove some or all of the Ramey Memo's ambiguity to give a dispassionate decryption, but this effort also yields merely an "interpretation" that likewise might suffer from the same inadequacies of the earlier attempts to read the memo using various methodologies. Without technological breakthroughs, there is low probability that the Ramey Memo will reveal anything further, much less conclusive information.

Some words seemingly can be read with near-universal agreement, which suggests that the memo references the Roswell crash debris. Yet, there is no such consensus about the key phrases. Without additional information or a clarification of what the memo says, it does not prove that what crashed had an alien origin. By the same token, there is nothing in the memo—as currently interpreted—that excludes an ETH explanation. The best that can be said is that *the document seemingly shows an interest in the debris recovered at Roswell without identifying that debris*. However, the information, documentation, and testimony about Project Mogul does not provide the ultimate answer. Dr. Crary's field notes indicate that Flight No. 4 was cancelled, and if it did not fly, then there was nothing from the Mogul team that accounts for the debris found by Mack Brazel. Skeptical arguments that a cluster of balloons flown later in the day was the real culprit likewise fail. The best information available suggests that *there were no Rawin radar targets attached to that cluster and that the cluster never left the confines of the various government ranges around Alamogordo Army Air Field*.

GENERAL DISCUSSION

Critical analysis of two key pieces of empirical information about the Roswell Incident shows that the physical debris found by Mack Brazel in 1947 is neither convincingly explained by Project Mogul nor obviously attributable to an ETH origin. As a result, this case should be character-

ized as "unsolved." But this does not mean that important insights and learnings have not come from the diligent work of independent investigators (cf. Table 1). To date, much of the information on the case has come from witnesses whose accounts certainly can be skewed through biased agendas or inaccurate memories. Although there are a few statements made on the record in the newspapers of 1947, those statements were mainly limited to the military officers involved in some way, either collecting the debris found near Corona, New Mexico, or identifying the balloon remains displayed in General Ramey's office. It wasn't until Jesse Marcel, Sr., went on the record in 1978 that the case began to expand to the point where we find it today. The ages of the witnesses in 1947 ranged from Jesse Marcel, Jr., who was eleven, and Frankie Rowe, who was twelve, to adults in their twenties, senior officers who were older, civilians such as Bill Brazel, Jr., who was twenty-two, and older ranchers and other civilians. The oldest of those interviewed was William Curry Holden, who was 96 when interviewed in 1992. The crucial point is that some of this testimony was gathered more than thirty years after the event, some of it much later than that.

Following the flow diagram in Figure 7, we find that the available empirical evidence does not converge on a clear solution. It is therefore "troubling or telling" that no official documentation or tangible data either exists or has been shared publicly that categorically explains the Roswell debris. It also seems incredulous that Weaver and McAndrew's (1994) investigation arguably failed to match the diligence and outcomes by ufologists into details of Project Mogul. Still, the empirical evidence yields six key findings for which any comprehensive and valid explanation of the Roswell Incident arguably must account:

- The "UFO" debris was unexpected by the local residents and military personnel.
- A staged press conference was needed to begin controlling the official narrative.
- The official narrative has changed three times over a period of 48 years.
- Project Mogul is now discredited as a viable explanation.
- The Ramey Memo is controversial and ambiguous evidence, but it likely affirms the military's interest in the debris without identifying its source or nature.
- To date, no known evidence conclusively resolves the case in terms of conventional technology vs. an ETH interpretation.

A grounded theory approach to these facts leads the author to two simple and unequivocal conclusions: (a) *The source or nature of the Roswell debris was something truly unusual or anomalous*, and (b) *The source or nature of the debris had meaningful ramifications for the military at that time*. The continued lack of transparency or resolution in this case might further suggest that those implications remain to this day. Pinpointing Project Mogul as the military's final answer (Weaver & McAndrew, 1994) implies that the Roswell debris was both US in origin and conventional in nature. Accordingly, two alternative scenarios are introduced by the discrediting of the Project Mogul hypothesis, i.e., the debris instead was either (a) conventional material or technology from a non-US source, or (b) unconventional technology or material of extraterrestrial origin.

Future investigations of these competing views are clearly warranted. Approaches can include searching for previously unknown or unexamined government or civilian documentation about any and all aspects of the event, as well as applying the latest qualitative and quantitative methods and cross-disciplinary efforts to existing evidence with the aim of extracting new insights or information. This case has been traditionally explored by maverick researchers operating individually, but new and significant advancements are perhaps more likely to come from adopting the model of scientific UFO panels that pool resources and apply cross-disciplinary expertise to targeted problems (e.g., Condon & Gillmor, 1968; Clemence, 1969; Kuettner et al., 1970; Sturrock et al., 1998). This tactic has been used to an extent on a smaller scale for Roswell-related research (e.g., Eberhart, 1991; Houran, 2005; Houran & Porter, 1998, 1999; Schollum, 2015), so it might be successful if expanded and supported with adequate resources. An ETH explanation of the Roswell Incident might nevertheless prove incorrect, but research suggests that no sociocultural crisis or collapse would ensue if extraterrestrial intelligence was confirmed (Alexander, 1994; Levin, 2012; Peters, 2011; Peters & Froehlig, 2008). Many people in the general population already believe in the existence of advanced extraterrestrial civilizations (Silva & Woody, 2022), and academics agree that the potential for learnings in this context would be enormous and transformative on both scientific and existential levels (for discussions, see Andresen & Chon Torres, 2022).

IMPLICATIONS AND APPLICATIONS

Social scientists from various sub-fields might identify and study those who support ETH interpretations of the Roswell "UFO" debris as examples of irrational or quasi-delusional beliefs, conspiracy theories, or ostension (i.e., the acting out of a legend narrative in real life). However, irrational beliefs (including conspiratorial thinking) often occur in healthy people due to improper or biased consideration of information or evidence (Pytlik et al., 2020; Ross et al., 2017; van Elk, 2015). This could likewise describe mainstream researchers who merely argue

from authority (Westrum, 1977) or otherwise uncritically accept problematic government or media narratives. Accordingly, the Roswell Incident might more accurately denote an example of "negative (or rejecting) gaslighting" on a coordinated and mass scale (Drinkwater et al., 2019, pp. 151–152). This term denotes the intentional act of using social conflict or identity forces in an attempt to normalize or demystify a genuinely anomalous event or experience of a witness or observer. Studying the military/government and media's reactions (both private and public) to the Roswell debris could perhaps serve as corresponding case studies in myth-making and narrative reality, especially as these processes relate to misinformation, disinformation, and so-called "fake news." In fact, the Roswell event might be a prime example of classic government disinformation, of which dozens of documented examples are known to exist (e.g., Baker et al., 2005; Hanyok, 2001; Wolf, 2001).

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BRIEF REPORT

Metarepresentations of Supernatural Belief and the Effect of Context on Cognitions

Malcolm B. Schofield

David Sheffield

Ian S. Baker

University of Derby

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HIGHLIGHTS

There is a difference between what people subconsciously 'believe' and what they publicly claim to believe. New evidence suggests that these subconscious beliefs can markedly influence how a person thinks.

ABSTRACT

This study aimed to see if context in the form of priming can alter a participant's thinking style based on their level of implicit association with either a religious or paranormal belief. This was based on the theory of alief, when a person's explicit belief and behavior are mismatched. This was also linked to dual process theory, with alief being analogous to type one thinking styles (fast and automatic). One hundred and seventy-two participants were recruited from the University of Derby and social media. Implicit association was measured using a modified Brief Implicit Association Test that looked at paranormal and religious belief. Explicit supernatural belief, cognitive reflection, metacognition, and confidence also were measured. A hierarchical multiple regression revealed that a lower belief in the supernatural (apart from psychokinesis), a religious prime, and high confidence predicted reflective thinking. Common paranormal perceptions, religious prime, and confidence were significant predictors in the model. It was concluded that the prime worked on a moral level and influenced someone with an already open mind to different beliefs to be more analytical, positive, and confident. This study does not support the theory of alief; however, it indicates certain beliefs are susceptible to a certain prime, and that a person can be influenced to be more analytical.

INTRODUCTION

The link between supernatural belief and intuitive thinking has been demonstrated in many studies (Schofield et al., 2020); however, the findings of these studies have been mixed. Some researchers have linked these mixed findings to different types of belief (such as religious or paranormal) having different thinking styles (Schofield et al., 2020). These studies generally measure explicit belief, what a person *says* they believe, rather than a measure of implicit belief, what a person *actually* believes. A priming paradigm, avoiding conscious intention, could be used to demonstrate whether these potential implicit beliefs have an impact on cognition. Priming effects have been examined for both religiosity (e.g., Bloom & Arikan, 2013; Shariff et al., 2008), and paranormality (Nees & Phillips, 2015; Pizzagalli et al., 2000). Bloom and Arikan (2013) demonstrated that priming can trigger specific types of implicit belief. Shariff et al. (2015) concluded that priming does not affect non-religious participants reliably and it might depend on a different style of activation for this group, indicating that a cognitive prime could be used to elicit a belief within a particular context. However, analytic thinking does increase religious disbelief (Gervais & Norenzayan, 2012). In addition, Nees and Phillips (2015) used priming in a paranormal context when looking at Electric Voice Phenomena (EVP) and found a clear indication that priming and context affect perception within a paranormal framework. Measuring explicit belief, implicit belief, and examining the effect a prime has on thinking style for different types of belief,

alongside metacognition (thinking about thinking), to measure how a person monitors their own thought processes, will provide a model of how what we believe can affect the way we think in certain situations. The nature of implicit belief is potentially tricky to conceptualize; one way this has been done, is to draw a distinction between explicit belief and the more implicit 'alief'.

The category of 'alief' arose from a belief vs. behaviour mismatch and the perceived discrepancy between explicit belief and behaviour (Gendler, 2008b). Alief is an intuitive reaction to a given situation that is based on a particular cognitive state. This state can be activated by either internal or external stimuli. The example that Gendler (2008b) uses is a person who is afraid to walk across a glass-bottomed bridge over a canyon, or a person who gets scared by a horror film, even though they know they are in no danger. These are however arguably rational fears for safety; it is not unreasonable to be afraid of falling from a great height. The repetitive behaviors that an OCD sufferer may engage in are also proposed to be an 'alief'; the repetitive actions not being 'believed' to really prevent a bad thing from happening. This could also be linked to negative metacognition (Zawidzki, 2019) or worrisome thoughts, and cause a problem when belief and alief do not match (Gendler, 2008a). Regarding religious and paranormal beliefs, some of the discrepancies in the research could be due to 'aliefs', particularly if intuitive dualists are implicitly attracted to certain beliefs (Bloom, 2009; Hood et al., 2012). This distinction between belief and alief has also been studied in terms of a Type One (intuitive) and Type Two (reflective) dual process framework (Kriegel, 2012).

According to dual process theory, Type One processing is intuitive, automatic, unconscious, and implicit. Type Two is reflective, flexible, wide-ranging, and explicit, and therefore controllable (Kahneman, 2011). Kriegel (2012) considers the mental state of alief and belief to be analogous to the two states within Dual Process Theory. Alief is analogous to Type One and belief is analogous to Type Two. However, previously it is beliefs themselves that are driving Type One thinking. This could go part way to explaining the mixed findings surrounding thinking styles and different types of belief, and ultimately help to establish the direct influence between belief and cognition (Schofield et al, 2020). Alief could be part of the intuitive system and influence it, but not totally separate from belief, as alief is just one aspect of belief. This provides a starting point and a way to conceptualize alief and belief; seeing them as dichotomous may not be helpful, and Gendler (2008b) does state that alief is a precursor to belief development. While the connection between alief and belief and dual process might seem attractive (almost intuitively so), caution should be taken when creating a bifurcation of mental states for convenience sake. That said, implicit attitudes have been studied in the past and Gendler (2011) refers to the Implicit Association Test as being a possible measure of alief.

Previous studies researching implicit belief focused on the paranormal (Stieger & Hergovich, 2013), the religious (Bassett et al., 2005), the supernatural and skepticism (Lindeman & Svedholm-Häkkinen, 2016), or the relationship between religious and paranormal (Weeks et al., 2008). Stieger and Hergovich (2013) showed that there is no correlation between implicit paranormal belief (measured using an implicit association test) and explicit paranormal belief, indicating that this is a valid area of enquiry. Lindeman and Svedholm-Häkkinen (2016) found that skeptics were not affected by a manipulation in the context of religious belief, but skeptics who were not analytic thinkers could be open to biases toward supernatural belief given the right conditions. This study questions the assumption that everyone has implicit supernatural belief; however, skeptics in the appropriate conditions can be primed using religious stimuli. This demonstrates that even though skeptics may explicitly disbelieve in religion, they can be primed to react to it. This could be seen as an implicit belief or an alief. The relationship between implicit and explicit attitudes has been seen to be affected by metacognition (Cooley et al., 2015), therefore it is not unreasonable to suggest that implicit and explicit belief could also be affected by metacognition.

Metacognition is how a person monitors their own thoughts and is often referred to as 'thinking about thinking' (Flavell, 1979), and has been studied alongside dual process theory, in relation to confidence and monitoring cognition (Mata et al., 2013). Baker and Morrison (1998) found that maladaptive metacognitions like worrisome thoughts were related to higher levels of paranormal belief. Elements of metacognition are correlated with paranormal beliefs, for example, Cognitive Self-Consciousness and Uncontrollability and Danger, but only for women (Irwin et al., 2012). However, while direct studies linking metacognitions and paranormal belief are sparse, they have been studied in areas alongside paranormal belief, for example, superstition (Cartwright-Hatton & Wells, 1997), psychoticism (Reeder et al., 2010), and schizotypy (Stirling et al., 2007). However, there is a dearth of studies relating directly to religiosity and metacognition. It has been posited that metacognitions can be used to train alief (Zawidzki, 2019), indicating that those with good metacognitive skills should be able to control their aliefs more, and therefore not be prone to priming. Those who exhibit negative metacognitions should be more prone to priming and their explicit and implicit beliefs should not match.

A measure of metacognition would provide the link

between implicit and priming paradigms and provide further evidence surrounding different types of belief and how they affect cognition. There is evidence that belief has an effect on reasoning and thinking style, however the measure of the belief could be having an effect. Dual process theory appears to link to belief strongly, but the role of implicit belief or alief is unclear. A priming paradigm could offer insight into the mechanism behind this process and metacognition would also show the awareness of a particular holder of belief of their perceived limitations.

Hypotheses

It is hypothesised that negative priming, negative belief, negative metacognitions, positive confidence, and negative implicit belief, will predict a reflective thinking style.

METHODS

Participants

This study recruited participants (n = 172) via social media, and from students and staff at the University of Derby using opportunity sampling. Ages of the participants ranged from 18 to 67 (mean = 28.34, SD = 10.19). The gender of the participants was 48 (27.9 %) male, 121 (70.3 %) female, 2 preferred to self-describe (1.2%), and one (0.6 %) preferred not to answer. Participants' self-identified religious belief included: Muslim, Hindu, Christian, atheist, none, and Jewish. One hundred and fifty-seven identified as students. Three hundred and sixty-one people attempted the survey with 172 completing it, giving a completion rate of 47.65%. Ethics were obtained through the University of Derby Human Sciences Research Ethics Committee, reference number 07-1718-MSp.

Instruments/Measures

An online survey (Qualtrics) was used to host the study.

Initial Condition Manipulation. Priming was carried out in the following way: There were three different contexts: religious, paranormal, and neutral. The context was manipulated by asking the participant to rank 10 statements. The religious prime group were asked to 'Please read the following Ten Commandments and rank them in terms of their importance to you'. These included: 'You shall have no other gods before me' and 'You shall not make for yourself an idol.' The paranormal prime group were asked to 'Please read the following paranormal statements and rank them in terms of how much you think they are likely to exist'. The statements included: 'Some individuals are able to levitate (lift) objects through mental forces' and 'Black magic really exists'. Finally, the neutral prime group were asked to 'Please rank these statements from the most too the least believable'. These statements included: '"Rhythm" is the longest English word without a vowel' and 'There is a city called Rome on every continent'.

The measures used in this study were as follows: 'Belief in the Supernatural Scale' (Schofield et al., 2018) to measure belief, MCQ 30 (Wells & Cartwright-Hatton, 2004) to measure negative metacognition, Cognitive Reflection Test–Long (Primi et al., 2015) to measure thinking style, and the Brief Implicit Association Test (Sriram & Greenwald, 2009) to measure implicit association.

Belief in the Supernatural Scale (BitSS). The BitSS (Schofield et al., 2018) has 44 items with five subscales. These are: 'mental and psychic phenomena' (16 items), 'religious belief' (10 items), 'psychokinesis' (4 items), 'supernatural entities' (7 items) and 'common paranormal perceptions' (7 items). The scores range from 44 and 308 and is scored using a seven-point Likert scale (strongly disagree to strongly agree). One item (39) is reversed. Items include: 'There are individuals who are messengers of God', 'There is both a spiritual as well as a natural side to reality' and 'Some individuals are able to levitate (lift) objects through mental forces. The scale demonstrates excellent validity and reliability (α = .97) (Schofield et al., 2018).

Metacognition Questionnaire 30 (MCQ-30). Metacognition was measured using the MCQ-30 (Wells & Cartwright-Hatton, 2004), a 30-item questionnaire. The MCQ-30 measures a range of Metacognitive Domains. The subscales being: Cognitive Confidence; Positive Beliefs; Cognitive Self-Consciousness; Uncontrollability and Danger; and the Need to Control Thoughts. Each of these subscales has 5 items. A Likert Scale is used to measure the levels from 1 (do not agree) to 4 (agree very much); a minimum score of 30 and a maximum score of 120 can be scored by adding the response to each item. Statements on the questionnaire include: 'My worrying is dangerous for me'; 'I monitor my thoughts'; and 'I have a poor memory'. Calculating subscales is achieved by adding the subscale scores and dividing by the number of items in that subscale. The Cronbach's Alpha Scores to calculate reliability for the full scale are: α = .93 showing good to excellent internal reliability and good convergent validity are also noted.

Cognitive Reflection Test–Long (CRT-L). The CRT-L (Primi et al., 2015) has 6 items that measure effortful reasoning. The items can be classified in three ways: correct answers, an intuitive (heuristic) incorrect response, or a response that was neither correct nor intuitive. The response format is a four-option multiple choice; a correct answer, an incorrect 'heuristic' answer, and two incorrect answers. Example items include: 'A bat and a ball cost £1.10 in total. The bat costs £1.00 more than the ball. How much does the ball cost?' (Correct answer = 5 p; heuristic answer = 10 p),

'In a lake, there is a patch of lily pads. Every day, the patch doubles in size. If it takes 48 days for the patch to cover the entire lake, how long would it take for the patch to cover half of the lake?' (Correct answer = 47 days; heuristic answer = 24 days). For this study, the scores were calculated so a higher score indicates reflective thinking.

Confidence Scale for CRT-L. Confidence in each answer given for the CRT-L was measured using the question 'How confident are you that you gave the correct answer? Please adjust the slider below to between 0 (totally not sure) and 100 (totally sure)' (De Neys et al., 2011). The participant could then use a slider on the survey tool to select the appropriate number.

Brief Implicit Association Test (BIAT). Due to the exploratory nature of the use of the measure, the BIAT (Sriram & Greenwald, 2009) was opted for over the more lengthy IAT (Greenwald et al., 1998). Unlike the IAT, the BIAT focuses on 2 out of the 4 categories (religious, paranormal, positive, and negative). This will enable the measuring of implicit association between the religious and the paranormal. The test was split into blocks of trials, and within these tasks participants are asked to press a specific key when shown a particular combination of stimuli; for example, 'press the left key when the paranormal word is combined with a good word'. These stimuli are in 4 categories (religious, paranormal, positive, and negative), and each category has 4 items. The words used are as follows: Religious: God, Religion, Worship, Sacred; Paranormal: Paranormal, Psychic, Haunting, Poltergeist; Positive: Wonderful, Best, Superb, Excellent; Negative: Terrible, Awful, Worst, and Horrible. The test was run as follows: The first 2 blocks are practice blocks and the remaining 4 blocks of trials are experimental and recorded. Within the blocks, there are 12 trials in the practice block and 20 trials in the experimental block as per (Sriram & Greenwald, 2009). Item, trial, and block numbers are based on previous research. Scoring the BIAT is based on the *D* measure (Greenwald et al., 2003) which give each participant a score ranging from –2 to +2. A positive score indicates an implicit religious preference and a negative one a paranormal implicit preference. There are other methods of scoring the task, however, this is seen to be the most effective (Greenwald et al., 2003). If error rates exceeded 20%, the cases will be excluded (Greenwald & Farnham, 2000).

Procedure

After participants were recruited, they were emailed details of the study and a link to the online survey that was hosted at www.qualtrics.com and were then briefed on details of the study and presented with the rights to withdraw, anonymity, and confidentiality. They were asked if they agreed and wanted to participate. They were then asked to create a unique ID code. Demographic data was then taken: age, gender, religious belief, occupation, and education. Each participant was placed in one of three different context groups, religious, paranormal, or neutral that acted as the priming conditions. This context was reflected in the brief. The participant also completed a task relating to the context that acted as a prime. Participants were then presented with the scales in a randomized order, and the BIAT. After the study, participants were debriefed.

RESULTS

Descriptive Statistics

Descriptive statistics were examined to determine the normality of distribution of the data, initially for all the scores (see Table 1).

The data were examined for normality by eyeballing histograms and boxplots, *z* skewness and *z* kurtosis, and finally *z* scores for outliers. Histograms indicated possible skewness and kurtosis in BIAT, supernatural entities, psychokinesis, religious belief, and confidence, and identified possible outliers in BIAT, metacognition, psychokinesis, and confidence. *Z* skewness and *z* kurtosis were examined within the priming groups for cases that lay outside of –2.58 to +2.58 (samples sizes between 100 and 200) (Field, 2013). Confidence appeared skewed (–6.45), as did psychokinesis (5.38), and religious belief (3.65). Kurtosis was present in the variables confidence (3.73), reflective thinking (–2.75), supernatural entities (–3.09), and common paranormal perceptions (–2.89). *Z* scores indicated confidence (–3.79), psychokinesis (3.23), metacognition (3.09), and BIAT (–3.09) scores being slightly out of range of the threshold of –3 to 3 (–3.23 being the lowest), but confidence being the only variable of concern with 3 participants below –3 (–3.02 to –3.79). While skewness and kurtosis were present in some variables and outliers were detected, the issues were minor and deemed to be within the assumptions of the test, meeting univariate normality (Field, 2013).

Main Analysis. Hierarchical multiple regression was used to examine if the BitSS subscales, negative metacognitions, confidence, and the BIAT were good significant predictors of reflective thinking. Variance inflation factor (VIF) values (largest = 4.51) indicated multicollinearity was not an issue, although the Durbin-Watson value (2.05) indicated no issue regarding correlation of adjacent residuals. Cook's Distances < 1, indicating no problems with outliers.

The predictors were entered in the following order, the block included the subscales from the BitSS and the three priming group manipulations. This was dummy-coded due to there being three groups of categorical data of neutral (n = 60), paranormal (n = 57), and religious (n = 55), with the neutral group being used as a

TABLE 1. Correlations, Mean, and SDs of the BitSS (and Subscales), Confidence, CRTL (Reflective), Negative Metacognition, and the BIAT

	Reflective Thinking	BitSS Total	Mental and Psychic Phenomena	Religious Belief	Psychoki-nesis	Supernatu-ral Entities	Common Paranormal Perceptions	Confidence	Meta-cognition	BIAT
BitSS Total	**−0.274									
Mental and Psychic Phenomena	**-0.306	**0.902								
Religious Belief	-0.024	**0.686	**0.364							
Psychokinesis	**−0.201	**0.754	**0.721	**0.337						
Supernatural Entities	**-0.211	**0.892	**0.69	**0.739	**0.534					
Common Paranormal Perceptions	**−0.396	**0.744	**0.782	*0.145	**0.709	**0.546				
Confidence	**0.248	0.034	-0.057	**0.226	0.038	0.045	*−0.144			
Metacognition	0.011	−0.066	−0.006	*−0.172	−0.001	−0.117	0.09	−0.003		
BIAT	*0.143	−0.116	**−0.199	*0.161	**−0.228	−0.034	**_ 0.288	**0.208	*−0.159	
Mean	2.436	142.494	3.692	2.88	2.241	3.519	3.003	76.983	65.401	0.312
SD	1.676	56.44	1.544	1.753	1.397	1.73	1.366	18.492	15.738	0.44

** Correlation is significant at the 0.01 level (2-tailed). * Correlation is significant at the 0.05 level (2-tailed).

baseline measure in Model One; with both the religious and paranormal being entered as separate variables and coded as 1 (0 being coded for the rest). This method is in line with the approach recommended by Hayes (2018). The dummy coding method to handle multi-categorical antecedents also has been seen to be effective for issues surrounding multicollinearity and model fit (Eze Francis et al., 2018). The three following blocks were entered separately, as follows: first confidence in Model Two, then metacognition in Model Three, and finally implicit association in Model Four. The second block indicated a significant change to the *F* value (p = .019), and the final two blocks did not indicate a significant change to the *F* value (p = .464 and p = .893). Thus, Model Two appears to account for the maximum variance in predicting reflective thinking. The regression model using the subscales from the BitSS, the three priming groups of neutral, paranormal, and religious with the neutral group being used as a baseline measure, and confidence to predict reflective thinking was significant ($F(8, 163) = 5.803$, $p < .001$), with 22% of the variance in the outcome being explained by the predictors ($R^2 = .222$, adjusted $R^2 = .183$). For full details of all the models, see Table 2.

There were significant positive relationships between religious prime (β = .157, p = .049) and confidence (β = .174, p = .019), and reflective thinking. And a significant negative relationship between common paranormal perceptions and reflective thinking (β = −.409, p = .002). There were no significant relationships between mental and psychic phenomena (β = −.057, p = .677), religious belief (β = −.004, p = .974), psychokinesis (β = .133, p = .237), supernatural entities (β = −.022, p = .882), and the paranormal prime (β = .102, p = .208).

TABLE 2. Model Progressions for the Regressions

	β	*t*	*p*	*R*	R^2	Adjusted R^2	*F* change	Sig *F* Change
Model One				0.441	0.195	0.16	5.664	<0.001
Mental and Psychic Phenomena	−0.066	−0.474	0.636					
Religious Belief	0.052	0.429	0.668					
Psychokinesis	0.166	1.474	0.142					
Supernatural Entities	−0.047	−0.319	0.75					
Common Paranormal Perceptions	−0.446	−3.384	0.001					
Paranormal Prime	0.113	1.386	0.168					
Religious Prime	0.172	2.136	0.034					
Model Two				0.471	0.222	0.183	5.651	0.019
Mental and Psychic Phenomena	−0.057	−0.417	0.677					
Religious Belief	−0.004	−0.033	0.974					
Psychokinesis	0.133	1.186	0.237					
Supernatural Entities	−0.022	−0.149	0.882					
Common Paranormal Perceptions	−0.409	−3.128	0.002					
Paranormal Prime	0.102	1.263	0.208					
Religious Prime	0.157	1.98	0.049					
Confidence	0.174	2.377	0.019					
Model Three				0.474	0.224	0.181	0.54	0.464
Mental and Psychic Phenomena	−0.058	−0.423	0.673					
Religious Belief	0.001	0.004	0.997					
Psychokinesis	0.138	1.226	0.222					
Supernatural Entities	−0.014	−0.095	0.925					
Common Paranormal Perceptions	−0.423	−3.197	0.002					
Paranormal Prime	0.11	1.35	0.179					
Religious Prime	0.158	1.984	0.049					
Confidence	0.171	2.323	0.021					
Negative Metacognition	0.053	0.735	0.464					
Model Four				0.474	0.224	0.176	0.018	0.893
Mental and Psychic Phenomena	−0.058	−0.419	0.676					
Religious Belief	−0.002	−0.015	0.988					
Psychokinesis	0.14	1.229	0.221					
Supernatural Entities	−0.013	−0.091	0.927					
Common Paranormal Perceptions	−0.422	−3.169	0.002					
Paranormal Prime	0.109	1.33	0.185					
Religious Prime	0.158	1.973	0.05					
Confidence	0.169	2.271	0.024					
Negative Metacognition	0.054	0.743	0.459					
Implicit Association	0.01	0.135	0.893					

DISCUSSION

The findings of the study indicated the model that explained the most variance included the subscales of the BitSS, the priming condition and confidence, significantly predicting reflective thinking style. Metacognition and implicit association added little and were not included in the final model. The significant predictors were as follows: common paranormal perceptions negatively, religious prime positively, and confidence positively predicted reflective thinking. The non-significant predictors were in the following directions: mental and psychic phenomena negatively, religious belief negatively, psychokinesis positively, supernatural entities negatively, and the paranormal prime positively. Overall, this showed partial support for the hypotheses.

The model predicted that a lower belief in the supernatural (apart from psychokinesis), a priming of an 'alief,' and high confidence predicts reflective thinking. The supernatural prime had an effect; however, it was only the religious prime, and not the paranormal, partially supporting previous research that examined religious (Bloom & Arikan, 2013; Shariff et al., 2008) and paranormal primes (Nees & Phillips, 2015; Pizzagalli et al., 2000). Whether or not this activated an implicit belief remains to be seen, but the results of this study do not support this, showing no effect in the model of implicit association. The religious prime did have an effect and according to this model the activation was in people who did not believe in common paranormal phenomena. The religious prime had an effect on thinking style, but this was not in the direction predicted. This finding conflicts with previous research showing that reflective thinking can affect belief, with analytic thinking increasing religious disbelief (Gervais & Norenzayan, 2012).

This further indicates that when the prime is tried in the other direction, it has a different effect, with the religious prime increasing analytic thinking. Furthermore, only the religious prime was a significant predictor. Supernatural belief also had an effect on thinking style, again in line with previous research (Pennycook et al., 2012), and this time in the direction predicted, apart from psychokinesis, which positively predicted reflective thinking. But only common paranormal perception was a significant predictor, and as with previous research this was a negative predictor of reflective thinking. Negative metacognitions and implicit association did not add anything to the model, although they did positively correlate (toward implicit religious belief) for metacognition, this goes against previous research that associated it with thinking style (Mata et al., 2013), but this finding could be a consequence of using a scale that measures negative metacognitions. Taken together, essentially, the model indicates that a religious prime, a lack of belief in common paranormal perceptions, and being confident, predict a reflective thinking style.

The significance of the religious prime seems to indicate that it has activated an alief, providing support for this theory, but if the analogy between type one (aleif) and type two (belief) thinking (Kriegel, 2012) holds, then the prime should have negatively predicted reflective thinking. However, this was not the case. Furthermore, it should be noted at this point that due to the correlational nature of the study, it cannot be claimed that the prime activated the alief and only that the prime is correlated with the thinking style. Taking this one step further, it would appear that if you did not believe in common paranormal perceptions and were confident, along with the prime, that this would predict a reflective thinking style. Rather than offer support for alief, this does indicate that the religious prime isn't priming an underlying alief; it is priming something else. Furthermore, the lack of significance of both implicit association and metacognition further diminishes support for alief. Correlations do indicate that elements of implicit association and metacognition have a relationship with belief, implicit association (toward implicit religious belief) positively correlating with religious belief, indicating there was no implicit/explicit mismatch. Also, negative metacognitions positively correlated with religious belief, indicating possible intrusive thoughts. This warrants further exploration.

One issue could be the nature of the prime. The religious prime could have been more of a moral prime. The rule-based nature of the prime may also have been a confound with rule-based primes being successful in prompting analytic thinking. This could account for the use of the Ten Commandments being successful. Also, the metacognition measure focused on negative metacognitions; therefore, an alternate measure of general metacognition is needed. This research leveraged standard statistical approaches grounded in Classical Test Theory. However, some authorities have strongly urged the use of Modern Test Theory (MTT) methods for improved measurement and model-building, especially as the assessment of paranormal belief and subjective experiences is inherently prone to response biases and other psychometric pitfalls (e.g., Lange, 2017; Lange et al., 2019). Thus, future research might re-examine the independent and dependent variables considered here using MTT frameworks. Additionally, it could be useful to test the relational patterns revealed by our regression analyses with more advanced modeling techniques that can sometimes account for attenuated results from measurement error (e.g., path analysis or structural equation modeling). We also refer readers to Laythe et al.'s (2021, pp. 142–143) summary of even more advanced options rooted in machine learning. Also, the use of dummy coding

in this way collapses the scatterplot into two box plot-like distributions, where the line of best fit is the slope created by the difference between the two groups. This means it only functionally mimics an ANOVA means test. Therefore, using categorical variables as predictors should be treated with caution.

Conclusion

This study indicated that primes do have an effect on certain types of belief. However, the extent to which this influences cognition needs further investigation. The primes themselves need more scrutiny to make sure it is the implicit belief that is being primed rather than a moral code being primed. While the nature of implicit belief is still unclear, belief does have an influence on thinking style. If this relationship can be reversed, and a type of implicit belief can be elicited by a prime of a certain thinking style remains to be seen. This study does not support the theory of alief; however, it indicates certain beliefs are susceptible to a certain prime, and that a person can be influenced to be more reflective.

IMPLICATIONS AND APPLICATIONS

This study implies that priming influences cognition. Furthermore, this could mean that primes could impact other beliefs. It would also be interesting to see if these findings can inform the study of paranormal or religious experiences and if priming is linked to experience and belief and to investigate the nature of this relationship. It would be interesting to see if priming can be used for beliefs such as conspiracy beliefs. Particularly appropriate given the current antivax and COVID situation. These methods could be instrumental in the fight against misinformation, disinformation, and so-called 'fake news'. While the alief question remains unanswered, more work in this area is needed to identify if there is a process occurring at an implicit level.

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ESSAY

Corona Discharges on von Reichenbach's Terrellae?

Marinus Anthony van der Sluijs

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HIGHLIGHTS

Light formations unlike known polar aurora phenomena were reported during classic experiments with a miniature model that simulated the earth's magnetic and electric properties. The nature of these observed anomalies remains intriguing and elusive.

ABSTRACT

In the 1840s, extensive experimentation led von Reichenbach to postulate an "Odic force" associated with "Odic light" or "magnet light," of which the polar aurora would be an example in space. The physical nature of the visible phenomena reported by his assistants during the experiments has never been satisfactorily identified. It is argued that "Od" compares to plasma or ionised gas, while at least a subset of the investigations, conducted on terrellae, represented the first experimental work on corona discharges. Accordingly, the glows on the terrellae cannot be directly compared to the aurora.

OD

The German baron Carl Ludwig von Reichenbach (1788–1869) was an eccentric naturalist, geologist, chemist, metallurgist, philosopher, and industrialist who embarked on an extensive study of disorders of the human nervous system upon his retirement from industry. Taking certain people diagnosed as "sensitives" into a completely dark room with magnets, magnetic devices, or crystals and allowing them ample time to adjust to the lack of light, he relied on their testimony in his theory of "a peculiar force in nature, which spans the whole universe . . . different from all hitherto known forces, here designated by the word 'Od'" (von Reichenbach, 1849a, p. 210; 1851, p. 221).[1] Fancifully named after the native Scandinavian god Odin (von Reichenbach, 1852, p. 198; 1860, p. 84), this "Odic force" was supposed to be tied to but nevertheless distinct from magnetism and electricity. It would manifest in "Od light," in five formal categories: glow-like, flame-like, thread-, fibre-, and fluff-like, smoke-like, and spark-like (von Reichenbach, 1849b, p. 53, cf. 435–436; 1851, p. 270, cf. 223).[2]

Von Reichenbach earned his reputation as an oddball even in his own lifetime primarily by his understanding

Carl Ludwig von Reichenbach

of Od as a form of "life energy" that produced the radiant wraiths of the newly dead and what some would now call "auras." The concept was similar to the *prāṇa* of Indian and the *qì* of Chinese metaphysics—though with an emphasis on electric and magnetic aspects, not on air and breath. In this respect, it resembled Franz Anton Mesmer's theory of "animal magnetism," which was beginning to fall out of fashion at the time (Alvarado, 2009, pp. 366–368, 375). Od can also be compared to younger vitalistic hypotheses such as Henri Bergson's *élan vital* and Wilhelm Reich's orgone. Quite different from the orthodox repertoire of intellectually acceptable notions at the time, such esoteric connotations, together with the claim that only "sensitives" were capable of perceiving Od, explain why the subject has always remained so odious to scientists.

OD AS PLASMA

The "sorcerer of Cobenzl"—as von Reichenbach was nicknamed, after his castle on the outskirts of Vienna—was far from incompetent or scientifically illiterate, however; he composed unsensational reports more sedulous than credulous. In hindsight, some of his observations on the Odic influence on living organisms prefigure the findings of Robert Otto Becker (1923–2008) and other bioelectromagnetists by more than a century; though many of these were dubious, they were not unscientific in essence. What von Reichenbach called "magnet light" (*Magnetlicht*) was in effect a successor to Edmond Halley's notion of "magnetic effluvia" rendered visible in the polar aurora (Halley, 1716, pp. 421–423; cf. Briggs, 1967, pp. 492–493; Hansteen, 1827, p. 340). Yet whereas Halley had only theorised its existence in analogy to electric glows seen in laboratories, von Reichenbach claimed to have observational evidence for it. Leaving the broader mystery of its physical nature for others to solve, a subset of von Reichenbach's experimentation undertaken in the years 1844–1847 appears to simply represent early unwitting work on self-sustaining gaseous discharges not powered by electrostatic friction. This can be gathered from the inclusion in many of the experiments of an armature or electromagnet and rarefaction by means of an air-pump:

> *The Odic light appearances of the magnet change under varied air pressure. They gain in strength considerably upon rarefaction of the air* (von Reichenbach, 1849b, p. 162 [cf. 159–161, 168, 172, 231–235]; 1851, p. 381 [cf. 378–380, 382, 386, 390, 442–444]).
>
> More beautiful, though, and more distinctly marked was the appearance on *electromagnets*. Here I was able to . . . heighten the appearances more and make them more clearly perceptible . . . (von Reichenbach, 1849b, p. 175 [cf. 176–178, 212–214, 222–223]; 1851, p. 394 [cf. 395–396, 426–428, 435]).[3]

Because the "magnet light" intensified with vacuum, it was arguably—at least in such cases—a glow or corona discharge produced before scientists comprehended that electric discharges can be sustained by direct current (DC). In the 1830s, the English scientist Michael Faraday (1791–1867) had revived the study of glow discharges and, inspired by Ørsted and Ampère, laid much of the groundwork for that of electromagnetism, proving the fundamental identity of static electricity, Voltaic electricity or electric current, electricity induced by a magnet, and animal electricity (Faraday, 1833; 1839, pp. 76–109). However, for years this work had failed to ignite interest (Hiebert, 1995, pp. 95–97). The faintness of the glows reported by von Reichenbach's volunteers must variously have been due to weakness of the electric component, wavelengths at the boundary of the visible spectrum, or a low degree of vacuum—close to normal air.[4]

O'Byrne (1926, pp. 110–117), a translator of von Reichenbach's work, cited visible discharges between a cathode and an anode in a vacuum tube as a method that might bring the effects of Odic force within the ken of "non-sensitives," but he insisted that Od, the electrified matter, had to be something different from electricity alone. The electron, electrical conductivity by ions, and ionisation of matter remained unknown during von Reichenbach's lifetime. Glow and corona discharges are electrical discharges that feature a visible plasma, that is, a visible partly ionised gas. "Od" though it may sound, von Reichenbach's imponderable force may actually be a state of matter that equates to plasma—equally unknown at the time, with the single exception of the "radiant matter" hypothesised by Faraday in 1816 (Jones, 1870, pp. 195–196, 268–270; cf. van der Sluijs, 2019, pp. 87–88; 2011, p. 663). Despite his Mesmerist proclivities and mystifying vocabulary, von Reichenbach would, accordingly, qualify as a respectable pioneer of plasma physics—and of plasma cosmology.

VON REICHENBACH'S WORK ON TERRELLAE

Von Reichenbach (1849b, pp. 234–239; 1851, pp. 445–448) postulated that the earth's magnetic field is generated by iron diffused through the earth's interior but is continually modified by "accessions" from the sun and moon. Specifically, he believed the iron to subsist in a cool, crystallised state, emitting Odic light that caused

the magnetisation. Like Halley before him, he regarded the northern and southern lights, or aurorae, as visible outflow of the earth's magnetic field.

One series of experiments aimed at deciphering the riddle of the aurorae involved terrellae "after the manner of Barlow" (von Reichenbach, 1852, p. 172; 1860, p. 76; cf. 1849b, p. 217; 1851, p. 431), which the Maverick referred to by the French "terrelles" (von Reichenbach, 1849b, pp. 210–224, 234–240; 1851, pp. 425–437, 444–449).[5] A terrella ("little earth" or "earthlet") is a miniature model of the earth, mostly used to simulate the earth's magnetic and electric properties. Von Reichenbach worked in succession with two smooth hollow globes of sheet iron, each composed of two hemispheres tightly fitted together, which he suspended by a silken cord passing through a hole in the joint between the halves. Inside each sphere he placed an electromagnet in the form of an iron bar wound with silk-covered copper wire. This was positioned upright inside the terrella, its ends being in immediate contact with the sphere. The two extremities of the wire were connected to an external Voltaic zinc-and-silver battery through two little holes in the sphere (von Reichenbach, 1849b, pp. 211, 218; 1851, pp. 425, 431).

Several light formations purportedly seen on these terrellae, in all categories of "Odic light," are worth singling out. An all-encompassing shell of light around the first terrella constituted one type of formation. One attendant, Sophie Pauer *née* Streicher (1791–1861), beheld "a delicate grey misty gauze spread all over the ball's surface, which she discerned most distinctly in profile and which rose above the ball's surface to a height of a centimetre" (von Reichenbach, 1849b, p. 213; 1851, p. 427). Von Reichenbach (1849b, p. 217; 1851, p. 431; cf. 1852, p. 172; 1860, p. 75) called this "*a luminous vapour shell*, a kind of delicate *photosphere*, which surrounds it," consisting of "an opaque veil of light, which does not rest on the globe's surface, but is located at a little distance from it, and floats freely in the air above its surface, like a spherical shell." A second distinct structure, showing again on the first terrella, was a radiant girdle around the equator. Sophie Pauer described this as "a more luminous, narrow and whitish-yellow ring laid all around the equator" (1849b, p. 213; 1851, p. 427). Another viewer, Cæcilie Bauer (born circa 1819), reported that "the equator itself formed a narrow, somewhat lightened band all round the globe" (1849b, p. 213; 1851, p. 428). Josephine Zinkel (born circa 1822), too, "perceived the belt that followed the equator around the globe," "a luminous streak, which follows the greatest circumference horizontally around and thus divides the globe into two halves, in an upper and a lower one," which is to be "understood like a fine comb with countless very short teeth, which sit up at right angles and point to the poles" (1849b, p. 214; 1851, p. 428).

Finally, perhaps the most significant morphology is that of vertical filamented beams above the poles. "Above the globe, as well as below it," witness Josephine Fenzl saw "lights as thick as an arm streaming out of the polar points, which then, according to her own expression, spread like open parasols over the globe, concentric with it, both above and below, but at a little distance from it" (1849b, p. 212; 1851, p. 426). Josephine Zinkel had it that the light emerging from the poles "formed towards the equator a great star, with apparently innumerable points, or rather thread-like radiant prolongations, which ran down the globe in colours . . ." (1849b, p. 214; 1851, p. 428). She "likened the whole Od-flame to a loosely-bound sheaf of grain, which, standing upright on the ground, bent over its ears and stalks in curves on all sides, so that they lay apart horizontally upon one another over the bundle" (1849b, p. 217; 1851, p. 430). Other descriptions she used were "an overhanging wheat-sheaf" and "a tassel turned the wrong way upwards" (1849b, p. 219; 1851, p. 432). According to Cæcilie Bauer, the continuous "luminosity of the globe over its surface" resolved itself into

> innumerable clearly distinguishable filaments, which to her seemed . . . about one millimetre thick (knitting needle, she said), and ran from the blue patch of the upper pole and the red one of the lower perpendicularly towards the girdle . . . She described these filaments as not so much independent isolated streaks, as rather merely lines of greater intensity of light, alternating with lines of lower intensity of light, so as to give a streaked appearance to the whole as if nothing but threads ran down from the poles. They were all of the colour corresponding to the point of the compass toward which they were directed . . . (1849b, p. 214; 1851, p. 428)

This woman, too, used the simile of a star, noticing how the coloured patches at the poles "became subdivided, and graded into the filaments which ran downwards over the succeeding zones; this gave the polar patches a star-like appearance;" "apparent projections and hollows developed and so formed a kind of star shape to the eye" (1849b, p. 215; 1851, p. 429).

At a later time, when the second, much larger terrella was taken into use, observer Marie von Augustin *née* Regelsberg von Thurnberg (1807–1886) noticed

> at both poles short luminous columns flowing out as a kind of vapour, reddish at the positive pole and blue at the negative one . . . These columns or stalks of light, as she called them, spread out

> at the top and turned over. She compared them with the image afforded by a palm-tree, where the leaves, directed at right angles to the stem, stretch out and diverge on all sides. (1849b, p. 218; 1851, p. 432)

Wilhelmine Glaser (born circa 1821), a different witness on the same occasion, resorted to the metaphor of a tree as well, as she "perceived the entire globe to be streaked in colours, from top to bottom":

> The coloured streaks were about of a hand's breadth where they passed over the equator, and were separated from each other by an opaque, indistinct, transitional streak of the same breadth, in which the colours were blended together. She saw a mass of blue light above, which she also described as resembling a tree of which the stem ascended from the pole and which lowered its branches away from each other, overhanging on all sides. (1849b, pp. 218–219; 1851, p. 432)

Another visitor, Anka Hetmanek (born circa 1824), again "saw streams of light issuing above and below from the globe; that is, from both poles, which spread out on all sides in the manner of a tree" (1849b, p. 219; 1851, p. 432). The baron concluded with respect to these "coloured meridians" running "from pole to pole":

> . . . now for the Od-flame. One such flowed out from each pole, perpendicularly to the surface of the globe, 5 to 6 centimetres in height and 3 to 4 centimetres thick, but then *it expanded on top and on all sides bent down parallel to the globe's surface*, broke up and frayed, and at once flowed out parallel into the air in filaments of Od-flame . . . The streaks of this Od-flame did not remain at rest, but flickered and scintillated constantly backwards and forwards, shortened and lengthened, shot out radiantly . . . *flaming lights exist over the poles of the magnet* . . . this *flaming appearance appears mobile, undulating, frequently serpentine, like rolls of riband blown about by the wind*; often enlarging and shrinking itself, then shooting out rays, scintillating, variegated, also *vaporous* . . . (von Reichenbach, 1849b, pp. 218, 217, 232; 1851, pp. 432, 430, 443)

> On top at the place where the north-pointing pole of the electromagnet was located, a column of light tending towards the blue rose hand-high over the ball, then bent over in all directions, like an opened umbrella, and streamed down all around over the ball, at a distance of two to three inches from it. From the other pole, the south-pointing one below, a similar tuft of fire ascended all around over the ball in reddish light. Both frayed and faded out before they reached the ball's equator. (von Reichenbach, 1852, p. 172; 1860, pp. 75–76)

Thus, each of the two iron globes appeared to provide support for the conclusion that "the 'Northern-Lights' are positive od-lights" (1852, p. 173; 1860, p. 76; cf. 1849b, p. 240; 1851, p. 449):

> . . . so we recognise in it a kind of *terrelle, which exhibits artificial northern and southern lights in miniature . . . Their poles emit . . . delicate light visible only in the darkness of night. High above both poles it turns over and flows on all sides towards the tropical zones*, broken up in the way of filaments and rays on the great terrestrial globe just as on the little *terrelle* . . . (1849b, p. 234; 1851, p. 444; cf. 1845, pp. 5, 23–26; 1849a, pp. 5, 19–22; 1849b, pp. 210–211, 217, 231–233, 239–240; 1851, pp. 22–23, 39–41, 425, 431, 442–443, 449; 1852, pp. 171–173; 1860, pp. 75–76)

AURORAE OR CORONA DISCHARGES?

The various luminous emanations from von Reichenbach's terrellae were reported with remarkable consistency. From a modern perspective, possible correlates in the geomagnetic dipole field are readily imagined: a fully ionised sphere—or "ionosphere"—around the globe, a ring current around the magnetic equator, and the field lines above the auroral ovals that outline the hollow centre of the toroidal plasmasphere. The fine filamentary structure that the savant's companions observed in the funnels and even the equatorial belt is characteristic of plasma, as is well known from the field-aligned rays in the aurora (e.g., Peratt, 2015, pp. 2, 22, 26, 41, 46). Of the three basic types identified above, von Reichenbach himself associated only the polar funnels with the earth's aurora. In the mid-19th century, the existence of the ionosphere and the equatorial ring current was not yet suspected. Neither these two structures nor the greater parts of the polar funnels—everything above the familiar auroral ovals—are normally seen to glow visibly.

The similarities between von Reichenbach's results and the auroral reality in space are in fact deceptive. For one thing, the actual earth's auroral rings comprise a dayside and a nightside sector formed by different mechanisms. On

the dayside, charged particles from the solar wind flow in directly and without much acceleration through the polar cusps, producing the near-continuous but usually feeble or subvisual daytime aurora as they collide with nitrogen and oxygen atoms residing in the polar upper atmosphere on that side. By contrast, particles diverted along the magnetopause create the more characteristic intermittent aurorae associated with geomagnetic substorms and storms by being accelerated from the central plasma sheet in the magnetotail or the radiation belts towards the nightside of the polar upper atmosphere and colliding with the same types of atoms there, with much more vigour than on the dayside (Simmons, 1998, pp. 247–251, 255–256; Eather, 1980, pp. 218–230; Iijima & Potemra, 1976). Evocative though the structural analogy may be between the polar funnels on von Reichenbach's terrellae and those in real space, it should, therefore, not be pushed too far.

That aside, the glows on these globes were probably of a wholly different character than the true aurorae. The visible aurorae are discharges in glow and arc mode occurring in the highly rarefied air of the upper atmosphere. Von Reichenbach did not use the air-pump in combination with the terrellae, so that the light effects seen over them all must have taken place at atmospheric pressure. Unfortunately, he did not provide a circuit diagram or illustration of the setup in his book, complicating efforts to picture the experiments accurately or replicate them. They do not appear to have involved a pair of electrodes. Taking into account that the Volt unit was only introduced in 1861, it is also unclear what voltage von Reichenbach applied to the apparatus. All these uncertainties notwithstanding, a barely visible direct-current corona discharge ionising the air around a spherical conductive surface might be the correct interpretation of the "Od light" on the terrellae. Corona discharges, also called incomplete discharges, tend to occur at air pressure and focus on sharp points, as in the classic St. Elmo's fire, but can also spread out over a single hemispherical or spherical electrode (Riba et al., 2018; Giao & Jordan, 1968). Due to the weakness of the radiation, they can be descried only in darkness. Moreover, most of the radiation falls within the ultraviolet spectrum (Riba et al., 2018, p. 3). While St. Elmo's fire is easily seen by anyone, the air being ionised enough, corona discharges can usually be detected only by the comparatively few humans who are capable of seeing ultraviolet or near-ultraviolet light or are otherwise equipped with acute vision:

> The corona discharge emits radiation in the 280–405 nanometer (nm) spectral range, mostly in the ultraviolet (UV) range, and therefore is invisible to the human eye. However, relatively weak emission at about 400 nm might be observed at night under conditions of absolute darkness. (Chan et al., 2008, p. 7-5)

Typically reporting a whitish-violet light, people who can make out this feeble light will have been the type considered "sensitive" by von Reichenbach. Aphakia, which is the condition of lacking a lens, was probably not the cause of their ability to see ultraviolet light, as it is usually associated with impaired vision and old age.

The particular manifestations beheld by the "sensitives" as polar rays read most like the streamer mode and the pulseless glow mode, fixed at one point, of a negative corona discharge; these two stages convert into one another with a change in voltage (Giao & Jordan, 1968, pp. 1208–1209, 1213, 1215; cf. Riba et al., 2018, pp. 4–5) (Figure 1 and Figure 2). The pattern that von Reichenbach called a "photosphere" must be a type of positive corona discharge on a spherical electrode known as Hermstein's glow, "an ionized layer adhering to the electrode surface," which forms "when the density of the negative space charge becomes high enough to completely suppress the onset streamers." (Giao & Jordan, 1968, p. 1209, cf. 1210, Fig. 4, 1210–1212, 1214). The filamentary structures on von Reichenbach's terrellae would have traced the electric field lines followed by ions created in the discharge. This does not preclude the involvement of the magnetic dipole field produced by the electromagnet, by which von Reichenbach meant to simulate the geomagnetic dipole field. According to a recent study, "the effect of the magnetic field" supplied by a permanent magnet on a direct-current corona discharge "increases as the degree of vacuum increases" and "is the most significant with the negative corona discharges rather than with positive corona discharge" (Elabbas, 2014, p. 189, cf. 191–194). This might mean that the magnetic dipole field around von Reichenbach's terrellae was modestly influential on the discharge. The equatorial ring was apparently perpendicular to the junction of the two hemispheres from which each terrella was forged. Hence it could not have been an artefact of the material imperfection along this circumference and was probably the faint spontaneous product of the dipole field.

An electric field of the order of 100 kV/m is typically required to create discharges in air. Corona discharges occur around power lines and in the laboratory when electric potentials of the order of 100 kV are reached. It remains a *desideratum* to know whether or not von Reichenbach possessed equipment capable of generating such voltages or whether any of the experiments perchance took place during conditions conducive to thunderstorms.

Figure 1. Direct-current corona discharge of negative polarity on a spherical surface, showing a stage with few moving streamers in blue to purple colours (from Riba et al., 2018, p. 10, fig. 4a left).

Figure 2. Direct-current corona discharge of negative polarity on a spherical surface, showing a stage with an amalgam of moving streamers blurring into a broad glow in blue to purple colours (from Riba et al., 2018, p. 10, fig. 4b).

CONCLUSION

In sum, von Reichenbach's *magnum opus* on terrellae could prove to be the earliest known experimental work on corona discharges. It is difficult to be certain on this count, however, as long as some of the specifics of his setup remain unknown, notably the achieved voltages. And did his work foreshadow elements of modern geophysical theory? Absent the aid of a crystal ball to gaze directly into the past, the only way to find out may be to keep the ball rolling on a veritable Od-yssey of unprejudiced research in real and space laboratories alike.

ACKNOWLEDGMENTS

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NOTES

[1] In citations from von Reichenbach's texts, page numbers in the original German edition are given before the numbers in the contemporary English translation. All translations quoted in the text of this article are mine.

[2] The English edition from 1851 translated "Incandescence," "Flame," "Threads, streaks, and nebulæ," "Smoke," and "Sparks."

[3] The armature is mentioned passim.

[4] For recent progress in the production of atmospheric-pressure glow discharges (APGDs), see Wang et al., 2018.

[5] Nahm (2012), in his informative overview of von Reichenbach's dramatic life and work, made no mention of the terrella work or electric discharges.

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ESSAY / INTERVIEW

Expanding Parapsychology Research: Learnings from a Beneficent Funder

Helané Wahbeh

Institute of Noetic Sciences
Petaluma, California

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HIGHLIGHTS

Funding in frontier science is sparse, but some sponsors clearly invest in certain lines of research as the most promising for future discoveries or advancements. This knowledge can help to guide new and established investigators alike.

KEYWORDS

Parapsychology, edge science, funding, phenomenology, bias, Bial, collaboration

Parapsychology is a research field that has received much less funding than other areas of science. For example, more than $5 billion was spent on mental health research in the United States in 2018. Only about $2 million was spent on formal parapsychology research that year. The total human and financial resources devoted to parapsychology since 1882 is at best equivalent to the expenditures devoted to fewer than two months of research in conventional psychology in the United States (Utts, 1995).

Charles Tart conducted the last known formal survey of psi funding in 1978 (Tart, 1978). He polled individuals at 14 identifiable parapsychological research laboratories in the United States. The individuals had to be full members of the Parapsychological Association who had been actively working and publishing in the field for at least five years and have at least one part-time colleague to qualify as a laboratory or center. He found that the total funds available per year for all respondents was $552,000 for the last five years. Tart reports, "While the mean level of year funding was $42,500, the median level was $17,000." That is, half the active research laboratories in the United States had yearly research budgets of less than $17,000. Annual budget funding estimates were $750,000 for the Mobius project (1979–1980) and SRI (formerly Stanford Research Institute) during the same period.

An informal discussion of current funding amongst parapsychology colleagues revealed varied funding levels. Smaller academic and nonprofit centers reported about $40,000 to $50,000 in annual funding, while larger nonprofits noted annual research budgets of about $500,000. Many also do not feel comfortable sharing information about their budgets, especially if the information is not already within the public domain. As far as I know, there has been no formal survey of parapsychology funding since Tart's survey. Despite the lack of specific total numbers for current funding, the amounts are much lower than those established for other areas of psychology.

This lack of financial support is critical. Resources for salaries, project expenses, and support staff are difficult to procure and maintain. Funding limitations also reduce the number of people entering the field, even though they may be interested in doing so. It stifles the whole field from expanding. I would even go so far as to say the limited resources can foster competition and a scarcity mentality amongst parapsychology researchers. That is, there is a general sense that there is not enough to go around, and researchers must fight for their own funding. While this is not unique to the parapsychology field, I feel these factors

can only harm the greater efforts of our community.

Why is there such a massive difference in funding between psychology and parapsychology? This is likely self-evident to this audience. Edge science research is still taboo within academia, and funding bodies are few and far between. While stigma remains around mental health disorders, the topic is not so taboo that discussions are blocked and funding is not available. On the contrary, we see billions of dollars funding the effort. In fact, $3.9 billion was allocated to the Substance Abuse and Mental Health Services Administration, and $1.2 billion to the National Institute of Mental Health in the United States in 2018. This robust funding exists partly because mental health issues do not challenge the materialistic worldview in the way that parapsychology does. Because despite evidence that the phenomena parapsychologists study are common, parapsychology research is still sidelined and seen as taboo (Cardeña, 2015). In my opinion, edge science research is just as critical and worthy as mental health research with as strong an impact on humanity.

So, the natural question is: How do we generate more funds to support our research efforts? Perhaps we can learn more about this from one of the largest foundational funders of parapsychology: Fundação Bial, or in English the Bial Foundation. The Bial Foundation was founded in 1994 by Bial, the leading Portuguese pharmaceutical company. The Bial Foundation was created as an independent nonprofit organization to promote scientific development. In 1996, the Bial Foundation began to organize the symposia—an event that brings together the international scientific elite in neuroscience and parapsychology.

Since the Grants for Scientific Research's establishment until the last 2020 Grants' edition, the Bial Foundation has supported 775 projects (452 from 2010–2020), supporting 1,624 researchers from 29 countries. About 50% of these grants involved parapsychology. The maximum funding level per grant was €50,000. One hundred and seventy-five projects are ongoing (Cordeiro et al., 2022).

Bial prides itself on the impact it has created with these grants in the dissemination of their results. These projects resulted in more than 2,000 papers that received approximately 35,000 citations. Of these, 1,606 were published in indexed journals (1,361 in journals with an average impact factor of 4.02, and 282 in journals with an impact factor >5).

I was able to interview the Bial Foundation Chairman Luís Portela to gain insight into parapsychology funding. It may seem sensational that the chairman of a pharmaceutical company is one of the greatest proponents of parapsychology research. How and why did he start funding the field? What are his thoughts on strengthening parapsychology research?

Bial Foundation Chair Dr. Luís Portela

INTERVIEW

Helané: Bial has a rich history. You assumed the presidency of Bial in 1979, and the Bial Foundation was created in 1994. Can you share how and why the Bial Foundation was created?

Luís: In my youth, I decided to become a doctor for two reasons: to enjoy being useful to others and desiring to contribute to humanity's spiritual enlightenment. I, therefore, intended to do research in neurosciences and parapsychology. However, my father passed away at the age of 50, when I was 21, and I felt the need to start working in the family's company—Bial pharmaceutical—while I was still finishing my medical degree.

When I finished my degree, I worked for three years at Porto's central hospital and was a professor of psychophysiology at the University of Porto for six years. At the age of 27, I won a scholarship to do my Ph.D. in psychophysiology at the University of Cambridge. But, given the admiration I had for my grandfather's work, the founder of the company, and my father, his successor, I chose to put aside my medical and research career to dedicate myself to the Bial company, of which I bought the majority of the shares. At that time, I promised myself that, when I had the means for it, I would create the conditions to support some researchers to do what I was giving up doing: research in neurosciences and parapsychology.

At the age of 32, in 1984, we were able to create the Bial Award, mainly to foster clinical research, which continues to this day. In 1994, we invited the Council of Rectors of Portuguese Universities to join the Bial company to cre-

ate an independent nonprofit institution of public utility that would manage the Bial Award and a Grants program for scientific research, which was accepted. Thus, the Bial Foundation was born and managed by representatives of Bial and the Council of Rectors of Portuguese Universities.

Helané: One of the primary research areas of the Bial Foundation funds is parapsychology. Can you share how and why you decided to include parapsychology as the primary funding focus?

Luís: At the time of the creation of the Bial Foundation, we assessed that, given the available funds, we would not be able to support, at least with some impact, research in the broad domain of health, so we decided, with regard to the grants, to focus on two areas. The representative of the Council of Rectors of the first administration of the Foundation, Professor Nuno Grande, suggested psychophysiology and parapsychology, which surprised me and led me to ask him the reason behind that choice. He said that psychophysiology was the field in which I would have done my Ph.D., and parapsychology was a field of interest of mine. Given that my company was going to fund the Foundation, the Council of Rectors deemed it fair to support the two fields of my interest. I was very touched by that and, obviously, gladly accepted.

Helané: How does the Bial Foundation set the criteria for its funding priorities? How are specific topics, theories, or research designs selected as high-priority vs. low-priority?

Luís: The activity of the Bial Foundation is focused on three programs: 1) three scientific awards, 2) scientific research grants, and 3) a symposium "Behind and Beyond the Brain" conducted every two years. The symposium provides a venue for the public presentation of our grantees' project results. It also gathers together neuroscientists, university researchers in the field of parapsychology, and other scientists (philosophers, mathematicians, physicists, etc.) to discuss the symposium's major theme. For example, this year's theme was the mystery of time.

The final responsibility always belongs to the Board of Directors. However, we have a Scientific Board, composed of 55 scientists from around the world and presided over by the neuroscientist António Damásio, which scientifically supports our offereings. For example, concerning the grants, when it comes to selecting the projects for funding, from among the hundreds of applications, the assessment is, indeed, made by the Scientific Board. As long as the projects fall within psychophysiology or parapsychology, they are selected for their quality. Since 1994, the Bial Foundation has funded 775 projects of scientific research, of which nearly half are in the field of psychophysiology, and the other half are related to parapsychology, be it on its own or in conjunction with neuroscience. These projects involved more than 1,600 researchers from 29 countries.

Helané: How should researchers think about research hypotheses and designs when engaging with organizations like the Bial Foundation?

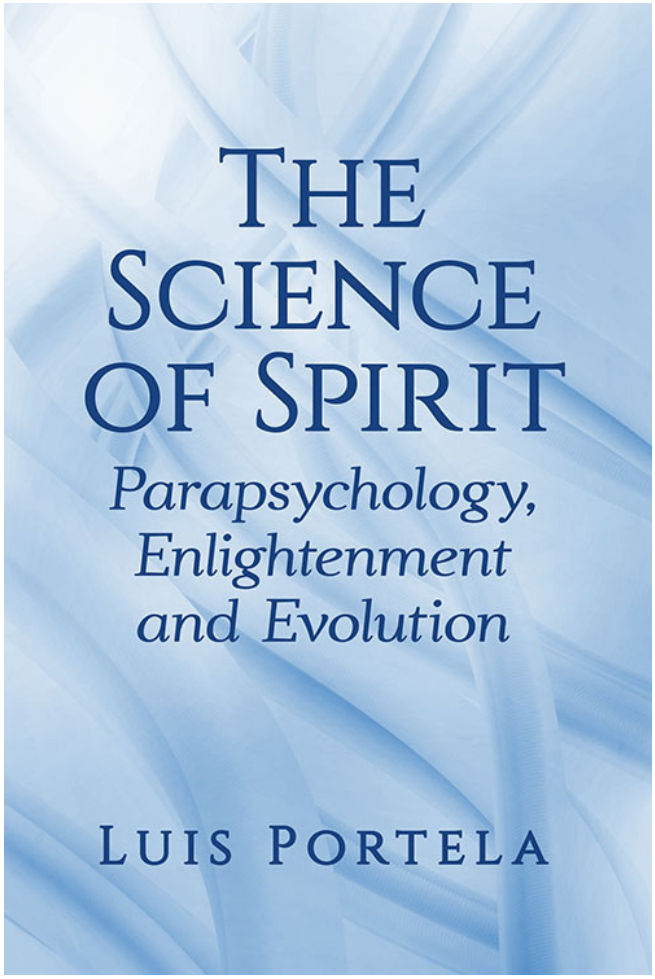
Luís: The Bial Foundation has a 100% patronage approach: It neither asks anything from its awardees and grant holders nor accepts anything from them. We want science to be done in complete freedom, without our interference, only with our financial support. As such, what we expect from our applicants is that they submit high-quality work, leaving the selection of the best to the independent jury of each of the three awards and our Scientific Board for the grants.

I believe that the applicants should focus, above all, on the intrinsic and scientific quality of their projects. I admit that is an important exercise for researchers in the parapsychology field because in the past, unfortunately, the investigation was not always carried out under the rigor of the scientific method, which is crucial for the credibility of the area.

Helané: Knowing you have a deep appreciation for these topics, as evidenced by your recently published *The Science of Spirit: Parapsychology, Enlightenment and Evolution*, what do you think are the most important research priorities for these domains?

Luís: I believe that during the 20th century, humanity has made a brilliant path in scientific and technological research, which allowed human beings today to know a lot more about themselves and the environment that surrounds them in comparison to a hundred years ago. However, unfortunately, parapsychological phenomena and consciousness have not been studied so much, which, in my opinion, led to a great imbalance. People became intoxicated with the study and mastery of matter, developing a hyper-materialist perspective, and [not] relegating the universal values and the urge to be, and seeking to be the best version of themselves [to a high importance]. Today's world shows great imbalances, be it social, environmental, economic, financial, etc.

As such, I believe that it is important, during the 21st century, to invest in scientific research of parapsychological phenomenon and the spiritual dimension in order to enlighten humanity. In my understanding, this research should not be done to demonstrate that this is correct or that it is false. It should be done only in search of the truth for truth's sake. I am convinced that, sooner or later, sci-



ence will demonstrate that some phenomena described since antiquity as miraculous or mysterious are purely false. But, I am also convinced that science will demonstrate the veracity of some other phenomena, discovering the scientific explanation and, probably, finding certain types of energy or energy use not yet known, the knowledge of which may allow a better individual and collective performance, as well as a greater inner balance, also at the individual and collective levels.

Helané: For the readers who have not yet read your book, could you briefly summarize your philosophy and insights about the role of exceptional human experiences (and notably psi phenomena) in human evolution and mainstream consciousness?

Luís: In this book, I aimed to show interesting results of scientific research in parapsychology, conducted in North American and European universities in the last decades that still are not very well-known. And then, cross those findings with the traditional knowledge from the field of spirituality while trying to give some clues focused on the advantage of each person trying to know themselves better and make their life path in a responsible and successful way.

I assume that the Earth functions as a world school, where we come to learn what we can with each passing day. In this school, we have plenty of opportunities to evolve that appear, as long as we are attentive and open to learning. To learn with everything and everyone: with the best, trying to follow their good examples, and with the worst, identifying their mistakes and trying not to repeat them.

In that sense, it is appropriate to steer our attention away from having and showing, and focus on being better every day, fixing our mistakes, and developing a path of self-perfection, which can be a source of great satisfaction, a reason for happiness. This dimension brings happiness much greater than the mere overstimulation of the senses so often sought by most people.

Helané: What do you think are the breakthrough areas that hold the most promise for advancing scientific theory?

Luís: I have the conviction that knowledge or wisdom is achieved through combining the efforts between multiple fields, from philosophy to physics, from medicine to psychology, and from biology to parapsychology. For this reason, at the Bial Foundation, we have fostered support for multidisciplinary projects. But, as I said, I think that parapsychology currently deserves a greater investment due to the fact that it was significantly less studied and developed during the 20th century. In the field of parapsychology, maybe it is important to study and clarify all aspects of its topic areas. We would try not to leave any grey areas, whatever the topic may concern.

But I also have the conviction that in research, it is very important to always maintain the rigor of the scientific method in a comprehensive, holistic way. Simultaneously, we can also consider direct experiments that are less objective but are still statistically demonstrated to be significant. And maybe even more important, that the research is conducted with authenticity, simplicity, detachment, and a strong sense of utility.

However, I deem important the work developed at the University of Virginia by the Division of Perceptual Studies (DOPS), the study of supposed past lives, which deserves special attention—work that has been confirmed and conducted by a significant number of researchers from other North American and European universities. Using rigorous scientific methods, the supposed past lives of a few thousand individuals have been documented from accounts related by children. The continuity of this work seems important to me. To continue researching how past lives are possible and developing scientific explanations for it. I think that physicists and philosophers should combine

their efforts with parapsychologists to further study reincarnation.

Helané: The Bial Foundation is one of the few foundations that fund this work. Why do you think there are so few organizations that support this work?

Luís: Parapsychology and consciousness have not been trending. The huge scientific investments and the enormous scientific and technological success of the 20th century created a perspective very focused on having, materiality, and consumption. The aspects more connected to being, universal values, spirituality, and parapsychology were left behind. Add to that the fact that, during the first half of the 20th century, some people discredited the field of parapsychology by fabricating or falsifying results and fraud on the part of participants. It is still a field hard to study due to the fact that many of the phenomena are spontaneous and sometimes unconscious, making them difficult to reproduce in a laboratory.

But the surge in the last decades of many researchers in North American and European universities conducting serious and profound work indicates that conditions are being created for the development of the field (Tucker, 2008). As such, various institutions giving financial support to the field have been arising as well.

Helané: How would you describe the role of funding in this research domain in terms of blocking or supporting continued progress in this area? For example, does the lack of funding provide an obstacle for research in this area?

Luís: I learned many years ago that the characteristic that really differentiates a scientific research project is its quality. A project with high quality ends up being carried out with more or less difficulties, but it is carried out. Of course, funding is important. If the funding chances are higher, it will be better for the project. Thus, I think a significant increase in funding is desirable for a strong and, if possible, rapid development of parapsychology. Not indiscriminate funding. An increase of funding opportunities directed to the development of projects of quality.

Helané: What role do you think taboos play in funding's availability? What strategies do you think are most effective for breaking through these barriers?

Luís: History shows us that the existence of taboos has always hindered scientific progress. But it also shows us that with patience, intelligence, honesty, and persistence, taboos have always been surpassed and science has always progressed. I have the profound conviction that it will be the same in the parapsychology field, as long as the researchers focus positively on the design of quality projects. The quality of the projects and the good results they may produce will be the best way to gain credibility for the field, attracting more and better researchers and attracting more and important funders.

Helané: Many researchers avoid pursuing careers in this area because of a lack of funding (or taboos). What advice would you give them? And what do you think are the most important things to consider for the future success of parapsychological research?

Luís: I think my previous answers demonstrate the importance I give to the qualitative aspect of the projects, which, in my opinion, should aim to be truly useful to humanity. Either in neurosciences or parapsychology, I think there is still much ignorance waiting to be solved. It shouldn't be hard to create truly interesting and useful projects. We will need patience, intelligence, honesty, and persistence to conduct the projects with pleasure and, that way, achieve important results.

In scientific research, as in any other activity, it is always important not to focus on the difficulties but rather on the solutions. Always use the power of positive thinking and a constructive attitude. And so, we verify that what is deemed as impossible is solely what is very difficult to accomplish. The path there may be worrisome and require effort, but, in return, it also gives enormous satisfaction to conduct and, above all, to conclude. The satisfaction extends to those who support it, namely those who fund it. Maybe the hardest things to accomplish are the most beautiful and the ones that give greater happiness to those who conduct them and those who support them.

COMMENTS

Dr. Portela's personal interest was what inspired the relatively large funding from the Bial Foundation in parapsychology, rather than a general trend. As Dr. Portela himself chose not to pursue his dream as a researcher in parapsychology, he was happy to be able to channel the profits from his company—the Bial Foundation—into the field. I also know that this support meets resistance. If you've ever had the pleasure of attending the Bial conferences, you know that it is an often divisive audience of parapsychology researchers and mainstream clinicians and researchers, many of whom find parapsychology research unacceptable. This is evident from the questions and often public criticism of the parapsychology talks. I appreciate Dr. Portela's courage to fund and promote parapsychology despite these obstacles. Perhaps the Bial Foundation's considerable parapsychology research and scholarship

funding will embolden other funders who are covertly interested in such topics. Perhaps the recent funding effort by Robert Bigelow for survival research is a harbinger of increased funding to come.

I do believe the tide is turning. Since I started in this field seven years ago, I have already seen a significant shift of the taboo. More of the general public are willing to talk about parapsychology topics openly. Numerous scientists in and out of academia are becoming interested, especially with the developments in quantum physics and cosmology and growing conversations about the non-local nature of consciousness and potential theories for consciousness as fundamental. Rising interest in the therapeutic benefits of contemplative practices and psychedelics adds to the swell.

Perhaps our field could foster increased collaboration and a more coherent front, so researchers in this field are well-poised to take advantage of funding opportunities that develop. For example, could multiple researchers collaborate on larger grants? Following Dr. Portela's advice for impeccable research methods, could more methods and analysis techniques be formalized into guidelines or structured protocols? Could scientific review become a norm? For example, the Koestler Parapsychology Unit provides excellent service to the field with its pre-registration process, including a very thoughtful scientific review by Jim Kennedy. Could we support or expand these efforts?

Furthermore, like the cases of Dr. Portela and Mr. Bigelow, parapsychology funding is often initiated by individuals who have had personal experiences that inspire them to support the work. Could direct experiences be provided so that more individuals could witness vetted individuals who can demonstrate such phenomena? I have found direct experience in concert with scientific evidence to be the most effective way to sway worldviews about parapsychology. The scientific evidence only goes so far if one has not experienced it; the direct experience without the rigorously collected data can be dismissed. The synergy between the two is potent.

Finally, I believe there is some personal work that we must embark on to clear past trauma many have experienced in this field. I imagine the majority of those reading this have faced some level of prejudice and blatant bias, especially around funding and paper submission. For some, this discrimination has even threatened their livelihoods. It can be challenging to be positive in the face of continued rejection. Could we support each other to release any individual or collective trauma our field holds for being the tabooed underdog? Can we shake off the continued rejections for paper submissions, etc.? Can we unapologetically stand for the work that we do? Picture standing with pride and confidence that edge science is a well-respected, valid, and impactful scientific field full of incredible scholars and researchers doing rigorous science.

Envision a world with many Dr. Portelas and Mr. Bigelows with abundant resources for all researchers already in the field, as well as those wanting to join. Picture dedicated chairs and departments established at multiple universities and institutes worldwide. Imagine a thriving scientific community of researchers and scholars supporting each other to conduct rigorous work, expanding our knowledge on edge science phenomena. Yes, I am the eternal optimist and hope this optimism infects you. The tide is turning. It must. The benefits we will reap are literally beyond our imagination.

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ESSAY

The Revival of Structuralism: The Periodic Table of Mental Science

Orogee Dolsenhe

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HIGHLIGHTS

Hidden patterns in subjective experience might be the basis for a 'periodic table' of mental science. New thinking indeed suggests that fifteen different sensory or perceptual modalities can be mapped in a systematic and unified way.

ABSTRACT

Physical science solved an age-old problem in the 19th century: What makes elements similar or dissimilar? Mendeleev generally is given credit for the discovery of the underlying structure of chemical elements, known as the periodic table of elements. Like chemicals, qualia seem to share different relationships within a modality and between modalities. Wundt's structuralism represents an early effort to build the structure of mind through data obtained by introspection. Unfortunately, as with many other subjects, structuralism has been victimized by behaviorism's domination. And the cognitive revolution did not completely eliminate the unfavorable status of consciousness, thus hampering the revival of structuralism unlike many other topics in psychology. With the subject of consciousness having been just about fully sanctioned by science beginning in the early 1990s, the time has come to build the periodic table of mental science by uncovering the hidden patterns of qualia. This paper examines three different scales of intrinsic patterns: the principle of opposition, the double-cone system, and the square of opposition. In addition, an effort is made to accommodate 15 different modalities in a systematic and unified way: chromatic, emotive, hedonic, acoustic, tactile, olfactory, gustatory, exteroceptive orientative, exteroceptive locus, exteroceptive motion, proprioceptive orientative, proprioceptive locus, proprioceptive motion, magnitude, and predicative.

One of the greatest achievements in physical science is the periodic table, for which Dmitri Mendeleev, the Russian chemist, is mostly given credit. It explained the age-old problem of the underlying interrelationships of chemical elements, chiefly, what makes them alike or different. His system serves as a unifying factor not only in chemistry but also in physics. The periodic law (or periodic table) has two dimensions: Horizontal rows or periods are arranged in order of increasing atomic number, while the vertical files (groups) manifest a certain degree of similarity in chemical and physical properties. The periodic table has become one of science's most well-known icons. Not only did the periodic table unveil the underlying reason for family likenesses among elements—it also identified elements. For instance, it revealed that salt is not an element but a compound of two elements (sodium and chlorine), and it was able to predict the properties of undiscovered elements.

In like manner, there are many different types of mental qualities: redness, sweetness, warmth, sadness, and so on. These are called "qualia." And just as with the chemical elements, some qualia seem similar to each other while others seem dissimilar. Thus, mental science faces a situation in finding interrelationships between qualia that is similar to the problem of chemical elements. While the 19th century physical scientists attempted to find the

hidden structure of the objective world, psychologists are faced with solving the same for the subjective world: What are the mental elements? What makes them similar or dissimilar? Mental scientists must find a unified system analogous to the periodic table.

A BRIEF SUMMARY OF THE HISTORY OF MENTAL ELEMENTS

Buddhist philosophy perhaps was the first to offer a discussion of mental elements under the etymology "dharma" (in Sanskrit) or "dhamma" (in Pali), for the basic, irreducible, self-existing mental elements (Ronkin, 2005). The traditional Theravada lists 82 dhamma categories which are grouped into four categories: *citta* (consciousness), *cetasika* (mentality), *rupa* (materiality or physical phenomena), and *asankhata* (nirvana) (Ronkin, 2005).

C. S. Peirce introduced the term *quale* in 1866 and C. I. Lewis (1929) was the first to use the term "qualia" in its generally agreed upon modern sense. Lewis sees qualia as the simple, sensory, building blocks of conscious experience (Keeley, 2009). Woodworth (1906) suggests that thought contains elements that are wholly irreducible to sensory terms.

Wilhelm Wundt (1897), known as the father of experimental psychology. suggests: ". . . psychical elements, or the absolutely simple and irreducible components of psychical phenomena, cannot be found by analysis alone, but only with the aid of abstraction." Wundt (1912) recognizes the importance of mental elements and boldly states: "The whole task of psychology can therefore be summed up in these two problems: (1) What are the elements of consciousness? (2) What combinations do these elements undergo and what laws govern these combinations?"

THE PRINCIPLE OF OPPOSITION

An interrelationship among qualia was noted by Aristotle, who suggested that all sensible objects contain the principle of opposition. In ancient Chinese metaphysics as far back as 4,000 years ago, the *I Ching* (known as the Book of Changes) describes three fundamental characteristics of human thinking: 1) bipolar organization of the dimensions of cognition, 2) attribution of positive to the yang pole and negative to the yin, 3) parallelism in the orientation of the dimensions in terms of underlying positiveness/negativeness (Osgood & Richards, 1973).

Consider the apparent interrelationship among qualia that can be seen in color. Yellow, for instance, seems more distant from red than does orange. Green seems even more distant from red, to the point that they are opposites. As far back as the 17th century, such obvious interrelationships have prompted color specialists to propose a geometrical model of the color system. Thus, the structural model of qualia actually began long before the periodic table of chemistry.

The modern approach of combining empirical data in the attempt to build the structure of mind began with Wundt. He was able to identify three bipolar dimensions of feelings: excitement–calm, pleasure–displeasure, and strain–relaxation (see Titchener, 1908). Titchener (1908) proposed that all sensations have four attributes: quality, intensity, clearness, and duration. He attempted to classify the structure of mind like the periodic table of chemical elements.

INTERRELATION AMONG MODALITIES

Interrelationships among qualia are also evident between different modalities. Sensory correspondence between modalities often is revealed in everyday language. For example, we say certain colors are loud, soft, warm, or cold. We describe certain cheeses as sharp and certain wines as smooth. And we label high-pitched sounds as bright and low-pitched sounds as dark.

One of the earliest attempts to investigate intermodality correspondence was made by Sapir (1929). He found that a particular vowel /i/ represents a smaller size than /a/ in native speakers of Chinese and of English. Newman (1933) not only confirmed the findings but also discovered that brightness and pitch also are associated with vowels: /i/ is brighter and has a higher pitch than /a/. Hornbostel (1931) argues that cross-modal brightness is more than an analogy; it is, he says, an "identical side" of different sensory systems. Hartshorne (1934) proposes sensory correspondence of all sensory modalities by introducing three bipolar dimensions corresponding with color: activity–passivity (red–green), joy–sorrow (yellow–violet), intensity–faintness (white–black). Hartshorne's model is remarkably similar to Wundt's (1912/1973) tridimensional theory of feeling.

Perhaps the most comprehensive empirical study of semantic structure was carried out by Osgood et al. (1957). They attempted to map words into various dimensions of meaning: "semantic differentiation." In their study, subjects would rate words with respect to 50 bipolar dimensions. The obtained data were then analyzed and condensed into three major factors: evaluation, potency, and activity. Interestingly, the three factors are quite similar to the three bipolar dimensions of Hartshorne (1934) and the tridimensional theory of feeling of Wundt (1912/1973). Marks (1978) made perhaps the most comprehensive

review of the studies of sensory correspondence and summed up by saying that different senses assist one another and share common phenomenological attributes.

Using 1096 schoolchildren as subjects, Simpson et al. (1956) found that violet and blue were associated with low frequencies, orange and red with intermediate frequencies, and green and yellow with high frequencies. Wicker (1968) based the two-dimensional map of color–sound correspondence upon similarity ratings between color and color, sound and sound, and color and sound.

One of the most well-known cross-modal translations is the association between color and emotion. For instance, blue is typically associated with sadness and yellow with joy. A stronger form of cross-modal translations is manifested in synesthesia. In synesthetes, one type of sensory experience provokes perception in another sensory modality. For instance, hearing the musical tone C induces seeing the color red. Interestingly, although there are some variations among synesthetes, the association between color and sound is often regular, systematic, and consistent from one person to another (Marks, 1978). In reviewing about 40 past studies on the correlation of colors and vowels, Marks (1975) concluded: ". . . a capacity for true synesthetic perception lies latent and dormant within most if not all people, ready to come forth when properly catalyzed. The potential to experience synesthesia is probably universal."

OSWALD'S DOUBLE-CONE SYSTEM OF CHROMATIC MODALITY

In the 17th century, several types of color systems had been put forward: a two-dimensional color chart, a two-dimensional color triangle, a color sphere, a color hemisphere, a color cube, a double tetrahedron, and an octahedron (see Oswald, 1931).

Perhaps the most advanced and certainly the most commonly used model of color was developed by Oswald (1931). Oswald (1931) developed the double-cone structural model (or the spindle shape) of color, which can systematically accommodate the three psychological dimensions of hue, saturation, and purity. The corresponding physical basis of hue is the wavelength of light. Brightness is the amplitude of light waves. The physical basis of purity is the amount of white light added to monochromatic light. The double-cone structural model not only can accommodate all colors in a simple, comprehensive, and systematic way, it also can show complementary hues and predict additive mixtures (Figure 1).

The Emotive Modality

Due to its dominance in human consciousness and behavior, emotion has been extensively studied by psychologists. Perhaps that's why the building of a structural model of emotion is far more advanced than that of other perceptions, with the exception of color. And the cross-comparison between colors and emotions is well ahead of those for other modalities.

McDougall (1921) was one of the first to indicate a correspondence between colors and emotion. Hartshorne (1934) also emphasized three bipolar dimensions by comparing emotions with colors: activity–passivity (red–green), joy–sorrow (yellow–violet), and intensity–faintness (white–black). Schlosberg (1952) demonstrated the correspondence experimentally by describing facial

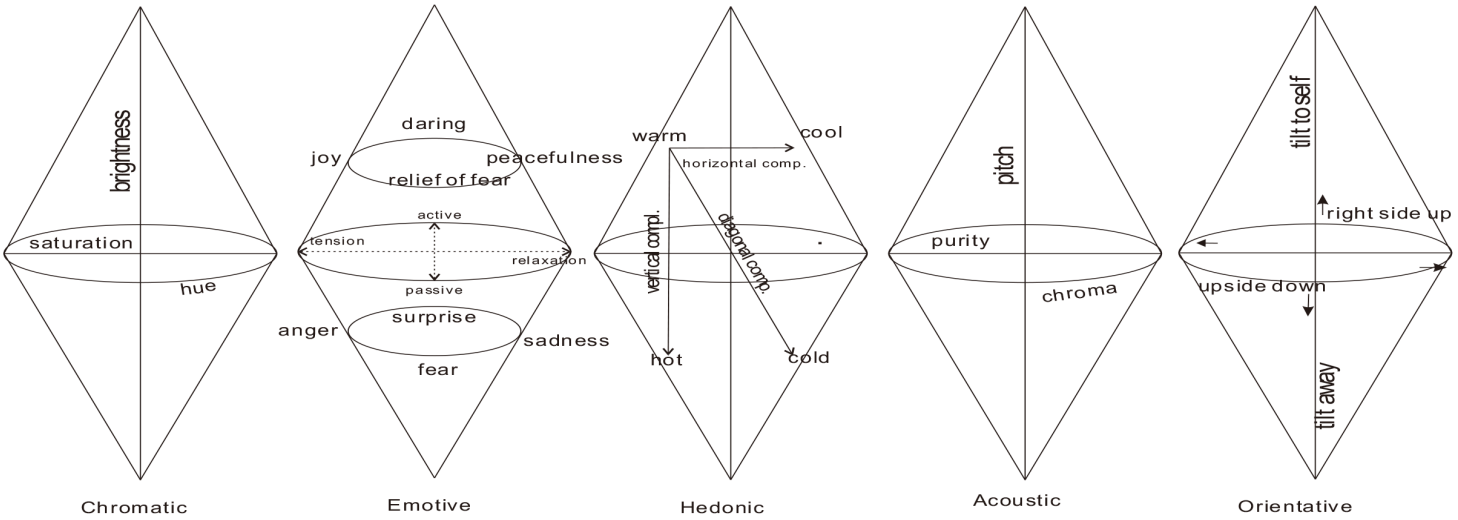

Figure 1. The unification of modalities.

expressions compared with colors and built a circumplex model of emotion (see Petri [1986] for an overview of the circumplex models of emotion). Just as color can be put into two dimensions (blue–yellow, red–green), he suggested two axes of emotion: pleasantness–unpleasantness and attention–rejection. Russell (1980) used 28 affective words in a multidimensional scaling method and also found two bipolar dimensions: pleasantness and arousal. And this two-dimensional arrangement of affective terms was pancultural (Russell, 1983). Plutchik (1962, 1980, 1983) departed from other models by proposing a three-dimensional version of emotion using eight primary emotions: anger, joy, acceptance, surprise, fear, sorrow, disgust, and anticipation. The third dimension he added is the intensity factor: The narrow bottom represents lower intensity while the top implies maximum strength. However, the dimension of intensity can be represented by the distance from the center of a circle as suggested by Schlosberg (1952). It seems that Plutchik (1962, 1980, 1983) added a third dimension without fully making use of the first two dimensions. The same problem also exists in the conical model of Daly et al. (1983).

A common problem in the study of cross-modal comparison is relying exclusively on primary colors. For instance, although there are nearly infinite shades of blue, we are culturally and linguistically biased toward the primary colors. The preconception of associating a type of emotion with a primary color may hinder the discovery of intrinsic relations between them. Therefore, the use of color patches that can show intermediate shades rather than relying on a few primary colors would be better for experimental studies. Thus, I propose the structure of the emotive modality is equivalent to the double-cone of the chromatic modality (Figure 1).

The Hedonistic Modality and the Notion of Square of Opposition

Despite the dominance of hedonic feelings in our lives, the hedonic structure lags behind the studies of emotion. Troland (1932) classified hedonic states into three categories: beneception, nociception, and neutroception. Beneception is pleasant feeling, and nociception is unpleasant feeling. Neutroception is neither pleasant nor unpleasant. Interestingly, Beebe-Center (1932) made associations between hedonism and color by attaching bright pressure to pleasant feelings and dull pressure to unpleasant feelings. Both Troland (1932) and Beebe-Center (1932) emphasized a hedonic continuum with a neutral state in the middle of two opposing extremes of pleasant and unpleasant feelings.

One of the most common hedonistic feelings is thermal sensations which have four different hedonic states: warm, cool, hot, and cold. Also, each thermal sensation has three complementary relationships with the others: the horizontal complementary, the vertical complementary, and the diagonal complementary. Warm–cool and hot–cold are horizontal complementaries as shown in Figure 1. The three complementaries are equivalent to the notion of the square of opposition, originated with Aristotle.

The sensations of warm and hot are associated with high temperatures while cool and cold are associated with low temperatures. The vertical complementary represents the opposition of positive (the upper parts) versus negative (the lower parts): Warm is pleasant, while hot is unpleasant; cool is pleasurable, while cold is displeasing.

The diagonal complementary relationship is apparent in our common experience. Suppose the thermostat of your house is set at 22° C throughout the year. During the hot summer, when outside temperatures rise above 32° C, you feel cool air when you walk into your house. The unpleasant feeling of hotness in the sun and the pleasurable sensation when you walk in the house represent the diagonal complementary. Consider also the freezing pain in winter weather and the warm sensation when you walk into the house. The hedonic state of the initial stimulus and that which follows represents exact opposites in quality (or the diagonal complementary). Both the uncomfortable feelings and the pleasurable feelings give us the double insurance of our well-being.

Thermal sensations invoke hedonic feelings that clearly have pleasant and unpleasant states as in emotion. While both emotion and hedonism are motivating factors in anyone's behavior, the former occurs generally in cognitive and sociological situations whereas the latter is geared toward physiological needs. Hedonistic feelings like hunger, thirst, pain, itchiness, etc., are directly connected to the needs and well-being of the body. It is as if the body rewards the organism with pleasurable sensations when it does something beneficial to itself but punishes it when it doesn't.

Besides thermal sensations, there are four other kinds of the hedonism: gastronomic, hydro, protective, and energetic. Gastronomic hedonism is involved with food intake and discharge (hunger, satiation, the urge to defecate, and relief from defecation). The hydro hedonism is the positive and negative feelings associated with drinking water and releasing it (thirst, quenching, the urge to urinate, and relief from urination). The protective hedonism is those hedonic qualia involved in protecting the body from injury or irritants: injurious pain, relief from pain, itching, and relief from itching. Energetic hedonism is the one responsible for motivating an organism to be active and to rest: boredom, stimulation, tiredness, and

the pleasure of rest. Energetic hedonism is a revised form of the square of opposition discussed earlier (Dolsenhe, 2005).

The Acoustic Modality

Hearing is the second most important sense and is the primary means of communication and music appreciation. Despite its importance, attempts to build the acoustic structural model began only recently. One of the earliest attempts to investigate the acoustic structure through speech perception was conducted by Miller and Nicely (1955) by employing confusions in noise among 16 English consonants. In their experiment, the subjects were forced to guess sounds which were spoken over a voice communication system with frequency distortion and with random masking noise. Shepard (1972) built a two-dimensional structure using the data of Miller and Nicely (1955). Wicker (1968) used intramodality comparison and intermodality comparison of patches of color and pure tones. In his study, subjects were to make similarity judgments between patches of color, pure tones, and patches of color and pure tones. Then, all the stimuli of both colors and pure tones were plotted in two-dimensional space. Simpson et al. (1956) earlier found that children associated violet and blue with low frequencies, orange and red with intermediate frequencies, and green and yellow with high frequencies.

I propose that all sounds can be plotted in the double-cone system, each with its corresponding item in the chromatic modality and the emotive modality (Figure 1). The acoustic modality should consist of three dimensions: The vertical axis represents pitch, the circle corresponds to tone chroma, and the horizontal distance from the center stands for saturation or purity. I would suggest that the voiceless sounds (*p*, *t*, *k*), which are closer to white noise, may be more toward the center of the equator section while voiced sounds (*d*, *z*, *b*) may be more distant from the center. The brighter sounds (*m*, *n*) are likely to be higher on the vertical axis.

The Tactile Modality

Though our skin is the largest sensory organ of the human body, adjectives describing the sense of touch are far fewer than for most other senses. Perhaps for this reason, a structural model of touch has not yet been proposed. Despite its linguistic limitation, some words describing touch have apparent opposite relationships: something can be as smooth as silk or as rough as tree bark, hard as rock or as soft as sponge, and slippery as the surface of ice or as sticky as chewing gum. I would propose our tactile sensation comes from the tactile modality that consists of three dimensions. The first dimension is sharp–round on the equator section of the double-cone. The sharp–round dimension is based on contacting an object's shape. Something can have sharp corners like a brick or can be round like a baseball. The second dimension on the equator of the double-cone is the high grating–low grating dimension. A lower frequency of grating means a larger size of grooves, and a higher frequency means a smaller size. Difference in frequency of grating can be understood with textural distinction. For instance, a burlap bag has a lower frequency of grating per unit area than does silk. Lederman & Taylor (1972) found that subjects could discriminate between spatial frequencies differing by as little as 3 percent. Gwosdow et al. (1986) reported that people tend to find high-grating material like silk more pleasurable than low-grating material like burlap. The third dimension is the vertical axis of the cone which is based on the degree of resistance when our body makes contact with the object. The upper cone section represents the degree of cushion or firmness. For instance, a pillow feels soft because it gives up resistance when the body pushes against it. Manufacturers of bedroom furnishings assign different degrees of firmness to mattresses. However, a concrete floor does not give, and this translates to the feeling of hardness. Although we might think that hardness may be the opposing quale to softness, stickiness may be more opposite to softness than hardness. I would suggest that the sensation of hardness is located in the equator section of the double-cone on the vertical axis. The lower section of the cone represents a sticky sensation, such as is experienced when we step on chewing gum.

The Olfactory Modality

Compared to color or sound, describing smell to someone is quite difficult. Our shortcomings of olfactory expression may arise from the fact that the human's sense of smell is comparatively less developed than that of other animals like insects or rodents.

One of the challenges for the attempt to develop a clasification system of odor is that there is no "obvious" or "direct" mode of objective description of the degree of similarity of odorants as seen in color and sound (Dubois, 2000). Two molecules of similar size and shape can be judged to be greatly different (Schiffman, 1974; Wright, 1982). The findings have all but ruled out Amoore's (1970) stereochemical model (which proposed that the shape of molecules determined the kind of odor we smell). To make the situation worse, the physiology of the olfactory receptors is still largely unknown (Sicard et al., 1997).

The discussion of smell classification goes back as

far as Aristotle. The most well-known model of olfactory qualities is Henning's (1916) smell prism. On the basis of similarities between more than 400 odors, he concluded that there were six primaries: fragrant, ethereal, putrid, spicy, resinous, and burned. In addition, he suggested that the prism is hollow and all intermediate smell should be located at the surface, not inside the prism. Utilizing a multidimensional scaling method based on reports of odor similarity, Wright & Michels (1964) and Schiffman (1974) were able to plot olfactory stimuli in a two-dimensional arrangement. I would propose that all odors can be plotted on the double-cone, and the method of intermodality comparison, particularly with colors, may be useful in finding the intrinsic structure.

The Gustatory Modality

We express whether food tastes good or bad. However, "taste" technically refers only to the perceptions that result from the contact of substances with the taste receptors of tongue, such as sweet, sour, salty, and bitter (Bartoshuk, 1971), whereas, the word "flavor" includes smell, touch, pressure, pain, and so on, in addition to taste (McBurney, 1978). Mozel et al. (1969) found that the ability to identify food substances is severely disrupted when odor perception is eliminated. Subjects were unable to identify coffee, garlic, and chocolate without smell.

Perhaps because of the substantial reliance of other modalities (i.e., smell, touch, etc.) in the intake of food, taste has the fewest number of stimuli among all senses. It is widely believed that there are just four primary tastes (Henning, 1927). Schiffman & Dackis (1975), however, proposed that there are other qualities not contained in the four primary components. They suggested three additional tastes: alkaline, sulfurous, and fatty. And these are not the product of olfaction because even anosmic individuals (those who cannot smell) are able to taste them. O'Mahony and Ishii (1986) also added another called "umami." The taste of umami should be familiar to people who have eaten foods like beef jerky or soup that contains monosodium glutamate. Because of the small number of gustatory qualia available, building the gustatory model is greatly hampered. Nonetheless, intermodality comparison may help to find the intrinsic structure of the gustatory modality.

The Exteroceptive Orientative Modality

Hebb (1949) proposed that lines constitute "primitive unities." His proposal was substantiated when David Hubel and Torsten Wiesel (1959) made one of the most important breakthroughs in neuroscience when they painstakingly began recording with microelectrodes from single cells in the visual cortex. They discovered that, unlike cells in the retina, cortical cells are selectively sensitive to specific patterns. By inserting an electrode into a neuron, they could measure the response of that neuron to various light patterns on the retina. One of the most dominant patterns that the neurons in the striate cortex respond to was orientation to lines and edges. Each cortical cell will respond vigorously to only a particular orientation. And if a line tilt is away from the optimum orientation, the cell's response drops off. The cortical cells in monkeys can distinguish the orientation to within 5–10°.

I would propose that the sense of orientation is also a separate modality. Therefore, it can be accommodated to the double-cone model. The equator section determines the angle of orientation: from 0° to 360°. Arrows would point to the left on 0°, to the top on 90°, to the right on 180°, to the bottom on 270°. Thus, the top half of the circle is right side up and the bottom half is upside down. The vertical dimension represents the tilting of a line either toward me (the top portion of axis) or away from me (the bottom portion of axis). Just as with any colors, any degree of orientation in the three dimensions can be plotted on the double-cone (Figure 1).

The discovery of neuronal selectivity on orientation by Hubel & Wiesel (1959) was essentially only on half of the equator section from 0° to 180°. That means there should be cells selectively responding to upside down orientation. In addition, there should be cells responding to a tilting toward me or a tilting away from me.

The Exteroceptive Locus Modality

All of us have a sort of cognitive map of the world we live in. We can tell where an object, a person, or a place is located relative to another. Spatial perception and its organization allow us to have a three-dimensional internal representation of the outside world. Kant (1781) suggested that space is an a priori form of perception; the property of spatiality precedes the content of sensory experience. His statement regarding space more than two centuries ago is remarkable because we do not normally ask whether space perception is a fundamental visual dimension. I would endorse Kant's proposal and say that, just as with color, sound, smell, taste, and orientation, the sense of location is an elementary modality which cannot be further reduced.

Following the seminal work by Hubel and Wiesel, there has been a flurry of investigations of neuronal selectivity. Among them, neuronal selectivity of locus was reported by Knudsen & Konishi (1978), who found that, in an owl, the unit (receptive fields) responded to sound only when the sound originated from a specific area of space. Interestingly, the nature and intensity of the sound caused

no changes in neuronal activity. Furthermore, the receptive fields of space in the owl are bimodal, responding to both auditory and visual stimuli (Knudsen, 1982).

Locus modality can be plotted on the double-cone system. On the equator section, the central point represents the center. The first dimension is left–right and the second is up–down. The third dimension (vertical) is distance from an observer, with far distance represented on the lower portion of the double-cone and close distance on the upper portion of it. Locus modality allows a sense of location of an object in a visual map and a relative sense between objects. In addition, locus modality enables us to perceive the 3-D shape of an object. For instance, when we see a cubical object like a pair of dice, the sense of 3-D is possible because we perceive some portions of the object is farther from us than others.

The Exteroceptive Motion Modality

An essential question about motion is whether it is a fundamental property or just the displacement of a visual image over time. Nakayama (1985) has discussed the often neglected yet very crucial issue of motion. He persuasively argues that mounting evidence leaves no doubt that motion is a fundamental visual dimension, meaning it is a primary sensory dimension and is not capable of further reduction. He invokes phi movement which is a phenomenon that stationary objects are seen as moving: When two light flashes appear about 100 milliseconds apart, observers see movement despite the inability to perceive an actual object moving across a gap. Gregory (1966) posits that senses of motion and position are separable dimensions. Furthermore, movement can be perceived not only by sight but also by sound and touch (Hornbostel, 1925).

I would support the idea that the sense of motion is an independent modality like the other modalities. Therefore, motion modality also can be expressed with the double-cone system. The first dimension is the motion of left–right. The second dimension is top–down. The third dimension is the sense of the motion coming toward an observer (the upper axis) or moving away (the lower axis) from an observer. And the farther away from the center of the double-cone, the higher speed of motion an object would appear to have.

The Proprioceptive Modalities

In the last three sections, we discussed the three types of modalities all dealing with the perception of an object: orientation, locus, and motion. We can also have those types of senses regarding our own bodies and limbs. The former three modalities are exteroceptions and the latter three are proprioceptions. The terms "kinesthetic sense" and "vestibular sense" are used to designate proprioceptive sensations. Kinesthesia means "perception of movement" in Greek, but it usually includes the sensation of static limb position (Clark & Horch, 1986). The vestibular sense is defined as the system that provides information about orientation, movement, and acceleration (Matlin, 1988). Kinesthesia and vestibular sense often overlap and can be somewhat confusing because the distinction is based on physiology rather than on any intrinsic modality differences. The vestibular sense comes from the central cavity of the bony labyrinth of the ear. The nonauditory labyrinthine organs are known as the vestibular system (Howard, 1986). Kinesthetic sense, on the other hand, arises from activity in sensory receptors that provide information about the angle of the joints, the lengths of the muscles and tensions they produce, and the rates at which these values change (Clark & Horch, 1986).

I would suggest that both kinesthesia and vestibular sense include three types of proprioceptive modalities: proprioceptive motion, proprioceptive orientation, and proprioceptive locus. And the proprioceptive modalities are identical to the exteroceptive modalities in their representation in the double-cone system. The proprioceptive orientative modality gives us a sense of uprightness, which is crucial in keeping oneself standing. We also can have a sense of our limbs using the proprioceptive orientative modality. The proprioceptive motion modality gives our body or limbs the sense of what direction we are moving in and how fast are we moving. The proprioceptive locus modality gives us the sense of where we are.

The Magnitude Modality

Kant (1781) proposed that our way of "sizing up" the world or estimating "how much" is an a priori concept. Korzybski (1958) also suggested that size and numbers represent a higher order of abstraction. Certainly, sizing up is one of our preoccupations: A house is big or small, a person is short or tall, a person is rich or poor, etc. A question we could ask is: Is magnitude inherent within an object or a person? Let's say your friend has $5,000 in his bank account. Is he rich? For an average person, $5,000 would be fairly large amount of money. But it's not considered such an exceptional amount by a millionaire. Thus, just as in thermal sensation, magnitude is not inherent within the objective world but is actually a product of subjective appraisal.

I would propose that the magnitude modality consists of three dimensions. First is the length–width dimension, and second is the number–volume dimension. The vertical axis represents larger magnitude on the upper cone and smaller magnitude on the lower.

The Predicative Modality

As discussed earlier, Aristotle was the first to discuss the notion of the square of opposition. It is a two-dimensional way of showing the interrelationship between the four types of predicates. Like thermal sensations, they show the three complementaries. Aristotle's square of opposition is, I believe, a section of the predicative modality. The predicative modality consists of three dimensions in the double cone. The first is the dimension of certainty. The words like *must*, *probably*, *perhaps*, *could*, *may*, and *possibly* characterize the confidence of sureness. The second is the dimension of proportion such as in the concepts of *all*, *virtually all*, *almost all*, *some*, *few*, *very few*, *virtually none*, and *absolutely none*. The third is the dimension of positive (the upper axis) or negative (the lower axis).

CONCLUSIONS

This paper focuses on the identifying and listing of mental elements and their relationships. It attempts to present a unified system of qualia consisting of 15 modalities: chromatic, emotive, hedonistic, acoustic, tactile, olfactory, gustatory, exteroceptive motion, exteroceptive orientative, exteroceptive locus, proprioceptive motion, proprioceptive orientative, proprioceptive locus, magnitude, and predicative, as shown in Figure 1. The proposed model posits that all modality characteristics have corresponding parts in other modalities. For instance, there are red-like emotions, red-like sounds, red-like hedonism, etc. The attempt to unify qualia is, I believe, equivalent to the effort to produce the periodic table in chemistry in the 19th century. It is a long overdue task in mental science.

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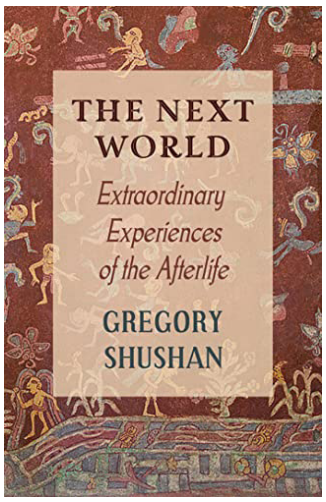
Journal of Scientific Exploration
Anomalistics and Frontier Science

BOOK REVIEW

The Next World: Extraordinary Experiences of the Afterlife by Gregory Shushan

Reviewed by
Michael Grosso



White Crow Books, 2022

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Our culture allows us to quantify death with precise statistics. We know that at least a million Americans so far have lost their lives to COVID-19. We have the daily numbers of mass killings in the United States; of those killed at the hands of Vladimir Putin's criminal war; of deaths due to starvation, specific diseases, obesity, psychosis, suicide, and so on. There are new technologies that claim they will be able to predict exactly when we will die from natural causes. And so on. What seems completely absent are platforms that entertain rational discussion of what exactly death and dying *are*, what they *mean*. What happens to a person when he or she dies?

Why so silent about this fundamental question? It turns out there is a small subculture of serious investigators curious about reports and conceptual issues that speak to this question. Gregory Shushan's book, *The Next World: Extraordinary Experiences of the Afterlife* is an original treatment of the subject, as wide-ranging in thought and feeling as it is rigorous and scholarly. Heartily recommended.

The book dwells largely on the near-death experience (NDE), but also reviews accounts of mediumship and cases of possible reincarnation. Modern NDE studies began in 1975 with the publication of Raymond Moody's *Life After Life*, but Shushan guides us through history and indigenous cultures in the far-reaching wider hunt for NDE-like experiences. The core elements of the NDE seem to reflect a universal component of human psychomental experience, and that would seem to point to the possibility of a 'next' world. One thing we may infer: For folks who manage to survive the death of their bodies, the next world must be a *mind-constituted* world.

Characteristic of the author's dialectical procedure, he does his best to review criticisms of the case for a next world. In most instances, Shushan handily disposes of the criticism, but continues to suspend judgment. His overall view seems to be that anything like scientific proof of life after death is not yet a plausible option. There are good rational reasons to believe in the reality of a next world, but they are yet to be altogether compelling. Universal agreement on the subject is bound to remain elusive, at least until some sort of reliable technology of communication is established. And yet the foregoing is not quite the point. Exploring the "extraordinary experiences" reviewed and assessed is an adventure in phenomenology, an exercise in evolving empathic attunement to the otherness of the transcendent.

The material covered is dense and varied. In the Introduction, we read:

> My main interest is in understanding why afterlife beliefs and experiences are similar across cultures, and why they're different. The true nature of NDEs is irrelevant to the idea that they can inspire, influence, and even give rise to afterlife beliefs. (p. 5)

It turns out that the similarities of belief and experience suggest a core pattern of reality. At once we are confronting a phenomenon that suggests the reality of an afterworld. The afterlife belief is historically a major part of most religious systems, but, in fact, evidence for an afterlife, as well as the concept of an afterlife, may be viewed as part of our natural existence, depending on no religious claims. This is compatible with whatever interpretation one might adopt to make ultimate sense of the data. We are all free to mythologize our own experience in our own way. For one person, the afterlife is integral to one's religion; but, for another, survival is simply a fact of nature, a consequence of the relationship between mind and body. There are, I suspect, many who recoil from the idea of belief in an afterlife, precisely because it may seem to entail religion. It might revive childhood fears of hell, drummed into them by Sunday School teachers.

Anyone interested in the "true nature of NDEs" (objectively real or delusory) has an immense possible database to work with. Shushan acknowledges there are "thousands of books" written on the subject, and he seems to have read a significant portion of them. As to the variety, ambiguities, and complexities of the available data, Chapter One, "Near-Death Experiences: Peeling the Universal, Cultural, And Individual Layers," lays the groundwork for the skeptical approach that follows. By skeptical I don't mean the stance of the disbeliever but the stance of the inquirer who suspends judgment until the inquiry is complete.

Not only are extraordinary afterlife experiences found in all cultures and times, the NDE, as Shushan points out, is an aggregate of distinct but related experiences, perhaps occurring to someone who is "near" death only in a psychological sense. Among the ND motifs are reports of out-of-body experiences (OBEs); encounters with deceased friends and loved ones; seeing or blending with a mysterious loving light presence; a panoramic memory of, and spiritual insight into, one's entire life; all of which generally results in a transformation of one's worldview, and, more telling yet, the acquisition of supernormal powers. Each of these motifs, say, the light presence, may occur outside the NDE. The elements of the NDE archetype are interpreted in various ways, depending on the various cultural settings—Christian, aboriginal, indigenous, classical Greek, the lower East Side of New York City, etc. Does an underlying, universal narrative emerge from the totality of these cross-cultural reports? Shushan's careful analysis concludes that the evidence reveals the active presence of a universal narrative suggesting the possibility of human postmortem existence. After taking into account the various individual and cultural colorings of the experiences, the outline of a universal story—*reality*—does emerge from the NDE multiplex. Then there is this crucial fact: The experience occurs even during cardiac arrest when the brain is deprived of oxygen, and consciousness is supposed to be impossible, according to current brain science. Just at the point where the mainstream view dictates no consciousness at all is possible, people are reporting conscious experiences, among the most intensely real and meaningful that they ever had.

Subsequent chapters provide an historical panorama of accounts of the near-death configuration in different epochs and different cultures. There is a chapter on near-death experiences in early civilizations, classical Greek and Roman, and one on shamanism and near-death experiences. In fact, we see how the NDE links shamanism to the rise of Greek philosophy and modern psychotherapy. The search takes us into the near-death worlds of Oceania and into the worlds of Victorian and Edwardian mediumship. The pointers for the momentous idea of a "next world" are diverse in names and specific narratives, as we should expect from across cultures, but at the same time they all repeat a single, overall transcendent message.

In addition to discussion of NDEs, there are chapters on mediumship and reincarnation phenomena. As for the latter, there is a fascinating discussion of "intermission" memories (Chapter Six), memories from the twilight state between death and one's rebirth. I was struck by one who described himself as if lost in nothingness, a graphic metaphor of unembodied existence for some people who feel lost in the cosmos. On a more upbeat side, "most met a god or god-like figure who helped them decide on their future parents. Many saw their future siblings" (p. 137).

The last chapter of the book poses the question, What kind of afterlife? Much is discussed, but the crucial insight is here stated (in the form of a question): "One might . . . ask, given the cross-cultural and individual differences between people across the world—religious, linguistic, social, environmental, and so on—why should anyone expect a single afterlife that would be the same for all humanity?" (164). The evidence, in fact, points to a great diversity of reported accounts, which in no way detracts from their credibility; our afterlives are likely to be about as wildly diverse as our current lives.

The philosopher H. H. Price wrote an essay comparing the next world to a dream world. The dream is a key part of the cycle of our waking, dreaming, and nonconscious existence. It may be the best available model of what a mind-based afterlife might be like. Shushan probes the possibilities of the dream model of the next world. What he reviews is a wide and varied range of conceptions of what the afterlife is like, from Tibetan Buddhism to native North American societies, and he repeatedly finds an explicit emphasis on the mind-dependent nature of the afterlife, a mind-world akin in different ways to dream-worlds, but a

world whose mental nature is expanded to include the normally quiescent so-called *paranormal* powers hid behind the innocuous-sounding letter of the Greek alphabet—*psi*. This is likely to involve any number of complications in the modalities of afterlife identity and creativity. What the author has shown is that some kind of a "next world" is a coherent theoretical possibility, based on a vast quantity of empirical data. We are invited to make of it what we will.

The book ends with two interesting appendices, the first titled "Extraordinary Experiences or Cultural Imagination: 'All In The Brain' Revisited." The discussion here focuses on what the author calls as "A Culture of Disbelief" (p. 180). In particular, he digs into the arguments of materialist scholarship as it appears in academic studies of religion. The chief target of the main academic assault is any claim in which something clearly *extra*physical presents itself. Sound the alarm—the door is open to magic, mysticism, mayhem!

The assault is extreme, inept though it be, and ends in many cases denying not just the meaningfulness of the experience but its very existence. The aim is to destroy the experience itself. One might indeed argue that the economic and technological rise of the religion of physicalism has led, directly and indirectly, to the invalidation of masses of deeply significant, indeed sacred, modes of human thought and experience. Nor would it seem extravagant to say we're talking about a species of crime against humanity. Much of this appendix is devoted to hammering home for the ironclad-skeptics the *reality* of OBEs and NDEs, etc.—realities they cannot explain physically and therefore pretend there's no there there.

The second appendix is about the near-death experience of Mrs. Leonora Piper. Mrs. Piper was one of the great mental mediums who also had an NDE. The appendix is confined to Mrs. Piper's NDE, which she underwent in 1896 during hernia surgery. The description of her NDE speaks for itself; it is both uniquely poetic and detailed but clearly contains the classic features of the experience: being out of the body, encountering a light, passing through a tunnel, sounds of transcendental music, seeing deceased friends and loved ones, and resisting the return to her body, the "depression" of it all, in contrast to her totally positive visitation to what seemed another, decidedly nicer world. I especially liked the "loose, Greek, flowing garments" and strains of music animating the air. She also noticed an open building where "some sort of educational work was being carried on" (p. 198), a portent, perhaps, of a new higher education to be explored in the next world, should we find ourselves still conscious after death.

Journal of Scientific Exploration
Anomalistics and Frontier Science

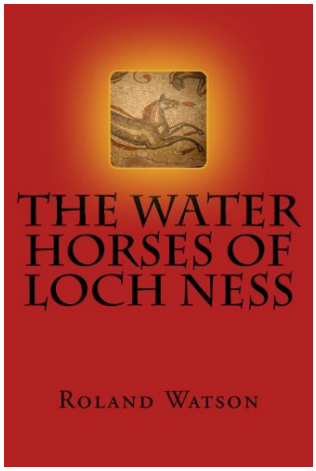
BOOK REVIEW

The Water Horses of Loch Ness by Roland Watson

Reviewed by
Henry H. Bauer

hhbauer@vt.edu

Virginia Polytechnic Institute & State University



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CONTEXT AND AUTHOR DISCLOSURES

This is an updated version of a review written in 2011 that somehow fell through the cracks of publishing procedures; a shorter version was posted on Amazon.com and is still there, dated 24 October 2011.

One reason for publishing it now is simply that it is an excellent book worth drawing to the attention of Scientific Explorers. Another reason is that we are working on an analysis of the spurious criticisms made, typically by self-styled Skeptics, about the reality of the so-called "monsters" of Loch Ness, the Nessies; and the book reviewed here includes some important original, even unique, relevant data.

The author of this review also discloses conflicts of interest: a long-standing belief in the real existence of Nessies[1] and congenial e-mail relations with Roland Watson.

For a reliable overview of the history of Nessie-hunting, I recommend Witchell (1989). For an illustration of how the same evidence may be used to support opposing beliefs, that Nessies are not real and that they are real, see chapters 1 and 2 in Bauer (1986).

CONTENT OVERVIEW

In *The Water Horses of Loch Ness*, Roland Watson presents a significant and original contribution to methods for evaluating and interpreting traditional stories and folklore.

Are Nessies real animals, or are they an entrepreneurial tourist trap capitalizing on folklore? Or are they perhaps supernatural entities?

Each of those hypotheses has its adherents, and they each offer evidence. Most cryptozoologists pursue the real-animals hypothesis. However, a British novelist and former PR executive confessed to inventing the creatures to help the hotel industry (Bauer, 1986, pp. 3–4), and an Italian journalist later claimed, separately and independently, to have invented the creatures[2]. Ted Holiday (1973), among others, envisaged a supernatural explanation.

In any event, it surely seems relevant that Scots folklore features such creatures as Water Horses, Water Bulls, Water Kelpies, to which are attributed a variety of characteristics. But relevant in what way? How to assess what lies at the root of this folklore?

The serious cryptozoological literature about Loch Ness mentions the legendary stories rather fitfully. Constance Whyte, in *More than a Legend* (1957), presented a determinedly empirical discussion of the evidence and referred to the difficulty in evaluating what local inhabitants have to say, citing the fellow who denied having seen the Loch Ness Monster, saying that he had however seen the Water Horse.

The debunkers try to make much of the fact that the big fuss arose in 1933, asking why Nessies only appeared then. As earlier possible mentions were uncovered, they would dismiss those as mere folklore, legend, myth. But, as Dmitri Bayanov has pointed

out with respect to yetis and their ilk, if anything like such creatures existed then surely they would have made their way somehow into folklore. A presence in traditional tales is no evidence that nothing real is at its root.

In *The Water Horses of Loch Ness*, Roland Watson gathers written accounts pre-dating 1933, more comprehensively than any earlier work about Nessies. He then uses this information in an ingenious manner. Is An Niseag, the water creature of Loch Ness, just like the kelpies and water horses and water bulls associated with other Scottish lochs?

It is not, it turns out. Half of all the books that mention such creatures mention the Loch Ness creature specifically; it is referred to more than twice as often as any other such entity.

Is that because Loch Ness itself was so often mentioned by geographers and others for some other reason than its Water Horses? How to estimate that?

By using Google Ngrams. That's one of the valuable things I learned about from this book. Google has scanned by now about 4% of all books ever printed, and this database can be searched to find how the frequency of use of a word or phrase has changed over time. For a quick overview, see http://books.google.com/ngrams/info; for the full treatment, see Michel et al. (2011).

Watson used Ngrams to determine how often Loch Ness itself had been mentioned. Only about as often as Loch Tay, and far less frequently than Loch Lomond, Loch Katrine, and Loch Awe. In other words, where Loch Ness is concerned, its Water Horses are a significantly more characteristic attribute than are such creatures in other Scottish lochs. Nessies are not the "usual" folklore associated "typically" with Scottish lakes.

Of course this does not establish for certain that Nessies are real animals, but it does put the kibosh on debunkers' arguments that Nessies are no more than myths, misperceptions, and 1930s tourist attractions. Nor does Watson attempt to extrapolate the evidence to that extent.

Indeed, a further attractive feature of this book is its determination not to shy away from any of the evidence, no matter that explanations are not yet forthcoming. Thus the land sightings—more than 30 of them—are pointed to; they pose real difficulties in identifying possible candidates for Nessie's identity. A survey of the candidates—fish, reptile, mammal, invertebrate—illustrates that no good explanation is yet at hand. In a more recent book, Watson (2018) offers a fully detailed and documented analysis of the land-sighting reports.

A particularly useful aspect of this book, at least for me, was Appendix B, "The ones that got away," which lists some of the claimed source-references to Nessies that others than the original author were unable to trace: John Keel's claim about an article in the 1890s in the *Atlanta Constitution*, and David James about a mention in Daniel Defoe's travel book. Watson also debunks an alleged Roman reference to a sea monster in a harbor named for Augustus, since the Fort Augustus at Loch Ness was not so named until many centuries later.

THE BOOK'S CONTRIBUTIONS TO THE LITERATURE

For the literature specifically about Loch Ness, the definitive list of reported sightings before 1933 is invaluable, as is the discussion of sources sometimes mentioned without specific citation. For the literature generally about cryptozoology, this book's approach is exemplary and, so far as I know, original and unique. All potential readers who do not yet know of, let alone use N-grams, this little gem of knowledge will be greatly appreciated.

Beyond the Nessie material and Google Ngrams, I also learned about the Moorov doctrine in Scottish law, which Watson mentions without explanation. Google helped me there immediately: The Moorov doctrine offers criteria for judging the reliability of corroborating evidence.

RECOMMENDATION

Highly recommended reading for anyone with even the slightest interest in the possible existence of animals featured in folklore myth and legend. Absolutely essential reading for cryptozoologists in general and Nessie fans in particular.

NOTES

[1] https://henryhbauer.homestead.com/LochNessFacts.html

[2] "Invention of Loch Ness monster, fortune-teller's misfortune and an amusing fraud". https://www.irishtimes.com/opinion/invention-of-loch-ness-monster-fortune-teller-s-misfortune-and-an-amusing-fraud-1.1237032

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Journal of Scientific Exploration
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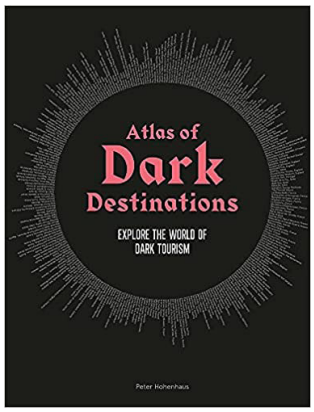
BOOK REVIEW

Atlas of Dark Destinations—Explore the World of Dark Tourism by Peter Hohenhaus

Reviewed by
Philip R. Stone

Institute of Dark Tourism Research
University of Central Lancashire, UK



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Dark tourism is an appellation defining travel to a diverse array of tourist sites that portray death, disasters, or calamities. For more than 25 years, dark tourism as an international subject of scholarly interest has drawn together multidisciplinary discourse, where the dominion of the dead collides with contemporary touristic consumption. In turn, dark tourism has opened scholarly scrutiny of our "Significant Other" dead and how societies deal with difficult heritage. Consequently, dark tourism is about polysemic touristic encounters with our memorialized dead, where a fine line exists between commemoration and commercialism. Dark tourism is inherently political and dissonant, as (re)presentations of our dead are imbued with sociopolitical bias, and remembrance is politically engineered and hegemonically orchestrated. Whereas heritage may produce narratives for dark tourism, it is the tourist experience that consumes such messages and co-constructs meaning-making. Indeed, dark tourism displays our fights, follies, failures, and misfortunes, and subsequent tourist experiences of our "heritage that hurts" mediates a sense of mortality at places of fatality.

Dark tourism has also piqued sustained interest from global print and broadcast media over the past decade or so. Undoubtedly, the provocative term for the media implies a focus on death and dying, though in reality, academic interrogation has demonstrated that dark tourism is more to do with life and the living. Dark tourism has also recently been brought to the public market with the first-ever tourist guidebook (Stone, 2021), published a week before *Atlas of Dark Tourism*, the subject of this book review. The publicization of the research field of dark tourism is now under way, and the *Atlas of Dark Tourism* has the potential to broaden the appeal of tragic memory to the lay market.

Yet, despite these new tourist guidebooks that allow visitors to sightsee in the mansions of our significant dead, the *Atlas of Dark Tourism* is neither a practical tourist guide nor a scholarly publication. With insensitive and inappropriate ratings of stars and crossbones for each site—a so-called "darkometer" arbitrarily created by the author—the book is confusing in terms of its readership objectives. Indeed, the naive "rating" of sites using stars and crossbones as symbolic markers of "experience" turns this volume from a useful tome to a tabloid manual. Consequently, the erroneous inclusion of the "darkometer" adds unnecessary sensationalism to visitor sites of tragedy and contested history. Therefore, this book serves as an interesting compendium of subjectively selected "dark" sites from the travels of Hohenhaus, a self-declared expert and dark tourist.

With a value for money price tag ($31), this hardcover A4 book comprises 352 pages and more than 300 potential visitor sites, with an abundance of full-color photos from across 90 countries. Visitor sites are geographically grouped for expediency, though some countries have more dark tourism sites than others. For example, Great Britain has eight allocated sites, Portugal only one, while Germany has twenty-six. Each of the "dark tourism" entries provides a descriptive but readable synopsis, some more succinct than others, and include former prisons, concentration camps, nuclear test centers, assassination spots, medical museums, and ghost towns. The book also includes a surprising feature

on volcanoes as dark tourism sites, especially considering that Hohenhaus states in the Introduction that he wishes to focus the book on modern (19th century onward) histories. Nonetheless, much of the book focuses on military sites or exhibitions, or places associated with the detritus of war and conflict. Combined with a military gothic font to highlight each visitor site, and with a black color design, this weighty book appears masculine rather than a universal "passport" of discovery it purports to be. Practical tourist information—that is, addresses, opening times, travel directions, websites, and a miscellany of other information that permits potential visitation to each of the sites—is omitted from the narrative. Therefore, this *Atlas of Dark Tourism* cannot serve as the tourist guide that it wishes to be.

Moreover, the uncritical descriptions of each site lack historical analysis and depth and, consequently, serve only as rudimentary introductions. Thus, this *Atlas of Dark Tourism* is not a history book either. That said, however, the tome may prove useful for students with limited historical knowledge who want a basic introduction to some potential dark tourism sites. Students may wish to further research the sites with case study approaches. Otherwise, this is not a particularly well-grounded book for historians or scholars of dark tourism. Instead, in terms of the public market, this book will appeal to the lay person who might wish to rudimentarily "dip into" the world of dark tourism.

REFERENCE

Stone, P. R. (2021). *111 dark places in England that you shouldn't miss.* Emons Publishers.

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Anomalistics and Frontier Science

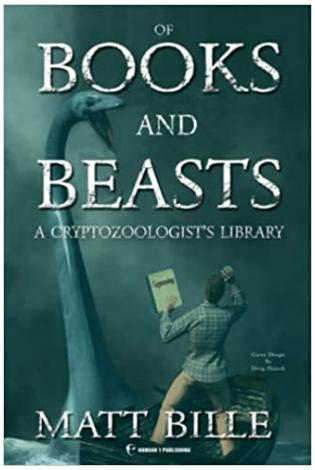
BOOK REVIEW

Of Books and Beasts: A Cryptozoologist's Library by Matt Bille

Reviewed by
Bruce A. Champagne

Bruce.champagne@lycos.com

www.brucechampagne.com



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Opportunities for the peer-reviewed presentation of specifically cryptozoological subjects are currently limited to the *Relict Hominoid Inquiry* (RHI, 2012–present) and the *Journal of Scientific Exploration* (JSE, 1987–present). As a result, books not only have an important role in providing historical information and current data and theory but can be preferable to other sources as the reader can evaluate the value of a book by the author's approach and sources. There are many cryptozoological titles but they are not equal in contribution, hence the value of this library.

DISCLOSURES

Matt Bille and I have been co-contributors (Heinselman, 2007) and have conversed about cryptozoology and natural history over the years. I was actually reading his book *Of Books and Beasts* when I was asked to review it and had already commented that his book has earned a place on the bookshelf as a great reference and a considered "must have" for those interested in the field. Currently, the book has positive reviews on the Amazon (Amazon Customer Reviews, 2022) and Barnes and Noble sites and elsewhere (Radford, 2022; Shuker, 2022), and won the 2022 General Nonfiction Award from the Colorado Authors League.

CONTENT OVERVIEW

Bille's suggested library of more than 400 titles and personal observations has particular relevance because of his extensive background in the field and his reputation. His opinion is valuable. One need only look at the book's acknowledgments to see that Bille has had contact with many of the major cryptozoological contributors and interested scientists of the last thirty years. Bille is also an author and blogger of science, space, technology, and cryptozoology-themed subjects and recognizes well-conceived and well-crafted writing.

My paperback copy of Bille's book was published by Doug Hajicek's publishing company Hangar 1 Publishing. It is my understanding that the first copies did not have a numbered index though my purchased copy does. Bille divided his effort into an Introduction, a 123-page section on general subjects of cryptozoology, a 96-page section dealing with related sciences, a third, crypto fiction section of 33 pages, a 17-page section titled "Marvelous Miscellany," an Afterword, acknowledgements, and a sectioned index of both titles and authors.

Bille made his selections based on a book's influence and its useful information. All books were personally read by Bille, less than 100 years old, and printed in English. Bille does not represent this book as an exhaustive list and allows that some titles were overlooked as a result of time constraints, finances, and life.

Bille's Introduction reminds the reader that "While cryptozoology can and should be a science, it is too rarely practiced using robust scientific methods. . . . Serious cryptozoological researchers who want to understand the field must start with reading." Perhaps, like me, you've heard researchers justify their unfamiliarity with subject literature or their lack of proper attribution by implying that their field activity makes up for their poor understanding and lack of testing and examination of previous data and theory. Unfortunately, the result is that cryptozoology can be strongly influenced by poor science.

Bille reviewed definitions of cryptozoology and proposed his own:

> A scientific endeavor that takes traditional zoological methods of animal location, collection, and identification (using field work, local reports of animals, chance discovery of trophies, etc.) and widens the aperture to consider animals based on evidence not firm enough or consistent enough to draw interest from most zoologists.

I applaud Bille's much-needed efforts to refocus and realign the field with foundational science.

The book is offered as a guide and basic reading list to current and future "generations" of cryptozoologists and other interested students. As intended, Bille's descriptions and "musings" will probably be valuable to the skeptic, the experienced, and those new to the field, and provide some important obvious and inferred reminders and clarifications for each group.

Though Bille is a self-described skeptic "in the proper sense of the word" in his acceptance of hypothetical species and his review of the literature describing them, Bille seems to understand his approach may also illustrate the differences in intention and utility by self-described skeptics.

Much to the frustration of both academics and citizen scientists studying cryptozoological subjects, the skeptical philosophy, with its acknowledged disciplinary conflict (Hill, 2016), is often encouraged and employed with the passionate default of cognitive bias, as opposed to being an implement of the scientific method. Just as with some of the cryptozoological proposals they scrutinize, skeptical exercises can commonly result in incomplete analysis and implausible proposals or superficial criticism. Perhaps future skeptics could provide more scientifically rigorous examinations of data and meaningful contributions with foundational reviews and more substantive examinations of some of the works Bille included.

However, both new students and researchers within the fields of cryptozoology must also improve in analysis, falsification, and presentation of their efforts and data. Bille also appropriately laments the scarcity of "robust" scientific methodology practiced by cryptozoologists. Bille is not wrong.

I'm fortunate to be able to participate with a cryptozoology-themed study group of citizen scientist cryptozoologists and objective professionals and academics, most of whom remain anonymous for the time being. The academics suggest that a cryptozoologist's work should always regard previous literature and work and be written and presented in an acceptable scientific format, and, of course, be defensible.

These practicing scientists and academics also encouraged cryptozoologists to discard the connections, Bille describes them as "entanglements," between hypothetical species and the paranormal, cloaking, mindspeak, portals, travel within dimensions, quantum physics, etc. Additionally, the inclusion of biologically implausible subjects (Mothman, Dogman, Jersey Devil, etc.) should have no place in cryptozoology when practiced as a science. Such incredible ideas negatively impact datasets, discourage the participation and support of academics and citizen scientists, compromise credibility, and relegate the study to folklore and popular culture.

The book's first section, "Cryptozoology Books," contains what Bille suggests "should be a basic reading list of the classics in the field." These foundational titles range in time from 1937 to 2021 and cover relict hominoids, aquatic megafauna, felids, birds, persistent dinosaurs and extinct Pleistocene mammals and marsupials, encyclopedias, location-specific overviews, and mothmen and dogmen (Bille is appropriately critical). I did note a few important titles that were not included. Among them was Mark Hall's *Thunderbirds* (2012), T. A. Wilson's *Bigfoot in Evolutionary Perspective* (2015), and William Munns' examination of the Patterson-Gimlin film *When Roger Met Patty* (2014).

The next section, "Related Sciences," is soundly introduced by Bille's opinion that the cryptozoological academic and field researcher, and even parent, "must know" foundational science. This section is a helpful support to the book's introduction of cryptozoology as a science. Included are titles on paleontology, evolution and extinction, dinosaurs, scientific literacy, exploration and field research, and environmental ecology. Bille suggests that familiarity with this material will help researchers "ask the right questions."

Formulating thoughtful and productive questions to evaluate plausibility and experimental design would go a long way to advancing the field generally and datasets specifically. Understanding evolution, selection, and scientific terminology is critical to developing theory and making defensible arguments and addressing the criticisms of the lack of critical thinking and of scientific falsification.

Section three, "Crypto-Fiction," begins with Bille's reminder that crypto-fiction probably predates written fiction and provides an opportunity to present ideas to the public. Bille has a significant background as a fiction writer and provides some experienced advice for both the new and established author.

The last section, "A Marvelous Miscellany," includes those titles Bille's suggests "are too important, interesting, or enjoyable to skip." These include field guides, explorations, paranormal compendiums (for their sources), anthologies, speculative evolution, regional reviews, folklore, fiction, conspiracy theory, natural history, and even coloring books. Bille concludes with a listing of his own authored titles.

PROS, CONS, AND CONTRIBUTION TO THE LITERATURE

Aside from a few more items that could have been included, this book contains many of the major titles, reads easily, and is enjoyable. Bille has made a meaningful contribution to the literature and field by evaluating and consolidating essential material. I would suggest of equal importance is Bille's effort to encourage a return or adherence to the scaffolding of critical thought, objectivity, and scientific methodology.

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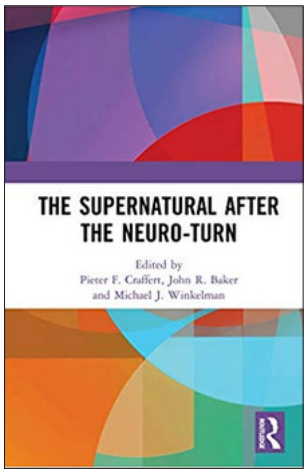
Journal of Scientific Exploration
Anomalistics and Frontier Science

BOOK REVIEW

The Supernatural after the Neuro-Turn edited by Pieter E. Craffert, John R. Baker, and Michael J. Winkelman

Reviewed by
Gerhard Mayer

mayer@igpp.de



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In the book chapter "Neurocognitive processes and supernatural beliefs," Andrew Newberg and David Yaden describe a basic cognitive process they call the "binary process." According to their model, this should contribute to the development of supernatural beliefs in addition to other processes. They understand this to be a general structuring process that organizes objects and abstract ideas into into dyads; that is, into relations of two, e.g., good and bad, happy and sad, natural and supernatural, right and wrong. The latter dyad points precisely to a problem I have with large parts of the book. The texts of the authors involved—all of whom are men—are almost universally characterized by such a binary thought structure. One of the editors, Pieter Craffert, a neuroanthropologist who teaches in South Africa, puts it succinctly in Chapter, "The *supernatural*: A range of neuro-cultural phenomena," when he writes:

> If transcendental theorists are correct, there is not only a whole range of entities and phenomena with powerful influences on the world that beg explanation, but the scientific enterprise as we know it needs radical transformation. If non-transcendental theorists are correct, their theories pose a challenge to nearly all religions as well as local and cultural explanations of the phenomena. (p. 24)

All authors of the book represent non-transcendental positions and attribute the category of the supernatural to "this-worldly" causes. It is not clear to me whether Craffert is aware that he leaves the area of scientific argumentation with the statement quoted above. With this escalation, there is only an either-or: Either they or we are right. It does not seem to occur to the authors that many of the non-transcendental explanations for supernatural interpretations of events and the arising of corresponding beliefs may be plausible and, in many cases, sufficient, but still do not capture the full picture. At least a more modest and reflective attitude that allows for this possibility is not reflected in the texts—with few exceptions.

The "Acknowledgements" state that some of the chapters collected in the book are based on papers presented at a symposium on *Where do we stand on the supernatural? An interdisciplinary exploration*, held at the University of South Africa in 2016. It seems to have been a very manageable group of participants. A group photo on the Internet shows 13 people. Only three of them, the three editors, contributed to the anthology. The presentations of the other participants obviously did not fit into its conception. Together they wrote the "Introduction;" Craffert contributed two chapters and medical anthropologist Michael Winkelman contributed three more. The remaining three chapters were written by anthropologist Charles Laughlin, neuroscientist Andrew Newberg, and psychologist David Yaden, as well as two psychologists, Yakov Shapiro and J. Rowan Scott. The selection of authors with a consistent, or at least for this occasion aligned, basic ideological stance with respect to the book topic under discussion—with one exception, which I will

discuss later—allows for a cursory treatment of the contents of each chapter and a focus on the underlying core ideas.

Chapters 1 and 6 are written by Craffert and provide an overview of cultural and religious concepts of the supernatural and ghosts. The author points out that these experiences and beliefs are reported in all cultures and during all time periods, and do not disappear with increasing [scientific] enlightenment and industrialization. Thus, the belief in ghosts and the belief in the supernatural are universal cultural anthropological constants.

In chapter 2, Laughlin uses a "neurophenomenological" approach and a biogenetic structural theory to describe the brain as a "cultural organ" whose primary function is to generate a "brainworld" (p. 42) that represents "a simplification of the real world" (p. 36). The complexity of our brain entails a high susceptibility to error. False causalities are produced because the true connections are invisible and remain hidden. Accordingly, the invisible is reified and attributed agency.

Chapters 3, 5, and 7, written by Winkelman, present his theory of the emergence of supernatural beliefs as part of the evolutionary process of human development in various aspects. The basic idea is that collective human rituals are evolutionarily biological and can also be found in primates. There, rituals strengthen group cohesion and therefore increase the survival chances of individuals and the group. In humans, they lead to religion and belief in the supernatural. The mimetic behavior of humans gives rise to shamanism. In Chapter 5, Winkelman discusses the neurophysiological basis for belief in the supernatural. Through innate and "hard-wired" cognitive operators, animism, archetypes, the collective unconscious, and even notions of supernatural powers are part of the human experience. For example, he describes animism as "a cultural universal that results from the interaction of several basic brain operators" (p. 92). Chapter 7 is devoted to one of Winkelman's specialties, shamanism and the techniques of shamanic journeying linked to altered states of consciousness (ASC), which he largely equates with out-of-body experiences. He also sees these shamanic techniques of conscious induction of ASC as human universals that bring evolutionary advantages, such as insensitivity to pain, increased vigilance, and the idea of being able to foresee the future.

Chapter 4 includes the paper mentioned at the beginning, written by Newberg and Yaden, which describes eight fundamental neurocognitive processes that can be associated with the formation of supernatural beliefs. These are, in addition to the "binary process" already mentioned, the "holistic process" that leads to a perception of wholeness, a sense of Oneness; the "reductionist process," which as a neurologically wired "Occam's razor" counteracts the formation of supernatural beliefs and promotes scientific approaches to the world; the "causal process" that searches for reasons and causes even where there are none; the "abstract process," which is responsible for category and concept formation; the "emotional value process," by which we evaluate our experiences and perceptions and come to fear or love supernatural beliefs and experiences; the "agency attribution process," by which we determine which subjects or objects in the environment have agency; and finally, the "reality attribution process," by which we determine what is real and what is merely imagination. Almost all of these processes are assigned a specific area of the brain and the brain is described as a "belief making machine" (p. 85). At least the authors admit that this assignment of brain structures, cognitive processes, and formation of supernatural beliefs is highly speculative.

This is particularly lacking in the contributions by Winkelman, who refers strongly to such concepts and uses them for apodictic statements, such as "This is the foundation of the human experience of the supernatural, something unseen but perceived to have mental capacities like our own" (p. 93). With the introduction and naming of various cognitive "operators" (social psychological operator, mimetic operator, isopraxic operator, etc.), his model seems to be assembled from a Lego set, with the plan and the result being fixed from the outset. Findings from evolutionary biology, anthropology, neurophysiology, cognitive and social psychology, religious studies, and cultural studies are taken into account only to the extent that they support his model. To take one example: Shamanism and imitation of the "other" (e.g., animals) play a major role in Winkelman's model. Anthropologist Michael Taussig has done research on these topics and written an influential and widely received book, *Mimesis and Alterity* (1993). But since his approach to understanding culture and culture formation, including his critique of anthropological reductionism, does not fit Winkelman's understanding, his theses are simply ignored. Or: Since Winkelman's evolutionist conception sees a direct connection between the alpha animal in a primate horde and the shaman in early human groups (p. 59), he writes "the shaman is typically male" (p. 58), although there are shamanic cultures with a clear dominance of female shamans, and in general the change or dissolution of gender identity plays a major role in shamanism (Bleibtreu-Ehrenberg, 1984; Tedlock, 2004, 2006). It is pure speculation that all this was different in early human groups and that the shaman was at the same time the leader of the group and a role model for the development of a concept of God.

Thus, in many places, and this does not only concern Winkelman's contributions, one can find simplifications that disregard essential discussions from the respec-

tive disciplines concerned. This concerns also and especially the findings of parapsychological and anomalistic research. If the authors would take these seriously, their constructions would suffer major cracks. Ultimately, they base their claim to absoluteness on the fact that psi phenomena can be traced back to perceptual illusions and cognitive dysfunctions. This blanking out is all the more surprising since both Craffert and Winkelman seem to be familiar with parapsychological literature and research. For example, Craffert suddenly cites experiments on *backward causation* and their "ample experimental evidence" (p. 42) and uses these findings situationally to support his model, but without considering them in their overall consequence for the existing non-transcendental worldview. His "reality" remains "classically three-dimensional" without, for example, admitting the possibility of further dimensions that elude our direct perception.[1] Winkelman also mentions in the context of shamanic practices "an enhanced ability to predict future conditions" (p. 142), although he is not clear about the underlying mechanism, except to say that future predictions are based on subconscious information.[2] One might conclude that the authors care more about their theoretical models than empirical facts, as is unfortunately not uncommon in the field of science.[3]

A few key statements can be distilled from the essays discussed:

- The formation of the belief in the supernatural brought evolutionary advantages (group cohesion, acceptance of hierarchical structures—alpha animal).
- The brain is extremely error-prone in its complexity and produces the supernatural (as misinterpretation). Complex brains are in some ways worse adapted to "reality" than simple ones.
- Nevertheless, the development of supernatural belief is not pathological or primitive, but a positive and often beneficial trait.
- Paranormal phenomena do not exist or do not enter into the models and therefore do not come into question as factors for the formation of beliefs.[4]

The overall positive assessment of the human inclination towards the supernatural fundamentally distinguishes the position of these authors from the classically skeptical and, in most cases, not scientifically but ideologically driven authors who have been waging the battle against superstition since the Enlightenment because it posed a massive threat to society.

What I have written so far has left out the last chapter of the anthology "Extraordinary knowing within the framework of natural science: Towards a theory of 'scientific mysticism'" by the two psychologists, Shapiro and Scott. Whoever reads the book until the end will be completely surprised with this text. With the exception of the fact that the authors take a non-transcendental position, give some importance to evolutionary biological considerations, and do not negatively evaluate supernatural beliefs, their approach has nothing to do with that of the other authors. For Shapiro and Scott, the findings of parapsychological research are central. They outline a model of dual-aspect monism and draw heavily on the ontological model of quantum mechanics of the U.S. physicist David Bohm (1917–1992). This chapter offers stimulating reading, which should not be seen as a supplement to the other contributions, but as a counterpoint and thus in a certain respect also represents a foreign body in the otherwise fairly homogeneous anthology. It contains sentences that are worth quoting, such as on the paradox of the *reductive epiphenomenalism of consciousness*: "I think reductively, therefore I am not" (p. 151) or a quotation from Charles Tart: "When data which make no sense in terms of the (implicit) paradigm are brought to our attention, the usual result is not a reevaluation of the paradigm, but a rejection or misperception of the data" (p. 156). They conclude their paper with well-considered sentences that also apply to parapsychological researchers:

> While many questions remain to be answered, it is clear that the categories of "paranormal" and "supernatural" are only a reflection of our limited understanding of the full scope and complexity that natural processes entail. By these standards, there are no "supernatural" phenomena in Nature but only *as yet unknown* principles that will be incorporated within ever more encompassing naturalistic and trans-materialist paradigms. (p. 167; italics in original)

My conclusion: Despite the considerable criticism I have of the content of the contributions to the anthology, and even without considering the last "deviant" chapter, I can recommend the book for those seeking a good overview of non-transcendental reductionist explanatory models for the emergence of supernatural beliefs and extraordinary experiences. For example, it is helpful to know the basic cognitive processes that *may* promote the formation of such beliefs. Just keep in mind that the authors present a simplistic picture and do not bring up, let alone discuss, contradictory findings.

NOTES

[1] "Taking subjects seriously does not mean adopting their explanation but finding an explanation for their experi-

ences and phenomena. Similar to our experience of a flat earth, which is a reasonable and rational conclusion drawn from experience, spirit beliefs are rational and empirical—even though mistaken when viewed from an etic perspective . . . Anecdotal evidence, even a billion people claiming to experience a flat earth, is not sufficient evidence for a cosmology. There is a difference between the experience of and belief in a flat earth and an actual flat earth" (pp. 18–19). Exactly the same reasoning could be applied to his statement regarding the discrepancy of subjective evidence of perception and its explanation.

[2] Michael Winkelman was invited by the Parapsychological Association in 2011 to give the Banquet Address at their 54th Annual Convention. It was entitled "Evolved Psychology and the Deep Structure of Psi: The Shamanic Paradigm."

[3] Of course, it could also be that the authors have chosen a simplistic presentation for reasons of publication strategy and have suppressed their actually more differentiated view.

[4] On the importance of such experiences for the formation of heterodox beliefs, see Mayer and Gründer (2011).

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Journal of Scientific Exploration
Anomalistics and Frontier Science

LETTER TO THE EDITOR

A Persistent Avian Formation on a South-Facing Slope along the Northwest Rim of the Argyre Basin

George J. Haas

James S. Miller

William R. Saunders

thecydoniainstitute@hotmail.com

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This report updates a paper published in this journal in the Fall of 2011 (Saunders et al., 2011). The original paper analyzed an avian-shaped feature that rests below a network of cellular structures found on a mound within the Argyre Basin of Mars. The paper included analysis of the avian formation by a geologist, Michael A. Dale, and a geoscientist, William Saunders, along with three veterinarians: Amelia Cole, DVM, Joseph Friedlander, DVM, and Susan Orosz, DVM. The area examined is located near 48.0° south and 55.1° and is supported by images from both Mars Global Surveyor and Mars Reconnaissance Orbiter (MRO) spacecrafts. The images reveal defining aspects of this avian feature, including a head, beak, body, eye, legs, foot, toes, wing, and feathers, which have been persistent over a period of more than 20 years. When taken together, these components induce the visual impression of an avian-shaped formation exhibiting a unique set of proportional features that include 23 points of anatomical correctness. A new MRO HiRISE image has been acquired revealing additional features and detail.

UPDATING WITH A NEW IMAGE

The new MRO HiRISE image of the avian formation was acquired in 2021. Independent researcher R. DeRosa filed a targeting request for higher-resolution images of the avian formation at the MRO HiWish site on March 9, 2014 (ID 97522). His request, which was titled "Region within Nereidum Montes," was granted, and the new image was acquired on January 1, 2021. The MRO HiRISE image ESP_020794_1860 (Figure 1) was released on January 14, 2021, and titled "Layered Knobs and Rectilinear Ridges in Nereidum Montes" at the Arizona University site (DeRosa 2014) and titled "Parrotopia" at the HiWish site.

The MRO HiRISE image ESP_020794_1860 (Figure 1) was acquired in winter during the early afternoon with a resolution of 50.4 cm per pixel (ESP_020794_1860 2021). This MRO HiRISE image provides the highest resolution to date and confirms all of the anatomical features observed in the earlier images with exquisite detail.

The avian structure (Figure 2) is composed of eight segments that include an extended right wing (1), beak (2), face (3), neck and crop (4), body (5), lower leg/foot (6), vent and tail feathers (7), and second leg/foot (8). These segments are differentiated by height, color, patterning, contour, and lithology.

ANATOMICAL ANALYSES OF THE AVIAN FORMATION

There are distinct anatomical similarities between the features found on the formation located at Argyre Basin (Figures 1 and 2) and avian species. The analytical drawing in Figure 3 identifies a set of 23 points of confirmation (labeled A–W) that provides evidence that the formation at Argyre Basin not only represents an avian creature, but that its sculptured features appear anatomically correct.

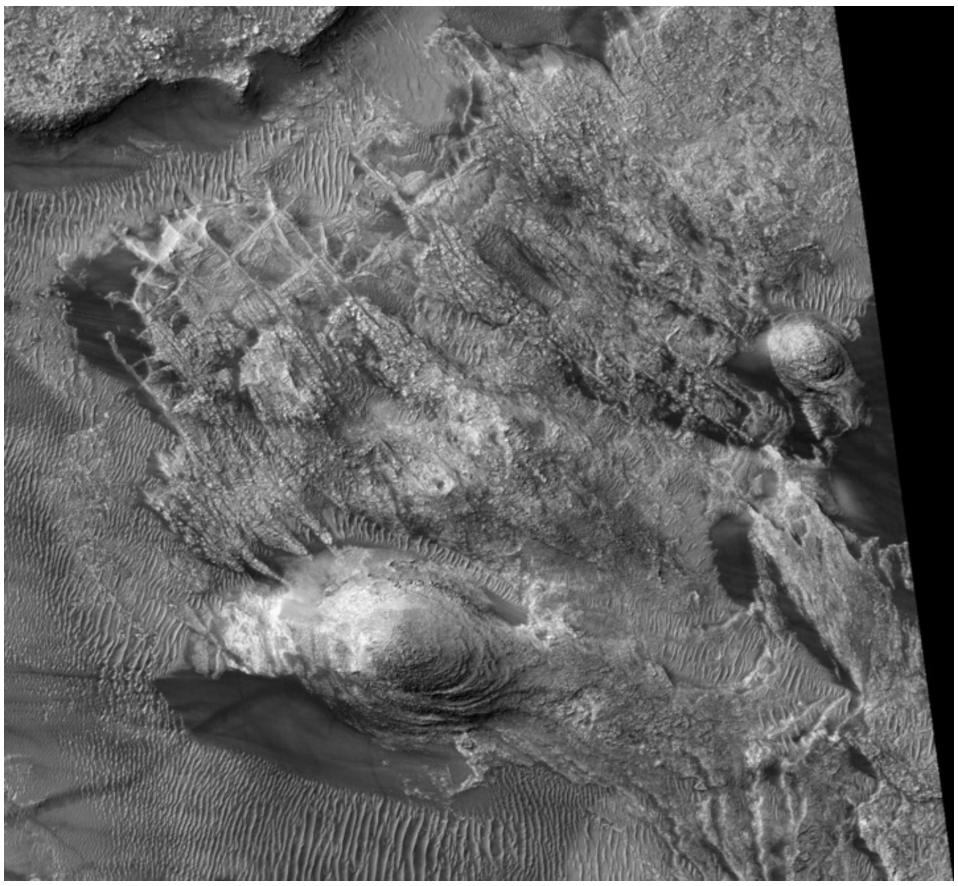

Figure 1. Avian formation. A portion of Mars Reconnaissance Orbiter HiRISE image ESP_020794_1860 (2021). Courtesy NASA/JPL/University of Arizona.

Figure 2. Eight segments of the avian formation. Detailed crop of Mars Reconnaissance Orbiter HiRISE image ESP_020794_1860 (2021). 1) extended right wing, 2) beak, 3) face, 4) neck, 5) body 6) first leg/feet, 7) tail feathers and 8) second leg/feet. Courtesy NASA/JPL/Arizona University. Notation and line annotations by George J. Haas.

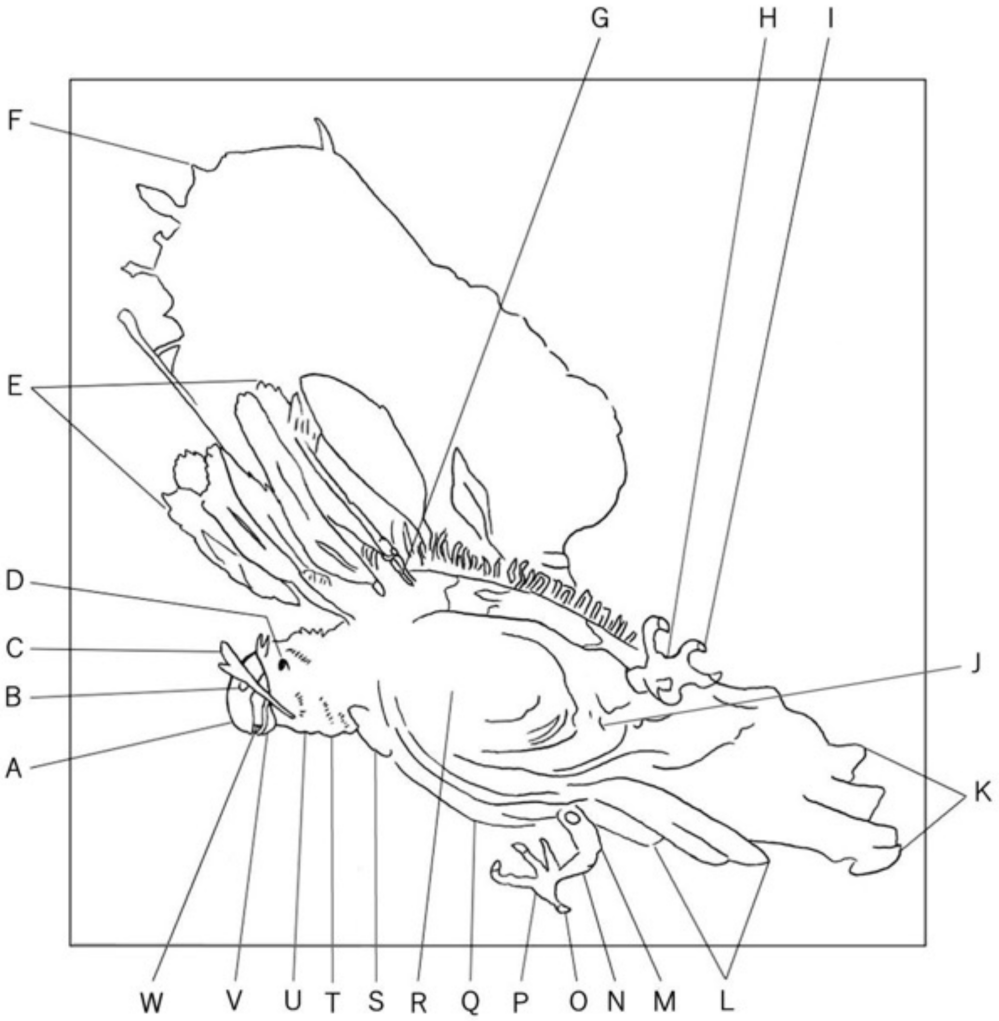

Figure 3. Avian formation. A) Beak. B) Cere. C) Crest. D) Eye. E) Primary flight feathers. F) Expanded wing. G) Feather shaft. H) Right foot and toes. I) Claw. J) Cloaca. K) Tail feathers. L) Upper tail feathers. M) Tibia. N) Tarsus joint. O) Claw. P) Left foot and toes. Q) Folded right wing. R) Abdomen. S) Crop. T) Neck. U) Head. V) Jaw. W) Tongue. Analytical drawing by George J. Haas, with notations by Erica Mollica, DVM. Image source: Mars Reconnaissance Orbiter HiRISE image ESP_020794_1860 (2021).

Examination of the formation at Argyre Basin (Figures 1, 2, and 3) reveals features of the avian species. Rostrally (left), one can see the beak, with its maxilla mandible surrounding the tongue. Features of the head are clearly visible. The cere is noted dorsal to the maxilla. The orbit, papillary margin, and opening of the external ear canal are evident. The head appears smooth and highly reflective. Down feathers are seen in the cervical area. Visible in the thoracic region is the left wing folded in a natural position. Primary feathers cover this region. Ventrally is the pectoral area ending at the point of the keel. Caudally (to the right) is the abdomen and right pelvic limb. Four digits, tarsometatarsus, and tibiotartus are clearly visible. The formation includes the proximal portion of tail feathers that extend from the rump (Saunders et al., 2011). There is also a protruding crop along the neck and evidence of a cloaca at the rear end of the avian formation. Just rostral and dorsal to the tail feathers is a left pelvic limb with four digits.

VETERINARIAN ANALYSES OF THE ANATOMICAL FEATURES OF THE AVIAN FORMATION

The first three veterinarians who contributed to the original paper were provided with copies of the 2021 MRO HiRISE of the Parrot Geoglyph and found all of its previously identified anatomical features to be consistent. They also identified a second foot, located just rostral and dorsal to the tail feathers, which included a left pelvic limb with four digits.[1]

A fourth veterinarian, Erica Mollica, DVM, contributed her analysis of the 2021 MRO HiRISE of the Parrot Geoglyph and found all of its previously identified anatomical features to be consistent, including the second foot. She also identified two additional anatomical features. She identified a cloaca (vent) at the rear end of the avian formation covered by shorter feathers on the lower abdomen, which appears as a slit that allows waste and eggs to pass. The second anatomical feature is the crop, a muscular pouch used as a storage compartment for food on the upper sternum area (thoracic inlet) in the avian formation. It is a dilated part of the esophagus and often protrudes out from the body.[2]

THE GEOLOGICAL CONTEXT FOR THE AVIAN FEATURE

The individual features that produce the avian formation within the Argyre Basin region of Mars are accurately depicted and remain clearly visible in the current MRO HiRISE image (Figure 1). The overall impression of this area of Mars is that regardless of the nature of the varied lithology or the nature of depositional and erosional agents, the avian-shaped formation is indeed exceptional in its physical appearance and anatomical completeness. While there are known geological mechanisms that can create the anatomical accuracies presented in this formation, the natural creation of a formation with 23 points of anatomical correctness seems to go well beyond the probability of chance. An expanded geological analysis of the avian feature can be found in the previous paper published in this journal during the Fall of 2011 (Saunders et al., 2011).

CONCLUSION AND RECOMMENDATIONS

With respect to the modeling of these anatomical features, the visual perception of this avian formation appears to have permanence and is not the result of a transient phenomenon or an illusionary projection. One interpretation is that this formation was originally a natural landform that was modified to illustrate the required features of a recognizable bird. Therefore, we conclude that the surface features that produce the unique avian components of this portrait are real and exhibit a level of consistency that is highly suspected of having artificial origins.

In an effort to expand our dataset, we recommend that NASA send a surface rover to examine the avian formation and utilize its new helicopter drone technology (Greicius, 2021). The drone can acquire close-up aerial views of these surface features from multiple angles and elevations. We also propose the use of the Shallow Subsurface Radar (SHARAD) instrument, which can detect changes in the electrical reflection characteristics of rock, sand, and any water that may be present in the surface and subsurface. High-density rock is highly conductive and provides a strong radar return. The instrument can detect changes in the reflection characteristics of the subsurface, caused by layers deposited by geological processes in the ancient history of Mars (Seu, 2006). If these avian features are found to be consistent, we would encourage the pursuit of a ground survey to determine the origins of this formation. We maintain that this site is a prime candidate for the study of potential archaeological artifacts on the surface of Mars.

ACKNOWLEDGMENTS

We would like to recognize the late Wilmer Faust for bringing this avian feature to our attention in 2002, and Zenith A. Haas for her assistance in completing this paper. Special thanks go out to the four veterinarians: Amelia Cole, DVM, Joseph Friedlander, DVM, Susan Orosz, DVM, and Erica Mollica, DVM for their in-depth analysis of the avian formation. We also acknowledge and thank NASA for the use of the Mars Reconnaissance Orbiter HiRISE image and its image data.

NOTES

[1] A personal communication with Amelia Cole, DVM, Joseph Friedlander, DVM, and Susan Orosz, DVM, April, 2021.

[2] A personal communication with Erica Mollica, DVM, Carroll Gardens Veterinary Group, February 27, 2022.

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Journal of Scientific Exploration
Anomalistics and Frontier Science

LETTER TO THE EDITOR

COVID: Orthodoxy or Conspiracy: Can the Center Hold?

Henry H. Bauer

Virginia Polytechnic Institute & State University

hhbauer@vt.edu
www.henryhbauer.homestead.com

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The conversation[1] between Walach and Bobrow about *The Real Anthony Fauci. Bill Gates, Big Pharma, and the Global War on Democracy and Public Health* by Robert F. Kennedy, Jr., deserves to be continued. It raises quite general issues about the role of science in modern society and the influence of politics. The two questions posed by Walach are a useful framework for discussion:

Is it true that we were faced with a "pandemic"?

Can we really trust our institutions?

A large factor in the contemporary dilemmas about COVID is that disagreeing groups offer their various dogmatic answers that are seemingly influenced more by guesswork and political attitudes than by facts of the matter.

The reality is that currently available data are not sufficiently complete or reliable to answer the first question. As Walach points out, the magnitude of the infection fatality rate (IFR) is a critical issue, and currently available data are inadequate, in particular as to the variation of IFR with age *and a detailed comparison of those numbers with the corresponding numbers for other respiratory infections*. Another important needed comparison with other infections is the overall excess mortality, where reliable data are again not yet available; that latter comparison is quite crucial in order to avoid distracting arguing about the basis for reporting COVID deaths, which varies by geographic region within countries as well as between countries and over time.

It does seem undeniable that *something relatively new* started happening towards the end of 2019, but it remains uncertain exactly what that has been, in particular whether it is radically different from the usual "flu season." The information promulgated by official agencies and the popular media is simply not yet reliable or complete.

That does present an answer to the second question, however: We cannot trust our institutions on this matter because they do not really know what they are talking about. That has been demonstrably obvious since expert advice and official actions have been different in different places and at different times: convincing, let alone conclusive, data are not yet available for deciding even in retrospect how effective or counterproductive were the various measures taken in different places such as masking, social distancing, closing of schools, and more.

A strong additional reason for not trusting the orthodox view is that the same authorities were wrong and continue to be wrong about HIV and AIDS. In particular, virology as a whole became unreliable over "HIV" by:

- adopting tests for which there is no gold standard because the virus itself has never been isolated in pure form from a supposedly infected individual without the intervention of purported amplification by growth in a culture medium
- enshrining the presence of apparent antibodies as proof of active infection
- unproven reliance on PCR as a supposed proof of active infection and measure of viral load

The contrarian claim that the very existence of the "HIV" virus has never even been established is increasingly supported by the lack of success *over four decades* to make a vaccine against it.

A significant point regarding COVID is the lack of cooperation and transparency from China, as well as the seeming inability of the Chinese authorities themselves to handle whatever is going on.

However, a strong argument against conspiracy theories is that incompetence is so much more common than active malice. There is also the sheer improbability that a conspiracy coordinated among Bill Gates, Anthony Fauci, the World Health Organization, and other actors and institutions could remain unexposed for a couple of years by leaks from internal whistleblowers.

Active malice is fortunately not common, but it is exemplified by Putin over Ukraine. It is not unlikely that deliberate spreading of misinformation about COVID is also occurring at Putin's behest, for example, the allegation that the United States has bio-weapons-research institutes inside Ukraine.

That illustrates the damaging influence of politics on matters of fact and science. One clear example of such influence over COVID is the case of Scott Atlas, who was for a brief few months an advisor in the Trump administration. Politically left-leaning media denigrated Atlas as unqualified and motivated by right-leaning political attitudes rather than by factual evidence; yet undeniable credentials show that Atlas was eminently qualified on matters of public health, and the record also shows that he criticized COVID orthodoxy from the very beginning, long before he joined the Trump Administration.[2]

Political preferences do unfortunately influence science in general nowadays.[3,4] For instance, dissent from HIV/AIDS or climate-change orthodoxy gets published (other than in the *Journal of Scientific Exploration*) only in politically right-of-center outlets.[5]

NOTES

1 Book review commentaries in *Journal of Scientific Exploration*, *36*(1), pp. 195–201.

2 Scott Atlas, "Will trust return?" in: *A plague upon our house: My fight at the Trump White House to stop COVID from destroying America*. Bombardier Books, 2021. https://www.youtube.com/watch?v=4mtz-GBgbS0&list=PLhYf_udPMpwyT6HUDD-vxjnC75diQVdpw&index=1&t=3186s

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5 For example: Henry H. Bauer, The mystery of HIV/AIDS, *Quadrant*, July–August 2006, 61–3; HIV tests are not HIV tests, *Journal of American Physicians and Surgeons*, *15*(2010), 5–9; Fact checking is needed in science also, *Academic Questions*, *34*(2021), 18–30.

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LETTER TO THE EDITOR

Reply to Bauer

Robert S. Bobrow

Department of Family, Population, and Preventive Medicine
Stony Brook University, New York

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"I don't care if it's horse piss. It works!"

— President John F. Kennedy

I don't know whom or what Dr. Bauer consults for his own medical care, but most of us rely on well-intentioned individuals doing the best they can with the tools they have on hand. Polymerase chain reaction (PCR), which can identify viruses and bacteria by their DNA or RNA within a few hours using a multiplication (amplification) technique, is such a tool. Identifying bacteria by culture takes several days, and viruses, which require living cells to reproduce, are difficult to culture at all. We use PCR to diagnose respiratory viruses such as influenza and Covid, as well as herpes viruses, bacteria that cause meningitis, sexually transmitted diseases, hepatitis B and C, and HIV. In the case of hepatitis and HIV, we measure "viral loads"—a quantification of virus particles using PCR. We can watch viral loads rise or fall as people become infected and then respond to treatment. This always correlates clinically: On becoming infected with HIV, the viral load rises rapidly coincident with flu or mononucleosis-like symptoms in at least 40% of people. The load reaches a peak at 6 months and plateaus (this is called the viral set point, and varies with individuals). HIV's target, the CD4 immunoactive cell, starts to decrease in number as soon as infection begins, and continues to decline as long as the viral loads are high. The higher the viral set point, the faster the decline. It may take 8–10 years until the CD4 count is low enough to cause symptoms, and then people become quite ill. Before antiviral therapy, more than 90% died. When I did hospital rotations in the late 1980s, an adult medical service of 12–15 patients always included at least one dying of AIDS, and usually someone younger than myself.

Another useful tool in medicine is antibody testing. The body makes antibodies in response to infections, and their presence, known as seropositivity, identifies past and present illness. It can take a number of days (for most infections) to weeks (Lyme disease) to months (HIV) for the body to mount a measurable antibody response. So culture and PCR, when available, are faster and allow treatment to begin. This is how well-intentioned people doing the best they can practice medicine.

Originally, we identified HIV-infected patients by their antibodies to HIV and declining CD4 cell counts. Then the technology to measure viral loads evolved. The HIV virus can be identified by viral culture or PCR in all seropositive HIV patients.[1,2] It can be seen with an electron microscope (Google it).[3] It parallels disease activity and CD4 counts rise as viral loads fall.

Now to Covid, which has killed more than a million Americans as of this writing. Prior

to vaccines, the greatest determinant of who died was age: 75% of deaths occurred in those over 65, as opposed to 4% in those under 45.[4] More than one in five persons 80 years or older who contracted Covid died (statistics from China; consistent with what I've seen in the US).[5] In 2018, less than one in a thousand of those over 65 with flu or pneumonia died.[6] Quite a difference. With influenza, the older and infirm are still generally at the greatest risk, but in some epidemics this varies, depending on immunity (antibody) levels from past infections in different age groups. In the great influenza pandemic of 1918, where an estimated 50 million died worldwide, younger people were at much greater risk of dying.

Influenza and pneumonia combined killed 55,672 Americans in 2017;[7] Covid killed an estimated 375,000 in 2020.[8] My hospital had more than 400 beds filled by Covid patients in April 2020. No flu epidemic ever approached that. Currently, the greatest determinant of who dies from Covid is vaccination status. An unvaccinated person was 58 times more likely to die (during Oct.–Nov. 2021) than a fully vaccinated and boosted one.[9]

Back to HIV. We still have no vaccine for it (nor do we have one for hepatitis C), but based on the premise that the HIV virus is the sole cause of AIDS, drugs were developed to target it. Now, using these drugs in combination, HIV-infected Americans who seek medical care before their infection is far advanced can expect a life expectancy approaching that of the general population.[10] Which is to say: "It works!"

NOTES

1 https://pubmed.ncbi.nlm.nih.gov/2298875/

2 https://www.ncbi.nlm.nih.gov/pmc/articles/PMC266626/pdf/jcm00079-0182.pdf

3 https://www.google.com/search?rlz=1C1OKWM_enUS787US787&q=Can+you+see+HIV+in+electron+microscope?&tbm=isch&source=iu&ictx=1&vet=1&fir=rV6Ubidn9gTpUM%252C1H0laxiAsNFYRM%252C_&usg=AI4_-kQce5G-SKLrGEtuFRdui4twPseBCg&sa=X&ved=2ahUKEwi5ieT3npz4AhUbdDABHRFyAVoQ9

4 https://www.cdc.gov/nchs/covid19/mortality-overview.htm

5 https://www.worldometers.info/coronavirus/coronavirus-age-sex-demographics/

6 https://www.cdc.gov/mmwr/volumes/69/wr/mm6940a5.htm

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Anomalistics and Frontier Science

LETTER TO THE EDITOR

COVID-19—Conspiracy or Not? Some Thoughts on Bauer and Bobrow

Harald Walach

hw@chs-institute.org

https://harald-walach.info

Next Society Institute
Kazimieras Simonavicius University
Vilnius, Lithuania

Change Health Science Institute
Berlin, Germany

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PLATINUM OPEN ACCESS

I am pleased about the discussion which my review of Kennedy's book *The Real Anthony Fauci* has stimulated. It is a signature of the COVID-19 crisis that scientific discourse has broken down. There seem to be only two camps: those who "believe" in the mainstream narrative that COVID-19 is a deadly killer virus with far above average Case Fatality Rates that could only be halted by drastic non-pharmaceutical interventions such as complete lockdowns, stay-at-home orders, and mask mandates; for which no treatment existed; which had to be spotted early by broad coverage of PCR tests even of asymptomatic people; and for which the emergency use admission of rapidly developed, badly tested vaccinations was the only and thus legally warranted action.

And on the other side the "conspirationists:" those who think that the whole story was overblown, that the virus was manufactured, either by China or some other secret service, to trigger a crisis that either served the pharmaceutical industry in developing and marketing a completely new brand of preventive pharmaceuticals, mRNA, and vector vaccines; and/or to issue vaccine passports that serve the larger purpose of having complete control of the population in a China-style system of social compliance points that allow access to privileges such as travel, holidays, etc.; or even a coup d'état serving to abolish our democratic system.

This thinking in camps is obvious in the public discourse in the mainstream media, TV, and print. It is obvious in the scientific discourse, where critical voices are sidelined into small outlets or penalized by retractions. It is obvious in the discussion between Bobrow, myself, and Bauer, with Bauer holding a middle ground by admitting to human incompetency as the most likely factor of the crisis (which I sympathize with, but won't discuss further). I will by no means be able to address all these points in a short commentary. Especially, I will not get into the HIV/AIDS-debate, as this is probably even more complicated than the COVID-19 debate. But let me pick out a few obvious points.

The Seemingly High Case Fatality Rate

Bobrow's reply to Bauer noted the enormous death toll of COVID-19 documented on dashboards all over the world, especially in and for the US. At the face of it, it is true: Many fatalities are attributed to SARS-CoV2 as the causative agent, many more than are usually registered for flu. However, there are a few points to be considered.

In no country has there been a clear definition what counts as a "COVID-19 death." In Germany, doctors and pathologists were explicitly forbidden by public health authorities to do autopsies in the first phase of the pandemic to ascertain causes of deaths, and I believe the same was true in many other countries. Some pathologists did so nevertheless and published findings that at most 30% to 40% of those COVID-19 deaths are directly attributable to the virus as a cause. The rest died of underlying diseases. This is a common pattern in old people: Their system is weak, and a respiratory virus kills off the patient. Now, interestingly, these data never saw the light of peer-reviewed day, be-

cause in one case the pathologist received an express order not to publish by his university. And I have it on trustworthy evidence that this order was underwritten by a threat to personal consequences. Why would that happen if we were talking about a purely scientific dispute?

So, we can take it that those numbers are completely unreliable, because they are not validated. Had our, and for that matter the US, public health authorities been interested in true causal attribution, they would have ordered well-taken samples of autopsies to determine causality and approximate percentage of deaths of people in whom PCR-tests for SARS-CoV2 was positively and truly attributable to the virus. The fact that this was not only not done, but actively sabotaged by authorities is a far cry from incompetence in my eyes, and if incompetence it would be a type that is punishable.

And in fact, as referenced by me in my original review: Meta-analyses of infection-fatality rate—this is quite different from case-fatality rates—revealed that there is no difference from influenza (Ioannidis, 2021). The overblown case-fatality rate is manufactured or an artefact, depending on your view, produced by counting every fatality with a positive COVID-19 PCR test as a COVID-19 death.

The PCR Test

Bobrow also pointed out that the PCR test allows us to diagnose viruses and determine the viral load. Both statements are only partially correct. For the question of what is being found and diagnosed is highly dependent on the primer samples and on the cycle-threshold used. The cycle-threshold, i.e., the number of amplifications that are being conducted, were originally 45 with the test published by Drosten and his team that became the blueprint and standard used by the WHO and other institutions (Corman et al., 2020). I have been and still am conducting expert interviews. I have spoken to academic experts who have been working with PCR tests all their careers. They confirm: Such a high amplification threshold is never used if one wants to detect live virus or infectious agents. And indeed, meta-analyses of studies show that beyond a cycle-threshold of 22 no infectious agent is discoverable, only RNA-fragments (Jefferson et al., 2020; Stang et al., 2021). These fragments can stem from a previous infection, they can be signs of contamination, but they do not constitute proof of infection, let alone infectiousness. Yet such PCR results have been used to determine "COVID-19 cases" and "COVID-19 fatalities." This is, again, a far cry from good laboratory and scientific practice.

Although it is a legal requirement to indicate the number of cycles used for testing, the official documents issued in Germany have, as a rule, not given this information. I know from talking to people working in such labs and from informal information that the standard practice was and still is to use 35 to 37 cycles of amplification, far beyond the 22 cycles known to be the threshold for identifying a viral load that is associated with potential infectivity.

Perhaps a clarification is in order here: PCR tests cannot, in principle, determine viral loads or infectivity because they have to break down all material, denaturalize it to test for DNA or RNA sequences. The conclusion that someone is infected or infectious, or to deduce the viral load associated with it, is entirely indirect and crucially dependent on the number of amplification cycles.

Now, given that all persons in relevant positions to make decisions about cycle thresholds and associated practices actually know this: Why would one want to gear the whole system of diagnostics towards oversensitivity? Does anyone have a natural and innocent explanation? I have so far not found one, and therefore can only conclude that our institutions are either incompetent (Bauer) or malicious (Kennedy and "conspirationists"), and most likely both.

The Novel Vaccines with Mandates, Vaccine Passports, and Aggressive Campaigns

The Public Health Emergency of International Concern (PHEIC) was declared by the WHO after a short deliberation period advised by a panel of experts—a fact that was even criticized by one of the recent pandemic preparedness exercises on a monkeypox emergency (Yassif et al., 2021). It is obvious: There would never have been a chance of having emergency-use admission to market these new vaccines without such a PHEIC. A second concern is the fact that there are no medications that can be used for treatment. I will deal with this latter point in the next paragraph.

These novel vaccines introduce a completely new pharmaceutical principle, and it is well worth remembering: This principle has so far failed to work in cancer, for which these techniques have been originally developed, and it has failed as an HIV vaccine, which is well-documented by Kennedy (2021). The company that developed those vaccines in Germany, BioNTech, had developed the technique as a cancer remedy. It did not work and the company was actually insolvent before Bill Gates came in and bought huge shares in it, a fact I have on good evidence from my interviews. Another of my interview partners was working on mRNA-based medications against cancer for the German government 10 years ago. They abandoned this research track because the substances violate one basic principle of pharmacology: The dose, or amount of stuff they produce, cannot be controlled. In other words: No one

knows how much of the end-product is being produced by the cells—and by which cells—the mRNA happens to be hosted in. This problem has not been solved. Thus, we are working with vaccines that violate one basic principle of pharmacology: to know with what dose of the end product we are treating an organism.

The second problem is that the lipid-nano particles that are used to package the mRNA are themselves toxic because they are highly inflammatory (Ndeupen et al., 2021) and do not have a human use clearance, to my knowledge in any country, certainly not in Germany and the EU. But they received indirect clearance with the emergency use of the vaccines. The risk–benefit ratio of these vaccines is terribly bad. We were the first to point this out (Walach et al., 2021a). That paper was retracted after protests and shortly afterwards republished (Walach et al., 2021b). One of the protests came from the head of pharmacovigilance in the Netherlands, whose data we had used. At the time, the pharmacovigilance data in Holland showed four suspected deaths per 100,000 vaccinations (now it is two). We used data from the then largest observational study to calculate that we are saving at the most six lives per 100,000 vaccinations.

Meanwhile, the six-month Pfizer study became available, which allowed us to calculate that in fact we are maximally saving five lives per 100,000 vaccinations (Walach, et al., 2022). The German pharmacovigilance data show that there were 1,802 deaths associated with COVID-19 vaccines as of September 30, 2021, which is more suspected deaths than that of all other vaccines together since inception of the database beginning of 2000 by a factor of 28, or by a factor of 560 more per year. One should consider that such passive monitoring systems like adverse reaction databases are underestimating effects by more than 80%, as direct comparisons and a meta-analysis of such comparisons in other cases show (Alatawi & Hansen, 2017; Hazell & Shakri, 2006). It is for future systematic cohort studies actively documenting benefits and risks that do not exist so far (Wu et al., 2021) to cast the final word. This is difficult, because all ongoing long-term studies have been unblinded so that no long-term control groups exist (Tanveer et al., 2021).

But perhaps one glance at the US all-cause mortality data gleaned from the CDC website says it all (Figure 1). Figure 1 presents the all-cause mortality dashboard data of the US Centers for Disease Control and Prevention website. The blue bars represent the weekly mortality data for the US. The orange and red lines represent the expected number of deaths and the upper boundary beyond which excess mortality occurs.

As can be easily seen on the left side, there is the last hint of excess mortality from the flu season of 2017/2018, which is more visible on older dashboards that go farther back. Then there is a small dip which signals less than expected mortality between January 2019 and the beginning of 2020. Then we see the sharp rise at the beginning of 2020, which is attributed to the first wave of COVID-19, and a smaller peak signaling the second SARS-CoV2 wave in summer 2020. That was when the vaccines were sent through regulation and the vaccination campaign began in the last weeks of 2020 and the beginning of 2021. This coincides with the largest excess mortality peak in the data

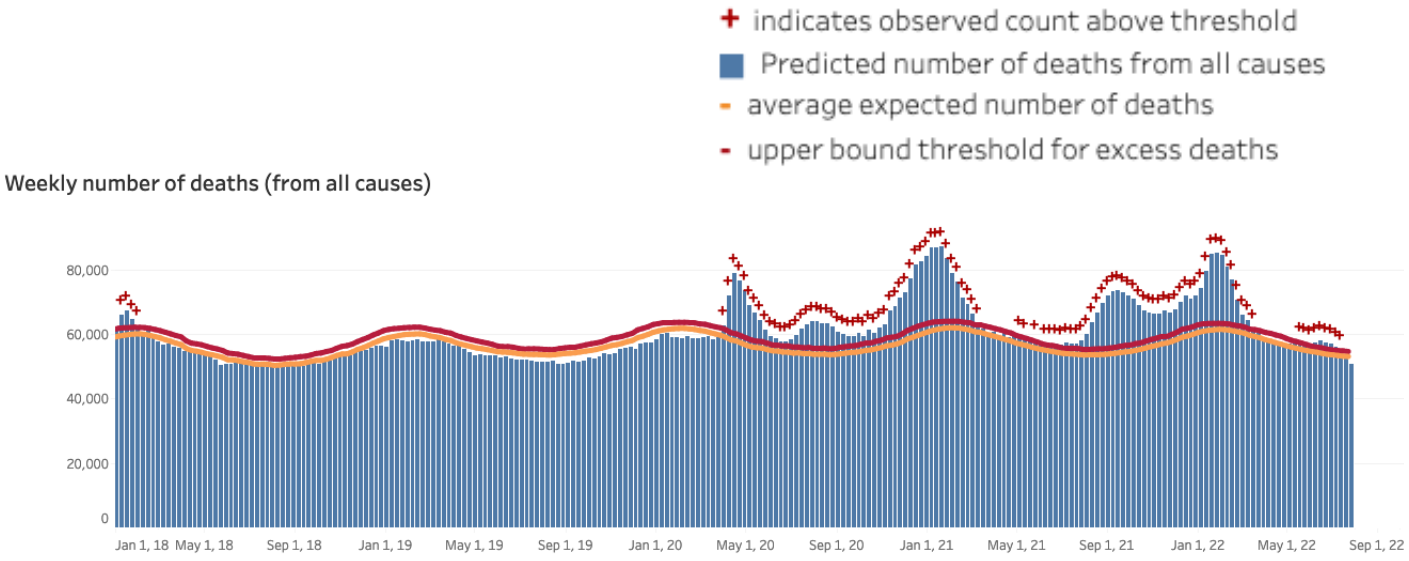

Figure 1. All Cause Mortality United States–Excess Death Rates CDC Data. https://www.cdc.gov/nchs/nvss/vsrr/covid19/excess_deaths.htm#dashboard

series so far. The vaccine did not seem to do what it was meant to do. It did not lower mortality rates. If it was causal for anything, then it was causal for increasing mortality.

A case can be made that vaccinating into a rising epidemic curve of any epidemic is a clinical stupidity. So, this might explain the peak at the start of 2021. But then, surely, one would have expected that the vaccinations did their job and prevented further deaths, at least of and with SARS-CoV2. And since this was supposed to be the major health hazard since the Spanish flu of 1918, there should have been a reduction of mortality later on, or even a below-average mortality. But what we see is the contrary: large peaks at the end of 2021 and beginning of 2022, which taken together outrun the supposed COVID-19 peak at the beginning of 2020 easily by an order of magnitude. Where, I would dare to ask, is the signature of a preventative effect of the COVID-19 vaccines? And let us remember: These vaccines were developed and sped through regulation to counter a fatal threat, a killer virus, which would be indomitable without a vaccine that should supposedly reduce mortality. After it became clear that the vaccines would not do what they were said to do, the claims were toned down to "reduce infection rates," then "reduce severity of the disease," "reduce the burden on the health system," none of which was actually proven to be true but was only claimed to be the case based on occasional and anecdotal evidence. Where are the hard data proving that these vaccines prevent deaths and prevent serious illness? I am sorry, Dr. Bobrow, but they do not exist, and I do not find the data in the references you mention.

What you can find, though, in the CDC data (though I won't go into the details here), following the link in Figure 1 and choosing different US States: The mortality peaks, numbers, and patterns for neighbor states, even counties, are so different that the pattern is incompatible with a unified cause such as an epidemically spreading virus that does not stop at state borders. Take Maine and Massachusetts as an example. In Maine you do not see any relevant peak until the end of 2021, while in Massachusetts you see a steep peak at the beginning of 2020 and then a smaller one at the end of 2021, exactly the opposite. The same is true in Europe: Belgium had some of the highest excess mortality data in Europe, while Germany, which borders Belgium, had the lowest before the introduction of the vaccines. Why would an infectious agent that is invariably deadly stop at the border? True, these are all-cause mortality data, and this is what is most interesting, because they are the most robust data. Perhaps they also tell us a different story? Not only that the vaccines were not effective, but that they were even dangerous? This is difficult to prove in the absence of control groups. We are currently trying to disentangle this with a modeling study.

But what is clear from the data is that the vaccine mandates, which are currently crumbling, were unwarranted where they have been introduced. Were they only introduced to enforce vaccine passports, including electronic monitoring systems, piggybacking on the mandates as "necessary" control procedures? We don't know. But I think it is obvious that the argument is not so far off the mark as some would like it to be. At least in Europe, I observed a vicious campaign against persons who refused the vaccine, with political arguments from the political Middle Ages based on "scientific" claims that were neither true nor scientific, as every new study that was published showed. What are we to make of such campaigns, supported by the most powerful TV and mainstream print media around? Had they spoken the truth, no one would have objected. The fact that a loud and vocal minority protested in numbers unseen and unheard of since the time of nuclear armaments in Europe back in the '70s and '80s is a hint that it was not the truth that was promulgated by politicians and media, but a political agenda.

The Myth of a Lack of Early Treatment for COVID-19

I am not a physician. So, I refer to secondary data. One of the prerequisites of Emergency Use Approval of the COVID-19 vaccines was, apart from PHEIC, the lack of potential early treatments. The CDC, NIH, FDA, and other guidelines said so and stipulated: Do not treat these patients unless deterioration sets in, and then start emergency treatment in a hospital. Early on critical care physicians published early treatment protocols (McCullough et al., 2021), and reportedly treated many hundreds of patients successfully with it without hospital admissions. Some of the agents, ivermectin for instance, were attacked by the CDC in broad campaigns. An independent group of high-profile US researchers at academic centers started a website, which I recommend all readers peruse: https://ivmmeta.com/ and https://c19early.com/#fpall. The first is a meta-analysis of all ivermectin studies for COVID-19 and the second compares different treatments for COVID-19. The first meta-analysis shows a huge benefit for ivermectin-treated patients. The second analysis shows the full range of treatments. Again, ivermectin ranges high, and other treatments, including vitamin D, are far more effective than the only one advocated by official sources in the US, remdesivir. Why, one might ask, is it that such a meta-analysis was not put together by official sources but by a crew of highly competent, yet anonymous academics in the US?

The simple fact that these data exist, that you will have trouble finding the website by a simple search, and that the

content of this website is actively and powerfully battled against by the most important public health authority in the US tells you that Bauer is right: Trust into institutions has eroded, to put it mildly. The question of whether the COVID-19 associated deaths were due to the virus, or perhaps due to the various NPIs and their distal consequences we have not even touched upon.

Conclusion

So, the facts are: The preconditions for the new vaccines did not exist from the beginning. The pandemic, although surely associated with a high number of fatalities, was not the killer pandemic it was said to be. The disease was treatable and severe consequences preventable in many cases. This was exactly what was prevented by official propagations and policy. At the same time, these preconditions, a dangerous epidemic with no treatment available, were necessary for a positive regulation for novel vaccines. These vaccines do not do what they were meant to do, yet the discourse about this fact is non-existent and is combatted at all levels. Exactly what should a rational agent conclude, who is neither in bed with Republicans nor Democrats, as I, as a European, am?

No, this is not political either, or a conflict between conservative or progressive, green-liberal or brown-revanchist, as so many columnists want it to be. There is a third position here, i.e. looking at the facts without preconceptions and then thinking about the consequences. And one can easily see: It's the economy, stupid. There is a famous wager by the 17th-century philosopher Blaise Pascal. He used it as an argument for the belief in God. I would like to slightly tweak it. We do not know the truth. But if the "conspirationists" are right and this whole issue is a big mistake on the part of our authorities to try to hide the disaster, or if there is some even more sinister goal behind it, then it is safer to follow this line of reasoning than to ignore this option and keep on trusting. I feel that the burden of proof has already shifted to those who believe that the mainstream narrative is correct and who stipulate that the vaccines are ultimately safe.

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Journal of Scientific Exploration
Anomalistics and Frontier Science

COMMENTARY

Introduction to the Special Subsection: Contemplating the BICS Essay Contest on Postmortem Survival

James Houran

editor@scientificexploration.org
https://orcid.org/0000-0003-1725-582X

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The answers we get from science depend on the questions that we dare to ask . . . and fund.

Ed Lantz (2022), SSE Symposium: Advanced Energy Concepts Challenging the 2nd Law—Panel Discussion (https://www.youtube.com/watch?v=B7DucTuE2Fk)

Well-known aerospace entrepreneur Robert T. Bigelow has contributed a great deal privately and publicly to science and technology over the years, including the realm of anomalistics and edge science [see: Kelleher, C. A., & Knapp, G. (2005). *Hunt for the skinwalker: Science confronts the unexplained at a remote ranch in Utah*. Paraview Pocket Books]. This generous support should be celebrated, as it is virtually unique in modern times where academic freedom and consequential funding are sparse in the controversial fields that *JSE* routinely spotlights. In fact, Bigelow recently formed the Bigelow Institute of Consciousness Studies (BICS) to support research on the ostensible survival of human consciousness after physical death and the potential nature of such a state. BICS therefore comprises an ongoing platform for exploration and education versus a singular act of support from a lone patron. Among the organization's first initiatives was a global campaign to solicit the best evidence supporting the notion of postmortem survival. This venture paralleled successful 'crowdsourcing contests' that some companies use to drive product improvements or innovations via public competitions with cash awards [see: Segev, E. (2020). Crowdsourcing contests. *European Journal of Operational Research*, *281*, 241–255. https://doi.org/10.1016/j.ejor.2019.02.057].

The media coverage that BICS garnered from the contest was a major success from a publicity standpoint. However, its format and outcomes have been the topic of much discussion and debate even among advocates of the survival hypothesis. In a very real sense, the BICS Essay Contest was not an end but instead the beginning of further scientific discovery and discourse. The observations, arguments, and insights from the winning entries hold important learnings about (a) the criteria and logic the judges used to evaluate the proffered evidence, and (b) the commonalities or discrepancies in the evidence and arguments that were deemed the most compelling [see: Tressoldi, P., Rock, A. J., Pederzoli, L., & Houran, J. (2022). The case for postmortem survival from the winners of the Bigelow Institute for Consciousness Studies essay contest: A level of evidence analysis. *Australian Journal of Parapsychology*, *22*, 7–29]. Therefore, it can be argued that the BICS essays transcend their status as end products by serving as valuable and new data points in a highly

contentious research domain. This view and approach motivated the creation of our Special Subsection, which was realized with the gracious support and tactical assistance of John Alexander, Stephen Braude, and Michael Sudduth.

Mr. Bigelow and Colm Kelleher in the first Commentary succinctly explain the original motivations and broader aims of the essay contest. This context gives the critical backdrop against which the subsequent Commentaries are set. Prominent skeptic of the survival hypothesis Keith Augustine next presents a detailed critique of representative essays that won BICS awards. The idea was not to invite a scholar to flatly discredit the essays but rather for an informed but critical eye to evaluate their quality of reasoning and consistency of evidence from a viewpoint that was perhaps underrepresented in the original pool of judges. A team of survival researchers and advocates (Stephen Braude, Imants Barušs, Arnaud Delorme, Dean Radin, and Helané Wahbeh) then provide counterarguments to Augustine in their Commentary, which he immediately addresses in a targeted rebuttal. Finally, this Special Subsection ends with an adversarial collaboration. Augustine is joined by fellow skeptic Etienne LeBel and survival agnostic Adam Rock, all of whom endorse experimental methods in this domain. This new team was tasked with finding common ground and a path forward that constructively advances the conversation. The result is a proposed investigation that builds on published work by some of Braude's team members, and other prior approaches, and aims to satisfy parameters set both by skeptics and advocates. Note that this effort draws on Honorton and Hyman's notable example with experimental research on putative psi [see: Honorton, C., & Hyman, R. (1986). A joint communiqué: The psi ganzfeld controversy. *Journal of Parapsychology*, *50*, 351–364] and can serve as the model for a pre-registered study protocol.

The following collection of material will not settle any debates, but it is intended to inform and motivate new research designs that leverage cooperative efforts and team participatory science. It might sound like an absurd assertion, but some skeptical researchers certainly share the same value set and sincere motivation for discovery as do many survivalists. It is thus the fervent conviction of the *Journal*'s editorial team that good faith collaborations are possible, advances in research designs and data collection can be achieved, and important new model-building and theory formation can be tackled with respect to the survival hypothesis. It will undeniably take time and continued financial support from bold institutions and benefactors (like BICS or the Bial Foundation) who rise to the challenge posed in Ed Lantz's introductory quote. Our team further hopes that this Special Subsection will be counted among the first steps in this direction, which is already being paved by the publication of some cross-disciplinary efforts [see: Parnia, S., et al. (2022). Guidelines and standards for the study of death and recalled experiences of death—A multidisciplinary consensus statement and proposed future directions. *Annals of the New York Academy of Sciences*. https://doi.org/10.1111/nyas.14740]. We thank all the contributors for their time and effort in preparing these invited commentaries. The opinions expressed here are solely those of the various authors and the usual disclaimers apply. Special appreciation goes to Brian Laythe and Adam J. Rock for their assistance with the editorial reviews of these collected works.

Journal of Scientific Exploration
Anomalistics and Frontier Science

COMMENTARY

The 2021 Bigelow Institute for Consciousness Studies (BICS) Essay Contest

Robert T. Bigelow

Colm A. Kelleher

Bigelow Institute for Consciousness Studies
Las Vegas, NV

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HIGHLIGHTS

The BICS essay contest successfully increased attention to the latest and perhaps most compelling scientific research on the topic of "life after death."

Robert T. Bigelow

ABSTRACT

BICS was founded in 2020 to communicate, facilitate, educate, and organize scientific research and exploration into the survival of human consciousness (SOHC) after permanent bodily death. In early 2021 BICS announced an essay contest on the best evidence for survival of human consciousness with $950,000 in prizes. Essay contestants were tasked to present evidence for SOHC after death beyond a reasonable doubt. "Beyond a reasonable doubt" is the most demanding and rigorous burden of proof in the criminal justice legal system and BICS challenged essay contestants to achieve this very high bar in order to maximize the focus on presentation of evidence from as many directions and sources as possible. By December 2020, seven respected judges from the research and academic communities were recruited. BICS received 1300 applications from all continents and invited 264 authors and groups of authors to submit essays. 204 essays were received from 38 countries. Following an intensive five-month judging process, a large group of essays was down-selected. So high was the standard among the dozens of essays that the judges had difficulty in choosing three essays for the top prizes. After several more rounds, eventually 29 essays were chosen and the prize money was increased from $950,000 to $1,800,000. Prizes were awarded at a gala in Las Vegas on December 4, 2021. Following the conclusion of the BICS essay contest, the organization is preparing for another major program. On September 1, 2022, BICS will unveil "Challenge 2023," which is a targeted grants program that is aimed at funding research proposals on SOHC with a particular focus on contact and communication with the afterlife.

INTRODUCTION

The Bigelow Institute for Consciousness Studies (BICS) essay contest that was held in 2021 was arguably the most successful essay contest in the history of Survival of Human Consciousness (SOHC) research, both in the United States and internationally. By awarding a total of $1,800,000 in prizes to 43 essay authors for 29 essays, the contest became a global phenomenon and galvanized more than 1300 contestants to submit applications to enter the

competition. The question that contest authors attempted to answer in no more than 25,000 words was: What is the Best Evidence for Survival of Human Consciousness After Permanent Bodily Death?

Out of the 1300 applications, BICS chose 264 individuals and groups to submit essays, of which 204 did so before the deadline. In the fall of 2020, BICS interviewed and recruited a team of scientists, physicians, and researchers to serve as BICS essay judges. Due to the high level of complexity inherent in the field of SOHC, BICS made the decision to cast a wide net for the recruitment of judges to incorporate broad expertise into the evaluation of the essays. It was the task of the seven judges to read, evaluate, and rank the 204 submitted papers.

The purpose of this article is to describe the origin and execution of, and next steps following, the BICS essay contest.

WHAT IS BICS?

BICS was founded in March 2020 by aerospace entrepreneur Robert T. Bigelow to support research into both the survival of human consciousness after physical death and, based on data from such studies, the nature of the afterlife.

Despite intriguing evidence, the number of research groups and funding devoted to investigating the SOHC beyond death is surprisingly small in the Western world. The purpose of BICS is to raise awareness among the public and within the scientific community of the importance and relevance of the survival of consciousness after death and the afterlife.

BICS hopes to serve as a facilitating and organizing catalyst within this field in North America, Europe, and South America. BICS hopes to facilitate communication and foster synergy between individuals and groups researching the subject. BICS also aspires to serve as a partner and coordinator in encouraging novel research initiatives in this unique area.

In April 2022, BICS released its Mission Statement:

> *The mission of BICS is to communicate, facilitate, educate and organize scientific research and exploration into the survival of human consciousness. These efforts shall focus particularly on consciousness surviving permanent bodily death. Additionally, research and investigations shall embrace all areas of human consciousness that can acquire information and/or cause effects upon space, time, animate or inanimate subject matter without the use of conventional means.*

BICS as a Facilitator of Communication:

- BICS will engage in outreach and communications with existing survival of consciousness (SOC) organizations and individuals based in the US and internationally. The communications will serve to foster commonality and to encourage discussions on projects and programs between groups and individuals.
- Publication of a six-volume set of 28 essays for distribution to hospices, selected religious organizations, university libraries, and other groups involved with the Afterlife.
- Beginning in 2024, depending on the pace of personnel recruitment, BICS will begin organizing BICS symposia, conferences, and small gatherings on the topic of SOC in different locations in the United States. These gatherings will be funded by BICS (there will be no registration fee).
- Outreach and communication of BICS material to psychiatrists, psychologists, religious groups, hospice, and other groups.
- Promoting communication by means of the BICS website and social media outreach.

Origins and Aims of the BICS Essay Contest

As of 2022, BICS is not aware of previous essay contests in the SOHC field from which to draw comparisons. The essay contest therefore could be considered a unique event in the history of SOHC research.

Opinion polls over decades in the United States and in many countries have consistently shown that a high percentage of the public believes in life after death (see referenced Roper Poll, Roper Organization, n.d.). This belief appears to be global and, according to a recent survey, is held by 79% of hunter–gather societies in different countries (Peoples et al., 2016). This acceptance is not necessarily shared by mainstream science. BICS initiated the contest knowing that a significant percentage of scientists and some sectors of organized religions were skeptical of, or even antagonistic toward, research into the SOHC topic.

The purpose of the BICS Essay Contest was threefold:

(a) Energize the field of SOHC after Death.

(b) Encourage more individuals and groups to learn about and take an interest in the field.

(c) Delineate the global state-of-the-art research in 2021 on SOHC after Death.

Energizing the SOHC Field

While there are numerous individual professors at several universities in the US studying survival, there is only one academic department affiliated with a University

staffed by several researchers and students (critical mass) in the US, located at the University of Virginia. The newly established Exceptional Experiences Research lab (EERL) at the University of West Georgia also has an academic affiliation. In addition, a number of high-quality research organizations such as the Institute of Noetic Sciences (IONS), the Parapsychological Association (PA), the Parapsychology Foundation (PF), the Society for Psychical Research (SPR), the International Association for Near Death Studies (IANDS), Windbridge Research Center, and others compete for limited funding and resources. There are scattered researchers and institutions in the United Kingdom, Europe, South America, and the Far East/Pacific that also are funded by small research grants.

It could be argued that the lack of research dollars, personnel, and resources over multiple decades may be one of the most important factors that has kept the SOHC field from thriving and expanding.

On September 1, 2022, BICS will announce a substantial grants program for 2023 that will help support research in the field of SOHC (see last page for more details). It is intended that an increase in grant funding may contribute to energizing the SOHC research field.

Encourage More Groups and Individuals to Enter the Field

BICS announced the essay contest with publicity in print journalism (*The New York Times*), TV reports, and multiple digital stories on the Internet. The result was many expressions of interest (more than 6000 emails to BICS) from more than 40 countries. The BICS application process stipulated that all entrants must sign a copy of the rules and BICS encouraged applicants to summarize their background and qualifications to enter the competition.

The application form (Appendix 1) contained the essay guidelines as well as a release agreement, in the following section BICS requested that applicants demonstrate some basic evidence of prior interest and experience in the area:

a) Applicants must provide evidence that they have investigated the topic of Survival of Human Consciousness after Death for 5 years or more. For multi-author submissions, only one author must have 5 years of experience in the topic, while the other authors can be less-qualified. Evidence of investigation such as published papers, monographs, books, or producers/directors of film or digital documentaries will be accepted. Embedded links to written or digital work will be welcomed as part of the application.

b) It may help if applicants are/were a member of relevant organizations, for example, the Society of Psychical Research, Society for Scientific Exploration, American Society for Psychical Research, Institute of Noetic Sciences, International Association for Near Death Studies, Rhine Research Center, Windbridge Research Center, Parapsychological Association, Eternea, Center for Research on Consciousness and Anomalous Psychology (CERCAP), Koestler Parapsychology Unit (University of Edinburgh, Scotland), NUPES—Research Center in Spirituality and Health (Brazil), or be a staff researcher (including emeritus) at an accredited university. Submission of curricula vitae is allowed.

c) Applicant may be an ordained member of a religious order or group. Formal ordination, or membership in religious orders, is relevant only if the affiliation relates to scholarly works in the body of the essay.

d) BICS included the statement: "*BICS understands that true (or veridical) evidence includes a combination of a wide variety of forms; scientific, experiential, witnessed, repeatable, anecdotal and otherwise persuasive far beyond rules of traditional evidence-based hypothesis tested research paradigms*" into the essay guidelines in order to broaden the scope beyond evidence from peer reviewed scientific research.

Further, in order to encourage a broader spectrum of applicants to enter the contest, BICS expanded the entry criteria and included the following paragraph in the application: "*BICS encourages, and will accept, the inclusion of credible eyewitness and witness testimony as valid evidence for the Survival of Human Consciousness after Death. Eyewitness testimony is a bedrock for conviction by juries and judges in the United States and in International Court Systems. Therefore, BICS will accept evidence and eyewitness testimony supporting the legal requirement that establishes proof beyond a reasonable doubt.*" This phrase "proof beyond a reasonable doubt" was included in an attempt to encourage essay applicants to cite additional evidence in their essays that lay beyond the traditional scientific standardized levels of evidence (see, for example Tressoldi et al., 2022). In doing so, BICS was placing a value on eyewitness testimony and personal anomalous experiences as potential supporting evidence for submitting a 25,000-word essay on the SOHC. In addition to scientific evidence hierarchy (Tressoldi et al., 2022) that included reviews of experimental studies, BICS was also interested in the potential inclusion of ethnographic and participant observer research in essays.

Both the publicity in the media and the broadening of the qualification process were overt attempts by BICS to encourage more individuals and groups to enter the essay competition and thus, potentially, to enter the field. Multiple anecdotal followup messages from essay authors informed BICS that the advertising of monetary rewards for essays was the major motivator for initiating research into multiple

SOHC areas they had not worked on before. Thus the essay competition played a role in broadening the SOHC field.

Delineate the Current Global State-of-the-Art Research on SOHC after Death

Evidence that the word had spread globally came from the 38 countries that hosted multiple applicants approved by BICS to submit essays. Table 1 summarizes the countries from which the majority of essays were submitted.

In addition, one essay was submitted by authors from each of the following countries: Kuwait, Hungary, Venezuela, Ecuador, Taiwan, Bolivia, Slovakia, Nigeria, Egypt, Belgium, Slovenia, Cuba, Sri Lanka, Singapore, Sweden, Dominican Republic, Austria, Romania, and South Korea.

The spectrum of countries spanning all continents indicated that research on SOHC was occurring across the planet and was somewhat independent of culture. These data, as well as the content of the essays submitted, gave an indication to BICS of the state of research into the SOHC across the planet.

Judging of the Essays

Between July and December 2020, BICS senior management recruited a panel of judges that had broad backgrounds in diverse subjects that collectively constitute the SOHC after death. BICS decided that simply having a panel of judges that had a deep level of expertise only in the SOHC would be insufficient to adequately judge the anticipated broad spectrum of technical (physics, biology, neuroscience, medical science) and participant observer research-based essays. Secondly, it was felt that judges with varied eclectic backgrounds in medical sciences, statistics, humanities, SOHC research, and theoretical physics would contribute expertise across the maximum spectrum of anticipated subject areas.

Who Were the Judges?

Jeffrey J. Kripal, Ph.D., is the Associate Dean of the Faculty and Graduate Programs in the School of the Humanities and the J. Newton Rayzor Chair of Philosophy and Religious Thought at Rice University. He is also the Associate Director of the Center for Theory and Research at the Esalen Institute in Big Sur, California.

Leslie Kean is an independent investigative journalist. She is the author of the award-winning *Surviving Death: A Journalist Investigates Evidence for an Afterlife* (Crown Archetype, 2017) and *UFOs: Generals, Pilots, and Government Officials Go on the Record* (Crown Publishing Group, 2010), a *New York Times* best-seller.

Christopher C. Green, M.D., Ph.D., worked for the U.S. government from 1969 to 1985 and was awarded the National Intelligence Medal for his work in forensic investigations. Currently, he applies his 40-plus-year background in neurophysiology and forensic neurology with a new clinical focus using high-field brain MRI to determine the differential diagnoses of neurodegenerative disease and study the neural aspects of cognition.

Brian Weiss, M.D., a graduate of Columbia University and Yale Medical School, is Chairman Emeritus of Psychiatry at the Mount Sinai Medical Center in Miami.

Jessica Utts, Ph.D., is Professor Emerita of Statistics at the University of California, Irvine. After receiving her Ph.D. in Statistics at Penn State University, she spent 30 years as a professor and associate vice provost at the University of California, Davis, before moving to UC Irvine.

Harold (Hal) Puthoff, Ph.D., is President and CEO of EarthTech International (ETI) and Director of the Institute for Advanced Studies at Austin (IASA). Earning his Ph.D. from Stanford University in 1967, Puthoff's professional background spans more than five decades of research.

Dianne Arcangel, M.S., is a former hospice chaplain, psychiatric hospital therapist, and Director of the Elisabeth

TABLE 1. Countries from Which the Majority of Essays Were Submitted

Country	# of Essays	Country	# of Essays	Country	# of Essays
United States	110	Italy	4	Japan	2
United Kingdom	15	Russia	4	China	2
Canada	7	Netherlands	3	Ukraine	2
Germany	6	Spain	3	Chile	2
Brazil	6	India	3	Finland	2
France	5	Mexico	2	Ireland	2
Australia	4				

Kubler-Ross Center of Houston. She served on the board of directors at the Rhine Research Center and the National Institute for Discovery Science (NIDS), where she also led trainings and conducted research.

All of the responsibility for judging of the BICS essay contest lay on the shoulders of the seven judges. There was no influence from senior BICS management on the judging process. The essays were chosen by majority rule with the central criterion being the cumulative evidence for Survival of Human Consciousness beyond permanent bodily death and beyond a reasonable doubt.

Because of the very large number of high-quality essays that BICS received, the judges spent up to five months of intensive work in evaluating, deliberating, arguing, and eventually making their decisions. The winners were chosen based on the power of the arguments presented and on a consensus of judges on how persuasively the essays made the case for survival of human consciousness beyond a reasonable doubt.

The Expansion of Prize Money

Most of the publicity surrounding the BICS essay contest focused on the fact that the top essay was to be awarded $500,000, the second prize was to be $300,000 and the third winning essay would receive $150,000. Within a couple of months, the panel of judges had already recognized that it would be exceedingly difficult to choose three winners, so great was the pool of high-quality essays. Robert Bigelow presented an immediate solution to the conundrum. He would add a further $550,000 to the winner's pot and award an additional $50,000 for each of 11 runner-up essays. This increased the total BICS prize money to $1,500,000.

Even when the first 14 essays had been identified and selected by the judges, there remained a significant number of high-quality essays that the judges continued to champion. At that stage it was decided to create an Honorable Mentions category for another 15 essays that the judges wished to recognize for the hard work and scholarship that they demonstrated. Robert Bigelow then decided that rather than just formally recognizing the group of 15 essays by publishing them on the BICS website, he made the decision to award these 15 essays an additional tranche of prize money. Robert allocated another $300,000 into the prize pot that was divided equally among the 15 Honorable Mention essays. Each of these essays was awarded $20,000. This brought the total prize money for all twenty-nine essays to $1,800,000, arguably an unprecedented level of financial support for this field of research.

Beyond a Reasonable Doubt

As BICS designed the essay contest, we considered the three legal standards involved with producing burden of proof. These were (i) preponderance of the evidence, (ii) clear and convincing evidence, and (ii) proof beyond a reasonable doubt.

Preponderance of evidence is more commonly used in civil lawsuits and the plaintiff must prove that their case against a defendant is more likely than not true. Clear and convincing evidence is utilized in family law cases and is the highest standard in non-criminal cases. Clear and convincing evidence requires proof that a fact is substantially more likely to be true than false.

Proof beyond a reasonable doubt is the main burden of proof in criminal cases. To convict a person in a criminal case, a prosecutor must prove the defendant is guilty beyond a reasonable doubt. This burden of proof indicates that the prosecution must demonstrate that there is no other reasonable explanation for the evidence presented during a criminal trial.

BICS opted to impose proof beyond a reasonable doubt to aim for the highest standards of evidence possible. The crossover between burden of proof in the legal and scientific systems has been ambiguous for years, although there have been attempts to create quantitative scales to correlate legal and scientific standards of evidence (see, for example, Weiss, 2003).

Weiss created a quantitative scale from 1 to 10 for legal standards of proof and defined preponderance of evidence as 5 out of 10, clear and convincing evidence as 8 out of 10, and beyond a reasonable doubt as 9 out of 10. The authors argued that a parallel scientific level of certainty to "beyond a reasonable doubt" would be "scientifically rigorously proven." Weiss characterized "scientifically rigorously proven" as evidence in which critical experiments give a clear and unambiguous result, excluding alternative explanations. Finally, because of extensive media coverage of criminal prosecutions over the past few decades, the phrase "beyond a reasonable doubt" was judged by BICS to be a familiar and understandable term to the public.

In late 2021, after the 43 authors had received their monetary prizes, all 29 winning essays were published on the BICS website (https://www.bigelowinstitute.org/contest_winners3.php) and have been available to the public to download free of charge. As of mid-2022, there have been thousands of downloads from North and South America, Europe, Asia, and Africa, indicating a sustained and broad global interest in reading the material. In addition, media coverage has increased the public appetite for this information.

BICS obtained agreement from the authors to publish

the winning essays in book form. As a further step in expanding global access to the information in the essays, 28 of the papers will be published as a multi-volume hardcover set of books that BICS will make available to interested individuals and groups.

In conclusion, BICS believes that some of the original three aims of the essay contest ((i) Energize the field of the SOHC after Death, (ii) Encourage more individuals and groups to learn about and take an interest in the field, and (iil) Delineate the global state-of-the-art research on the SOHC after Death in 2021) were achieved.

What is Next? The BICS Challenge for 2023

BICS is following up the 2021 Essay Contest with a new program called "The Challenge," to be unveiled on September 1, 2022. The Challenge comprises a significant grants program that initiates in 2023 and is targeted toward researchers in the field of the SOHC. The three main bounding parameters for The Challenge are:

- The research is to be exclusive to the survival of human consciousness beyond permanent bodily death.
- The research is to be exclusively focused on Contact or Communication with the "Other Side."
- The research should preferably break new ground.

BICS requires all applicants to submit information about themselves and their capability to conduct this research.

Important Dates for the BICS Challenge for 2023

—BICS Announces the 2023 Challenge: 9/1/2022

—Applicants send their required information to BICS and BICS notifies selected applicants of acceptance to submit a grant proposal: 11/1/2022–1/1/2023

—Applicants submit proposals: 1/1/2023–4/1/2023

—Proposal submission cutoff date: 4/1/2023

—Accepted Grantees announced: 8/1/2023

—Conclusion of Work and Final Reports Due: 5/1/2024

A brief summary of grant proposal requirements may better prepare readers to submit proposals to BICS for Challenge 2023:

- Contact and Communication with "the other side" is the exclusive focus of the grants program. However, this does not include contacting alleged deceased relatives.
- BICS requires a preliminary application procedure to validate that applicants have the required background and experience to submit proposals for the BICS Challenge. Applicants will be requested to submit brief descriptions of their education, backgrounds, and experience in researching the SOHC. Secondly, applicants are requested to submit a summary of the proposed research that contains sufficient information for reviewers to evaluate the proposed research. Applications will need to be submitted between 11/1/2022 and 1/1/2023. Successful applicants will be notified that they are authorized to submit proposals on or before 1/1/2023.
- The total proposal (in 12-point font) must not exceed 10,000 words (resumes or CVs do not count in the 10,000-word limit).
- Any significant upfront dollar amount needs to be justified. BICS will automatically disqualify a proposal that requests the majority of funds upfront.
- The proposal must have:
  - A Table of Contents
  - Abstract at the beginning of the proposal
  - Objectives the grant or proposal intends to achieve
  - A budget
  - A breakdown of milestones with deliverables indicating the dollar amount to be paid to the grantee upon satisfactory completion of each milestone
  - A section describing why the proposed work is important and novel and which gives details of experimental designs and controls.
  - A Conclusion and Summary at the end.

In conclusion, the popularity and global reach of the BICS essay contest showed that SOHC research is an active scientific discipline in multiple countries and continents. The anecdotal evidence gleaned from the 204 essays submitted indicates that the contest was responsible for attracting additional people and groups to research and write essays on the SOHC topic. BICS hopes to follow the success of the essay contest with the new grants program, BICS Challenge 2023, which may further energize the SOHC field.

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APPENDIX

Bigelow Institute for Consciousness Studies

BICS ESSAY Competition Rules, Qualifications, Confidentiality and Media Release Form

1. **Essay Application**
    a. All Essay Competition Entrants must first apply to BICS.
    b. Prior to essay submission, each prospective entrant must submit this signed application by email describing their background and qualifications to receive BICS approval to enter the Awards competition (see #2 for more detail).
    c. All applicants must sign releases to BICS for any and all media interviews and publicity. By signing this document applicants agree to make themselves available for media (TV, radio, newspaper, social media) interviews if their essay is declared a winner.
    d. Only those entrants with approval from BICS will be eligible to enter the Essay Competition.
    e. Preliminary Applications must be received by BICS **January 26-February 28, 2021.** People who have been approved to submit an essay will hear from us on or before February 28, 2021. If you have not been approved, you will not hear from us.

2. **Essay Applicant Qualifications**:
    a. Applicants must provide evidence that they have investigated the topic of Survival of Human Consciousness after Death for 5 years or more. For multi author submissions, only one author must have 5 years' experience in the topic while other authors can be less qualified.
    Evidence of investigation such as published papers, monographs, books or producers/directors of film or digital documentaries will be accepted. Embedded links to written or digital work will be welcome as part of the application.

1899 W. Brooks Ave, North Las Vegas, NV 89032 P (702) 639-4440 F (702) 688-6776

b. It may help if applicants are/were a member of relevant organizations for example Society of Psychical Research, Society for Scientific Exploration, American Society for Psychical Research, Institute of Noetic Sciences, International Association for Near Death Studies, Rhine Research Center, Windbridge Research, Parapsychological Association, Eternea, Center for Research on Consciousness and Anomalous Psychology (CERCAP), Koestler Parapsychology Unit (University of Edinburgh, Scotland), NUPES – Research Center in Spirituality and Health (Brazil) or be a staff researcher (including emeritus) in an accredited university in the world. Submission of *curricula vitae* is allowed.
c. Applicant may be an ordained member of a religious order or group. Formal ordination, or membership in religious orders is relevant only if the affiliation relates to scholarly works in the body of the essay.

3. Essay Question

Each essay must address the following question:

What is the best available evidence for the Survival of Human Consciousness after Permanent Bodily Death?

4. Essay Submission Deadline

Essays must be emailed to, and received by, BICS by 5.00pm Pacific time **August 1, 2021**

5. Judging

a. Essays will be judged by a panel of five internationally renowned experts.
b. Judges' decisions are final
c. Winners will be announced on **November 1, 2021**
d. All judging deliberations and inter-BICS discourse in any form are proprietary to BICS and will not be disseminated to candidates or to the public.

6. Essay Formatting

a. Essays must be in English, typed in 12 font, be up to 25,000 words not counting references.

b. Each submitted essay must detail all citations and references of information sources and evidence. Paraphrasing other people's work must be referenced.

c. References must be numbered in the body of the text and arranged numerically in a reference section at the end of the essay. Footnotes are also acceptable. References can be in the styles of the writers, but should use the American Psychological Association Manual style for guidance and cite: Author(s), (Year) volume, page number(s) and book or journal title / publisher.

7. Essay Guidelines

a. Quoting sacred scripture is an expression of faith. For the purposes of this contest, faith is not sufficient and does not constitute evidence, although it may well inform or influence the specific argument and choice of materials used. Therefore scriptural quotations would not be accepted as evidence beyond a reasonable doubt, that is, they will carry no authority for the sake of this contest because they claim as much for the believer. Of course quotations from scholarly faith-based literature if relevant to experimental research of others, or non-evidentiary material is permitted [eg if related to case or subject of past research criteria being analyzed]. BICS understands that true (or veridical) evidence includes a combination of a wide variety of forms; scientific, experiential, witnessed, repeatable, anecdotal and otherwise persuasive far beyond rules of traditional evidence-based hypothesis tested research paradigms.

b. All essays must be original, unpublished, and not submitted elsewhere. Authors are free to refer to, explain, analyze and cite, their own previous work in their essays.

8. Copyright & Essay Publication Rights

a. Authors retain all rights to their work.

b. BICS reserves the right to publish the winning entries on this website, in a possible proceedings volume and in the media.

c. Derivative publications in other media are permitted, if/as permitted by the publication's rules.

9. Beyond A Reasonable Doubt

BICS encourages, and will accept, the inclusion of credible eyewitness and witness testimony as valid evidence for the Survival of Human Consciousness after Death. Eyewitness testimony is a bedrock for conviction by juries and judges in the United States and in International Court Systems. Therefore BICS will accept evidence and eyewitness testimony supporting the legal requirement that establishes proof beyond a reasonable doubt.

10. Applicant Participation, Confidentiality Agreement and Media Release

WHEREAS, Bigelow Institute for Consciousness Studies ("BICS") is sponsoring an essay contest for the best research and/or presentation regarding the survival of consciousness beyond physical death (the "Contest").

WHEREAS, each applicant in the contest ("Entrant") will be required to provide an essay supported by studies, research and other empirical evidence supporting survival of consciousness after death (collectively the “Entry”) in order to participate in the Contest.

WHEREAS, each Entrant will be required to electronically submit their Entry (an essay together with all supporting research) by August 1, 2021 at 5.00 p.m. Pacific Daylight Time, through the BICS website (bigelowinstitute.org).

WHEREAS, as a condition to participation in the Contest, each Entrant must agree to and sign this consent and release, according to the terms set forth below.

NOW, THEREFORE, in exchange for, and in consideration of, the opportunity to enter the Contest, the receipt and sufficiency of which is hereby acknowledged by the Entrant’s signature below, Entrant hereby states, avers, and agrees as follows:

A. **RELEASE AND CONSENT OF CONTEST ENTRANT**

By submitting my Entry to the Contest, I hereby grant BICS and its designees the perpetual, worldwide right to reproduce, promote, and otherwise use my Entry and/or its contents in any media advertising, promotional, and/or any other purposes, as BICS and/or its designees may determine or see fit, without having to seek permission from, and without consideration or notification

to Entrant or any third party. Entrant acknowledges and warrants that it holds all rights to the Entry and all supporting research submitted in support thereof. I also agree that the entry may, in BICS' sole discretion, be posted online at the contest website and/or any other website as determined by BICS, for visitors to the website(s) to view.

I understand that at or for purposes of this Contest, my image may be captured and used by either still photography or video recording. I agree to allow my photo, video or film likeness to be used for any legitimate purpose by BICS, the contest event holders, producers, sponsors, organizers and or assigns. I further agree to participate in any interviews with media or other promotional activities, (health permitting), designated by BICS if I am the recipient of any award through the Contest (as long as BICS pays the reasonable travel expenses, including airfare and lodging, associated with such promotional activities, which must be pre-approved by BICS in writing before BICS has any obligations). I further acknowledge and agree that all enforcement of any Contest rules and the determination of the Contest winner(s) shall be the exclusive right, and at the exclusive discretion, of BICS, and/or BICS' judges. I further acknowledge and agree that BICS shall have the right to disqualify and remove me from participating in the Contest at any time, and for any reason, which reason need not be provided to me.

In consideration of my Entry and permitting me to participate in this Contest, I hereby take action for myself, my executors, administrators, heirs, next of kin, successors, and assigns as follows: (A) I agree to Waive, Release and Discharge BICS, the Contest Judges, as well as any other persons, vendors, entities, and/or businesses participating in staging the Contest which are not otherwise named herein, as well as their directors, officers, employees, volunteers, agents, Contest holders, Contest promoters, Contest sponsors, Contest volunteers, Contest permit grantors, Contest property owners, from any and all liability for my death, disability, personal injury, property damage, cell phone damage, property theft, lost income, or any other losses, costs or actions of any kind which hereafter may accrue to me by virtue of my participation in this Contest, or my travel to or from BICS or any location necessary for promotional purposes if I am a winner of any award through this Contest; (B) I agree to Hold Harmless and Indemnify the entities or persons mentioned in this paragraph from

and against any and all liability, losses, costs and expenses (including attorneys' fees) incurred as a result of my Entry being submitted to BICS in violation of any terms of this Agreement or any rules adopted by BICS for this Contest, including, but not limited to, any failure on my part to obtain the releases required from any and all persons who may claim any interest in my Entry or the contents thereof. I have read and fully understand the above important information, warning of risk, assumption of risk and waiver and release of all claims. Participation in the Contest will be denied if I have not signed this waiver before the start of the Contest.

B. **WARRANTY OF CONTEST ENTRANT**

I represent and warrant that I own all right, title and interest and have obtained all appropriate permissions and releases to grant BICS the right to use and publish my Entry, the content thereof, and all research associated therewith, which shall include but not be limited, all right, title and interest to any patent, trademark, trade secret, copyright or other proprietary rights, including but not limited to, privacy and publicity rights, in and to, my Entry, to enable BICS to review, judge, use and publish my Entry. I further represent and warrant that I have obtained a Participant Release Form from any identifiable person who appears in, is heard in, has allowed the use of his/her name, likeness, or voice in the Entry, or that has otherwise contributed to the Entry. If Entrant is a minor, I have received the written consent of Entrant's legal guardian.

I hereby agree to indemnify, defend and hold harmless BICS and its affiliates, licensees, successors and assigns from and against any and all liability, losses, costs and expenses (including attorneys' fees) incurred as a result of any Entry submitted to BICS in violation of any terms contained in this Agreement or in any rules for the Contest that may be promulgated by BICS. I further agree that contacting the judges for any reason is forbidden and could result in disqualification from the Essay Contest.

C. **NONDISCLOSURE AGREEMENT OF CONTEST ENTRANT**

In consideration of my Entry and permitting me to participate in this Contest, I hereby take action for myself, my executors, administrators, heirs, next

of kin, successors, and hereby agree that I will not share any information whatsoever, including, but not limited to, the following:

(a) I will not disclose, or take any unauthorized pictures, still or moving of, any portion of any of the Contest proceedings, Contest judges, Contest managers, Contest presentations, Contest holders, Contest promoters, Contest sponsors, Contest volunteers, Contest permit grantors, Contest property owners, as well as their directors, officers, employees, volunteers, and agents;

(b) I will not directly or indirectly disclose to any person or entity (including, without limitation, to any media source or social media source or any other social networks, websites or the internet) any information of any kind relating to or concerning the Contest, the Contest Judges, my Entry, or BICS, including its affiliates, officers, shareholders, directors, managers, members, representatives, licensees, successors and assigns, as all such information is strictly confidential. I understand and agree that disclosure of any of the information prohibited by this Paragraph can only be permitted with an express, written agreement signed by BICS.

I understand and agree that all such information is confidential and that the dissemination of any such information shall constitute a breach of this confidentiality and non-disclosure Agreement and will cause BICS irreparable harm, not readily measurable in money.

D. **MISCELLANEOUS**

(i). Applicable Law. This Agreement shall be construed in accordance with the laws of Nevada, with regard to its choice of law provisions. If legal action is initiated relative to this Agreement or the rights or obligations of any party hereunder, such action must be initiated, maintained and continued in Clark County, Nevada.

BICS shall have the right to take all measures necessary to protect its rights, including without limitation, the right to seek civil and criminal penalties for any violation of this Agreement. Further, BICS shall have the right to all other remedies at law and in equity, including injunctive relief.

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(ii). Interpretation. The provisions of this document should not be interpreted or construed in favor of or with prejudice against any particular party, but in accordance with the general tenor of the language used.

(iii). Counterparts. This Agreement may be executed in two or more counterparts. A set of counterparts containing the signatures of all parties hereto shall have the same effect as a single Agreement containing the signatures of all parties.

(iv). Further Assurances. The Entrant(s) hereby agree to cooperate with BICS and to execute and deliver all such further instruments and documents and to do all such further acts and that may be reasonably requested to do from time to time in order to carry out the provisions and purposes of this Agreement.

I understand that confidentiality and non-disclosure are of the utmost importance and, by my signature below, agree to the terms herein.

Background and Qualification Statement. This section MUST be filled out with as much detail as possible. This section will be evaluated by BICS staff and the evaluation will play a significant role in approval for submission of essays.

My background and qualifications to submit an essay to the BICS competition are:

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For multi-author essays, each individual author must: (a) provide their names and addresses on this application, (b) they must separately sign this application and (c) they must agree on this application to divide the prize money equally among each author. Thus, for multi-author works, this application must be printed, individually signed, scanned and emailed back to BICS.

I have read, understood and agree to abide by, the rules, confidentiality and media release requirements for the BICS essay competition

Co-Author #1

Signed_____

Date_____

Name_____

Address_____

_____

Email Address_____

Co-Author #2

Signed_____

Date_____

9

Name_____

Address_____

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Email Address_____

Co-Author #3

Signed_____

Date_____

Name_____

Address_____

_____

Email Address_____

If there are more than three co-authors, the additional coauthors must sign, date, and give their names, mail addresses and email addresses.

Journal of Scientific Exploration
Anomalistics and Frontier Science

COMMENTARY

How *Not* To Do Survival Research: Reflections on the Bigelow Institute Essay Competition

Keith Augustine

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HIGHLIGHTS

An evaluation of essays offering the best evidence for "life after death" finds that the winning entries not only conflicted with well-established biomedical knowledge, but were inconsistent with each other.

ABSTRACT

The recent Bigelow Institute contest rewarding the "best" evidence for life after death epitomizes much of what's wrong with the current state of survival research, its participants constituting a who's who list of contemporary survival researchers. Cases that are regularly hyped as among the best evidence for an afterlife are all too often easily susceptible to normal explanations—if only survival researchers would give them a chance. The consistently negative results of 121 years of experimental survival research ought to have spurred soul-searching questions for survival researchers by now. And if we treat discarnate personal survival as a scientific hypothesis, then researchers are rationally obliged to seriously consider biological facts that tell against it, too. Limiting one's inquiry to attempts to only collect data that might confirm survival is one of the chief hallmarks of pseudoscience, and it's sadly a feature, not a bug, of the survival literature. This systematic review reveals that survival researchers would better serve science by setting aside their feelings and heeding what the data are telling them, for the probabilities should drive our beliefs, not the other way around. Is discarnate personal survival likely to occur in light of the total available evidence? The overall evidence doesn't even make personal survival more probable than not.

KEYWORDS

Mind–body problem; neuroscience; psychical research; tests of survival; total evidence requirement

Note: Citations from the BICS prize-winning essays have asterisks after the year (2021*).
Note: The author declares no financial interest in the Bigelow Institute for Consciousness Studies monetary prizes.
Note: Informal logical fallacy names have been bolded.

INTRODUCTION

This journal's former Editor-in-Chief has already commented on the shoddy state of survival research (Braude, 2021a), but as a sympathetic friend his criticisms have been relatively lightweight. From the perspective of an outsider, he goes easy on fellow survival researchers, pulling his punches. I have no such compunction. In what follows, I will call out bad behavior and—more importantly—poor reasoning when survival researchers engage in it.

The recent essay contest funded by the Bigelow Institute for Consciousness Studies (BICS) epitomizes much of what's wrong with the current state of survival research, its participants constituting a who's who list of contemporary survival researchers. Long gone are the days of C. D. Broad, E. R. Dodds, or Gardner Murphy. Though these eminent contributors to the field have had a few comparable successors, it's a pity that they are so few and far between compared to the early decades of the Society for Psychical Research (SPR).

On or about January 21, 2021, BICS announced that it was accepting submissions on the question: "What is the best available evidence for the Survival of Human Consciousness after Permanent Bodily Death?" (Rules & Regulations, 2021, §3). Entrants had until February 28 to apply to be eligible to receive substantial monetary prizes (from $500,000 for first place to $20,000 for last), at which point they would be notified if they had been cleared to compete. Submissions were due by August 1, and 29 winning essays were announced on November 24. BICS's six judges were slated to read and evaluate 204 submissions of up to 25,000 words (~50 single-spaced pages) each, excluding references, and then collaborate on how to rank the winning essays. Going by the upper length limit (with no lower limit) and the earliest possible date to apply to be considered (January 26), that tasked the six judges to read, evaluate, and collaborate on potentially as many as ~10,000 pages of text in at most just under 10 months, at a potential rate of 1000 pages per month, or ~33 pages per day, every day.

Of course, most eligible essays would likely be submitted much later than the earliest possible application date, and many would not come near the upper length limit. Nevertheless, this breakdown does raise the question of whether the six judges had enough time to really *evaluate* the submissions that they received. A more pointed question concerns the *aim* of the competition itself. If BICS wanted an *objective* assessment of the state of the survival evidence, why not instead commission an evidence review (not an essay contest) by *independent* judges, such as those in the biomedical field who have *not* published in the survival literature, to avoid potential conflicts of interest?

But these questions are neither here nor there, as I will comment on the content of eight select essays of the 29 winners in order to produce a manageable critique. These include the top three prize-winning essays by Jeffrey Mishlove, Pim van Lommel, and Leo Ruickbie; the two essays by survival researchers who have sought hard *experimental* evidence of personal survival, Julie Beischel and Sam Parnia (with Tara Keshavarz Shirazi); the essay by Arnaud Delorme, Dean Radin, and Helané Wahbeh (hereafter DRW) because they are seasoned experimentalists who propose future studies that could meet this standard of evidence, and because they systematically review the field as a whole; the essay by Michael Nahm because, in addition to evaluating the survival evidence, he addresses the most substantial challenges to personal survival published in recent years (though space precludes me from addressing the latter); and the essay by Stephen Braude, then editor of this journal, because of his command of how to *evaluate* evidence in addition to his knowledge of the survival evidence itself.

IN SCIENCE THE QUALITY OF THE EVIDENCE IS PARAMOUNT

Set aside (for the moment) that **cherry-picking** evidence that might favor discarnate personal survival, while ignoring or cursorily dismissing any evidence against it, is inexcusable. Even in inviting only potentially favorable evidence, BICS contest requirements were conflicting. In one breath entrants were informed that "We are seeking hard evidence 'beyond a reasonable doubt'" (About BICS, 2021), while in the next they are told that sufficient evidence "includes a combination of a wide variety of forms; scientific, experiential, witnessed, repeatable, anecdotal and otherwise persuasive far beyond rules of traditional evidence-based hypothesis tested research paradigms" (Rules & Regulations, 2021, §7). Elsewhere, entrants were told that "BICS will accept evidence and eyewitness testimony supporting the legal requirement that establishes proof beyond a reasonable doubt" (Rules & Regulations, 2021, §9), but then a wide net was cast: "BICS envisions the essays['] focus to be on scientific evidence as well as objective and subjective supported documentation" such as "special cases, including older cases, from very credible witnesses," "photographic or electronic data," "all available literature," "highly validated and authenticated human experiences," and "other relevant sources" (whatever that means) (About BICS, 2021). The quality of the evidence on offer appears less important to BICS than ensuring that the evidence provided ostensibly favors discarnate personal survival, whatever its quality.

The conflicting messages are reflected in the win-

ning submissions themselves. Nahm's entry emphasizes that "In court, a striking agreement of more than 30 eyewitnesses would carry enormous weight" (2021*, p. 19), though one wonders, "agreement about *what*?" Are different witnesses asked about the same events? Or are they just asked to give testimony, leaving it to investigators to then tie together claims made by different witnesses into a common theme? Did investigators ask leading questions, or open-ended ones? Do different witnesses corroborate each other, or do they provide testimony whose differences investigators gloss over to fit a coherent narrative? Are the different testimonies truly independent of each other, or were they intermingled or informed by a common third source? And so on.

Nahm later writes that impartial judges "would take eyewitness testimonies just as seriously as they would do in other contexts" (2021*, p. 66). While Elizabeth Loftus's (1979) seminal research into the reliability of eyewitness testimony provides all sorts of reasons to hesitate to rely upon it so heavily (as survival research typically does), what DRW say about it in their prize-winning essay is more than sufficient:

> eyewitness testimony would not convince those who also take into consideration the relevant literature from the neurosciences, clinical, cognitive, and perceptual psychology, and court cases. Research in those disciplines has shown that eyewitness testimony is not as reliable as one might hope because perceptions and memories are easily distorted. (2021*, p. 3)

They cite the Innocence Project, writing:

> Of 375 wrongful convictions they investigated, an alarming 69% were due to mistakes in eyewitness testimony. Cumulatively, those innocent people served 5,284 years in prison. In 21 cases, the accused was sentenced to death before being exonerated by DNA evidence, and in at least one case, the accused was executed before DNA evidence proved his innocence. Thus, when it comes to matters of life and death, which arguably includes the question of survival, reliance on eyewitness testimony is both legally and scientifically questionable. Ultimately, we know that eyewitness testimony is not persuasive for many because agnosticism about survival persists despite an abundance of eyewitness reports. (DRW, 2021*, p. 3)

So, although Nahm concludes that "the available evidence for survival of human consciousness after permanent bodily death clearly matches the standard of proof beyond a reasonable doubt" (2021*, p. 66), survival agnostics might well note that there's an abundance of eyewitness reports for the existence of the Loch Ness Monster, too, that they find just as unconvincing. For all the talk about courtroom standards of evidence, empirical survivalists have habitually engaged in a hitherto-unacknowledged *evidential* sleight of hand by demanding that the "defense" (survival skeptics) produce their own counterevidence to offset the "prosecution's" (survival proponents') weaker testimonial evidence for personal survival, all the while seeking to rule as inadmissible the defense's much stronger "DNA evidence"—the chiefly neuroscientific evidence that our mental lives cannot be sustained absent a functioning brain. (I will return to this point later.)

Prospective Experimental Tests of Survival

DRW "anonymously surveyed 422 academic scientists and scholars from major universities in the United States" (2021*, p. 26) about ten proposed survival experiments "to see which of those studies, if successful, they would find most persuasive" (2021*, p. 1). This is exactly the sort of controlled experimental research that survival researchers ought to be doing, in the spirit of previously attempted "tests of survival" (or of mind–body separation). Now the only thing that's left to do is actually *perform* these experiments. Despite the logical possibility of living-agent-psi (LAP) interpretations (or other nonsurvivalist paranormal interpretations), most "academic scientists and scholars" would surely be satisfied with, say, replicable positive results from Parnia's AWARE II study. Indeed, DRW go on to note that "The most frequently selected study was a controlled, prospective experiment that would result in veridical out-of-body perceptions during a near-death experience, followed by experiments involving mediumship and reincarnation" (2021*, p. 1).

It's significant that DRW's survival-agnostic[1] academics' most selected experiments have *already been done* and failed to produce the desired results. The actual outcome of several decades of such experiments (over a century's worth for mental mediumship) "continues to frustrate researchers" (Holden, 2009, p. 210) and ought to have spurred soul-searching questions for survival researchers by now. In response to a chapter invitation on historical mental mediumship research, logician Roy Sorensen wittily wrote to me:

> Thanks for your invitation! I do not have anything to offer. But you should invite Henry Sidgwick to contribute. He pursued psychical research and saw his death as an opportunity for further

> research. To forestall fraud, he arranged codes with his executors. I believe some mediums claimed to be channeling the great philosopher. But none got through the security arrangements. Sidgwick's failure to reply to invitation would be of more evidential significance than mine! (personal communication, April 22, 2012)

Negative outcomes are only frustrating if you want the experiments to come out a certain way. In lieu of remaining frustrated by failing to get the data that you were hoping for (as many pharmaceutical company CEOs surely have been at times), survival researchers would better serve *science* by setting aside their feelings and heeding what the *data* are telling them. One possibility stands out among the rest for its sheer simplicity: perhaps out-of-body experience (OBE) adepts and near-death experiencers (NDErs) cannot describe remote visual targets under controlled conditions because nothing leaves the body during OBEs or NDEs that could perceive them.[2]

Scientifically, a pharmaceutical company cannot loosen its testing conditions (e.g., by relaxing the blinding of participants during a trial, or by settling for anecdotal evidence instead) until its favored drug produces the results that it had hoped for. Rather, it is expected to *test a different drug*. So, too, empirical survivalists should test another hypothesis when all of the experiments that they have attempted to confirm discarnate personal survival have failed. While some survivalists (e.g., digitalists or "resurrectionists") can abide such outcomes, the failure of such simple tests of survival is incredibly problematic for *empirical* survivalists,[3] such as those who herald mental mediumship as providing the best evidence available for personal survival—"no other body of evidence comes close" (Braude, 2021b*, p. 29)—unless we are simply talking about the best of a bad lot (van Fraassen, 1989, p. 143).

Where Have All the Deceased Survival Researchers Gone?

The closest thing to scientific "proof" of an afterlife was pursued over 100 years ago and has continued since, as much survival research involves doing the same thing over and over again and expecting different results. The conclusion of the most recent write-up of a postmortem test of survival speaks for itself:

> Eight people made attempts to [psychically] read the [audio] tape in 1996, during his lifetime, but none was judged by Charles to be successful. He died on 29 August 2005, aged 91. Since then, 35 psychic sensitives from all over the world have taken up Charles's challenge and have been in touch with us . . . Sadly, none of the attempts came anywhere near this wording [of an audio-taped poem], or of the images it conveys . . . We are grateful to Charles for initiating this experiment, and to the many people who have collaborated in it; but it cannot be judged to have been a success. Perhaps Charles lost interest? Perhaps he was unable to "get through?" Perhaps none of the sensitives were in tune with him? Perhaps, perhaps, perhaps. (Perry & Fontana, 2009, pp. 11–12)

This is just one of countless attempted direct tests of survival (or of mind–body separation), many of which haven't been written up. And although the outcome of this recent experiment *was* written up, to my knowledge this essay constitutes the first time that its write-up has been so much as *cited* in any psychical research journal in the 13 years since its publication.

So experimental designs to test survival are nothing new (cf. Carrington, 1957, pp. 131–133; Levin, 1994; Quid & Dallas, 1920, pp. 278–283). A partial list of deceased "participants" in such tests, most of whom were survival researchers in life, includes:

- Frederic W. H. Myers d. 1901 ("no resemblance" found between suggestion and note)
- Richard Hodgson d. 1905 (all attempts to get invented word "stabdelta" were misses)
- Harry Houdini d. 1926 (wife & medium already knew code: "Rosabelle believe")
- Thomas Edison d. 1931 (conveyed name of his hometown wasn't any of ten code words)
- Oliver Lodge d. 1940 (piano notes CEGEDFEDCEGEDEC)
- Grandmother of Judith Skutch Whitson d. 1971
- T. E. Wood d. 1971
- J. Gaither Pratt d. 1979
- Clarissa Mulders d. 1982
- Robert H. Thouless d. 1984
- Arnold Barber d. 1989
- Susy Smith d. 2001
- Elisabeth Kübler-Ross d. 2004
- Charles Fryer d. 2005
- Frank C. Tribbe d. 2006
- Ian Stevenson d. 2007
- Arthur S. Berger d. 2016

(Anonymous, 1989; Bauer, 2017, pp. 316–317; Berger, 1988, p. 106; Berger & Berger, 1995, p. 141; Cohen & Skutch, 1985, pp. 47–50; Dunninger, 1935, pp. 69–79; Fox, 2007; Gay et al., 1955; Lodge, 1905; O'Shea, 2018; Price, 1975, p.

25; Roach, 2005, pp. 163–164; Schwartz & Russek, 2001, p. 82; Smith, 2000, p. 236; Stevenson et al., 1989, pp. 330–331; Tribbe, 1980; Verrall, 1906, p. 252)

While some mediums were asked to describe the contents of sealed envelopes or provide auditory information, most direct tests of survival involve asking living persons to posthumously reveal to a medium key words, phrases, or mnemonic devices, ostensibly unknown to any living person, that would decipher encrypted messages or open user-set combination locks (leaving it to living researchers to transpose key words into numbers). About 24 combination lock tests (some included in the list above) are in the possession of the University of Virginia Division of Perceptual Studies (Greyson, 2009). Berger reported having the encrypted messages of "a hundred or so participants" nearly 40 years ago (Cohen, 1984, p. 94). On Smith's defunct Afterlife Codes website, cryptologist Craig P. Bauer reports: "[T]he fate of the messages enciphered through it is uncertain. It is known that there were about 1,000 people registered" (2017, p. 345). Two decades ago, Smith herself wrote: "There are vast numbers of people registering their secret messages with us. Surely codes will begin to be broken" (2000, p. 214). After 121 years of such simple tests, only undeniably fraudulent mediums (Spraggett & Rauscher, 1973) or cryptologists (Bean, 2020; Gillogly & Harnisch, 1996) have ever been able to solve them. Perhaps the "telephone to the dead" under development by Beischel's mentor (Gary Schwartz) will function better than *Theranos's* Edison device—but I'm not holding my breath (SoulPhone Foundation, 2020).

Moreover, experiments to detect OBErs' "astral bodies" (Alvarado, 1982b; Blackmore, 1982/1992, pp. 213–224; Irwin, 1985, pp. 232–235; Stokes, 1997, pp. 46–47) or have them identify visual targets (Alvarado, 1982a; Blackmore, 1982/1992, pp. 189–199; Irwin, 1985, pp. 235–236; Stokes, 1997, pp. 46–47) were systematically conducted from the late 1960s to early 1980s without proffering convincing evidence of either. There have also been some half a dozen target identification experiments during out-of-body NDEs since (Beauregard et al., 2012; Greyson et al., 2006; Holden & Joesten, 1990; Lawrence, 1997, pp. 158–159; Parnia et al., 2001; Parnia et al., 2014; Sartori, 2004). As with cipher and combination lock tests of mental mediumship, history repeated itself in the hype ahead of the *results* of the AWARE study (Parnia et al., 2014). Despite dubious *anecdotal* claims of successes (Abbott, 1908, p. 32; Burkhardt, 1921; Greaves, 1967; Myers, 1903, pp. 182–185; Rivas et al., 2016, pp. 29–55; Salter, 1958; Stevenson, 1976, p. 219; Underhill, 1885, p. 435), collectively there have been quite a large number of attempts to demonstrate discarnate personal survival and/or mind–body separation using a variety of *controlled* experimental designs over a long stretch of time, and yet their outcomes have been underwhelming.

This raises an obvious question: If communication with the dead occurs, as the vast majority of empirical survivalists evidently believe, then why have we heard *nothing* from any of these deceased psychical researchers, many of whom were dedicated to providing "proof" of discarnate personal survival during life? Why can't a single one of them "authenticate" their continuation (or come as close to that as possible) by providing their "passwords" to a medium (or as an ostensibly reincarnated child—à la Berger, 1991—or via EVP/ITC, for that matter)?

In her prize-winning BICS essay, Beischel does not mention such tests directly, but does seem to try to preempt questions about them, writing:

> During any research reading, we need to ensure that we only ask the mediums to report the types of information they usually report. Since this does not include winning lottery numbers, combinations to locks, or what color shirt the sitter should wear tomorrow, I didn't ask for any of those things in my experiments. Additionally, although in your physical life you are regularly known by your personally-identifiable information (PII), like your name, date of birth, social security number, address, and phone number, these are not the types of information mediums are regularly observed reporting, so I didn't ask for those during research. (Beischel, 2021*, p. 23)

This is disingenuous. Postmortem tests of survival never concern requests of the deceased for winning lottery numbers, *numerical* combinations, or fashion advice. Rather, they concern requests for simple information akin to the last name of a deceased person purportedly haunting a location. More to the point, the fact that mediums do not provide such information is not a *reason* why they do not provide it (or cannot provide it). Elsewhere, Beischel *has* speculated about such reasons: maybe "the combination to the lock holds no interest or has been forgotten. Perhaps not all types of stored memories are retained after death. Maybe the medium's consciousness filters out information for which she does not have a personal reference" (2007, p. 62). Countless possibilities are *imaginable* here, but surely the most parsimonious explanation is that the deceased simply have not survived as conscious individuals who could convey the keys.

Moreover, if we can telepathically/clairvoyantly retrieve information—whether from the living, the dead, or the inanimate—why have such tests bore so little fruit? Their failure gives the scientific community good reason

to doubt the existence of extrasensory perception (ESP) of *any* sort akin to why many scientists doubt the existence of psychokinesis (PK): if it's real, why can't anyone demonstrably move an object for any distance behind sealed glass? If seers can provide accurate specifics about future events that defy chance, then why have premonition registries (Ruickbie, 2021*, pp. 48–51), which securely document precognitive claims *before* prophesied events, produced hits less than 1% of the time (Shadowitz & Walsh, 1976, pp. 116–117), if at all? (West, 1948a, p. 268). The question is particularly pointed today, when just about anyone can preregister predictions online to validate their timing through chain of custody safeguarding or distributed blockchain records. As the late magician Christopher Milbourne points out: "Many brilliant men have investigated the paranormal but they have yet to find a single person who can, without trickery, send or receive even a three-letter word under test conditions" (1970, p. 37). Berger himself wrote:

> Those who urge these theories must explain why there have been no successes by psychics not only to discover the Thouless keys but also the secret keys of other people who set locks, such as Pratt, or enciphered messages, such as that of the deceased Clarissa Mulders of the SRF [Survival Research Foundation]. Or, for that matter, the living Susy Smith, also of the SRF, who has issued challenges [to 'read her mind' while she's still alive] in this country and abroad, so far not met, to anyone to try to get the secret keys she used to encipher her messages. As cogent explanations have not been offered, the lengthy and growing list of failures diminishes the 'Super-ESP' and other hypotheses. (1996, pp. 48–49)

"Other hypotheses" include interpretations of replicable positive results in terms of telepathy solely among living agents (whether "super" or not), their paranormal access to psychic reservoirs or place memories, demonic/interdimensional/extraterrestrial influences (e.g., Hales, 2001, pp. 342–344), and, of course, discarnate personal survival. Put differently, the perpetual failure of direct tests of survival would seem to indicate that neither LAP nor "otherworldly psi" (Stoeber, 1996, pp. 1–2) exist.

WHAT DOES THE TOTAL AVAILABLE RELEVANT EVIDENCE TELL US?

When assessing the prospects for discarnate personal survival, failing to countenance the evidence favoring the dependence of consciousness on the brain commits the **cherry-picking fallacy**, which one may define (ironically following E. F. Kelly) as "preventing accumulation of evidence favoring any opinion one happens not to like" (2016, p. 593). There is a great deal of data from neuroscience, behavioral genetics, and evolutionary psychology, among other places, that constitutes much stronger evidence against discarnate personal survival than the parapsychological evidence offered in its favor. Ignoring such counterevidence, or waving it away by reinterpreting it so that it never counts in one's evaluation, is the true "a priori dismissal" (Tart, 2007, p. 251) here—and hardly constitutes a *scientific* approach. Nothing about requiring psychical researchers to consider the *totality* of the evidence, not just the particular evidential corner that interests them, involves maintaining that survival is "'impossible' in an aprioristic way" (Nahm, 2021*, p. 4). Well-supported beliefs must be proportioned to *all* of the available relevant evidence, giving more weight to stronger sources of evidence when different sources conflict.

Those survival researchers who address "empirically-grounded indicators of extinction" (Lund, 2009, p. 24) rarely challenge the *reliability* of such evidence, so I will limit my comments to their attempts to reinterpret it away. It's easy to show that the chiefly neuroscientific data constitutes *evidence* against discarnate personal survival (and strong evidence at that). Imagine two mail bins, one labeled "outgoing mail" and the other labeled "incoming mail." Relabel them "individual consciousness requires brain functioning" and "individual consciousness does not require brain functioning." Concisely state on paper strips some *representative*,[4] agreed-upon facts that scientists have discovered about the mind's link to the brain, such as:

- Minds mature as brains mature
- Childhood mental development halts when childhood brain development halts
- Minds degenerate when brains degenerate (due to old age or traumatic brain injury)
- Creatures with simple brains have simple minds
- Creatures with complex brains have complex minds
- Sickening/injuring the brain sickens/injures the mind
- Mental dispositions can be inherited from one's parents
- Mental desires can be induced or eliminated by brain stimulation
- Mental disorders can be cured by altering brain chemistry with drugs
- Mental disorders can be brought on by altering brain chemistry with drugs

Now task everyday persons (undergraduates, perhaps)

to complete the following exercise. Take each paper strip (datum) and place it a bin (hypothesis). Each strip has to be placed in one or the other bin, not both or neither, as prima facie the evidence at hand is relevant to *which* of the two is true. There are no additional bins (hypotheses) because the proposition "either a functioning brain is required for this, or one is not" is a tautology (i.e., is necessarily true so long as individual consciousnesses and functioning brains exist), and the truth of one of those disjuncts entails the falsity of the other. Finally, assume that organisms' minds operate uniformly (in the same general way) across individuals. Given these stipulations, if ordinary people *had to* pick one or the other bin, in which bin would these representative, agreed-upon facts be placed, nine times out of ten? That is, under which of the two hypotheses (required, or not required) would the listed facts be *more expected*?

Putting the question this way countenances the trivial point that one can always contort any hypothesis to fit any facts, just as one can hammer at a square peg to force it into a round hole. The key to assessing degree of evidential support is to start with what the most basic version of each hypothesis predicts. What do their *simpliciter* versions—the hypotheses unamended with auxiliary assumptions, or at most only amended with agreed-upon/confirmed auxiliaries—lead us to expect?[5]

Consider one of the symptoms of long COVID: "brain fog." Why should the mental processes of an independent mind, one capable of functioning after death at least as well it did during the pinnacle of life, be vulnerable to something as clearly biological as a viral infection? Such biological vulnerability makes sense if mental activity is realized by underlying biological processes in the brain. But it makes little sense otherwise. Or consider Curt Ducasse's proposal that mental capacity (or the need for it) causes brain complexity, rather than the other way around:

> [T]he parallelism between the degree of development of the nervous systems of various animals and the degree of their intelligence . . . is alleged to prove that the latter is the product of the former. But the facts *lend themselves equally well* to the supposition that, on the contrary, or at least in equal measure, an obscurely felt need for greater intelligence in the circumstances the animal faced brought about the variations which eventually resulted in a more adequate nervous organization. [emphasis mine] (1951, pp. 456–457)

The idea that a line of giraffes could "strive" to reach higher tree tops for their leaves, subconsciously "willing" a change in their descendants' genotype to allow them to develop longer necks, has long been discredited. Variation in a population of organisms—some giraffes have longer necks, others have shorter ones—is more than sufficient to account for evolutionary change without postulating Lamarckian "striving" or Ducasse's "obscurely felt need": if longer necks increase fitness, then longer-necked giraffes will tend to live long enough to reproduce and pass on their genes more often than shorter-necked giraffes, leading to an increase in neck length over the generations. If the data of evolutionary biology do not "lend themselves equally well" to classical Lamarckism as they do to Darwinian natural selection, how does the comparable idea that a pre-existing mind mysteriously "strives" to become more intelligent, and an animal's neural architecture responds to this yearning, fare any better? Is the biological consensus in either case mere prejudice, or is it justified?

The idea that minds could unwittingly impel a considerable degree of neural development is certainly less credible than the idea that greater neural resources simply enable greater mental proficiency. On the face of it, it certainly *seems*—to an awful lot of people—like brain development is the engine pulling the train. After all, no one believes that the significantly developmentally delayed will ever be able to simply "concentrate" or "meditate" themselves out of mental retardation, but modifying their neural architecture directly would be promising if only we knew enough to be able to produce intended improvements without producing disastrous unintended consequences (as lobotomies once did). For, although we can do both to some degree, we can affect a person's mind much more *profoundly* by manipulating his brain chemistry than we can affect that person's brain chemistry by manipulating his mind.[6] This *empirical* discovery is what warrants taking the brain to be primary and the mind secondary, regardless of one's preferred mind–body theory. To all appearances, significant mental development tracks significant brain development, not the other way around.

It's thus unwise to **jump on the** empirical survivalist **bandwagon** and declare of the dependence of consciousness on the brain: "Its widespread acceptance in Western cultures is merely socioculturally conditioned" (Nahm, 2021*, p. 66). Its *discovery* is no more Western imperialism than is the replacement of the ancient demonic theory of disease with the germ theory. It's simply scientific progress. Accepting it as highly probable requires no "prior—and even cherished—antisurvivalist metaphysical commitments" (Braude, 2021b*, p. 51), or "materialist dogma" (van Lommel, 2021*, p. 17), *at all*. To say that dependence thesis proponents "regard survival 'impossible' in an aprioristic way" (Nahm, 2021*, p. 66) merely attacks a **straw man**, probably because it is easier to defeat a caricature than their actual arguments.[7]

Neuroscientist Sam Harris eloquently argues:

> Science is not in principle committed to the idea that there's no afterlife, or that the mind is identical to the brain, or that materialism is true. Science is completely open to whatever in fact *is* true, and if it's true that consciousness . . . can be dissociated from the brain at death, that would be part of our growing scientific understanding of the world, if we could discover it. And there are ways that we could in fact discover that, if it were true.
>
> The problem is there are very good reasons to think it's not true. And we know this from 150 years of neurology where you damage areas of the brain and faculties are lost, and they're clearly lost, it's not that everyone with brain damage has their soul perfectly intact [and] they just can't get the words out, everything about your mind can be damaged by damaging the brain. You can cease to recognize faces, you can cease to know the names of animals but you still know the names of tools . . . I mean the fragmentation in the way in which our mind is parcellated on that level of the brain is not at all intuitive, and there's a lot known about it. And what we're being asked to consider is that you damage one part of the brain and . . . something about the mind and subjectivity is lost, you damage another and yet more is lost, and yet if you damage the whole thing at death, we can rise off the brain with all our faculties intact, recognizing grandma and speaking English. (Harris, 2011, 1:10:39–1:12:18)

Here philosopher of mind Colin McGinn poses a fair question: "Why does brain damage obliterate mental faculties if minds do not owe their existence to brains?" (1999, p. 27). For a less direct, but no less relevant kind of evidence, consider my paraphrase of philosopher Mathew Iredale's upshot: "The greatly enhanced mental powers of human beings, compared to those of our primate cousins, are a clear result of the enlarged brains that we possess but that they do not. But then how could human minds retain their impressive mental faculties in the complete absence of brain functioning after death?" (Augustine & Fishman, 2015, p. 232). Even former SPR President C. D. Broad acknowledged that tight mind–brain correlations

> strongly [suggest] that minds depend for their existence on bodies; in which case, though survival may still be abstractly possible, it is to the last degree unlikely. At death there takes place completely and permanently a process of bodily destruction which, when it occurs partially and temporarily, carries with it the destruction of part of our mental life. The inference seems only too obvious. (1925, p. 533)

Was Broad mistaken that the *empirical* conclusions of Harris and others are "only too obvious"? If so, *why* was he mistaken? The *actual* arguments of dependence thesis proponents are much more powerful than empirical survivalists typically let on. Even some of their own have acknowledged as much:

> A homunculus residing in a separate mental world, and able to survive the death and destruction of the brain, would, presumably, not be itself impaired by the brain damage: *its* mental universe would be left essentially intact. The damaged brain would be unable to respond as fully to the action of the homunculus upon it, and this impairment would result in problems in communication, and control, and in the reciprocal action of sensing. But the representation of the afflicted part would not disappear from the patient's mental universe itself, as is suggested by the evidence: the patient should not be *puzzled* to discover that there is a left arm connected to his body; the patient should "know" that he has his left arm, even though he has recently been deprived by brain damage of the ability to directly sense or control it. (Stapp, 2009, p. 139)

In this very journal, in fact, developmental biologist Michael Levin pointed out that facile analogies with television sets (e.g., Sheldrake, 1991, p. 117) don't even begin to do justice to the *actual* evidence that neuroscience has uncovered about the mind's relationship to the brain: "If, when one pulls out a certain transistor, the TV show does not stop but rather shows the protagonist start to walk on his hands for the rest of the program, one starts to suspect that some important aspect of the fundamental information content was indeed directly related to the hardware that was removed" (Levin, 2005, p. 634). Appeals to casual soundbites like "correlation is not causation" are not serious responses to this evidence (e.g., Grossman, 2008, pp. 231–232), and distinctions between "functional dependence" and "existential dependence" (e.g., Carter, 2010, pp. 20–21) make no difference since both rule out *discarnate* personal survival (Swinburne, 1997, p. 310).

The irony of Nahm's statements on the matter should not be lost on us. In one breath he quotes former SPR President Hans Driesch, who represented the last gasp of vital-

ism in biology, that we "must look for exceptions, because exceptions are the best means for avoiding dogmatism" (2021*, p. 64). In another breath, he regurgitates the Jamesian argument "that it is *principally impossible* to prove that brain chemistry produces consciousness" given that "all we can observe are 'concomitant variations' of brain states and states of consciousness" [emphasis mine] (2021*, p. 3). The fact of the matter is that such concomitant variations *are* evidence, no matter how staunchly empirical survivalists fight to the death to pretend otherwise. We use them *all of the time* to infer causation, whether the inference is that smoking causes lung cancer, radiation exposure causes leukemia, large greenhouse gas concentrations cause global warming, or brain functioning causes mental functioning. Science is not cafeteria Catholicism, where you get to pick the empirical conclusions that you like and toss out the rest. There are *principled* reasons for when one should infer causation from correlation (Augustine & Fishman, 2015, pp. 204–211; Weisman, 2015, pp. 102–103), and it is **special pleading** to pretend that those reasons do not apply when the causal inference is simply not to one's liking. Science proceeds in the interest of probable truth, not that of validating one's personal proclivities.

According to *what principled reason*, then, can we rule the neuroscientific evidence as inadmissible? Not wanting to deal with powerful counterevidence is not an epistemic principle, but a fallacy (**confirmation bias**). Failing to deal with it shirks one's epistemic responsibilities; it is merely aiming to confirm what one wants to hear, not seeking the truth. Braude, at least, grants as much elsewhere, writing that "physiological evidence apparently casts doubt on the survivalist position" (2005, p. 245). But he immediately follows that up with the caveat that "good survival evidence has a theoretical pull in the opposite direction and poses an apparently comparable prima facie challenge to the anti-survivalist" (2005, p. 245).[8] The key word here is "comparable." The fact that the reliability of the evidence itself is the focus of critics of psychical research, but *not* the focus of critics of neuroscience, suggests that the two cases are anything but comparable. And, like other survival researchers, Braude himself opts to instead focus on the *interpretation* of the neuroscientific evidence (Braude, 2006), widening the opening for **motivated reasoning** to drive his conclusions rather than the evidence itself.

If empirical survivalists insist on maintaining that, as a matter of science, individual human consciousness is "beyond the brain" (Mishlove, 2021*) in the sense of not requiring brain functioning *at all* in order to exist/occur, they should at least *try* to show (not merely assert) that (1) the dependence thesis does not predict this evidence, or else that (2) the independence thesis would lead us to expect the same evidence just as much. Neither is plausible. Long ago, C. S. Peirce defined a prediction as an observational consequence, *derived from* a hypothesis, that would be "a matter of course" were that hypothesis true, but surprising otherwise (1903/1974, p. 117). Philosopher of science Elliott Sober formalized the concept thus:

> The Surprise Principle describes when an observation O strongly favors one hypothesis (H_1) over another (H_2). There are two requirements:
>
> (1) If H_1 were true, you would expect O to be true.
> (2) If H_2 were true, you would expect O to be false.
>
> That is, (1) if H_1 were true, O would be unsurprising; (2) if H_2 were true, O would be surprising. (2012, p. 30)

This is the basic idea behind inference to the best explanation that improves upon the old hypothetico–deductive method. That known mind–brain correlations are "a matter of course" under the dependence thesis, but surprising under the hypothesis that mental processes are independent of brain functioning, has already been amply demonstrated in my response to Ducasse and in the quotations from Harris, Stapp, and Levin above, among other places (Augustine & Fishman, 2015, p. 234; Olson, 2021, pp. 90–91).

In the near-century since Broad's (1925) classic, why is the late Douglas M. Stokes the *sole* psychical researcher to press the evidence *against* personal survival, rather than try to dispose of it as quickly as possible? As Stokes himself observed, "At times, it seems that it is almost a definitional requirement that parapsychologists believe in psi or personal survival" (2016, p. 184). One should not have to sign a doctrinal statement of faith that he will affirm personal survival, or at worst be completely agnostic about it, in order to commensurately contribute to psychical research. If researchers aim to treat discarnate personal survival as a *scientific* hypothesis, then they are rationally *obliged* to seriously consider facts that tell *against* it. If you claim to be doing *science*, you cannot limit your inquiry only to attempts to collect data with the potential to confirm survival, or at worst only fail to provide evidence in its favor.[9] Doing so is one of the chief hallmarks of *pseudoscience*, and it's sadly a feature, not a bug, of the survival literature. In *empirical* inquiry, one must *also* consider evidence that *lowers* the probability of discarnate personal survival well below 50–50 odds—*particularly* when that evidence is stronger than any potentially favorable evidence:

> All inductive reasoning, including explanatory reasoning, is subject to a total evidence requirement. It's relatively easy for facts to offer evidential support for any hypothesis or theory. Every instance of the fallacy of **stacking the deck**—only considering the evidence that favors one's preferred theory—demonstrates this truism. And it's just as easy for any evidential status to diminish with the acquisition of new facts. For this reason, we have to consider as many salient facts as possible, especially facts that (greatly) lower the plausibility of a hypothesis. (Sudduth, 2021, p. 945)

The BICS contest exemplifies the worst of this pseudoscientific tendency, rewarding those who can provide evidence that would "prove" *that* personal survival happens, not those who can provide evidence that would determine *whether* personal survival happens. After all, BICS was openly founded "to support research into both the survival of human consciousness after physical death and, based on data from such studies, the nature of the afterlife" (About BICS, 2021). Clearly, those promoting research "into" the survival of human consciousness and what the afterlife is like have already decided that there *is* an afterlife to have a certain character. If their organizational statement of purpose isn't explicit enough for you, consider *their* characterization of the competition itself: "The goal of the essay contest is to award contestants for writing papers that summarize the best evidence available *for* the survival of human consciousness after permanent bodily death" [emphasis mine] (About BICS, 2021). The answer having already been decided, go forth and back it up with whatever you can find.

But this is not science; antivaxxers and climate change deniers can appeal to some small subset of the total relevant evidence, too, and ignore any evidence that contradicts their beliefs. Would it go uncommented upon if an "evidence-based" contest asked: "What is the best available evidence that the Holocaust did not happen?" Any essay meeting that requirement would have no obligation to address the (substantial) evidence that the Holocaust was a genuine historical event. If Holocaust deniers are not within their epistemic rights to make this sort of move, neither are empirical survivalists. Alternatively, imagine a parallel universe where *independent* geological estimates of the age of the Earth happened to date the planet much younger than the time necessary for biological evolution to occur. Would we tolerate the omission of such a fact from biology textbooks, on the grounds that biologists don't do geology? Not if they were promoted as scientifically authoritative.

RANKING THE SURVIVAL EVIDENCE

DRW rank nine categories of "survival evidence" according to their assessment of the evidential strength of each, from the strongest to weakest sources of ostensible evidence for discarnate personal survival. They evaluate this evidence using a classroom "grading system, where the grades provide criteria for the credibility of the evidence," ostensibly emulating "several established ways for evaluating the efficacy of pharmaceutical drugs, medical interventions, and other forms of observational or empirical evidence in the life sciences," such as scoping reviews, systematic reviews, and meta-analyses (DRW, 2021*, p. 8). Their systematic review was designed to be both scoping and systematic, "scoping in that it considered a wide-ranging overview of the relevant evidence, and . . . systematic in that we developed a grading system that was uniformly applied to each of the evidential categories." In addition, DRW aimed to evaluate "representative examples of evidence rather than attempt to examine all possible studies or methods within each category" (2021*, p. 8).

The evidentiality of a source could be rated as strong (A), good (B), suggestive (C), unclear/conflicting (D), poor (E), or no evidence (F). While most classroom grading in the US doesn't distinguish between E and F, the difference is moot since in practice the authors rate the categories between a B+ at best (mental and physical mediumship) and a C at worst (spontaneous and induced apparitions, after-death communications). Cases of the reincarnation type (CORT) and NDE reports come in second with a grade of B-, followed by electronic voice phenomena/instrumental transcommunication (EVP/ITC) and reports of deathbed visions at a C+. Notably, although none of the nine sources are rated as unclear/conflicting (D), some likely would be so rated even by other psychical researchers sympathetic to survival—for example, reports of EVP/ITC or of induced apparitions ("scrying"). Presumably the authors only consider spontaneous memories of previous lives in the section on reincarnation, saving comment on those induced under hypnosis for the "Induced Experiences of Survival" section, for this very reason. DRW conclude that section with the comment: "The evidential grade assigned for induced experiences is C because nearly all available evidence is anecdotal, and none is prospective" (2021*, p. 23).

In most classroom settings, A indicates "excellent," B indicates "good," C indicates "satisfactory," and D indicates "poor" or at least "needs improvement." If DRW are really emulating medical standards for evaluating efficacy, one would think that any source deemed to be nearly all anecdotal and none prospective would come in at a D at best. And reported NDE content across cultures (and even within them) certainly warrants the conflicting characteriza-

tion (Augustine, 2015b, pp. 542–550; Belanti et al., 2008; Groth-Marnat, 1994; Lester, 2015, pp. 645–646). Moreover, if we take these different sources to be evidence about the character of an actual afterlife, there are telling conflicts *between* the sources about that character. For example, is "going discarnate" so lucid that it's impossible to forget, as suggested by OBErs and NDErs who ostensibly "return to the body," or are discarnate memories immediately/gradually suppressed once normally embodied, as suggested by CORT? Such conflicts cast doubt on Nahm's purported "interrelatedness of the different survival phenomena that lend support to each other" (2021*, p. 65, Fig. 4). And they are all the more telling because Nahm, at least, *concedes* them, concluding: "the *qualitative strength* of NDEs [B- in DRW] is 'relatively low' (2) because most are subjective experiences that take place during times of unconsciousness, and they are clearly culturally influenced," adding that claims of veridical paranormal perception during "critical brain conditions" are weak because "there are usually only a few eyewitnesses who can support the statements of the experiencer in an unambiguous manner" (2021*, pp. 16–17).

But these are quibbles. On the face of it, using long-standing medical principles to evaluate evidence is an encouraging way to parse the issue (cf. Augustine & Fishman, 2015, pp. 206–208, 278n9). And DRW are wise to avoid distinguishing LAP from survivalist interpretations of this evidence here, as the age-old LAP–survival debate looms large as a *distraction* from assessing the state of the survival evidence itself. A better approach, or at least one more congenial to the researchers outside of psychical research that DRW seek to engage, is to wrap all paranormal interpretations into a single umbrella paranormal hypothesis (Augustine, 2015a, p. 35, 41n43) and compare *that* to conventional explanations of the survival evidence. Whether we should interpret that evidence in nonsurvivalist paranormal terms instead of discarnate personal survival is best left for a separate discussion (in DRW, 2021*, pp. 34–35).[10]

One final methodological concern: using letter grades to signify the evidentiality of each of the nine sources of survival evidence is helpful, but the underlying criteria used to assign those grades are questionable. For example, DRW's grade criteria decision matrix includes problematic criteria like "No plausible materialistic (psychology or neuroscience) explanation" (2021*, p. 11, Table 2). As Sudduth (2021) has shown, cases that are regularly hyped as among the best evidence for survival are all too often easily susceptible to normal explanations, if only survival researchers would give conventional explanations a chance. It's also often unclear how to validate the reliability of this structured grading system given that the authors speak of general lines of evidence from each source, rather than, say, evaluating the evidential features of the (heralded) best cases from each of the sources considered (as Gauld, 1982, pp. 32–108, 178–182 does for mental mediumship and CORT, and Sudduth, 2016, pp. 47–133 does for OBEs and NDEs, mental mediumship, and CORT). DRW's appeals to dubious examples of supposed evidence for survival will make both concerns clear in what follows.

As we proceed through each type of survival evidence, keep in mind that when assigning specific letter grades to the credibility of particular evidential claims, DRW determined that "none of the categories achieved an A level," defined as "strong evidence" (2021*, p. 10, Table 1). The overconfident claims of other winning contestants of having "unequivocally disprove[n] the modernist view that consciousness ends with bodily death" (Mishlove, 2021*, p. 93), or that "the statistically significant scientific evidence described above, collected under randomized, controlled conditions in order to address falsifiable hypotheses, meets if not surpasses what could be considered proof beyond a reasonable doubt in a court system" (Beischel, 2021*, p. 62),[11] stand in stark contrast.

After explaining their procedure, DRW go on to evaluate their nine types of survival evidence in order from best evidence (B+) to worst (C). They cite the long-heralded mental mediumship of Mrs. Piper (but cf. Dodds, 1934; Gauld, 1982, pp. 109–118; Moore, 1981, pp. 82–101), drop-in communicator cases like that of Runki's leg (cf. Braude, 2003, pp. 43–51; Moore, 1981, pp. 115–126; Sudduth, 2016, p. 97, 97n17), the use of proxy sitters in historical trance mediumship, and the Pearl Curran/Patience Worth case (cf. Braude, 2003, pp. 170–173; 2021*, p. 30; Diliberto, 2010) before characterizing Beischel's contemporary triple-blind laboratory mediumship research as having used "rigorously controlled protocols [that] have demonstrated that some mediums can accurately gain information well beyond chance expectation" (DRW, 2021*, pp. 13–14). However, in making this determination, they cite sources written prior to or simultaneously with an independent assessment of that research that I commissioned (Battista et al., 2015), and thus are not responsive to its criticisms, which I previously paraphrased:

> [T]he contributors canvass how Beischel and Schwartz use two different ways to describe the same data in order to overstate the force of their results, their use of statistically invalid analyses and concepts that render their results "statistically meaningless," their failure to disclose the only statistically meaningful data that they have, their use of procedures prone to "inflate the rate of false positives," the openness of their experi-

> mental design to merely "collecting data until positive results emerged," and how optimizing the differences between sitters' actual readings and their control readings "essentially rigged the experiment to produce the result that they wanted" (pp. 619–625). While Matlock believes that the statistical flaws present in their triple-blind study "appear to be corrected in a follow-up quintuple-blind study," there is no way for anyone to know since "the details of its implementation have never been published . . ." (Augustine, 2016, pp. 230–231)

Beischel has yet to respond to these criticisms or release the requested raw data to allay these concerns (in either her triple-blind or quintuple-blind study), either in print or on the Windbridge website.

The sources that I cite above (like those below) argue that the evidence in the other ostensible survival cases that DRW cite is weak. The confounding cross-correspondences are also summarized, though the likelihood that the investigators, not the deceased, are the ones fitting these pieces together into a pattern is not acknowledged (cf. Braude, 2003, pp. 95–99; 2021*, p. 34; Moore, 1981, pp. 102–114; Moreman, 2003, 2004). Christopher M. Moreman's defense of his replication using pseudoscripts is telling:

> [M]y study was designed to find whether the patterns and meanings detected in the original scripts might or might not be the result of chance combined with the ingenuity of the investigators. Certainly, the design that I used permits more than one conclusion, though the results of my study have demonstrated only one. *If my scripts had not produced similarly striking patterns to the original C-Cs, then the conclusion would have been quite different*. [emphasis mine] (2004, p. 60)

Here, too, restricting who can access the raw data seems to be an issue:

> E. J. Dingwall, in a recent "blast" at psychic researchers, claimed that the Society for Psychical Research refused to permit adequate access to independent investigators in the matter of the famous cross-correspondences. He claims that people who want to know details of those cases will still meet every sort of obstruction, evasion, and refusal of requests to verify details of the stories in question. If I had the knowledge of Mr. Dingwall, I might have been even more troubled by Gauld's acceptance of the reports on cross-correspondences and related matters at more-or-less face value. I reiterate my disappointment in the lack of adequate discussion of such matters in this book. (Dilley, 1984, p. 68)

DRW also mention supposed instances of xenoglossy/glossolalia (cf. Thomason, 1984) and the manifestation of previously unmanifested skills (cf. Braude, 2003, pp. 117–118) as potential evidence for personal survival. Nahm similarly lists "the following three facets of mental mediumship [that] are often regarded most compelling":

> - Astonishing quality and quantity of accurate information conveyed by seemingly purposeful communicators via extraordinarily gifted mediums
> - Drop-in-communicators
> - Cross-correspondence (Nahm, 2021*, p. 11)

Nahm adds that, on some (not all) occasions, Mrs. Piper "was even observed secretly by private detectives to ascertain that she didn't acquire her knowledge via mundane information channels" (2021*, p. 12). Here DRW's "No plausible materialistic (psychology or neuroscience) explanation" criterion rears its ugly head, particularly the qualifier "plausible." Context is important, too; the fact that historical trance mediums' accurate statements must be fished out of reams of twaddle (James, 1909, p. 115) is surely relevant to any plausibility assessments here, as is the agreed-upon fact that a significant proportion of the entities that they claimed to contact were undeniably fictitious constructions of the mediums' own minds. Certainly the latter more than offsets any gain provided by appealing to the "never caught cheating" card, which is hardly conclusive in any case since Mrs. Piper had access to gossip within a large web of her community connections (Gauld, 1982, pp. 36–37). (And empirical survivalists seem more willing than others to overlook instances where mediums *have* been caught cheating anyway.) Like Old Testament miracles conveniently tucked away from the prying eyes of modern television cameras, Nahm acknowledges that "for many decades, extraordinarily gifted mediums, drop-in communicators, and cross-correspondences haven't been investigated, presumably because suitable mediums and researchers were simply not available," and thus despite the potential for rigorous scientific investigation of trance mediumship today, "the investigability of the most compelling aspects of mental mediumship is only 'relatively low'" (2021*, p. 13). Given the weight that both DRW and Nahm give to historical trance mediumship, readers may be surprised to read Nahm's overall assessment:

> However, because all communication with ostensible interlocutors from the beyond must be conducted via a medium serving as intermediary, and because these mediums are often in trance, even veridical information provided by these ostensibly deceased individuals is still prone to being attributed alternatively to 1) the retrieval of latent forgotten knowledge, or 2) a psi-conducive dissociated state of the medium that enables the retrieval of information clairvoyantly or telepathically, but without entailing a factual deceased communicator. Therefore, the qualitative strength of mental mediumship cannot be regarded as "high." (Nahm, 2021*, pp. 13–14)

By contrast, DRW conclude:

> The evidential grade assigned to mediumship is B+ because these cases represent some of the most compelling evidence for survival, including studies with objective data, multiple independent researchers reporting similar results that do not require statistical arguments, and effects that are observable in real-time. While some mediums were found to be fraudulent, others studied for decades were not. The reason mediumship does not achieve an A grade is that one could argue that the results could be achieved through forms of [psi-in-the-lab] or [psi-in-the-wild]. (2021*, p. 15)

It's notable that although psychical researchers often take suggesting the possibility of fraud to be the refuge of scoundrels (e.g., Carington, 1940, p. 265; Sidgwick, 1882, p. 12), the fact that it does pervade the history of mediumship ought to spur them to reconsider. Moreover, fraud would mimic psi pretty precisely, since it is purpose-made to do exactly that in these contexts (e.g., Spraggett & Rauscher, 1973), and the most dramatic examples of psi-in-the-wild not only fail to rule it out, but sometimes even *detect* it (see below).

It's therefore rather surprising that DRW also assign a high B+ grade to the evidence from physical mediumship since it serves as an exemplar of fraudulent phenomena. They dash through the history of fraud in this setting, the fact that the 19th-century physical medium Daniel Dunglas Home was never definitively exposed to be engaged in fraud, and the suspect Scole sittings in the 1990s. On Scole, they oddly write that "no one has been able to demonstrate how this series of events could have been accomplished by fraud" (2021*, p. 16). In fact, however, it is *well-known* that a great deal of positive evidence of fraud was uncovered in these sittings (e.g., Cornell, 1999, p. 402), and thus that no "psychokinetic effect on photography" need be invoked *at all* to account for the "detailed text [that] was produced on film" kept in a padlocked box (DRW, 2021*, p. 16). For one, the characteristics of that very text are one of the key pieces of positive evidence of fraud (Cornell, 1999, p. 398; Gauld, 1999, pp. 413–414). For another, the box was found to be easily opened by normal means (Gauld, 1999, pp. 404–405), after which it was replaced with secure envelopes and then a secure box, both controls curiously having the effect of preventing any further text from appearing on film (West, 1999, p. 393). The more recent and similarly suspect Felix circle sittings (Braude, 2014, 2015, 2016; Nahm, 2014, 2015, 2016) are unmentioned. DRW also inform us that "Fraud was never detected in" the early 20th-century Kulski molds (wax casts of human hands), even though plausible normal ways of producing them are not hard to come by (Polidoro, 2009).

The history of exposures of fraud in these investigations, the typical need for darkness in order for the phenomena to manifest (Cornell, 1999, p. 403; West, 1999, p. 394), and the likely use of shills to help produce effects, all of which DRW note (2021*, p. 17), "ought reasonably to beget a suspicion against all relations of this kind" (Hume, 1748/2000, p. 89). In light of its history, DRW's conclusion comes off as outright Pollyannaish: "The evidential grade assigned to physical mediumship is B+ because of the striking nature of the legitimate phenomena and multiple witnesses. However, there are fewer than ten highly credible cases, so confidence in these cases is not sufficiently high to rate an A" (2021*, p. 17). Contrast their take on what they deem "the legitimate phenomena" with that of late poltergeist investigator A. D. Cornell:

> One must nevertheless take into account the possibility that they were so enthralled by the dramatic performance of it all in the dark that they accepted without questioning enough whether it could have other than a paranormal explanation. Alan Gauld has shown how in the dark the padlocked Alan box could be opened and closed in a matter of seconds. The Dragon film images were all taken from an easily available book and displayed clear signs of how they could have been produced by normal means. The same applies to the Ruth film handwriting, which has all the appearance of a photographed hand-traced copy of the reproduction, slightly reduced in size, of the original page corrections in Christie's Catalogue. In view of the normal explanation that could be given for many of the phenomena, one is bound to ask whether a high proportion if not all were wrongly interpreted . . .

> What better way could the claims of the Scole Group be verifiably presented (and those of any other physical séance circle) than to have a replayable continuous infra-red video record verifying some of the physical effects for the whole world to see? The fact that such promises are repeatedly made by physical mediums but never come about, or are side-stepped at the last minute (as has been my experience several times), may well indicate that no such record is likely to be made. Such reluctance to allow what is really going on in the dark to be seen in every detail may well indicate a recognition that it would reveal too much and could sound the death knell of its practice. (Cornell, 1999, p. 398, 403)

If DRW mean to include the Scole sittings and the Kulski molds as examples of "legitimate phenomena" and "highly credible cases"—and why else would they summarize them here if they do not—then their grading system, though promising in concept, is fundamentally flawed in execution. Too much subjectivity is introduced when the letter grade that one assigns relies on contentious criteria like "No plausible materialistic (psychology or neuroscience) explanation" or "Not likely fraud" (DRW, 2021*, p. 11, Table 2), since one's judgment on those matters relies on faith in how hard survival researchers have worked to look for such alternative explanations while simultaneously aiming to find evidence *for* discarnate personal survival, which undoubtedly disincentivizes them from looking too hard (cf. Braude, 2021b*, pp. 29, 31–32; Sudduth, 2021).

The absence of clear-cut permanent paranormal objects (Beloff, 1990, pp. 191–202; Polidoro, 2009; Tort, 1991) produced by physical mediums should clue in any reasonable person of the dubious reliability of this phenomena as a source of evidence for the paranormal in general, let alone for discarnate personal survival. And of course there was never any need to invoke the existence of deceased human spirits to explain any genuine paranormal effects from physical mediums anyway, should there be any.

DRW assign CORT a B-, mainly because "there are no prospective studies, and this phenomenon does not lend itself to strict controls" (2021*, p. 18). While both of those features indeed reduce the evidential value of such cases, their conclusion is nevertheless somewhat surprising since, like Nahm, most survival researchers tout CORT as constituting "the best" of the survival evidence. Against the grain, Braude's prize-winning essay gives good principled reasons to rank CORT as less convincing evidence than historical cases of mental mediumship—namely, the failure of CORT investigations to rule out conventional explanations in practice and their reliance "on evidence that's dauntingly difficult to investigate and evaluate" (2021*, p. 34). As a result, Braude concludes that CORT "are too often hobbled by investigative intricacy, psychological superficiality, and a failure to deal in an empirically-informed way with challenges from the Unusual Suspects" (2021*, p. 48), such as the dissociative skills and latent abilities that can fully account for the mental mediumship of Pearl Curran/Patience Worth in conventional terms.

While Braude's assessment of CORT is consistent with DRW's ranking, it's unlikely that DRW were aware of these reasons, for Braude only expressed his change of heart about the evidential value of CORT recently, in his contemporaneous prize-winning essay itself, and largely due to the then-unpublished findings of Sudduth (2021), which exposed the sloppiness of the investigation of a long-overhyped CORT (one ranked as the second-best "before case" by Nahm).[12] It's notable that, in contrast to DRW's ranking and Braude's evidence-based change of heart, Nahm makes CORT central to his case for discarnate personal survival, going so far as to characterize it as "the *core evidence* for survival," labeling the remaining three types of survival evidence that he looks at more deeply—after-death communications (ADCs), NDEs, and mental mediumship—"ancillary evidence" (Nahm, 2021*, p. 65, Fig. 4).

DRW note that "In a number of these cases, alternative mundane explanations could not be found" (2021*, p. 17). That's undoubtedly true, but *why*? Could the absence of credible conventional explanations of CORT be an artifact of the fact that they were not investigated deeply enough? This is not some mere possibility; Sudduth (2021) has already *demonstrated* an example of it in what Nahm deems to be his second-best before-case (2021*, p. 28, Table 2), which Nahm characterizes as "impressive" (2021*, p. 28, Table 2), indeed "quite remarkable" (2021*, p. 26n12), and even "well-documented" (2021*, p. 26). Moreover, this was evidenced for purportedly one of *the best* kinds of CORT, those "before-cases in which the statements had already been recorded *before* the families met and there was [supposedly] little chance to add correct information" (Nahm, 2021*, p. 17). Nahm primes us to believe that "Retrospective tampering is much more difficult and unlikely in these cases"—of which there are 31 out of over 2,500 total CORT (~1%)—"thereby rendering their essential features much more authentic" (2021*, p. 24). From this flawed assumption, Nahm extrapolates much more than the evidence can support:

> Because of this information exchange between the families, one would expect a higher percentage of correct statements given by the interviewees in CORT when statements were recorded only *after* they interacted ("after-cases")—compared

> to those in before-cases in which the statements had already been recorded *before* the families met and there was little chance to add correct information. Furthermore, one would expect the total number of correct, incorrect, and unverified statements to be lower in before-cases.
>
> In a study comparing both types of CORT, however, they yielded approximately equal percentages of correct statements. The average overall number of statements was even *higher* for the before-cases. (2021*, p. 47)

This would be impressive only if normal/conventional sources of information for ostensibly anomalous knowledge were not present in before-cases, and we already know that they *have been*.[13] In the absence of (1) a list of all of the alternative mundane explanations that were considered and (2) a detailed explanation of how each of these were definitively ruled out, what more can someone approaching this evidence with an evaluative eye profitably say?[14] While the use of leading questions certainly creates "a problem assessing the information provided under direct questioning" (Braude, 2021b*, p. 30), this methodological concern pales in comparison. DRW's "No plausible materialistic (psychology or neuroscience) explanation" criterion isn't some minor worry, but the *crux* of any evidential assessment here. The boggle factor for CORT requires the assumption that there is no normal source of any (nonspurious) factual correspondences. That element of mystery has to be maintained for these cases to warrant further parapsychological investigation; otherwise, as soon as a conventional explanation surfaces, a case loses interest.

Nahm masks his **hasty conclusions** with a number of qualifiers, such as "Given these cases are authentic . . ." (2021*, p. 44). Presuming that conventional explanations have really been ruled out is an important example, but there are others, and they are representative of the hasty conclusions of other prize-winning essays. Consider that, even if CORT have entirely psychosocial origins, there will be patterns in the data that one has collected about them (as there are in, say, alien abduction experiences). Survival researchers can easily sift through some data, find some patterns, and then retroactively declare these patterns to be "predictions" of the reincarnation hypothesis. But are they really its predictions? A simple thought experiment provides an answer: prior to having reviewed any data, would you have said, based on the reincarnation or simple survival hypothesis alone, that you would have *expected* to find some particular datum? Can you honestly say that in hypothesizing reincarnation you would have predicted (*pre*-Stevenson) that we should find feature X prior to collecting any CORT, back when you had no idea whether or not X was actually present in CORT?

To be a genuine prediction, a particular item has to be *derived from* a hypothesis in some way. It has to either be deductively entailed by the hypothesis (as the phenomenon of falling apples is logically entailed by Newton's theory of universal gravitation), or else made at least more probable than not by the truth of that hypothesis. There are ways to show that a particular outcome would be more probable if a hypothesis were true than if it were not (such as the previous section's endnote *argument from* analogy comparing the mind and brain to software and hardware, respectively, to derive which facts would be more expected on the dependence thesis than on its antithesis). But *some* inductive argument has to be given for *why* we should expect a particular fact to be found were a particular hypothesis true, and one must show that it is a *good* argument (e.g., by showing that there are more relevant similarities than dissimilarities between analogues). If one cannot do that, then there's no reason to call a particular outcome a *prediction* of a hypothesis. Anyone can just mold a hypothesis to fit whatever data one has at hand, in what philosophers of science deride as *accommodation* rather than hypothesis-derived *prediction*.

Nahm's reincarnation hypothesis "predictions" are paradigm cases of accommodation. First, he argues that "cases involving young children who speak spontaneously about past lives are most compelling because they are less prone to being created artificially than cases involving adults, be it purposefully or involuntarily" (2021*, p. 17). The existence of play with imaginary friends and other kinds of pretending suggests otherwise, and there are undeniably "artificially created" childhood CORT (Chari, 1978, pp. 317–319; Cook et al., 1983, pp. 133–134; Stevenson et al., 1988, pp. 22–26). Second, Nahm regards as supporting evidence the fact that "about 20% of CORT subjects report having memories of events that occurred during the intermission between their death in the previous life and their birth into the current life" (2021*, p. 18), given that there could have been *none at all*. But this commits the **fallacy of understated evidence**. In assessing rival hypotheses,

> one should use a more specific evidence statement that one knows to be true instead of a less specific one whenever different results would be obtained by doing so. For example, when comparing the hypothesis that Mona intends to harm Lisa to the hypothesis that she intends to benefit her, one might seek to determine how probable it is (antecedently) on each of the two hypotheses that Mona is bringing a butter knife to her meeting

> with Lisa. To ignore this more specific knowledge and focus instead just on the more general knowledge that Mona is bringing a knife (of some sort) to that meeting would commit the "fallacy of understated evidence." (Draper, 2020, pp. 179–180)

Here Nahm disregards the *absence* of reports of intermission memories in the vast majority (~80%) of CORT. That is, in taking the existence of any intermission "memories" to be evidential, he disregards the more specific issue of why there are so few of them. On the face of it, if one can really remember aspects of an even *older* past life, then one should (usually) also be able to remember aspects of a more recent (and perhaps half-a-century-long) intermission period between that life and the current one, all else held equal (assuming that before-life memories function like those already known to exist, anyway). Third, Nahm presumes that the birthmark evidence supports the reincarnation hypothesis, but never shows us *how* that hypothesis—when unamended with numerous *untestable* auxiliary assumptions that do the work of yielding that "prediction"—leads us to expect "physical features such as birthmarks or birth defects that can contribute to the identification of a matching previous personality" (2021*, p. 19). It's worth noting that here Nahm also appeals to the number of eyewitnesses interviewed, as if the sheer quantity of witnesses tells us anything about the evidentiality of their testimony. As I previously wrote about reports of veridical NDEs:

> Their value ultimately depends on how well they can be corroborated by *independent* testimony (i.e., testimony where agreement between witnesses is not simply the result of witnesses having talked among themselves before an investigator interviewed them, or the result of independent witnesses gleaning congruent information from the same third party). [emphasis mine] (Augustine, 2019, p. 595)

One last thing is worth noting about Nahm's assessment of CORT. He's unimpressed with Stevenson's critics, asking rhetorically:

> Was Stevenson, a trained psychiatrist who had even published a book on psychiatric examination in 1969, really so naïve that he exposed himself to all this stress and danger for four decades without ever realizing that every case he investigated rested on misinterpretation and fraud, as some of his critics presume? (2021*, p. 22)

This fails to take into consideration the power of **motivated reasoning**, which is transparent in Stevenson in other places. For example, on cross-cultural comparisons of NDE reports, which Nahm concedes are characterized by more differences than similarities (2021*, p. 18), Pasricha and Stevenson wrote of reports of encounters with others in NDEs: "For Americans this is usually a deceased relative or friend; for Indians it is usually the messengers (Yamdoots) of the god of death. The variations in the persons of the 'next world' do not weigh against (or for) their reality" (1986, p. 169). Some NDE content variations do carry such weight (Augustine, 2015b, pp. 549–550), but more to the point, those less invested in Stevenson's mission—such as those agnostic scientists that DRW aim to persuade, or the suicidologist David Lester (2015, p. 639)—might see these comments as instances of wishful thinking.

More surprisingly, DRW rank NDE reports as providing the same degree of evidential support for personal survival as CORT, grading them at an equivalent B-. Such equivalency might be justified if it based solely on the similar lack of (successful) well-designed prospective studies of paranormal perception or influence in OBEs or NDEs, and the concomitant inability to rule out normal sources of information or influence in anecdotal cases. And indeed, this at least seems to be a factor, as DRW acknowledge that "from a strict evidential perspective, the degree of confidence that can be assigned to them is low" given the absence of strong evidence for veridical paranormal perception under controlled conditions: "There are no cases of OBEs associated with NDEs that could be verified under strictly controlled, *planned* conditions" (2021*, p. 19). In contrast to DRW, psychical researchers have tended to hold up CORT and historical mental mediumship as among the best sources of evidence for personal survival, typically giving much less evidential weight to OBEs or NDEs. Braude, for example, concludes that "the case for survival receives very little *independent* support from OBEs, NDEs, and apparitions" (Braude, 2003, pp. 280–281), and Nahm considers them "ancillary" rather than equivalent to CORT in terms of their evidential strength (though for Braude, this is only secondarily due to the weakness of the evidence itself).

For Braude, even if we *had* an evidentially ideal OBE/NDE or apparitional case, it's less clear that personal survival is what such a case would be evidence *of* (compared to an ideal CORT or mental mediumship case). It's thus not without reason that the three classics (Braude, 2003; Gauld, 1982; Sudduth, 2016) assessing the overall survival evidence produced in the last 40 years concentrate on the evidence from mental mediumship or CORT even though there is an equally old and large psychical research literature on apparitions and astral projection/remote sensing

(going back to the early days of the SPR).

However we compare them to the other sources, the evidence for personal survival from OBEs/NDEs *is* weak. Mishlove is seemingly unaware of how awkward the evidence that he cites for a survivalist interpretation of NDEs becomes. A single NDEr self-reports (after the fact, of course) having accurately learned the outcome of a future presidential election and a Superbowl game during her NDE (Mishlove, 2021*, p. 25), and along with some secondhand reporting from van Lommel, the case for prophetic NDEs rests. But it shouldn't, as Kenneth Ring systematically investigated prophetic visions (PVs) during NDEs in the early 1980s, concluding: "at least some of the specific predictions that have been made by near-death survivors who have reported PVs have been *wrong*. Another [salient feature] is that, to my knowledge, there are only retroactive claims of successful predictions" (1982, p. 66). As noted in an earlier section, in the age of the Internet there's no excuse *not to* preregister predictions online in order to securely validate their timing—unless, of course, one doesn't really possess knowledge of future events.

Nahm argues that "evidence favoring the notion that brain chemistry cannot fully account for OBEs and NDEs comes from their occurrence in indistinguishable manners under conditions ranging from optimal oxygen supply in the brain to virtually no oxygen supply" (2021*, p. 15; cf. Fischer & Mitchell-Yellin, 2016, p. 82; van Lommel, 2021*, p. 7). Simply substituting Nahm's "OBEs and NDEs" with "realistic hallucinations" in that sentence makes plain the shakiness of the argument. It's also undermined by the discovery of even more hypnagogic-like dreamlets than classic NDEs during cardiac arrest:

> Although the AWARE study was designed to vindicate the view that NDEs are not hallucinations, the results ironically have had the opposite effect. The study found that of the fifty-five reported cardiac arrest experiences, forty-six (84 percent) were clearly dreamlike hallucinations, with the remaining classic NDEs constituting only 16 percent (nine of fifty-five) of the total (Parnia et al., 2014). These results alone are sufficient to refute premature arguments that it is simply impossible for the brain to generate any experiences during cardiac arrest, and thus NDEs cannot be brain-generated hallucinations (see, e.g., Greyson, 2010; van Lommel, 2006). They also raise the possibility that classic NDEs are simply a subset of these dreamlike hallucinations. Perhaps the more coherent of the dreamlike narratives simply get labeled as reports of NDEs because they happen to have an otherworldly theme (because expectation of imminent death during cardiac arrest calls up afterlife imagery at the time). (Augustine, 2019, p. 595)

Contra Nahm, the crucial evidence for veridical OBEs (during NDEs or otherwise) is also weak. OBEs that can be timestamped as occurring when brain activity is not "sufficient to enable the accurate perception of events," Nahm argues, "provide considerable evidence for the notion that in these situations, human consciousness operates independently of brain states" (2021*, p. 15). But where are these simultaneously veridical *and* timestamped experiences? On the one verifiable recalled event during the AWARE study, Parnia and Shirazi write: "the recalled experience relating to actual events occurring in the resuscitation room was verified as being accurate, correct, and *consistent with* real events that had occurred some 3–5 minutes after the heart had stopped and when the brain was *expected to be* either severely disordered or not functioning" [emphasis mine] (2021*, p. 49). First, note that a detail can be "consistent with" an event without referring to it (e.g., Sudduth, 2021, p. 1006); and second, that the claim that the brain is (effectively) offline during the "consistent" event is just a *conjecture*, not a *fact*.[15] Compare Braude: "Even those sympathetic to NDE research would probably admit that this body of evidence is not the best evidence of survival . . . NDE studies face the notorious problem of accurately timestamping the NDEr's experience—something that can only be attempted after resuscitation" (2021b*, p. 48). Even if we could accurately timestamp *when* an NDE occurred, Braude adds, we still face an *additional* problem noted by Cook et al., (1998), namely: "If we don't know the physical or physiological conditions required for ordinary cognitive functioning (much less optimal cognitive functioning), we should be wary of drawing conclusions about the significance of the evidence" (Braude, 2021b*, p. 49). In other words, there's an inherent Catch-22 in Nahm's argument that "neurophysiological models cannot account for conscious awareness during apparent states of unconsciousness such as in critical NDEs or Juan's evident coma" (2021*, p. 15). Namely, if neuroscience cannot accurately determine when we should be consciously aware, then near-death researchers cannot argue that awareness occurred at a time when it would be neurally impossible, and thus is anomalous.

Nahm seems oblivious to yet another Catch-22, writing: "it is intriguing that blind people, even those blind from birth, report having NDEs that include visual imagery *comparable to that in [the] NDEs of those who can see*" [emphasis mine] (2021*, p. 15). This immediately recalls a famous thought experiment in the philosophy of mind (Byrne, 2020; Shoemaker, 1982) and raises the question: how could we possibly know whether congenitally blind

NDErs actually "see" what sighted NDErs see? Without any frame of reference for what constitutes *sight*, congenitally blind NDErs might *report* experiences comparable to those reported by sighted ones, coopting the same language that they picked up from the sighted. But what reason could we ever have to believe that their visual language refers to actual *visual experiences*? Whatever experiences congenitally blind NDErs are referring to when they use visual words, they almost certainly are not referring to *vision*. Harvey J. Irwin picks up on this point, writing:

> A very short note . . . reiterates the proposal that while blind NDErs and OBErs may depict their experiences in terms of visual impressions, this tendency simply involves the unwitting reformulation of an experience of mindsight in terms of constructs that the experient herself or himself can comprehend. This concluding chapter might better have given more emphasis to the most fundamental implication of the project's finding of NDEs in congenitally totally blind people, namely, that *the perceptual-like impressions in NDEs and OBEs evidently are not perceptual at all*. [emphasis mine] (2000, p. 112)

To overcome the main difficulties with the state of the evidence itself here, we need (1) replicable positive results from experiments designed to test veridical paranormal perception during OBEs/NDEs under *controlled* conditions in which (2) the experiences can be definitively time-stamped to a period when brain activity is virtually nonexistent, such as during the deepest hypothermia of cardiac standstill. But we lack cases meeting *either* requirement, let alone both (e.g., Beauregard et al., 2012; Horizon Research Foundation, 2010), so questions about how to best interpret such purely hypothetical cases are moot.

DRW next assign EVP/ITC a C+ grade, which seems overly generous. They note that "misinterpretation of signals from mundane sources is an obvious problem," but the most prevalent factor is likely "substantial noise, giving rise to auditory pareidolia, the tendency to subjectively perceive meaning in randomness" (a factor obviously exploited in paranormal reality television). As seasoned experimentalists, DRW rightly propose that, for those who find this research worthwhile, "independent judges should be asked to assess, under blinded conditions, if they hear the same material" and that "objective methods, like spectrographic analysis of purported voices, should be performed" (DRW, 2021*, p. 20), noting that the latter was reported in at least one study (MacRae, 2005). Other studies have found little reason for parapsychologists (unlike psychologists) to pursue this line of research (Barušs, 2001; Ellis, 1978). DRW's conclusion:

> The evidential grade is C+ because in most cases (not all), claims of voices or messages are determined subjectively, and even in cases where there is some objective evidence, the effects could still be attributable to [psi-in-the-lab], [psi-in-the-wild], or to mistakes of perception. (2021*, p. 20)

Indeed, perceptual misinterpretation seems sufficient to account for such phenomena even to fellow psychical researchers. In their recent overview of the subject, Mark R. Leary and Tom Butler write that "some debunkers do not seem to recognize the fallacy of concluding that all purported EVP are due to mundane causes simply because some of them clearly are" (2016, p. 347). The flip side is that some EVP investigators do not seem to recognize the fallacy of **shifting the burden of proof** in claiming that there are unknown paranormal processes, over and above known normal ones, involved in EVP. He who makes a claim assumes the burden of showing what he claims, period.

DRW grade deathbed visions an equivalent C+ "because all the evidence is anecdotal, and the experience itself, even if partially confirmed by other witnesses, is reported by a living person with impaired functionality" (2021*, p. 21). Citing the famous Osis and Haraldsson (1977) study, Nahm concludes that such visions "display an autonomy of their own that seems largely independent from the mental dispositions of dying individuals and their brain chemistry" (2021*, p. 19). He bases this on four postulates that the study tested (and did not find), but only one of these is more than weak evidence for a survivalist interpretation: "Patients dying rather unexpectedly and in the expectation of recovery should report more visions related to this world whereas patients dying in the expectation of death should report more otherworldly elements including deceased individuals" (Nahm, 2021*, p. 10). The fact that the study didn't find this is certainly interesting, but could easily be an artifact of the selective response or selective memory of the medical practitioners who provided the second-hand reports of the visions—that is, those who witnessed counter-to-expectation incidents might have been more likely to respond to the questionnaires than those who did not, or those who witnessed both might have been more likely to remember, years later, the more dramatic counter-to-expectation incidents than the incidents that would confirm this natural expectation.

The remaining three sources—apparitional experiences, induced experiences, and ADCs—received DRW's lowest assigned grade, C. On apparitions, DRW conclude that "despite a few cases with multiple witnesses, the rest of the available evidence is anecdotal and there are numer-

ous potentially mundane explanations," such as that (in addition some witnesses priming others) "the perceived [collective] apparition may be explained by group exposure to environmental factors that correlate not only with feelings of anxiety and/or disorientation but in extreme cases with hallucinations" (2021*, p. 22). Ruickbie more optimistically quotes Myers's 1886 conclusion that crisis apparitions (those within 12 hours of death) "are perceived by their friends and relatives with a frequency which mere chance cannot explain" (2021*, p. 28). This is often inflated to the specific claim that crisis apparitions occur 440 times more often than would be expected by chance (Sidgwick et al., 1894, pp. 247–248), but the mathematical reasoning behind that figure is dubious. Apparitions researcher G. N. M. Tyrrell concluded that he could not "attach any importance to [that] numerical conclusion" (1943/1953, pp. 19–20), and West's later investigations could not corroborate a single crisis apparition report (1948b, p. 196; 1990, p. 200). Even experimental designs utilizing observers or instruments as apparition detectors might mistake for anomalous common expectations about which locations are eerie, or simply detect drafts, changes in air pressure, pollutants, static electricity, infrasound, or artificial or natural sources of electromagnetic radiation (Stokes, 1997, pp. 175–176). Other features of apparitional experiences outright signal a conventional explanation (Augustine, 2015a, pp. 20–22).

As broadly as DRW define ADCs, quoting Susan Kwilecki—most often being ambiguously constituted by "an intuitive sense of presence, in vivid dreams, or in meaningfully timed appearances of birds or butterflies" (2021*, p. 24)—it's not surprising that these would rank as one of the weakest sources of survival evidence. When Mishlove reports that "Uncle Harry actually visited me in a dream when he died" (2021*, p. 6), I'm reminded of awkward Bible study questions like "Did God come to Abimelech in a dream, or did Abimelech simply have a dream about God?" (re: Genesis 20:3; cf. Hobbes, 1651/1994, p. 247). And while Nahm gives ADCs their due—"All cultures had or still have their seers, healers, or shamans who communicate with the deceased" (and notably with nature spirits, angels, demons, and gods, too)[16]—he concurs that the evidence that ADCs are nonillusory is weak:

> Obviously, the conditions of observation as well as the witness testimonies of ADCs are often not satisfactory. ADCs frequently come as a complete surprise, even on the sickbed. Also, most are only reported by a sole witness, or at best by a few individuals. And even in these collective cases, the witnesses may report divergent observations: Tom may report having seen a bright light, but Jerry may in addition report having seen a human shape in this light. Hence, ADCs imply a degree of subjectivity even in collectively perceived cases, which impedes the formation of an objective judgment about the witness testimonies. These aspects of ADCs are also relevant for their interpretation in terms of survival. On theoretical grounds, it is often not easy to determine whether an apparition perceived only fleetingly was created by the deceased individual him- or herself or was a hallucination of the living percipient. All this contributes to rendering the qualitative strength of ADCs "relatively low." (2021*, p. 11)

THE MIND–BODY PROBLEM, BOTCHED

Far too often, empirical survivalists' statements about the mind–body problem betray a stunning lack of familiarity with basic philosophy of mind, including material that would be typical of a freshman-level introduction to philosophy (PHIL 101) *unit* on the philosophy of mind, to say nothing of material from philosophy of mind *courses* (or textbooks). Statements made in some of the BICS contest-winning essays are no exception. In his third-place essay, Ruickbie briefly quotes philosopher and neuroscientist Alva Noë verbatim: "Consciousness does not happen in the brain. That's why we have been unable to come up with a good explanation of its neural basis" (Ruickbie, 2021*, p. 63). Now admittedly, that's not a bad **quote to mine** for one who maintains that consciousness can function completely independently of brain activity. But context is important. On Noë's next page we read: "At present, we have no better understanding of how 'a vast assembly of nerve cells and their associated molecules' might give rise to consciousness than we understand how supernatural stuff might do the trick" (2009, p. 6). Since by "supernatural stuff" Noë means Ruickbie's preferred alternative, this statement is telling. Noë is saying that the current neuroscientific understanding of consciousness is woefully inadequate because it's hardly any better than *that* antiquated notion! That's the *opposite* of an endorsement of Ruickbie's view.

Noë's actual position is that consciousness does not exist *only* in your brain, but *also* in other parts of the body and in the surrounding physical environment. The reference to "the biology of consciousness" in his subtitle might have been a clue. Noë is quite explicit about it:

> Maybe consciousness is like money. Here's a possibility: my consciousness now—with all its particular quality for me now—depends *not only on* what is happening in my brain but also on my history and my current position in and interaction with the wider world. [emphasis mine] (2009, p. 4)

In this sense, "there is no principled reason not to think of the wristwatch, the landmarks, the pen and paper, the linguistic community, as belonging to my mind. The causal processes that enable us to talk and think and find our ways around are not confined to what is going on in our skulls" (2009, p. 82). This simply expands philosopher of neuroscience Andy Clark's extended mind/embodied cognition thesis, which maintains that cognitive "operations are realized not in the neural system alone but in the whole embodied system in the world" (2008, p. 14). We could justly call Noë's view the "extended consciousness" or "embodied consciousness" thesis (cf. Chalmers, 2019, pp. 17–20), as he simply enlarges Clark's view to include not just cognition, but conscious experience itself. Ruickbie's use converts Noë's actual meaning into an **argument from ignorance**: we don't know *how* brain activity gives rise to consciousness, therefore it must not give rise to consciousness. If the argument were that we don't know *how* migrating birds navigate, therefore they must *not* navigate, it would not impress. Nor should it here.

Given the abstractness of Noë's view, Ruickbie's 180° misreading is perhaps forgivable. Others' mischaracterizations are much less so. Nahm defines "the physicalist model" as one that is "based on the assumption that consciousness can be explained by physics and its derivate, chemistry" (2021*, p. 5) before conflating materialism/physicalism, like too many other psychical researchers, with the notion that having a functioning brain is a necessary condition for having consciousness (the dependence thesis, Nahm's Jamesian "production hypothesis" being one version of it). Yet the very philosophers of mind who famously *press* that consciousness cannot be "explained by physics and its derivate, chemistry" simultaneously (and *explicitly*) maintain that it cannot persist in the absence of some underlying physical substrate, either (Chalmers, 1996, p. 121; Koch, 2012, p. 152; Strawson, 2006, p. 7). They thus illustrate that it is possible to hold both thoughts in one's head at the same time—if only one would try.

Nahm is absolutely right that "nothing in physics and chemistry predicts that protons, electrons, atoms, or molecules will produce something like consciousness" (2021*, p. 3), if by consciousness one means the qualitative experience of "what it's like" to, say, taste chocolate. And yet consciousness nevertheless seems to be as much a part of the natural world as anything else; it is found across the animal kingdom in varying degrees, strongly suggesting that it is a ubiquitous *biological* phenomenon, rather than something altogether different in kind (just consider how biological the feeling of pain or desire is). If that's right, then consciousness cannot survive biological death. It **does not follow** from the inability to explain *how* consciousness arises from matter that it does not so arise, and in fact its ubiquitousness throughout the biosphere positively suggests that it does (though see McGinn, 1999, pp. 89–95 and Nahm, 2021*, p. 64 for ways to get around this). And the distinctively *individual* consciousnesses necessary for *personal* survival almost certainly so arise.[17]

At one point, Braude subtly hints that "reductive physicalist views about the nature of mentality" are not equivalent to the mind's "apparent causal dependence on the body or the brain" (Braude, 2021b*, p. 2) by distinguishing them, but says no more about the difference. Where he has said more, he has been less careful, writing about "the lingering lure of physicalism" and citing the survival evidence as a challenge to "reductionistic physicalism and epiphenomenalism" since it "calls into question familiar forms of physicalism" (2005, pp. 241–242). The distinction matters for two crucial reasons. First, many contemporary philosophers of mind have been highly critical, for different reasons and for a long time, of both reductive physicalism and epiphenomenalism (which in the 19th century was a kind of *substance dualism*, à la Thomas Henry Huxley, before emerging as a kind of property dualism). Their often persuasive (if not decisive) criticisms simply *do not touch* the dependence thesis—and so are irrelevant to its viability. Second, there's a pernicious, subtle misdirection (or **red herring**) involved in changing the subject from the *evidence* for mind–brain dependence to the *metaphysics* of mind, as if one is attempting to render the evidence *against* discarnate personal survival inadmissible by simply redirecting attention away from it. One's particular theory of mind is irrelevant since some version of *every* mind–body theory is compatible with mind–brain dependence. Attempts to shift the conversation notwithstanding, the issue for discarnate personal survival isn't about *which* theory of mind one adopts, but rather how *whatever* theory of mind one adopts must be modified to do justice to discovered mind-brain correlations.

DRW fare no better, assuming almost definitionally that rejecting discarnate personal survival "begins with a reductionist materialist 'you are your brain' perspective" (2021*, p. 36, Fig. 2 caption). Under this understanding, computationalists and other functionalists are effectively materialists, "materialism" broadly encompassing any mind–body position that excludes the mind's ability to function independently of the brain—even though functionalists like the late Jerry Fodor were among the first to criticize reductive materialism and offer alternatives to it! They could rightly be called materialists, but certainly not *reductive* materialists. Moreover, computationalists and other functionalists would never say that you *are* your brain; at most, they would say that you are instantiated *in* a human brain, but you could've been instantiated in something else—like a silicon network, an extraterrestrial brain, or even an astral body or nonphysical substance (it's just

that, as a contingent matter of fact, a brain is what happens to instantiate your mind). An empirically *informed* functionalism thus rules out discarnate personal survival, but functionalism itself need not do so.

Similarly, DRW ironically cite "growing academic interest in notions like idealism, panpsychism, and neutral monism" (2021*, p. 33), as if *any* version of panpsychism or neutral monism—or any *informed* version of idealism—would license discarnate personal survival (and the presumed death of "nihilism" that DRW mistakenly think would come with them). Panpsychism and neutral monism exclude the metaphysical *possibility* that consciousness could persist apart from matter *in principle*. Indeed, the *coiner* of the term "neutral monism," Bertrand Russell, wrote:

> Although metaphysical materialism cannot be considered true, yet emotionally the world is pretty much the same as it would be if the materialists were in the right. I think the opponents of materialism have always been actuated by two main desires: the first to prove that the mind is immortal, and the second to prove that the ultimate power in the universe is mental rather than physical. In both these respects, I think the materialists were in the right. (1928/1986, p. 150)

As for panpsychism, even if we posit microexperience at the level of electrons, as panpsychists are wont to do, it won't be *human-level consciousness* that's fundamental. Human-level consciousness will only arise when a number of these microexperiential particles combine in the right way, namely to form a functioning brain (or at least some comparable physical structure). What makes us uniquely us—the "various motives, interests, and other attitudes idiosyncratically appropriate to that individual" (Braude, 2021b*, p. 17)—won't survive the brain's destruction at death even if "experience" in some generic sense persists. As philosopher Josh Weisberg notes, "when the basic substance is configured in the form of a brain, it then realizes phenomenal as well as physical properties. But that need not be the case when the fundamental stuff makes up a table" (Weisberg, 2012, §3g). Discarnate personal survival is *not possible in principle* if any of reductive materialism/type identity, functionalism/token identity (including computationalism), Aristotelian hylomorphism, Spinozaic dual-aspect theory, property dualism, or Russellian monism (whether "neutral" or otherwise) are true.

Although it's certainly true that when one rejects reductionist materialism (or accepts it![18]), "then some form of psi can be considered" (DRW, 2021*, p. 36, Fig. 2 caption), it's notable that virtually all philosophers of mind who reject reductionist materialism (or any other mind–body theory) do so for reasons that have nothing to do with psi; indeed, they are typically either skeptical or agnostic about the existence of psi,[19] discussing only its logical possibility and not its actual reality (e.g., Drange, 2015, pp. 331–332; Kim, 2015, p. 347n2).

Worse still, discarnate personal survival is very likely false *even assuming traditional Cartesian substance dualism, more contemporary non-Cartesian forms, or idealism*, though none of these positions *require* mind–brain dependence.[20] David Hume captured this point well without going through our alphabet soup of mind–body theories. Suppose that we grant substance dualism, that *some* sort of pre-existing consciousness is added to the brain during embodiment. Even so, *our* consciousness would not survive the destruction of the brain that enabled it:

> [W]e have reason to conclude from analogy, that nature uses [the spiritual substance] after the same manner she does the other substance, matter. She employs it as a kind of paste or clay; modifies it into a variety of forms and existences; dissolves after a time each modification; and from its substance erects a new form. As the same material substance may successively compose the body of all animals, the same spiritual substance may compose their minds: Their consciousness, or that system of thought, which they formed during life, may be continually dissolved by death; and *nothing interests them in the new modification*. The most positive asserters of the mortality of the soul, never denied the immortality of its substance. And *that an immaterial substance*, as well as a material [one], *may lose its memory or consciousness appears*, in part, *from experience*, if the soul be immaterial. [emphasis mine] (Hume, 1755/1987, pp. 591–592)

Brain activity may thus be a necessary condition for having human consciousness on *any* theory of mind.[21] Enter a non-Cartesian dualist: "Nor should we think it contrary to the self's status as a substance that its existence may be thus causally dependent upon the functioning of another, distinct substance—the brain, or more generally, the body" (Lowe, 1996, p. 41). Given the evidence for such, even an apologist for traditional Cartesian dualism—who believes that *some* immaterial part of us can survive—does not believe that a *discarnate* can have a mental life:

> The soul is like a light bulb and the brain is like an electric light socket. If you plug the bulb into the socket and turn the current on, the light will shine.

> If the socket is damaged or the current turned off, the light will not shine. So, too, the soul will function (have a mental life) *if it is plugged into a functioning brain*. Destroy the brain . . . and the soul will cease to function, remaining inert. But it can be revived and made to function again by repairing or reassembling the brain—just as the light can be made to shine again by repairing the socket or turning on the current. [emphasis mine] (Swinburne, 1997, p. 310)

Now admittedly, philosopher Richard Swinburne only concedes that the mind depends on the brain in order to function, not to exist. But that is a distinction without a difference, for personal survival is possible for "souls" *only when they are embodied*. So even on his most liberal interpretation of the mind–brain data, a *conscious mental life* cannot exist in the absence of brain functioning. Thus, *even assuming interactionism*, having a functioning brain (or similar physical substrate) is a necessary condition for having a mental life, at least for biological organisms—"and thus conscious experience must end when the brain ceases to function" (Gennaro & Fishman, 2015, p. 105). When even a traditional Cartesian dualist advocates the dependence thesis against discarnate personal survival, empirical survivalists ought to stand up and take note (cf. Braude, 2005, p. 244; Stairs & Bernard, 2007, p. 301; Sudduth, 2016, p. 27). After all, were it not for his prior religious conviction that God will resurrect us with the necessary bodies/brains to save us from annihilation or permanent unconsciousness, Swinburne would be making my case for me.

DRW take one last stab at securing personal survival through a theory of mind:

> [I]f consciousness does not emerge from the physical world but the other way around, as proposed by many esoteric traditions and the philosophy of idealism, then the answer to the question of survival is easy: Of course consciousness survives. It was here before the emergence of the physical world, and it will continue afterward. (2021*, p. 33)

But this is too quick. On idealism, the physical world is a mere appearance; only the mental is real. While I see little reason to affirm it (Augustine, 2016, p. 224), it's worth explaining why not even the truth of idealism could secure personal survival, given the empirical evidence against it.

Idealism describes what we normally think of as physical objects/events/processes as mental objects/events/processes. But this reframing does not change in the slightest the evidential implications of the *observed* correlations between mental processes and brain processes. Ostensible firewood will still ostensibly burn if placed in an ostensible fire. What fire does to an object—regardless of whether it's "really" physical, or just seems to be—is governed by laws of nature, our (approximate) knowledge of which are grounded by empirical *observation*. On something like George Berkeley's idealism, there are *perceived* objects in our individual minds, and then there are—external to us—*perceived* objects in the mind of God. A piece of firewood is ultimately just a perception in the mind of God, a divine perception that we somehow *also* perceive (or represent in our own minds). Our (internal) perceived object represents or is caused by God's perceived object (which is external to us). It still has an independent/objective existence outside of any of our individual minds (inside of God's mind), just not as part of a *physical* external world. So the falling tree makes a sound even if there's no one in the forest to hear it because God "hears" it. An idealist merely substitutes "it falls in the physical world" with "it falls in God's mind." The divine mind is what's fundamental, and our (unfundamental) minds somehow partake in what happens in it. Since our minds are derivative (like the firewood), we could disappear from God's consciousness (ceasing to exist as individual consciousnesses) just as easily as a dream character can cease to exist when the dreamer stops dreaming about him.

Note that nothing *empirical* has changed in this redescription. Individual brains still exist as external objects in an objective/independent divine mind, and they still stand in the exact same law-like relation to events in your individual mind as they would under realism about the physical world (otherwise one slips into solipsism, which everyone eschews). There is still a natural law—if natural is the right word for a fundamentally mental reality—that determines how your mind (your internal world) and your brain (an external object in God's mind) interrelate. Materialists track the underlying metaphysical reality in terms of physical laws, dualists in terms of psychophysical laws, and idealists in terms of psychological laws. On idealism, brains may not exist as *physical* objects, but they still exist as objects apart from to *our* own minds, so our minds still stand in a law-governed relation to those separate divine objects that we call brains. The dependence of consciousness on the brain in order to exist/occur is just such a relationship. As survivalist philosopher David Lund points out, if observed mind–brain correlations are "best interpreted as indicative of a natural law that conscious states exist only in association with brain activity, then it is a matter of natural law that we will not survive the destruction of our brains" (2009, p. 19)—even if we construe "brain activity" as activity in the mind of God rather than in the physical world.

Since idealism is pure metaphysics, it shouldn't come as a surprise that its picture of reality will be empirically

indistinguishable from that of its antithesis, realism. Our daily lives would be like living in a Matrix in which there are never any glitches to reveal the true underlying reality. Idealism is a rather abstract thought experiment, akin to the notion that you might really just be a brain in a vat and mistakenly think that you have a body, or be a victim of René Descartes' evil demon. But it's also a picture that we have no positive reason to affirm. Sure, it could theoretically be true, but if the world appeared and functioned in exactly the same way as it would if it were false, what would it matter?

What all of this means is that there is no general position on the mind–body problem that is inconsistent with the dependence of consciousness on the brain, not even one that denies that brains "really" exist at all. Whatever underlying metaphysics one adopts, all of it is empirically indistinguishable or operationally equivalent. If the destruction of the mental object in God's mind called "your frontal lobe" results in the destruction of a mental capacity in your own mind, then we have empirical evidence that our minds depend upon brains, whatever "brains" ultimately are.

CONCLUSION: NOT MUCH BETTER THAN RELIGIOUS FAITH

Champe Ransom recently wrote: "I admired Stevenson for his effort to obtain some real evidence for (as opposed to merely having faith in) the existence of reincarnation" (Ransom, 2015, p. 574). I share this attitude. I have respect for the project of psychical research in aiming to investigate the survival question scientifically, ostensibly respectful of the evidence. What I take issue with is the execution of that project, which is often (thankfully not always) anything but scientific.

Rhetoric about "scientism's dark shadow" (Mishlove, 2021*, pp. 10–13), how materialists would ask "why bother" if confronted by a drowning child (Tart, 2009, p. 298), DRW's claim that acknowledging that we might not live forever and ever and ever "leads to exaggerations of the worst vices of humanity: envy, greed, and selfishness" (2021*, p. 33), and so on, has no place in *science*. Survival researcher Charles Tart confesses: "If materialism is really true, my reaction is eat, drink, and be merry (and don't get caught by others if they don't approve of your pleasures), for tomorrow we die—and life doesn't mean anything anyway" (2009, p. 20). Pity that he thinks so little of himself that he believes that he would only behave morally if he could benefit from it in some way, such as in return for some postmortem reward. If that's really true—and I doubt that it is—then it's more of a reflection on the person who thinks that way than on any particular metaphysics. A moral person would help others not for the social credit it that generates for himself, or to benefit himself in some other way, but simply *for the sake of the other people* helped—regardless of that person's picture of the world. Simply believing in ghosts doesn't make evil people good, and not believing in them doesn't make good people evil.

These are all **arguments from consequences**, and good science can never be built upon a foundation of *fallacies*. Nor is it served by showmanship (e.g., Carter, 2011, p. 48), or transparent **appeals to emotion**:

> Materialism tells us that there is no purpose to anything. When we die, we are forever extinguished, and our atoms are recycled into other purposeless creatures. Eventually, all the suns will burn out, the universe will grow cold, and by a random fluke, the whole meaningless cycle might begin again. (DRW, 2021*, p. 33)

Going forward, will survival research be marked by throwing red meat to your base, or by investigating issues dispassionately for *everyone*?

I have seen this all before—in religion, not science. DRW could have easily cribbed their grievances from creationists railing against the ill effects of accepting that biological evolution occurs:

> 13. Belief in special creation has a *salutary influence on mankind*, since it encourages responsible obedience to the Creator and considerate recognition of those who were created by him.
>
> . . .
>
> 15. Belief in evolution has historically been used by their leaders *to justify a long succession of evil* systems—including fascism, communism, anarchism, Nazism, occultism, and many others.
>
> 16. Belief in evolution and animal kinship *leads normally to selfishness, aggressiveness, and fighting between groups, as well as animalistic attitudes and behavior* by individuals. [emphasis mine] (Morris, 1972, pp. vi–viii)

Like acknowledgement of the occurrence of wars, a scientific picture of the world might well be "deeply unsatisfying" to certain people. There might even be "harmful effects of absorbing a picture of reality that children begin to learn as soon as they enter the (secular) educational system" (DRW, 2021*, p. 33)—imagine the horror if we taught them about the Holocaust, too! But that is no more reason to reject a scientific picture than it is to deny the existence

of wars. The religious *neutrality* of, or tolerance of religious diversity in, the public educational system is not the same thing as inculcating materialism. School children do not get days off from school in honor of materialist holidays, and materialists do not overtake school boards. What children and adults are inculcated with is not materialism, but knowledge, and it's not the fault of "materialists" that humanity only has beliefs about, and not knowledge of, spiritual realms. It's not up to those who believe in less than you to defend their absence of conviction. It's up to you to justify your postulation of more than they postulate.

The fact that spirits (or demigods, or whatever) are not among the class of things known to exist is not anybody's fault. Empirical survivalists do their brethren a disservice by so freely falling back on insinuations, the use of loaded terms, and other informal fallacies in place of addressing the actual points made by their opponents. Contra parapsychologist John Palmer, such behavior isn't so much "offensive" (2016, p. 251) as it is annoying, as it requires interlocutors to waste time addressing **non sequiturs** rather than arguments of substance. It's long overdue for tribal commentators to put away childish things and acknowledge that other people are perfectly within their rights to come to conclusions different from your own. You'll survive—I promise.

In any case, if empirical survivalists are going to present themselves as rational empiricists, it would seem incumbent on them to do some survey research to actually find out (rather than presume) the reasons that people have for their skepticism about personal survival. Research, perhaps, like this:

> These survey results demonstrate that *regardless of current belief in the survival of consciousness*, religious or spiritual affiliation, or occupation, *there were three experiments whose positive results would be the most persuasive* for believing in the survival of consciousness after death: OBE/NDE, mediumship, and reincarnation. Interestingly, our evidential letter grades are reflected in these survey results. That is, academics who were presumably not especially familiar with the survival literature selected experiments that were rated the highest on our grading scheme. [emphasis mine] (DRW, 2021*, p. 31)

Many are thus skeptics of discarnate personal survival simply because the evidence in its favor is hardly compelling. We don't have a SoulPhone *yet*, after all. Elsewhere, even DRW concede the point themselves: "so far, no category of evidence has achieved a grade of A. This provides ample room for skepticism among those who remain agnostic about survival" (2021*, p. 26). Indeed, the very fact that every single one of the 422 academics whom they surveyed responded positively to seeking replicable positive results from at least one of their 10 proposed experiments testifies to their lack of dogmatism. The idea that others don't believe the same things that you do because they are immovable dogmatists is merely what people who are insecure in their own faith tell themselves to assuage their own doubts. The survey results speak for themselves: "Not surprisingly, the non-religious respondents showed low confidence in survival and paranormal belief. However, they still selected the OBE/NDE experiment as the second most persuasive, and the mediumship and reincarnation experiments as the first and third most persuasive, respectively" (DRW, 2021*, p. 30). The actual presence of immovable dogmatism is unnecessary when using unchangeable **talking points**.

So much for skepticism about survival stemming from some rigid adherence to fundamaterialism, reductionism, scientism, pseudoskepticism, or whatever other pejorative is the word of the day. (One might as well throw in antifa at this point.) On the contrary, there's a stunning similarity between survivalist apologetics and those of fundamentalist Christians.

Tart even goes so far as to create a kind of materialist catechism he dubs "the Western Creed" since there'd be no **windmills** for him **to tilt at** if he didn't invent them himself. He invites readers to "do responsive recitation . . . and then repeat it out loud in a solid, formal way, as if you were pledging allegiance to your flag or reciting a creed in church" (2009, p. 27). What possible purpose could this exercise have other than to encourage readers to reject ideas based *purely on their emotional reactions* to them? One might as well create "the Realist's Creed" in which one makes a mantra out statements like "thousands die horrific deaths in elective wars," "many animals must kill or starve," or "over 99% of all species that have ever lived are now extinct," and then ask participants how they feel afterwards. Pretty bad, no? Well, simply stop believing that depressing facts are true. Climate change solved! In any other context this would be called living in denial. A mere **appeal to emotion** is a rhetorical strategy, not a rational argument, as it does not present any *grounds* for adopting a particular position on an issue.[22] Like DRW, Tart goes on to conflate materialistic consumerism with materialist metaphysics.

Empirical survivalists can wag their fingers at those who disagree with them all that they want, but it will be to no avail. Skeptic shaming will never be an adequate substitute for presenting a strong argument or providing clear, genuinely scientific evidence for one's position. While social media may have lowered the bar for online discourse, I expect better of published work. If skeptics pigeonholing

proponents is irritating, set an example by not engaging in the exact same behavior that you decry when those in the other "tribe" exhibit it.

Are we rationally permitted to believe in personal survival? (Braude, 2021*, p. 1). Sure. As Alvin Plantinga has famously argued, we might well be rationally permitted to accept the religious belief system instilled in us in childhood in the absence of evidence, and even have no compunction to ever review the available (historical) evidence relevant to its truth or falsehood even when that could help settle the question for us (2000, pp. 416–417, 420–421). If what rationality permits is the bar, it's a rather low bar.

A much more interesting question is not whether we *can* rationally affirm personal survival, but whether we *have to* rationally affirm it. Being rationally permitted to believe a proposition is much less compelling than being rationally obliged to do so, since in the latter case it would be positively irrational not to affirm personal survival. I suspect that Braude adopts the weaker standard out of epistemic responsibility because he knows (unlike some heedless, brash, and particularly vocal empirical survivalists) that the evidence favoring personal survival leaves much to be desired; he has, after all, intellectually honestly noted some of the weaknesses in that evidence himself, even while endorsing personal survival.

But what serves the interest of science is trying to get at *what's likely to be the case*, not merely what's permissible to believe. The probabilities should drive our beliefs, not the other way around. Does the evidence rationally compel belief in personal survival? It doesn't even come close. The evidence doesn't even make personal survival more probable than not.

NOTES

[1] "We selected [US] academics as likely to represent a subpopulation that would be more agnostic about survival than the general population due to their [supposed] immersion in the Western scientific worldview" (DRW, 2021*, p. 28). The **prejudice** that the general US population is hostile to personal survival due to Western inculcation is not evidenced in popular culture, where paranormal television develops a large niche following and usually includes no skeptical commentary whatsoever, since including it would reduce the air of mystery, making such programming less entertaining and hurt ratings. Moreover, if only as much as 10% of the US population identifies as an atheist, skepticism about an afterlife from the "30% to 40% who are unsure or do not believe" is rather unlikely to be due to either "the conviction that philosophical materialism is the only valid way to understand reality" or "a firm commitment to atheism" (DRW, 2021*, p. 2). More likely is that a larger segment of the general population is suspicious of things like mediums' ability to communicate with the dead due to a widespread perception that mediums are often involved in fraud, which isn't exactly historically unwarranted (e.g., Nahm, 2015; Spraggett & Rauscher, 1973; West, 1999), or lack experience of anything unequivocally paranormal.

[2] Cf. Robert Thouless on his innovative cipher tests of mental mediumship: "If . . . all attempts to carry out this and related tests do fail, this will obviously strengthen the case for non-survival" (1984, p. 24), even though "survival is consistent with the possibility that there can be no communication between those still living in their physical bodies and those whose bodies have died" (1984, p. 25).

[3] By an "empirical survivalist" I simply mean "someone who both believes in survival and believes that there is empirical evidence that is at least suggestive of survival" (Sudduth, 2016, p. 50).

[4] I add this qualification because empirical survivalists are constantly on the lookout for *any* neurological outliers that might be taken to defeat this otherwise strong body of diverse evidence unfavorable to survival (e.g., DRW, 2021*, pp. 11-12; Kelly et al., 2007, p. 411; van Lommel, 2021*, pp. 17–18, 26), even when they demonstrably fail to defeat it (Augustine & Fishman, 2015, pp. 248–251; Stokes, 2016, p. 172; Weisman, 2015, pp. 101-102).

[5] This is key because otherwise the auxiliaries are doing all of the work to yield the predictions that the hypothesis, by itself, would otherwise give us no reason to expect. Moreover, whenever you conjoin an auxiliary to a hypothesis you are reducing its overall probability to some degree because now you are testing two non-100%-probable hypotheses, or three, or however many auxiliaries you add *in addition to* the primary hypothesis itself (which is why auxiliary assumptions are sometimes called auxiliary hypotheses). The reduction in probability can be small if the added auxiliaries are *themselves* highly confirmed (e.g., a 96% probable hypothesis × one 96% probable auxiliary yields a 92% probable hypothesis, all else held equal—though see Plantinga, 2000, p. 402 on dwindling probabilities). But if one's auxiliaries are untestable in principle, the highest probability than can in fairness be allotted to them is 50% (the same 50–50 odds for the auxiliary as for its negation). This is what makes the upshot of Sudduth (2016) so devastating to empirical survivalists: other Bayesian values held equal, a 96% probable simple survival hypothesis × one 50% probable auxiliary yields a 48% probable "bulked-up" survival hypothesis—that is, one this is not even minimally more probable than not! ($p > 50\%$).

[6] This is exactly what would be expected if brain activ-

ity underlies mental activity. Liken mental processes to computational processes governed by programming rules (software), but enabled by physical infrastructure (hardware). Even though one could do both, one can disrupt how well a computer program functions much more profoundly by manipulating the underlying hardware than one can disrupt how well the underlying hardware functions by manipulating the computer program (though Stuxnet illustrates the latter well, if we regard centrifuge controllers as part of the hardware rather than supplemental to it). That's because functioning hardware is what grounds/enables the computational processes to occur/exist, just as brain functioning evidently grounds/enables mental processes to occur/exist in biological creatures.

[7] On this issue, Nahm also invokes a **double standard**, writing of the dependence thesis, "it is impossible to prove it from a purely logical perspective," even though, incredibly, he had *just written* "we usually don't speak of 'proof' in sciences like psychical research" (2021*, p. 66). What justifies Nahm raising the bar for neuroscientific evidence while lowering it for evidence from psychical research?

[8] Braude is absolutely right that "the very best cases are rich enough to give us pause" (2021*, p. 19), which is notably not the same thing as being rich enough to *overcome* the independent, robust evidence from ethology, comparative psychology, evolutionary psychology, behavioral genetics, developmental psychology, clinical neuropsychology, psychopharmacology, and so on. By any reasonable definition, the findings of these diverse fields are *relevant evidence* here—not a mere "metaphysical axe to grind"—particularly when even the best real-life survival evidence is admittedly "consistently frustrating in one way or another" (Braude, 2021b*, p. 19).

[9] Cf. scientific *relativist* Paul Feyerabend: "The crank usually is content with defending [his] point of view in its original, undeveloped, metaphysical form, and he is not at all prepared to test its usefulness in all those cases which seem to favor the opponent, or even to admit that there exists a problem" (1964, p. 305).

[10] In contrast to DRW's discussion, all but the last of Mishlove's five reasons for rejecting LAP interpretations of the survival evidence (2021*, pp. 89–91) are substandard. More damning is that there is abundant positive evidence *against* survivalist interpretations of that evidence (Augustine, 2015a, pp. 20–31; Augustine, 2015b, pp. 530–540, 545, 549–553, 556, 558, 561n6, 562n12; Blackmore, 2015, pp. 395–396, 399; Braude, 2003, pp. 24, 66–67; Crookall, 1972, pp. 89–90; Dodds, 1934, pp. 156–162, 171; Fenwick & Fenwick, 1997, p. 41; Fox, 1920/1962, p. 82; Gauld, 1982, pp. 32–33, 109–118, 146, 219, 228; Green & McCreery, 1975, pp. 18, 168–170, 205–206; Grey, 1985, p. 37; Holt, 1919, p. 203; Lester, 2015, pp. 640, 642–643; Lindley et al. 1981, p. 109; Murphy, 1945, pp. 87–90; Sudduth, 2016, pp. 61–62n11, 71, 97, 121, 127, 190–191, 221–223, 231, 273–275; Tart, 2009, pp. 217–218, 266–267). I have in mind here, for example, mediums ostensibly communicating with deceased persons who are later found out to be still alive, but whom the sitters believed to be dead at the time—which is more compatible with mind-reading than communication with nonexistent deceased persons. Swinburne lists: "First, there are no cross-checks between mediums about the alleged present experiences of the dead in the afterlife. Mediums never give independently verifiable reports on this. Second, their reports about the present alleged experiences of the dead are themselves very banal. Yet one would expect because of the total lack of evidence of dependence of the dead on their past bodies, that they would live in a very different world, and that this would emerge in their reports of that world" (1997, p. 303).

[11] There's an implicit conflict between Beischel's evidential standards and those of DRW. First, DRW feel compelled to include among the items in their evidential grade criteria decision matrix "multiple independent researchers reporting similar results *that do not require statistical arguments*" [emphasis mine] (2021*, p. 15), presumably because of the perception that purely statistical arguments are too easily manipulated (cf. Huff, 1954). Second, DRW keep in mind the various replication crises that have come to light since Ioannidis (2005) in the social sciences—e.g., "only about 20% of such results are independently reproducible even when these upper criteria [odds of 20 to 1] are achieved"—when noting that much higher odds against chance are *standardly* required to establish the existence of even conventional effects in "harder" sciences: "odds of 1,000 or even 10,000 to 1 may be required to persuade peers that something interesting is going on . . . In physics, where odds of a million to one are required to claim a provisional 'discovery,' it is not uncommon for such discoveries to later be invalidated as a mistake" (2021*, p. 37). So, even if it's true, it might not mean much that the statistical "evidence for psi is comparable to that for established phenomena in psychology and other disciplines" (Etzel Cardeña, in Beischel, 2021*, p. 35).

[12] Granted, this is only one case—but if reincarnation researchers can overlook such obvious conventional explanations in that recent heralded case, why should we have any confidence that they haven't done so in numerous other such cases that haven't been as thoroughly investigated? Moreover, why don't these researchers publish their unedited case reports on the Internet (with

personally identifiable information redacted when appropriate) to allow for *independent* analysis?

[13] So much, then, for Nahm's response to conventional counterexplanations of CORT already noted in the literature: "none of the critiques listed above applies to the strong before-cases in which written documents were made before the previous personalities were identified and the families met" (2021*, p. 40). And I haven't even mentioned how spurious *specific* correspondences between one's life and that of a (supposedly reincarnated) person can be manufactured from whole cloth due to the law of near enough (Sudduth, 2021, pp. 999–1000, 1006; cf. Angel, 2015, pp. 575–578), even when supposed correspondences are *conflicting* (Sudduth, 2021, p. 1022n62).

[14] Mind you, contest participants could respond that such details can be found in the outside sources that they cite, but do they really expect the contest judges to track down these sources for further information *in addition* to evaluating the 204 submissions that they received? It seems that, at the very least, an abstract-like summary of (1) and (2) could have been provided.

[15] van Lommel writes: "So how do we know for sure that all functions of the brain have ceased during cardiac arrest? Many studies into induced cardiac arrest in both human and animal models have shown cerebral function to be severely compromised during cardiac arrest" (2021*, p. 10). But his cited human and animal studies predate those led by Lakhmir S. Chawla (2009, pp. 1096–1098) and Jimo Borjigin, which found otherwise. Thus Borjigin and colleagues characterize Parnia and Shirazi's equivalent claim that "the brain as an organ loses function within seconds of the heart stopping" (2021*, p. 63) as an "unsupported belief that the brain cannot possibly be the source of highly vivid and lucid conscious experiences during clinical death" (Borjigin et al., 2013, p. 14436).

[16] Mishlove seems to regard this as evidential, in which case he's making an **appeal to popularity**: "A belief in postmortem survival of consciousness is common to every culture, nationality, religion, and linguistic group in every region and historical period on Earth. Every single one!" (Mishlove, 2021*, p. 9) As Mark Twain once quipped, "One of the proofs of the immortality of the soul is that myriads have believed it. They also believed the world was flat" (Twain, 1902/1935, p. 379). Mishlove thinks that our "modernist" "current technological era is historically unusual" (2021*, p. 10) in allowing open doubt about personal survival, but as classicist Richard Lattimore points out, "there are several pagan epitaphs in which death is spoken of as an everlasting sleep . . . if it is qualified as ultimate or eternal, then sleep is annihilation; a dreamless sleep is a complete suspension of all sensation" (1923, p. 78). Moreover, such doubt can be found in the Indian Carvaka, the author/s of Ecclesiastes, ancient Greek poets like Moscus, Taoism founder Chuang-Tzu, the towering philosopher Aristotle, the musical theoretician Aristoxenus, the geographer Dicaearchus, the Sadducees, the Epicureans, the Confucian school of Hsün-tzu, the ancient Roman poets Horace or Lucretius, the playwright Seneca, the encyclopedist Pliny the Elder, the first-century Chinese philosopher Wang Ch'ung, the Stoic philosopher Marcus Aurelius, the anti-Buddhist Fan Zhen, the medieval Arabic astronomer Omar Khayyám, the 11th-century Syrian poet Al-Ma'arri, the Renaissance playwright Montaigne, the unparalleled dramatist William Shakespeare, and so on. Survivalists have had no more monopoly on culture than they have had on science, even before the Enlightenment, or outside of the West, or both.

[17] Note also that if personal survival really did occur for human beings, the continuum of consciousness across the animal kingdom implies that it would occur for earwigs, too (Broad, 1925, pp. 530–532), raising the question of just how many afterlives a survivalist has to unparsimoniously posit in order to maintain a consistent belief system—presumably afterlives not only for the individual members of extant species, but for the members of long-extinct species as well. Upon seeing death in the wild, those reluctant to posit so many "ghosts" might well conclude that the fate of human beings after death is no different than that of any other organism (cf. Ecclesiastes 3:18–22).

[18] Similarly, Nahm erroneously assumes that, unlike veridical perceptions during NDEs that could be chalked up to residual brain function (and presumably normal perception), for "veridical accounts of events that occurred in this intermission [between lives] . . . the only option left for physicalists is postulating that these cases are not authentic" (2021*, p. 18). But LAP interpretations (e.g., citing clairvoyance or retrocognition) are as open to "physicalists" as anyone else. To wit: "It should not be thought . . . that all parapsychologists are necessarily committed to a dualist interpretation of the mind–body relationship . . .[M]any exponents prefer to think of psi as essentially a function of the brain, or of some special brain mechanism or process" (Beloff, 1987, p. 586).

[19] Like most "physicalists," I refrain from invoking LAP because I'm simply unconvinced of its existence. Nahm believes that I reject all LAP interpretations as "incredibly *ad hoc*" (2021*, p. 59) when it's only *unlimited* LAP—i.e., superpsi—that's problematically unfalsifiable (Augustine, 2015a, p. 33). Nahm's error stems from his by now inexcusable conflation of the two: "the living-agent psi model is also called the 'super-psi' model" (2021*, p. 49). This conflation is **question-begging**: "This term points

to the fact that psi of an enormous quality and quantity is required to explain all facets of survival phenomena" (Nahm, 2021*, p. 49). It's also pernicious, for the **loaded term** superpsi is used to deliberately **shift the burden of proof** off of empirical survivalists and on to their critics by **poisoning the well**. And it seems to **straw man** critics: "ordinary vanilla psi is sufficiently mysterious to account for most, if not all, of the evidence for survival" (Radin, 2008). Why is Radin wrong? If you merely change your vocabulary, you never have to offer a reason why. Sudduth's neutral term LAP doesn't imply anything about how "much" psi (if any) is required to explain the survival evidence, which is the same amount of psi in any case! As Braude notes, "survivalists are committed to positing comparably impressive psi on the part of the deceased or the living" (2017, p. 155). Continuing to conflate the two is thus a mere "logical sleight of hand" (Sudduth, 2016, p. 290). Indeed, Braude notes that this point is so obvious that it's remarkable that so many empirical survivalists have "missed" it: "This is so easy to see, it's quite astonishing that many works on survival fail to acknowledge it . . . Although this is not a difficult point to grasp, prominent writers on survival seem curiously oblivious to it" (2021*, p. 5).

[20] Consider that the ancient Greek philosopher Epicurus, famous for his argument that "death is nothing to us" since we will not exist to experience it, believed in a kind of physical soul that disintegrates at death along with the normal physical body's atoms.

[21] Ironically, Ruickbie quotes Cyril Burt maintaining that "the brain functions, not as a generator of consciousness, but rather as a two-way transmitter and detector; i.e., although its activity is apparently a necessary condition, it cannot be a sufficient condition, of conscious experience" (Ruickbie, 2021*, pp. 65–66). If brain activity is a necessary condition for consciousness, even if it "transmits" rather than wholly "generates" it, then consciousness cannot exist in the absence of brain activity, as Swinburne notes.

[22] And when this is presented in order to persuade an audience to adopt a belief for other than rational reasons, it's a fallacy. Tart adds: "Hopefully there'll be some long-term change in beliefs" as a result of the rote recitation he recommends in his Western Creed exercise (2009, p. 31).

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COMMENTARY

Not So Fast: A Response to Augustine's Critique of the BICS Contest

Stephen E. Braude[1]
Imants Barušs
Arnaud Delorme
Dean Radin
Helané Wahbeh

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HIGHLIGHTS

A prior cynical review of the outcomes of a recent contest on the best evidence for "life after death" arguably rehashes many familiar and trivial criticisms of paranormal research. Problems with survival-type studies exist, but some evidence seems much stronger than what skeptics assume or conclude.

ABSTRACT

Keith Augustine's critical evaluation of the essay contest sponsored by the Bigelow Institute of Consciousness Studies (BICS) is an interesting but problematic review. It mixes reasonable and detailed criticisms of the contest and many of the winning essays with a disappointing reliance on some of the most trite and superficial criticisms of parapsychological research. Ironically, Augustine criticizes the winning essays for using straw-man arguments and cherry-picked evidence even though many of his own arguments commit these same errors.

Preliminaries

Augustine's detailed essay is an interesting—and at times, frustrating—stew. It mixes reasonable criticisms of the BICS contest and many of the winning essays with lazy reliance on some of the most tired and shallow critiques of parapsychological research.

We agree that Augustine has identified some areas of concern about the BICS contests' design and execution. One of those is BICS's controversial reliance on the legal standard of proof beyond reasonable doubt. Moreover, Augustine provides reasons for questioning both whether the judging was sufficiently objective, and also whether judges (even competent and objective judges) could have properly evaluated the enormous body of submissions in the time allotted for that task. Granted, the BICS contest may have successfully and commendably brought widespread attention to the fact that serious survival research and scholarship exist. But it did not discover or create an authoritative consensus about what the "best" evidence is, much less clarify the principles by which ostensible survival evidence should be evaluated. Augustine argues that it would have been better to "commission an evidence review (not an essay contest) by *independent* judges, such as those in the biomedical field who have *not* published in the survival literature, to avoid potential conflicts of interest."

Our response to Augustine's critique has some unavoidable limitations. Augustine offers many criticisms of the winning BICS entries he selected for discussion, and we cannot assess them all. In fact, we prefer to shelve discussion of the messy particulars in Augustine's selection of essays, thereby sparing the reader from being drenched in minutiae. Besides, there are bigger concerns that take priority. We need to examine major and pervasive deficiencies in Augustine's discussion—for example, his reliance on straw-man or other notoriously unacceptable tactics, his

refusal even to *mention* positive evidence, and his failure to realize that there is nothing privileged about the many assumptions he brings to the table.

The Big Picture

As we will see, convenient lacunae in Augustine's critique allow him to make his skeptical position seem more substantive than it really is. For example, Augustine carefully avoids discussing two matters of great importance: (1) not simply the strongest reasons, but any reasons for challenging his negative appraisal of particular cases, and (2) arguments exposing how unverified assumptions and hasty inferences pollute the received view of the relevant physiological data. In fact, Augustine entirely ignores McTaggart's subtle re-framing of the issues (discussed below and in Braude, 2003), showing how we can modify our language to purge it of some conventional metaphysical presuppositions, and thereby allow us to see the physiological evidence in a more survival-friendly light, as if through a different metaphysical lens.

Turning now to the details of what Augustine says, consider the following illustration of point (1) above. Augustine writes,

> . . . the fact that historical trance mediums' accurate statements must be fished out of reams of twaddle (James, 1909, p. 115) is surely relevant to any plausibility assessments here, as is the agreed-upon fact that a significant proportion of the entities that they claimed to contact were undeniably fictitious constructions of the mediums' own minds. Certainly the latter more than offsets any gain provided by appealing to the "never caught cheating" card, which is hardly conclusive in any case since Mrs. Piper had access to gossip within a large web of her community connections.

This passage commits several sins. First, as far as clearly fictitious mediumistic control personalities are concerned, even if one grants the reality of survival, the existence of these controls would not be surprising. They might even be exactly what many survivalists expect. An extensive body of research, primarily studies of hypnosis and personality disorders (especially MPD/DID), reveals how dissociative and other altered states can unleash remarkable displays of creativity or previously latent abilities, including precisely the kind of creative achievements seen in mediumistic control personalities (as in the case of Patience Worth—see Braude, 2003). Augustine ignores those bodies of work, apparently unaware of the complexities of dealing with what Braude (2003) has called the *Unusual Suspects*—namely, rare or abnormal processes, such as a combination of dissociation and latent creative capacities, or exceptional (e.g., "photographic") memory, or something analogous to extreme or rare forms of savantism, where we find remarkable skills existing alongside cognitive and physical deficits that ordinarily prevent the manifestation of those skills.

Moreover, in the passage above, Augustine tries to dismiss Mrs. Piper's case with an undefended appeal to the medium's presumed access to "gossip within a large web of . . . community connections." But he ignores the reasons many reject that explanation of Mrs. Piper's successes. Are there grounds for thinking that Mrs. Piper's access to gossip *actually* played a role? Augustine does not mention any. Moreover, there are many instances throughout Mrs. Piper's career where allegations of fraud or cryptomnesia are particularly implausible. But Augustine ignores those as well. In fact, he ignores William James's comment that Mrs. Piper "showed a most startling intimacy" with sitters' family affairs, "talking of many matters known to no one outside, and which *gossip* could not possibly have conveyed to her ears" (James, 1886, pp. 15–16). Similarly, he ignores the impressive successes of Mrs. Piper's G. P. communicator. Thirty of the 150 sitters introduced to G. P. were people known to the living George Pellew, and G. P. recognized twenty-nine of them. The thirtieth, whom he failed to identify at first, was someone who had grown from a girl to a woman since the last time she saw the living G. P. The G. P. communicator interacted appropriately with these sitters, and he seemed to know a great deal about their lives and relationships with Pellew. It is both illuminating and refreshing to compare Augustine's cursory dismissal of Mrs. Piper's mediumship to accounts by Alan Gauld, who dives deeply into the small and often revealing details and painstakingly evaluates them (Gauld, 1982, 2022).

Perhaps Augustine believes he is under no obligation to consider apparently positive evidence of Mrs. Piper's paranormal abilities—presumably on the grounds that conventional explanations have already been vetted by the scientific community and must therefore always be chosen over unconventional ones. But whether or not that is the case, Augustine's flippant appeal to gossip is a poor excuse for an explanation of Mrs. Piper's mediumistic success. At best, it is a promissory note for an explanation. If Augustine wants to dismiss Mrs. Piper's abilities, he must demonstrate that his gossip hypothesis has some evidence in its favor, and also that it is adequate to a wide range of facts. But Augustine avoids mentioning—much less discussing—evidence favorable to Mrs. Piper.

Granted, Augustine mentions that private detectives tailing Mrs. Piper never found anything suspicious. But he is mute on the significance of the many times Mrs. Piper

got intimate hits with anonymous sitters she was meeting for the first time—including proxy sitters and people who, during the medium's visit to England, happened to be travelling through Cambridge. So although it is certainly relevant that Mrs. Piper was never caught cheating, survivalists do not need to rely on a never-caught-cheating card. Augustine simply ignores the strongest reasons for thinking that cheating is highly improbable.

With regard to point (2) above, there are serious reasons for relaxing our commitments to standard interpretations of the neurophysiological data and entertaining possibly radical alternatives. Some famous experiments in the 1920s by psychologist Karl Lashley illustrate this clearly. Lashley thought he knew where memories would be stored in a rat's brain. But he found that no matter how much of a rat's brain he surgically removed, trained rats continued to run their maze. And when Lashley reached the point in his surgical marathon where the poor critters were unable to run a maze, they were unable to do *anything* (Lashley, 1929). So some—but not Lashley—concluded that a rat's memory is not localized at a specific place in the rat's brain. Rather, memories are *diffusely* localized, much as information is diffusely distributed in holograms.

This proposal catapulted Karl Pribram to the status of a pundit.[2] However, to someone not antecedently committed to the received wisdom about mind–brain relations, Lashley's experiments take on a different sort of significance. They suggest that memories are not located *anywhere* or in any form in the brain. More generally, they suggest that the container metaphor (that memories and mental states generally are *in* the brain or in something else) was wrong from the start, because memories (and mental states generally) are not *things* or *objects* with distinct spatiotemporal coordinates.

But this takes us into deeper metaphysical waters than we need right now. What is important here about the Lashley example is that it illustrates, first, how possibly unrecognized assumptions undergird our understanding of Nature, and also how those assumptions infiltrate our ways of speaking. That is why scientific reform can initiate linguistic reform. We will return to this topic below, when we consider McTaggart's position.

As Augustine noted, the BICS Rules and Regulations informed entrants that "BICS will accept evidence and eyewitness testimony supporting the legal requirement that establishes proof beyond a reasonable doubt." Although there is much one could say about the appropriateness of that legal requirement in survival research, Augustine goes in a different direction. He cites the work of Elizabeth Loftus (Loftus, 1979), and he claims that "seminal research into the reliability of eyewitness testimony provides all sorts of reasons to hesitate to rely upon it so heavily (as survival research typically does)."[3] However, Augustine seems unaware that *in that same work* Loftus actually made a solid argument *against* the view that eyewitness testimony is generally unreliable and malleable. We will look at the argument shortly.

It is easy to see why one might worry about the reliability of first-person observation or memory reports—perhaps, reasoning as follows: Some first-person reports are clearly unreliable and malleable. For example, visual illusions, pareidolia, etc., are both common and quite real, and they make it easy to misinterpret what one is experiencing. Similarly, in *staged incident* experiments, subjects are taken by surprise to witness a carefully prearranged event such as a confrontation or dispute. But when asked what they saw, they usually get some critical details wrong—for example, who pulled out a gun first. Moreover, memory reports can easily fall victim to the gnawing tooth of time. These errors can only further reduce the veracity of testimony generally.

But that argument misses the point. Visual illusions, staged incident errors, and so on do not diminish the veracity of reports from the *strongest* cases where (for example) the light was good, the investigators knew what they were doing and were experienced in detecting fraud, specific measures were taken to minimize the possibility of fraud from the start, the phenomena occurred slowly enough to permit careful close-up examination, and witnesses had plenty of time to examine the setup and could monitor the phenomena closely while they occurred. That is why Crookes' accordion test with D. D. Home and the 1908 Naples sittings with Eusapia Palladino are so important (see Braude, 1997). Moreover (as we note below), there are good reasons for thinking that memory reports may not be as fragile as many suppose.

At any rate, Loftus argued sensibly for the view that people remember certain types or details of events better than others. She noted that experiments have confirmed the commonsense observation that eyewitness reports are more reliable when the perceived events or objects are observed repeatedly or for extended periods (Loftus, 1979, pp. 24–5). Thus, she approvingly quoted D. S. Gardner's observation that

> The extraordinary, colorful, novel, unusual, and interesting scenes attract our attention and hold our interest, both attention and interest being important aids to memory. The opposite of this principle is inversely true—routine, commonplace and insignificant circumstances are rarely remembered as specific incidents. (Loftus, 1979, p. 27)

That is why one can argue plausibly that in the *best* in-

vestigations of physical mediumship—the ones that matter—conditions of observation were actually *conducive* to reliable eyewitness reports (Braude, 1997). So we challenge Augustine to (1) demonstrate his command of the details of Crooke's accordion test with D. D. Home, or the 1908 Naples sittings with Palladino, (2) demonstrate his grasp of the reasons why many believe those details rule out fraud, (3) explain credibly how observers could have been (or were actually) mistaken about what occurred, and (4) explain why we should believe that fraud actually occurred.

A more sophisticated and nuanced view of first-person reports would acknowledge, first, that *all* first-person observation and memory reports are only conditionally, rather than intrinsically or categorically, acceptable. Our decision whether or not to accept a particular report depends on various factors. Some of the most important of those factors are: (a) the capabilities and interests of the observer; (b) the nature of the object allegedly observed; and (c) the means of observation and the conditions under which the observation occurred. In judging the reliability of observation claims or memory reports, we weight these factors differently in different cases. But in general, it matters: (a) whether the observers are trained, sober, honest, alert, calm, attentive, subject to flights of imagination, fortunate enough to have good eyesight, and whether they have any strong prior interests in observing carefully and accurately; (b) whether the objects are too small to see easily, whether they are easily mistaken for other things, or whether they are of a kind whose existence cannot be assumed as a matter of course (e.g., unicorns, pixies, Elvis sightings); and (c) whether the objects were observed close at hand, with or without the aid of instruments, whether they were stationary or moving rapidly, etc., whether the observation occurred under decent light, through a dirty window, in the midst of various distractions, and so on. The best cases of ostensible physical mediumship easily survive such scrutiny (Braude, 1997).

Furthermore, it is worth noting that even though first-person reports are only conditionally acceptable, we rely on them all the time, usually successfully, in our daily commerce with others. Indeed, we *must* do so. As philosopher C. A. J. Coady argued, "our normal cognitive practices are underpinned by our reliance upon what others tell us" (Coady, 1992, p. viii). However, pursuing the topic of testimony further will take us too far afield.

Nevertheless, one final point is worth mentioning. Ironically, a well-known Loftus example of a malleable and false traumatic memory helps make the case for her opponents. She cites baseball pitcher Jack Hamilton's memory of hitting batter Tony Conigliaro in the face with a fastball. Although Hamilton claimed to remember the event perfectly, he remembered it as happening during a day game, when in fact the game was played at night. Of course, it is contentious (to say the least) that the time of day was a "critical" detail (Loftus, 1993, p. 531). But the important point is that Hamilton remembered that he hit Conigliaro in the face. Indeed, one would think that this is precisely the sort of case that would be embraced by the authors Loftus was opposing. After all, the allegedly critical detail concerning the time of the game was not traumatic. Thus, Hamilton's failure to remember it is compatible with the claim that traumatic or highly unusual or dramatic events leave an indelible impression on the mind. However, the traumatic part of the event, hitting the batter, seems to have been etched in Hamilton's memory (see Olio, 1994, for a similar observation). So, ironically, Loftus's strategy adds support for the sensible view that traumatic or dramatic events (like hitting a batter in the face) are more indelible than non-traumatic or less-arresting features of the incidents (such as the time of day). Loftus has certainly not shown that first-person reports are unreliable generally or, in the best cases, easy to dismiss. (For further discussion of Loftus's dishonesty and confusions, see Braude, 1995, 1998.)

Let us turn now to Augustine's lopsided focus on alleged experimental failures. Once again, his comments disappoint. For example, he ignores one of the clearest lessons we have learned from parapsychology experiments—namely, that subjects in ESP tests often either focus on something more personally meaningful or interesting than the official target, or at least get distracted by, and focus on, some minor feature of the target, thereby making it difficult to distinguish near hits from misses. The Maimonides dream telepathy experiments provide some dandy examples (see Ullman, et al., 2002). But of course, this may also occur with any ESP test, including Augustine's favored combination-lock tests of survival, where it is especially easy to imagine more arresting targets.

Imants Barušs provides another example:

> It was found in the PEAR research that remote viewers were often distracted by more interesting objects than the official target . . . [For example] in 1993, during a class field trip to the PEAR lab, one of the students went to New York City for the evening . . . She chose the Empire State Building while we tried to remote view where she was. I saw her at a small Catholic church in a square with pigeons walking outside. She and her friend sat in the pews on the right hand side. There was a slimy guy in the back of the church who freaked them out, so they left. That was all correct, except that it occurred later in the evening, not at the time we

> were remote viewing back in the lab at Princeton. This is an example of a more interesting target drawing a remote viewer's attention than an official target. (personal correspondence, 4/19/2022)

At one point Augustine counters the survivalist's reliance on first-person accounts by saying "survival agnostics might well note that there is an abundance of eyewitness reports for the existence of the Loch Ness Monster, too, that they find just as unconvincing." He is specifically concerned here with Michael Nahm's questionable contention that the reliability of testimony is enhanced when there is agreement among a multiplicity of observers. But Augustine did not seize the opportunity to note that conditions of observation matter more than the number of witnesses. In the most reliable first-person reports in parapsychology, conditions of observation are actually conducive to accurate reporting. For example (as we noted earlier), in the best cases of physical mediumship, observers can study the phenomena closely while they are occurring, and experimental controls can make cheating improbable. Obviously, there is nothing comparable in alleged Nessie sightings—certainly no close-up observation.

Furthermore, the strongest cases of physical mediumship, and macro-PK in general, do not seem vulnerable to concerns about the fallibility of memory over time. For one thing, observers sometimes write their accounts at the time or shortly thereafter. And the shocking or dramatic phenomena described in those reports seem to be of the kind that Loftus and Gardner regard as conducive to reliable first-person reports.

It is curious how Augustine fails to acknowledge that our description of Nature rests on independently unverified presuppositions, required simply to get any inquiry off the ground. As the history and philosophy of science demonstrate, those assumptions are not sacrosanct, and they can later be abandoned and replaced. But Augustine seems to think that anti-survivalist or survival-agnostic judgments about the significance of empirical data are assumption-free, or at least freer than the claims made by survivalists. That is why he repeatedly appeals to "what the [neuroscientific] data tell us," as if one could grasp what the data mean independently of any deeper assumptions, or perhaps as if Augustine's assumptions—and the entire conceptual and scientific framework of which they are a part—are somehow unassailable. For example, he writes,

> Negative outcomes are only frustrating if you want the experiments to come out a certain way. In lieu of remaining frustrated by failing to get the data that you were hoping for . . . survival researchers would better serve *science* by setting aside their feelings and heeding what the *data* are telling them.

Let us set aside the snarky and condescending tone of this passage. It is enough to note that it is one of several instances in which Augustine either seems blind to his own first principles or else treats them as somehow privileged.

It is particularly disappointing that some of Augustine's arguments are variants of familiar superficial attacks on parapsychology. Augustine repeatedly claims that survival researchers have a long track record of failures when trying to elicit evidence of survival, as if that lends support to the view that there is no evidence of survival *at all*. But he does not consider whether test conditions might inhibit performance, and he greatly underestimates how tricky it is generally to determine ahead of time how a parapsychological test or experiment will turn out. That trickiness is not surprising, considering that researchers do not know what kind of human ability they are trying to wrestle from a real-life setting and then study under artificial controls or other novel background conditions. But then they do not know whether their planned experiments are even appropriate to the phenomena, and also whether the demands of the experimental setting tend to frustrate the quest for positive results. Of course, Augustine also lacks that knowledge, but—apparently undaunted—he nevertheless purports to know what we should expect to find if the phenomena under investigation are real.

We have seen that anti-survivalists must do more than assert that evidence suggesting survival can be accounted for by appealing to the possibility of fraud or other Usual Suspects. They must wallow in the grubby details and show that fraud (or whatever) is either likely or actual. But we need to be fair here, because survivalists have an analogous duty. In order to explain away or dismiss experimental failures, they must do more than appeal to the mere possibility of psi-inhibitory conditions. They must also provide reasons for thinking that those conditions were actually or probably obtained. And if they fail to mount that defense, then critics can justifiably complain that survivalists do not take experimental failures as seriously as they would take successes.

In any case, not all parapsychological tests (including survival investigations) have failed. For example, although Mrs. Piper's results are often ambiguous and messy, the investigation of her mediumship counts as a failure only on an indefensibly strict standard of success, one which we reject in many domains. That is why a baseball player who gets a hit 1 out of 3 times is considered excellent.

Furthermore, we *know* that most (if not all) human capacities are situation-sensitive. And we know that even the best modern or contemporary remote viewers (e.g., Joe

McMoneagle, Ingo Swann, and Pat Price) do not always get a hit. But then contrary to what Augustine suggests, a test subject's misses—even consistent misses—do not clearly (if at all) cast doubt on the reality of the phenomenon under investigation. We also have good reason to believe that if psychic abilities are real, their manifestations may be disguised and subtle—for example, in the interest of our psychological well-being (see, e.g., Eisenbud, 1970, 1982, 1992). But Augustine does not even entertain that option, much less evaluate it carefully. Instead, he again avoids discussing evidence suggesting survival at the level of detail and sensitivity to the nuances of human behavior that the best cases deserve.

Benjamin Franklin once quipped: "Clean your finger before you point at my spots." We have noted that one of Augustine's main tactics is to avoid discussing psychic successes and to concentrate instead on failures to elicit psi on demand—for example, unsuccessful efforts to get OBErs and NDErs to identify remote targets. Yet he accuses survivalists of cherry-picking evidence favorable to their position. Furthermore, Augustine seems to infer not simply that nothing psychic was happening during the tests of OBErs and NDErs, but more likely, given his broad skepticism about things paranormal, that nothing psychic *could* occur. Regrettably, Augustine never clarifies this. But he has a recommendation. He urges us to attend to *what the data tell us*, and when we do that,

> One possibility stands out among the rest for its sheer simplicity: perhaps out-of-body experience (OBE) adepts and near-death experiencers (NDErs) cannot describe remote visual targets under controlled conditions because nothing leaves the body during OBEs or NDEs that could perceive them.

But that is hardly the only—or most plausible—interpretation of the negative results. And even if we agree that nothing leaves the body in these cases, that is not enough to support a more sweeping skepticism about the reality of psychic functioning in general, or about the specific ability to accurately describe remote targets. One of the few things we know about psychic abilities—in addition, we would say, to their existence—is that they are psychodynamically complex. Moreover, we know that RV superstars often display distinctive RV abilities and lack others. For example, Ingo Swann was able to direct the operator of a mini-submarine to the location of a sought-for and previously undiscovered ship wreckage. And Hella Hammid (also in the sub) successfully described the objects they would find at the site. But apparently Ingo and Hella were unable to exchange the tasks. We also know that the ability to demonstrate ESP or PK *reliably* seems to be quite rare, even if psychic experiences can occur to virtually anyone under the right conditions. But then we must exercise caution in interpreting a parapsychology experiment's negative results. Augustine presumably knows this, but he nevertheless fails to consider what kind of ability is under investigation. He does not even entertain the counter-proposal that when OBErs and NDErs fail to identify remote targets in formal tests, perhaps they are simply not particularly good at it—or good at it in formal tests or under mental or physical duress.

After all, there is no evidence that people generally, or randomly selected people, are good at remote viewing, or as good as the small number of outstanding RVers. But then we can say, plausibly, that the ability to remote view is genuine (as RV superstars demonstrate), but like many normal abilities it is not widely or evenly distributed, and it is also situationally fragile. That is what the data, both negative *and positive*, tell *us*.

Augustine apparently considers encrypted messages and combination-lock tests to be the gold standard for testing mediums. But he claims repeatedly that all such tests have failed. He writes, "While some mediums were asked to describe the contents of sealed envelopes or provide auditory information, most direct tests of survival involve asking living persons to posthumously reveal to a medium key words, phrases, or mnemonic devices, ostensibly unknown to any living person, that would decipher encrypted messages or open user-set combination locks." Then a few sentences later, "After 121 years of such simple tests, only undeniably fraudulent mediums (Spraggett & Rauscher, 1973) or cryptologists (Bean, 2020; Gillogly & Harnisch, 1996) have ever been able to solve them." Predictably, Augustine does not consider the option that the tests were psi-inhibitory. He also does not indicate what his position would be if the tests were successful. Would he concede that the positive results count as evidence of survival? That would help clarify how open-minded he is about evidence of the paranormal.

Augustine also approvingly cites magician Christopher Milbourne's claim that "Many brilliant men have investigated the paranormal but they have yet to find a single person who can, without trickery, send or receive even a three-letter word under test conditions" (1970, p. 37). Now that may have been true in 1970, but since then James Carpenter conducted an experiment that successfully transmitted the word "Peace" (Carpenter, 1991). However, even if Milbourne's claim had been true today, what would it have shown? As we have noted, Augustine does not consider the strongest features of the cases he discusses. He simply focuses on ways in which a case like that of Mrs. Piper falls short of an ideal. But then, when we take into

account not just the strongest evidence of remote viewing abilities, but the totality of evidence in parapsychology, the failure of OBErs and NDErs to succeed in formal or controlled tests is ambiguous and certainly not impressive or clear enough for us to conclude that the subjects totally lack the ability being tested. And it fails even more clearly to support a general skepticism about the reality of paranormal phenomena.

Lurking below the surface is an interesting and serious problem which Augustine does not consider at all—namely, whether we can ever confidently assess success or failure in *any* parapsychological test. For one thing, those who originally designed encrypted message or combination-lock tests were making various assumptions about what it is like to survive death—for example, whether (or to what degree) the channels of communication are noisy, and whether the deceased would even care about communicating with the living.[4] But then, test failures would at best only disconfirm a particular *model* of personal survival. Moreover, this problem is an instance of a more general and very serious difficulty faced by both survivalists and anti-survivalists—namely, that most (or perhaps all) of the time, we have no idea what is really going on in a parapsychological experiment.

Parapsychologists try to study phenomena which, if real, could apparently subvert any experimental controls. Yet researchers too often assume, tacitly and naively, that subjects will use only the psychic ability being investigated, that they will use that ability only after the experiment has begun, and that experimenters, judges, and mere bystanders will use no psychic abilities at all to influence the outcomes. But this supposed "gentleman's agreement" as Eisenbud (1963, 1992) called it, is clearly preposterous. We have no reason to think that people will be so well-behaved in exercising their psychic abilities, and we also have no idea what issues or hidden agendas might be on the minds of participants and onlookers, perhaps motivating them to influence the results of the tests (Braude, 1997). This is the recalcitrant problem of sneaky or naughty psi. Unfortunately, we cannot pursue it here.

We know that evidence of paranormality can be captured even in a skeptical environment, especially if investigators act sympathetically and respectfully, as in the painstaking 1908 Naples sittings with Palladino (Feilding et al., 1909). We should also recall that the parapsychological community has a very good track record of identifying colleagues who are psi repressive (e.g., John Beloff) and psi conducive (e.g., Helmut Schmidt). And there is good reason to believe that this difference has much to do with the experimenters' beliefs, personality, and the quality of their interactions with subjects.

However, Augustine's assessment of encrypted message and combination-lock tests seems viable only when we regard both experimenters and subjects as psychological stick figures, unburdened by self-defeating character traits and untroubled by the concerns, fears, hopes, and other frailties that plague most of humanity. The issues here coincide with some of those discussed in connection with the replicability problem in parapsychology (see Braude, 2018).

In any case, Augustine's narrow focus on subjects' failures to get hits is very much in the spirit of a foolish claim made by psychologist C. E. M. Hansel in the 1983–84 BBC documentary "The Case of ESP." Hansel said that to demonstrate telepathy, one need only tell him what he is thinking. But we wonder what Hansel would have said had he been challenged to personally demonstrate penile erection, then and there, on camera and on demand. If Hansel failed that test, would he have been logically compelled to admit that he just cannot "get it up"?

It is no secret that abilities vary distinctively from one person to another. For example, some porn stars could probably ace the challenge we imagined presenting to Hansel. Also, one cannot infer that people have a certain ability (e.g., to compose music, or psychokinetically raise a table) just because they have certain other abilities in the same general domain (e.g., to play the trumpet, or psychokinetically nudge a matchstick). Savants illustrate this dramatically. Calendar calculators tend to be accurate only within specific ranges of years, and those ranges differ from one individual to another. And although calculators might be able to perform rapid and complex operations concerning dates or remember extremely long numbers, they might be unable to do simple addition or change a dollar bill. The famous calculating twins, George and Charles, amused themselves by exchanging 20-digit prime numbers, and they could factor any number presented to them, but they could not count to 30 (Sacks, 1985). Another savant could rapidly solve complex algebraic problems in his head, but he seemed unable to comprehend even simple principles of geometry (Treffert, 1989).

Along the same lines, our intuitions about what to expect from a person—either generally or in specific circumstances—are notoriously unreliable. Consider: We know that good hypnotic subjects can make themselves anesthetic in response to suggestion. And we also know that this ability can *take forms no one predicted*—e.g., becoming anesthetic in an area corresponding to no natural anatomical region (e.g., in the shape of a band around the arm) (see Janet, 1901; Myers, 1903). Similarly, David A. Oakley observed that "conversion symptoms defy the normal rules of neuroanatomy and neurophysiology" and that "hypnotically suggested anaesthesia of a hand . . . will typically show a glove pattern with sharply defined boundaries in

apparent correspondence to a naïve understanding of sensory innervation patterns" (Oakley, 1999, p. 244).

Augustine seems to adopt yet another familiar, and tawdry, skeptical strategy. It is revealed in the following passage: "DRW also inform us that 'Fraud was never detected in' the early 20th-century Kluski molds (wax casts of human hands), even though plausible normal ways of producing them are not hard to come by." But that is as far as Augustine takes his discussion of Kluski. He fails to provide even a single example of a plausible normal counter-explanation of the Kluski molds, and as usual, he does not discuss the strongest reasons for rejecting skeptical counter-proposals. Skepticism is easy when one ignores relevant details.[5] For a better-informed and more balanced discussion of Kluski, see Weaver (2015).

Similarly, Augustine writes, "The history of exposures of [mediumistic] fraud . . . the typical need for darkness in order for the phenomena to manifest . . . and the likely use of shills to help produce effects, all of which DRW note, 'ought reasonably to beget a suspicion against all relations of this kind.'" But since he consistently ignores the cases that most effectively resist the easy appeal to fraud and poor controls, Augustine comes uncomfortably close to another familiar and worthless skeptical claim—namely, that the results of parapsychology experiments should be rejected as long as fraud is possible. But of course, fraud is possible in any experiment and in any branch of science. What matters is not whether fraud is possible, but whether it is actual, and whether (or to what extent) the evidence for a properly conducted experiment or investigation outweighs the evidence for fraud.

Moreover, although there is a clear and rich history of mediumistic fraud, and although that history illustrates why experimenters must exercise caution and impose good controls, one cannot generalize from tainted cases to impugn the entire body of mediumistic evidence. That is one reason why the strongest cases are the ones that matter. And of course, to illustrate why those cases matter, one must look at their details and consider (say) what we know about the experimenters and experimental controls. But that requires considering apparently positive evidence at a level of detail that Augustine consistently avoids.

Another passage reveals Augustine's all too easy reliance on undefended assumptions. "The absence of clear-cut permanent paranormal objects produced by physical mediums should clue in any reasonable person of the dubious reliability of this phenomena [sic] as a source of evidence for the paranormal in general, let alone for discarnate personal survival." Apparently, Augustine assumes that a physical medium's paranormally produced objects should at least sometimes be permanent, or perhaps that they should be more common. But why? We find no argument for those assumptions, and they are certainly not obviously true. Augustine comes uncomfortably close to presumptuously dictating to Nature the forms in which he will accept her secrets.

In any case, if apport phenomena are genuine, they would be examples of permanent paranormal objects. That would be true even if (as some maintain) the apported objects existed previously at another location. Perhaps Augustine would simply dismiss the evidence of apportation. But that would be irresponsible. The serious literature on apportation, while not extensive (see, e.g., Nahm, 2019; von Ludwiger & Nahm, 2016), deserves a careful, open-minded evaluation, and it should range over both mediumistic and poltergeist contexts.

Another revealing passage is the following:

> Nahm disregards the *absence* of reports of intermission memories in the vast majority (~80%) of CORT. That is, in taking the existence of any intermission "memories" to be evidential, he disregards the more specific issue of why there are so few of them. On the face of it, *if one can really remember aspects of an even older past life, then one should (usually) also be able to remember aspects of a more recent (and perhaps half-a-century-long) intermission period between that life and the current one*, all else held equal (assuming that before-life memories function like those already known to exist, anyway). (italics added)

This is a good example of how unverified, controversial assumptions can be enlisted when convenient. What is the basis for Augustine's claim italicized above? Why assume this is how before-life memories operate? We barely understand how memory works in *this* life, and we are still puzzled by the memory anomalies displayed by mnemonists and savants, as well as people suffering from dementia. Augustine once again understands and criticizes the way survivalists often import unstated and undefended assumptions into the debate, but he apparently misses his own frequent deployment of the same strategy.

This reminds us of another, related, example, showing how controversial assumptions are unavoidable when trying to interpret the evidence for survival. In a *JSE* paper published in 2000, David Bishai addressed the familiar anti-survivalist argument that "reincarnation appears to be refuted by population statistics" (Edwards, 1996, p. 227). He sketched a simple "circular migration model" that accounts for the data from a reincarnationist perspective, and he showed that different assumptions about the "dwell time" between incarnations yield different predictions about the peak of human population growth. But more important,

Bishai showed that metaphysical assumptions are unavoidable no matter where one stands on the issue of reincarnation and population growth. Specifically, he noted that the alleged incompatibility between the reincarnation hypothesis and the facts of population growth rests on the very controversial assumption that "the mean duration of stay in the afterlife has been constant throughout human history" (Bishai, 2000, p. 419). Presumably, Edwards was unaware that his own position rested on that assumption. Ironically, then, Edwards' purportedly hard-nosed and condescending attack on reincarnation is as deeply and inevitably metaphysical and debatable as the view he opposes.

McTaggart and the Significance of Physiological Data

Survivalists maintain that we, or something essential to who we are (our mind or soul), can persist even when our bodies die. And Augustine believes this puts survivalists in an awkward position empirically, because they cannot explain away a large and respectable body of neuroscientific data suggesting that survival is impossible. Now we agree with Augustine that the study of survival must respect the data, whatever the data might be. And we also agree that some evidence seems prima facie to cut against the survival hypothesis. Indeed, we agree there is a huge body of research pointing, at least on the surface, to the apparently intimate connection between brain states and mental states—and in particular, the causal dependence of the latter on the former. So Augustine is justified in insisting that survivalists confront the challenge posed by the evidence of mind–brain correlations. However, he again resorts to straw-man tactics, making the following condescending criticism.

> According to *what principled reason*, then, can we rule the neuroscientific evidence as inadmissible? Not wanting to deal with powerful counterevidence is not an epistemic principle, but a fallacy (**confirmation bias**). Failing to deal with it shirks one's epistemic responsibilities; it is merely aiming to confirm what one wants to hear, not seeking the truth.

Similarly, he writes, "concomitant [mind–brain] variations *are* evidence, no matter how staunchly empirical survivalists fight to the death to pretend otherwise."

But who, exactly, is engaged in this epic struggle? Who believes that concomitant mind–brain variations are not evidence? Augustine again offers an implausible or inaccurate characterization of his survivalist opponents. Moreover, he seems simply to have missed the point. Survivalists need not consider the correlations to be non-evidential. But what are they evidence *of*? Augustine's anti-survivalist position is only an option, and probably it seems compelling primarily to *those antecedently committed to, or caught in the grip of*, a prevailing conventional scientific view of the world. Similarly, who is actually guilty of claiming that neuroscientific evidence is *inadmissible*? In fact, its admissibility is precisely why survivalists make the effort to find viable alternative accounts of the data! Moreover, Augustine ignores the strongest reasons for thinking that the best cases *cannot* be explained away in conventional scientific terms. Indeed, conventional science has *already* failed to accommodate the most robust evidence in parapsychology. So it is not as though there are no chinks in that armor.

At any rate, survivalists must say something about how mental states or characteristic chunks of personal psychology might persist in the absence of brain activity. More specifically, they must explain why, if mental states can occur independently of bodily states, they *seem* in so many respects to be bodily dependent. Typically, survivalists do this by arguing that the brain is merely one kind of physical *instrument* for expressing mental activity.

Predictably, most anti-survivalists find that hard to swallow (e.g., Edwards, 1996), and Charles Richet offered an analogy to explain why (Richet, 1924). In doing so, he anticipated a position many neuroscientists and others would probably now express somewhat differently, but no more cogently. Richet observed how certain changes to the brain affect and sometimes seem to obliterate memory. And he noted that survivalists regard the brain as "only an instrument, which is unable to respond unless it is intact" (Richet, 1924, p. 109). Although Richet did not object to that claim, he found it incredible to assert further that this instrument is not necessary for memory and other cognitive functions. He wrote,

> It is as if I were to say that in an electric lamp the passage of the current and the integrity of the mechanism of the lamp are not necessary for the production of its light. (p. 109)

Many find that analogy, and others like it, to be very seductive, and Augustine apparently thinks they pose an insurmountable challenge to the survivalist. However, the analogies are likely to be irresistibly seductive only (or primarily) to those who have already internalized the conventional wisdom regarding mind–brain relations. But, as we will see below, those who regard the brain as an instrument mediating the expression of mentality can approach the matter from a much different perspective.

Regrettably, Augustine ignores some intriguing argu-

ments from philosopher J. M. E. McTaggart, showing how survivalists can transform the prevailing scientific perspective on the data into something more survival-friendly. Notice first that survivalists hold that the self—whatever, exactly, it may be—is not something identical with one's physical body or a part of the body (e.g., the brain). Nor is it something totally causally dependent (or supervening) on part of one's physical body. Instead, survivalists could say that the self, whatever exactly it is, and as we know it both introspectively and through our earthly interactions with others, is something that *has* a body.

Now we realize that this locution may strike some as intolerably quaint at best, and possibly question-begging at worst, since it may presuppose precisely what is at issue: namely, that the self might not be embodied. However, survivalists must be allowed to use the locution that the self has a body. *Pre-theoretically*, it is no less legitimate than the competing, and equally theory-laden, terminology of anti-survivalists. Moreover, if survivalists are right, then reality is profoundly different from what conventional science proposes, and one would expect that to require some modifications to our usual ways of speaking.

Granted (and as Augustine notes), a great deal of evidence of mind–brain correlations seems to cast doubt on the survivalist position. It is precisely what draws many people to some form of the identity theory or epiphenomenalism. But McTaggart argued that survivalists can concede that Richet's analogy of the electric lamp is forceful and that correlations pose at least an initial challenge to their position. However, survivalists contend that other bodies of evidence exert a theoretical pull in the opposite direction. How, then, can survivalists argue for the superiority—or just the adequacy—of their point of view? According to McTaggart, one strategy would be to offer competing analogies that are at least as weighty as analogies apparently favoring the anti-survivalist. We will consider one such analogy shortly.

Moreover and perhaps most important, McTaggart argued that anti-survivalists make several *unwarranted inferential leaps* when they interpret the evidence. For example, no matter how intimate the mind–body connection seems to be, the data would show, *at most*, "that *some* body was necessary to my self, and not that its present body was necessary" (McTaggart, 1930, p. 104). But even that may be going too far; strictly speaking, the data show us only what *is* the case, not what *must* be the case. Thus, the data do not establish limits on the possible manifestations of selfhood. Specifically, nothing in the data compels us to conclude that a self must be linked to a human body or any kind of physical body. So on an even more circumspect or conservative appraisal of the data, we might conclude simply that "*while a self has a body*, that body is essentially connected with the self's mental life" (p. 105). McTaggart argued,

> . . . it does not follow, because a self which has a body cannot get its data except in connexion with that body, that it would be impossible for a self without a body to get data in some other way. It may be just the existence of the body which makes these other ways impossible at present. If a man is shut up in a house, the transparency of the windows is an essential condition of his seeing the sky. But it would not be prudent to infer that, if he walked out of the house, he could not see he sky because there was no longer any glass through which he might see it. (p. 105)

McTaggart makes a similar point with regard to the more specific, and apparently intimate, causal relation between brain states and mental states.

> Even if the brain is essential to thought while we have bodies, it would not follow that when we ceased to have brains we could not think without them . . . It might be that the present inability of the self to think except in connexion with the body was a limitation which was imposed by the presence of the body, and which vanished with it. (p. 106)

McTaggart's view is insightful. Strictly speaking, the evidence for mind–brain correlations does not show that selfhood or consciousness is *exclusively* linked to bodily processes, much less the processes of any particular physical body. We noted earlier that survival-unfriendly interpretations of the neurophysiological data may seem initially compelling because their presuppositions are widespread and deeply rooted. And if so, it may be a useful intellectual exercise to try to divest ourselves of those presuppositions and then take a fresh look at the data. We might find, then, that McTaggart's (or some other survivalist) interpretation seems more immediately appealing. It is therefore regrettable that Augustine does not rise to the challenge.

Moreover, it is not clear to what extent anti-survivalists can legitimately cite neurophysiological data in support of their position. After all, in the debate between survivalists and anti-survivalists, *both* positions are in question. In fact, one would *expect* survivalists to consider alternative interpretations of the neurophysiological data apparently unfavorable to their position. After all, data do not come pre-interpreted. They must always be evaluated in the light of a background theory (or a set of basic presuppositions). Often enough (and as we saw earlier in connection with

Lashley), what we take to be obvious interpretations of the data may reveal more about our unexamined theoretical presuppositions, or lack of imagination, than they do about the phenomena in question.

Augustine offers a list of "agreed-upon facts that scientists have discovered about the mind's link to the brain," and he maintains "that the chiefly neuroscientific data constitutes *evidence* against discarnate personal survival (and strong evidence at that)." But some of the items on the list are contentious, and all seem to be precisely what we would expect if the brain is merely an instrument for the expression of mentality. The alleged facts are:

1. Minds mature as brains mature
2. Childhood mental development halts when childhood brain development halts
3. Minds degenerate when brains degenerate (due to old age or traumatic brain injury)
4. Creatures with simple brains have simple minds
5. Creatures with complex brains have complex minds
6. Sickening/injuring the brain sickens/injures the mind
7. Mental dispositions can be inherited from one's parents
8. Mental desires can be induced or eliminated by brain stimulation
9. Mental disorders can be cured by altering brain chemistry with drugs
10. Mental disorders can be brought on by altering brain chemistry with drugs

But this list will not stop survivalists in their tracks. Consider: Advocates of the brain-as-instrument view would presumably be quick to embrace items 1–6 and 8. From their point of view, as the brain develops (or deteriorates), what can be expressed though it changes accordingly. Similarly for 9 and 10: We could equally say that drugs correct, impair, or simply modify the working of the instrument for expressing mentality. And the remaining item on the list:

7. Mental dispositions can be inherited from one's parents

may not state a fact at all. We wonder: Why not say "learned/absorbed" rather than "inherited"? Although some genetic data suggest that certain personality traits and talents are inherited through DNA, Augustine cannot simply assume that this alleged regularity is an example of nature and not nurture. After all, many (perhaps most) families do not exhibit this generational continuity. In fact, children often have attitudes, dispositions, and preferences that conflict with those of their parents. Are we really to believe that Augustine does not know this?

Finally, one cautionary terminological point merits a few comments. On the list above (in fact, throughout his paper), Augustine mentions various alleged causal connections between two things: the brain and the mind. Now, we know what the brain is; it is a squishy physical object. But the *mind* is not clearly an object at all. We know that various physical states seem to influence mental states and behavior. But the "mind" (like "personality" or "character") is merely a shorthand, a general term, for a class of mental events, just as "the weather" is a general term for the class of meteorological events, and "the economy" stands for a class of financial transactions. There is certainly no need, and arguably no justification, for reifying the mind, weather, or the economy. Moreover, thinking of the mind as an entity—a piece of ontological furniture—surreptitiously tilts the discussion in favor of the anti-survivalist. It suggests, right from the start, that the mind and brain are on the same ontological level—or at least that they are neighbors. And no doubt that encourages some to identify the mind with the brain, or at least to posit intimate causal relations between them.

Summing Up

Augustine has squandered an opportunity to advance the debate over survival. What's needed are novel proposals, not the tired and transparently defective skeptical arguments on which he often relies. As far as dialectical and conceptual sophistication are concerned, Augustine's critique could have been written in the 1950s and 60s, when arguments similar to his were all the rage among psi skeptics.

As we noted at the beginning of this reply, Augustine makes some reasonable comments on the BICS contest and the winning papers he selected for discussion. But much of the time he offers arguments which can only seem credible to someone ignorant of the relevant evidence. No wonder, then, that Augustine so often fails to mention the strongest reasons for rejecting his charges or suggestions of fraud, malobservation, and other Usual Suspects.

Moreover, we have also seen that Augustine avoids discussion of clearly relevant data or lines of argument that challenge his point of view. Indeed, that may be his principal dialectical strategy. And although he charges survivalists with straw-man reasoning, that is something he often does himself, either by describing the opposing survivalist position in perhaps its least plausible form, or by simply charging survivalists with positions they (or at least the best of the lot) do not hold.

Hopefully, we will eventually see commentaries on the

BICS contest that grapple more constructively with the issues, and which demonstrate a more thorough grasp of the relevant empirical and philosophical landscape.[6]

NOTES

[1] Readers may wonder exactly how five authors collaborated on writing this reply. In order to express our comments in one "voice" (so to speak), Braude did the writing. But the text incorporates many corrections and suggestions from the co-authors, which led to numerous revisions, perhaps setting a world record.

[2] Granted, most neuroscientists are unfamiliar with the logical and conceptual errors in positing memory traces. Like Pribram, when confronted with challenges to their views on memory, their first impulse is to simply modify the nature of the trace (say, as a dense neural network) and ignore the reasons for regarding trace theory as deep (or disguised) nonsense. Moreover, the arguments for the vacuousness of trace theory are hardware-independent. No matter how they are configured, it is relatively easy to show that memory traces are impossible objects. For more details, see Braude (2014), "Memory without a Trace."

[3] It is unfortunate that both BICS and Augustine use the term "eyewitness testimony." That term is intimately tied to concerns about guilt or innocence in a legal setting. But first-person reports in parapsychological or survival research do not count as eyewitness testimony in that legal sense. That expression means one thing in a court of law and another when we are considering how to evaluate ordinary first-person observation and memory reports. Hence, our preference for referring simply to first-person reports.

[4] See Sudduth (2014, 2016) for a penetrating discussion of auxiliary assumptions.

[5] Similarly (we hasten to add), belief in survival is easy when one ignores relevant detail.

[6] Many thanks to Michael Sudduth for his helpful comments on an ancestor of this paper.

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COMMENTARY

Final Reply: When Will Survival Researchers Move Past Defending the Indefensible?

Keith Augustine

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HIGHLIGHTS

The survivalists' response to the author's skeptical review did not confront the novel criticisms and arguments made against the BICS essay evidence. Such a candid and deep engagement with fundamental issues is needed to advance the question of 'life after death.'

ABSTRACT

The failure of five psychical researchers to confront my critique of Bigelow Institute contest-winning essays with counterpoints or concessions responsive to its novel criticisms is disappointing. Their defensive and scattershot reply lost sight of whether the critiqued essays met their directive to provide "hard evidence 'beyond a reasonable doubt'" of the survival of human consciousness. Those who claim that science should expand its metaphysically conservative picture to include things otherwise not known to exist assume the burden of showing what they claim. My interlocutors' almost exclusively testimonial evidence does not adhere to the long-standing scientific principles required by the scientific community. For the kind of evidence that could be publicly confirmed is simply not the kind that survival researchers have been able to provide, just as we would expect of a hodgepodge of deception, embellishment, malobservation, misreporting, self-deception, and so on; but which would be surprising on the hypothesis that discarnate personal survival occurs. The survival evidence does not even survive elementary scrutiny, let alone outweigh our everyday experience of the biological fragility of our own minds. The *totality* of the evidence renders discarnate personal survival highly unlikely. Attempts to reinterpret this evidence away through various analogies fail because a hypothesis that makes false predictions, like that of the independence of individual consciousness from a functioning brain, will continue to make them no matter what analogy one uses to illustrate it.

KEYWORDS

Analogies; brain damage; falsifiability; McTaggart; testimony

With as many as five psychical researchers working together to respond to my critique of select BICS contest-winning essays, their failure to confront that critique with counterpoints (or concessions) responsive to its general criticisms is disappointing. The lost opportunity is particularly disappointing with a philosopher of Stephen Braude's caliber at the helm, and with commentators whose own essays were the ones most at issue. Their defensive and scattershot reply focusing on tangents—not the central points conveyed by my section titles—contrasts sharply with my rather basic, well-evidenced, and well-structured overview of systemic deficiencies across the winning essays, deficiencies that are symptomatic of long-standing shortcomings within contemporary survival research itself.

Given their constraints, my interlocutors could not respond to *everything* that I said. But as I will show, their neglect of my arguments goes well beyond this. By at the outset acknowledging as valid *only* my prefatory passing criticisms—the bare mentions that I characterized as "neither here nor there"—they blatantly downplay the force of my substantial and cogent criticisms, **poisoning the well.**

Braude et al. (2022) characterize my criticisms of their (and other BICS contestants') arguments as "lazy" and "shallow." This characterization is not only false or misguided, but ironic. Where I've offered novel criticisms otherwise absent from the survival literature, my commentators simply regurgitate familiar arguments that they've made before. Since the work that they regurgitate did not anticipate my novel points,[1] they never respond to them. The commentators can hardly be faulted for failing to anticipate an argument that they have never heard before. But they most certainly can and should be faulted for failing to confront a novel argument with a novel response. That failure to adapt is the epitome of the "lazy reliance" on "tired" arguments that they claim to eschew.

The charge of shallowness seems to stem more from **hasty presuppositions** about my aim and directive than from anything that I wrote. If I had wanted easy targets, I could have focused exclusively or primarily on the top three prize-winning essays by Jeffrey Mishlove, Pim van Lommel, and Leo Ruickbie.[2] Instead, I concentrated on the essays written by the three most promising contributors, given the quality of their previous work: Stephen Braude (2021*), Dean Radin (in Delorme et al., 2021*), and Michael Nahm (2021*).

By the time that I had finished addressing these three essays alone, I had already exceeded allotted word count limits twice, even while aiming to make individual points succinct, particularly in the largest section on ranking the survival evidence.[3] Prior to this, I had already scrapped the development of entire sections on items like Nahm's feeble attempt to come to grips with the most substantial recent challenges to discarnate personal survival, and on the pressing need for survival researchers to substitute their habitual use of **loaded terms** with viewpoint-neutral language, among other things.

In the retained sections, supporting examples and analyses were often merely cited rather than even paraphrased—even when they strongly *substantiated* my point—due to limitations of both space and time (e.g., citations on untried tests of survival, especially problematic aspects of Mrs. Piper's mediumship, evidential weaknesses in touted drop-in communicator cases, plausible explanations of the cross-correspondences in terms of chance, the most damning evidence of fraud in the Scole and Felix circle sittings, features of apparitional experiences inconsistent with survivalist interpretations, and so on). Only those that follow up on my citations (typically listing specific pages) will discover many of the finer points underlying my conclusions, but one must compromise somehow, and it's better to refer readers to extant literature (that many are likely unfamiliar with) than to provide no way for them to follow up at all. Some highly relevant literature wasn't even cited *at all* in the interest of moving on—for example, the large corpus of *corroborating* literature on the unreliability of eyewitness testimony *since* Loftus (1979) (which, incidentally, even Delorme et al., 2021* only mention rather than cite). For space, I didn't even *define* more than a couple of the over two dozen *types* (not instances!) of fallacies extracted from the selected BICS essays in the hope that unfamiliar readers would look them up for themselves.

I thus had to be *very* selective about what I responded to in any of the other five essays (Beischel, 2021*; Mishlove, 2021*; Ruickbie, 2021*; Parnia & Shirazi, 2021*; van Lommel, 2021*), only addressing the most key material in them that tied into what Braude, Delorme et. al, or Nahm had said, or what I had said in response to them. In any case, it seems perverse to complain in one breath that I didn't *say enough*, and then in the next that I said *too much*: "Augustine offers many criticisms . . . and we can't assess them all. In fact, we prefer . . . sparing the reader from being drenched in minutiae." Pick your grievance. Perhaps Braude et al. (2022) might extend the principle of charity to those who face the same editorial choices that they do, but have the audacity to disagree with them.

The "Best" Survival Evidence: Mental Mediumship

Let's take a look at my supposed "major and pervasive deficiencies," starting with my supposed "refusal" to acknowledge positive evidence of discarnate personal survival. Braude et al. (2022) credit me for noting that, at

times, private investigators kept Mrs. Piper under surveillance, and I at least mention the proxy sittings that they later press. They then fault me for failing to address "the impressive successes of Mrs. Piper's G. P. communicator" in my BICS critique.

Their mischaracterization notwithstanding, it's already clear that I didn't quite fail "even to *mention* positive evidence," but let me explain why I referenced the positive evidence that I did (and not other possible examples). I specifically mentioned both Mrs. Piper sometimes being tailed by investigators and the use of proxy sitters in historical trance mediumship research, *because Delorme et al. (or Nahm) brought them up*. I did not mention the celebrated GP control sittings *because Delorme et al. (or Nahm) did not bring them up* (although Braude and Mishlove did).[4]

In previous work, where I had more space, I *did* provide strong reasons why we should not take Mrs. Piper's GP control to be the deceased GP,[5] though Braude et al. (2022) would have had to have familiarized themselves with my work to know that. But regardless, they are surely well aware of similar reasons pointed out long ago by E. R. Dodds and Alan Gauld. For example, GP is clearly among the controls that Gauld has in mind when he asks: "Why should the medium's influence so often intervene and override the control's *just when the latter is going to exhibit literary and philosophical information greatly exceeding the medium's*? And why should it intervene to force the control to appear to give a blessing and *a certificate of genuineness to perfectly preposterous 'controls' who can be nothing other than fictions* dreamed up by the medium?" [emphasis mine] (Gauld, 1982, p. 146; cf. pp. 114–115). Compare Dodds:

> The main points are the shiftiness displayed even by highly veridical communicators like 'George Pelham'; their confident statements in cases where they can hardly fail to know that they are lying; the habitual lameness of their attempts to answer direct questions; and above all their acceptance of bogus personalities as genuine spirits (e.g. 'George Pelham' guaranteed the authenticity of 'Phinuit' . . .). (1934, p. 171)

Different examples are highlighted by Michael Sudduth: "In some cases G. P. *incorrectly* reported on some alleged current or recent event that involved friends or family, which he claimed to have observed" (2016, p. 81n9; cf. Gauld, 1982, pp. 115–117).[6] I could go on (cf. Sudduth, 2016, pp. 221–223, 231, 273–275, which I cited in note 10 of the critique).[7] The regurgitation of Braude (2003)'s comments on dissociative identity disorder might well substantiate that "the existence of these controls would not be surprising," but it's their *nature* or *characteristics*, not their mere existence, that's problematic for survivalist interpretations of mental mediumship. Satisfying oneself with the stock answer that perhaps "the channels of communication are noisy" does not do justice to the facts highlighted by Dodds, Gauld, Sudduth, and others, either.[8] Given the more sensible option that *all* of the conjectured spirits are characters in a production, I seriously doubt that the 422 survival-agnostic academics surveyed in Delorme et al. (2021*) would be very impressed with the hypothesis that genuine discarnates blend in with majority fictitious ones.

Braude et al. (2022) further complain that I am "mute on the significance of the many times Mrs. Piper got intimate hits with anonymous sitters she was meeting for the first time—including proxy sitters and people who . . . happened to be travelling through Cambridge." Whenever I was aware of relevant extant literature that I did not have the space to expand on, I cited it. Literature on the problematic evidentiality of anonymous/proxy sittings was not cited because, as far as I know, little such literature exists. But in light of the recent discovery of how easily spurious correspondences could be generated from whole cloth in cases of the reincarnation type (CORT) (Sudduth, 2021; cf. Angel, 2015, pp. 575–578), I did ask Michael Sudduth for his take on whether similar spurious correspondences could have plausibly arisen in historical mediumistic proxy sittings. After all, veridical information fished out of *reams* of mediumistic twaddle might well be conventionally explicable by the law of near enough (cf. Sudduth, 2021, pp. 999–1000, 1006).

Sudduth opined that the law of near enough "probably has a lot of explanatory mileage" in accounting for proxy sittings when combined with their other features (personal communication, May 9, 2022). As far as I know, a systematic study of this possibility has yet to be carried out, and I'd certainly encourage psychical researchers to pursue one. However, we are not totally in the dark here. After noting that proxy sittings reduce "whatever evidential value dramatic portrayals of deceased personalities might have [since] the proxy cannot attest to that evidential value" (Moore, 1981, p. 88), philosopher Brooke Noel Moore adds in a footnote (taking a cue from the late philosopher Antony Flew):

> There is reason to believe that the number of true statements produced in proxy sittings is substantially lower than it is in standard sittings . . . Stevenson [1977] indeed states that in contemporary research (i.e. since 1960) with its stricter controls, when the medium deals not with the person wanting information but with his representative, "no positive results have been obtained." This certainly suggests that the impressive results some-

> times obtained in standard sittings may be due to factors other than communication with the dead. (Moore, 1981, p. 88n13)

If Moore's suspicion is right—that when nonverbal communication is controlled for (since proxy sitters are blind to whether or not the specifics provided by mediums are accurate), the number of accurate statements made in comparably long/detailed mediumistic sittings decreases—this outcome would be rather telling. The likelihood of spurious correspondences goes up the fewer the hits and/or the more copious the material to sift through. And whether sittings were by proxy or not, it seems unlikely that all of that copious twaddle and nonverbal communication was faithfully transcribed/noticed (which is why police interrogations are now standardly video recorded).[9] As the late mathematics popularizer Martin Gardner pointed out, **motivated reasoning** could have easily led psychical researchers to record mental mediums' ambiguous statements as direct hits (1992, p. 228). The store of normal/conventional sources of information that early SPR mediums could draw on is likely larger than appears in their transcripts.

Testimonial Evidence and the Burden of Proof

According to Braude et al. (2022), a survival skeptic "must do more than assert that evidence suggesting survival can be accounted for by appealing to the possibility of fraud[10] or other Usual Suspects." And in general, that expectation seems reasonable enough. But whether it is reasonable here depends upon *what the skeptic is trying to accomplish in a given context*. Some skeptics may simply point out that empirical survivalists *have not made their case for personal survival* without committing to a position on the survival question (e.g., Kastenbaum, 1986; Lester, 2005; Sudduth, 2016). Others may be more pessimistic than optimistic about the prospects for discarnate personal survival and offer their reasons why (e.g., Lamont, 1935/1990; Moore, 1981). Others still may have a particular take on the issue, but reserve making a full-blown case for their position for elsewhere (if they desire to make a case at all), limiting their comments to *the particular task at hand*.

My directive for the initial critique was to critically evaluate the arguments for discarnate personal survival presented within a manageable subset of *the BICS contest-winning essays*, which in turn had been called upon to provide "hard evidence 'beyond a reasonable doubt'" of the survival of human consciousness (About BICS, 2021). I took it upon myself to go further and question the essay contest's *goal* "to award contestants for writing papers that summarize the best evidence available *for* the survival of human consciousness after permanent bodily death" [emphasis mine] (About BICS, 2021) because a *scientific* (or even legal) investigation of evidence does not proceed in such a partisan way, and because survival researchers have *consistently* sought evidence ostensibly favoring personal survival without giving due care to potential evidence *against* it, both within and outside of the BICS contest.

To be sure, *he who makes a positive claim has the burden to back up that claim*. But limitations of space (and time) often preclude a commentator from being able to adequately defend every claim that he makes in a given context, and so he has no choice but to direct readers to other treatments where the particular claim in question *is* investigated in more depth. That's why we all use *citations*.

Moreover, given that a commentator cannot include *everything* that he might say in a finite critique, one must prioritize, giving more weight to bigger claims over smaller ones. Claims of having "proven" discarnate personal survival beyond a reasonable doubt, or of having "unequivocally disprove[n] the modernist view that consciousness ends with bodily death" (Mishlove, 2021*, p. 93), are examples of bigger claims. Claims about, say, "Crookes' accordion test with D. D. Home" are comparably smaller ones, especially given their dubious relevance to the survival question. Since such bigger claims can remain untouched regardless of whether or not specific smaller ones are defensible, it makes sense to give more priority to an overview of the broad outlines of the relevant evidence as a whole, especially given the breadth of the essays that I was invited to critique.

When it comes to the bigger claims, it's the job of survival researchers trying to make a positive case for the existence of conjectural forces or entities to rule out potential conventional explanations. This would be just as true if the subject were tachyons rather than spirits. It's not the job of those who hesitate to make another's leap to demonstrate that others' conjectures do not correspond to anything real (especially when establishing that a thing exists is usually easier than establishing that a thing does *not* exist). After all, *survival researchers—particularly* in the BICS essays—are the ones trying to persuade thus-far unpersuaded "agnostics to accept the existence of survival" (Delorme et al., 2021*, p. 33). Luminaries like Ian Stevenson recognized their burden here and, to their credit, took the mantle. It's perfectly reasonable for a person discussing survival research to take a position along the lines of: "I believe all of the same things that you do . . . *except* that I'm not convinced that discarnate personal survival actually occurs." Let's be loud and clear about this: empirical survivalists are the ones making the bigger positive claim that *science* should expand its metaphysically conservative picture of the world to include things otherwise not known to exist.

It's also perfectly reasonable to step back and take a look at the big picture. One can reasonably argue along the following lines. None of the arguments *in favor of* the existence of an all-powerful, all-knowing, and perfectly good God are very compelling *even by theistic philosophers' standards*. On the other hand, such philosophers struggle with how to reconcile the existence of such a being with a world that has long been steeped in suffering. The parallels here should be obvious. None of the variable-quality ostensible evidence for discarnate personal survival is very compelling even by parapsychological standards (cf. Delorme et al., 2021*), and those psychical researchers who acknowledge that contrary evidence should count for something struggle with how to reconcile discarnate personal survival with independent, well-vetted evidence from cognitive neuroscience (and elsewhere) (Stokes, 1993; cf. Stairs & Bernard, 2007, p. 301). When two sources of evidence appear to conflict, is it not more reasonable (absent further evidence) to give greater weight to the more reliable of the two? (cf. Rowe, 2007, pp. 159–160).

If I had thought that skeptics were "under no obligation to consider apparently positive evidence of Mrs. Piper's paranormal abilities" *at all*, then I would have never mentioned specific points like "historical trance mediums' accurate statements must be fished out of reams of twaddle" (Augustine, 2022). My interlocutors' hyperbole aside, I do think that because "conventional explanations have already been vetted by the scientific community," they should be given some *precedence* over explanations invoking conjectural forces or entities. So, like Delorme et al. (2021*)'s survival-agnostic academics, I would certainly give precedence to explanations in terms of known substances like cheesecloth or artificial cobwebs over those in terms of unknown substances (or "manifestations") like ectoplasm. Greater weight should be given to conventional explanations for the same reason that we should take appearances to be what they seem to be in the absence overriding positive reasons to think that we are misled. Of course, the latter is complicated in psychical research since the opportunities to be misled increase with the involvement of human intermediaries (like mediums or "chosen" experients) who may engage in deception or self-deception. In investigations of tachyons, by contrast, the experimenter's self-deception is still possible, but there are fewer sources of such confounds, and scientific scrutiny can ultimately sort this out, as it did in the cases of N-rays and cold fusion.

It's worth adding that to the extent that the existence of conjectural forces or entities is *not* scientifically established, paranormal explanations don't really *explain* anything at all. They are just an umbrella catchall of the negation of the conventional/normal explanations that researchers have *thought of* (maybe they didn't think of everything) and that *don't fit*. To have a real scientific *explanation*, we need to know *something* about the positive characteristics of the 'explaining' hypothesized force or entity—*what it is*—not simply *what it is not* (Augustine, 2015, p. 34). Until then, the label psi is just a placeholder or promissory note for an explanation. That's why *it's solely by convention* that we don't include unknown lights in the sky, unidentified living creatures, or other Forteana under the umbrella of psi. (If conventionally inexplicable, are ghost lights ostensible spirits, ostensible extraterrestrial probes, ostensible plasma-based cryptids, or something else entirely? No one will ever be able to say without some verifiable positive characterization of what they are.)

Braude et al. (2022) have much worthwhile to say about testimonial evidence—on various perceptual and cognitive errors, conducive conditions of observation, accounts of purported events recorded soon after they allegedly occurred,[11] observers' erroneous reports following experiments with staged "paranormal" events (e.g., Jones & Russell, 1980), and so on. And what they have to say about these things deserves unpacking *somewhere*, by *somebody*. But since there are more important fish to fry, I'll keep my comments on testimonial evidence limited, as I did in the opening critique itself.

None of my interlocutors' (sometimes valid) points detract from the fact that testimonial evidence falls short of the gold standard demanded by *the scientific community* for exceptional claims. The evidential strength of commonly uncross-examined testimony of purported events occurring under uncontrolled conditions pales in comparison to that of experimental evidence. Experimental evidence has *survived greater scrutiny*, after all, because of the protocols planned and implemented *before* experiments took place, in addition to being subsequently checked by protocol reviews and attempted replications. When more rigorous experiments are conducted to replicate the findings of uncontrolled or poorly controlled experiments, it is not unheard of for an "effect" to evaporate altogether (as much in parapsychology as anywhere else). The implementation of properly controlled experiments at least *reduces* the number of confounds contaminating other kinds of evidence, then, if earlier findings cannot be replicated when it is no longer *possible* for confounding variables to influence outcomes. Evidence obtained when cofounding variables can be ruled out as potential sources of effects constitutes much stronger evidence than testimonial evidence that comes with varying degrees of arguable corroboration. If lead author Braude wants to take his coauthors to task on this, I recommend that he prepare a separate critique of Delorme et al. (2021*).

Does Physical Mediumship Provide Good Evidence of Survival?

Throughout their reply, Braude et al. (2022) chide me for failing to address all things paranormal, or specific kinds or instances of parapsychological evidence that have little to do with ostensible evidence for discarnate personal survival. Why they ever expected me to do so is beyond me, and hardly different from what most BICS contest participants themselves did (for obvious reasons). Even if covering more had been part of my directive, I had neither the time nor the space to do so. And while I did incidentally *mention* the overall state of the evidence for other kinds of psi (macro-PK, precognition, and telepathy) to illustrate a point or two, it was never my intention to assess anything *other than* ostensible evidence for what theologian Michael Stoeber has called "otherworldly psi" (1996, pp. 1–2). Should I be faulted for failing to address the crop circle evidence, too?

While one could commit to a document review on the physical mediums that thrived in the century before last, as I said in my BICS critique, "there was never any need to invoke the existence of deceased human spirits to explain any genuine paranormal effects from physical mediums anyway, should there be any" (Augustine, 2022). This point is bolstered by *Braude himself*, whose *Immortal Remains* only mentions D. D. Home *once*, in the context of what he characterizes as an unconvincing or fictitious mediumistic communicator *pretending to be the deceased Home* (2003, p. 60). Eusapia Palladino isn't even mentioned there *at all*. So Braude evidently does not regard the cases involving either of them as ostensible evidence for discarnate personal survival rather than, say, recurrent spontaneous psychokinesis (RSPK). If Braude himself says so little about Home and Palladino *in his own classic on evidence for an afterlife*—let alone in his BICS essay—why would he expect *me* to comment on such cases?

Moreover, the *sole* winning BICS essay (not among those that I critiqued) that does more than cite sources on these mediums—that of Survival Research Institute of Canada's Walter Meyer zu Erpen—concludes:

> Home is touted as the physical medium never exposed in fraud. Based upon understanding of misobservation in dimly-lit séance rooms, reports of Home's miraculous feats were probably embroidered. In any case, the physical feats of Palladino and Home did not add to the evidence for life after death. (2021*, pp. 44–45)

If even survival researchers, Braude or otherwise, doubt the value of these cases as ostensible evidence of discarnate personal survival, it would have been out of place for me to critique them.

While physical mediumship is arguably relevant to the survival question, only *those cases that constitute potential evidence for discarnate personal survival* are germane to my critique. It's no more incumbent on me than it is on Braude et al. to engage in a never-ending game of Whack-a-Mole challenging every proffered instance of an anomalous effect that has convinced somebody or other that there is something interesting going on that demands novel explanation. So, like most BICS contestants and Journal readers, I don't *pretend to* have "command of"—let alone much interest in—"the details of Crooke's accordion test" and the like, any more than Braude et al. pretend to have a command of or interest in the last 50 years of cryptozoological research into the existence of Bigfoot (even if such things actually exist).

Nevertheless, I will make some general comments given (1) how much space Braude et al. devote to this digression and (2) the fact that some of the evidence from physical mediumship—as secondary as most psychical researchers take it to be—could be deemed relevant to the survival question. In the "strongest cases," we're told, (a) "investigators knew what they were doing" or were "experienced in detecting fraud," (b) taking "specific measures . . . to minimize the possibility of fraud from the start" where (c) "phenomena occurred slowly enough to permit careful close-up examination" and "witnesses had plenty of time to examine the setup and could monitor the phenomena closely while they occurred," sometimes even (d) in "good light." Which of these features are present in the highly investigable séances that take place *today*, rather than those conducted with the long-dead physical mediums of yesteryear? All but the last of these features were said to be present in both the Scole and Felix circle sittings that still managed to produce evidence of fraud, and whatever events physical mediums allow sitters to see, they go to great lengths to prevent those events from being video recorded in good or infrared light. If readers want a sense of the comparably underwhelming effects that physical mediums can produce in sittings utilizing the more stringent precautions available today, they need only read Nahm's (2015) brief missive on what he characterizes as merely "promissory mediumship."

Little wonder, then, that Braude et al. default to cases of physical mediumship contemporaneous with when medicine was still unclear on whether or not germs were the causes of disease, or whether flies spontaneously generate directly from rotting meat. From the perspective of their coveted survival-agnostic academics—not to mention that of everyday people who already believe in survival—Home's mediumship is about as likely to engender confidence in paranormal manifestations as the legend of

the Bell witch. The specifics of how Home accomplished Victorian-era feats *that have no contemporary parallels*—much like Mrs. Piper's mental mediumship[12]—seem rather moot if neither Nahm, Braude, nor any other investigator can capture comparable demonstrations today using modern tools that are more than capable of clearly documenting events through high-resolution closed-circuit, livestreamed, or otherwise unalterable video recording from multiple angles. When simply *informed* of the general character of this evidence, most people (survival skeptics or not) would be compelled to ask: *where have all the bona fide physical mediums gone?*

A final complaint warrants comment. Where I noted that normal ways of producing the Kluski molds were available—citing a source on how to produce them—Braude et al. complain: "But that's as far as Augustine takes his discussion of Kluski. He fails to provide even a single example of a plausible normal counter-explanation . . ." The section in which the comment was made was already more than twice as long as any other section, so I intentionally cited outside sources for additional details in that instance—and many others in that section, even when the unspecified details substantially bolstered my point—because I had a lot of ground to cover and had to compromise *somehow*. Considering that Delorme et al.'s (2022) systematic ranking of the survival evidence (that I was there emulating) devoted a total of *four sentences* to the Kluski molds—and that none of the other BICS essays that I critiqued mentioned *at all*—my interlocutors' complaint seems rather hypocritical. And interested readers should certainly follow-up on Braude et al.'s citation on the molds—the details of which their reply also "tawdrily" left unspecified—no less than my own.

An endless debate over the strength of inherently weaker testimonial and other poorly controlled sources of evidence could be avoided altogether, of course, if only Braude et al. had more rigorous experimental evidence to offer. But one cannot produce evidence akin to an Earth-bound extraterrestrial artifact, a Bigfoot skeleton, or a working SoulPhone if the hypothesized entities never existed in the first place.

Advancing Beyond Eternally Debatable Evidence

On permanent paranormal objects (PPOs), Braude et al. write: "Apparently, Augustine assumes that a physical medium's paranormally produced objects should at least sometimes be permanent, or perhaps that they should be more common. But why? We find no argument for those assumptions." Given the limited scope of my initial critique, the lack of an argument here should not surprise them. But I can provide one now and clarify my position.

The impermanence or rarity of PPOs isn't the problem, although both are a symptom of it. The problem is that, in practice, the evidence for putative paranormal effects is *perpetually ambiguous*. Compare cosmologist Sean Carroll's concern with attempts to confirm a positive characterization of negatively defined entities like unidentified aerial phenomena (UAP/UFOs):

> The argument that UFOs are not aliens is not mostly about priors, it's about likelihoods. If UFOs were a combination of glitches/test flights/weather, we would expect to see fuzzy inconclusive images. If they were aliens, we'd expect to see something very different. (Carroll, 2021)

That is, we should hesitate to interpret UAP as evidence for the extraterrestrial visitation hypothesis not so much because crossing the vast distances of interstellar space is potentially insuperable, but because as a matter of fact, *we simply do not find the sorts of evidence that we would expect to find* were extraterrestrial visitation occurring. If extraterrestrials were regularly visiting Earth, why would evidence of their presence *always* fall within the narrow range of possibilities that we might call the *perpetually ambiguous* range? There is a wide continuum of conceivable evidence consistent with extraterrestrial visitation, ranging from no evidence at all to undeniable evidence (indigenous peoples did not eternally debate the presence of European colonists, for example).

Researchers like Braude et al. maintain that however deficient the *publicly available* evidence for paranormal effects, for a select chosen few (e.g., Home's sitters), the evidence is undeniable (akin to witnessing an extraterrestrial spacecraft 10 feet above one's head for 20 minutes). By their lights, undeniable evidence of such effects is thus possible—but happens to only reveal itself to the few who have received personal revelations. Stepping back and looking at the big picture, one cannot help but ask: in the plentitude of reports of ostensible paranormal phenomena, why is it *always* the case that *the kind of evidence that could be publicly confirmed* is never the kind provided? Why does the actual evidence offered never deviate from the eternally ambiguous? This consistent, historical *feature of the evidence* would be surprising for a *genuinely anomalous* phenomenon, but exactly what we would expect of a hodgepodge of deception, embellishment, malobservation, misreporting, self-deception, and so on. If the word of Elisabeth Kübler-Ross is anything to go by, full-bodied apparitions of deceased human beings are available for extended conversations with the living—just not with anchors on live television for all the world to see. Videos of "apparitions" fleetingly appearing in backgrounds and disappearing around corners in the

same manner as living persons abound, but videos of apparitions interacting closely with a video camera for extended periods of time, or fading away just when an in-focus video camera is trained on them, are much harder to come by. This is akin to large triangular spacecraft that purportedly hover over cities for hours—but only at night, for daylight evidence would be too unambiguous. It's a rather convenient coincidence that such novel forces or entities would only ever reveal themselves in a manner that is indistinguishable from misperceptions, photographic artifacts, and the like. What Braude calls the Usual and Unusual Suspects explains this *persistent pattern in the data themselves* far better than either discarnate personal survival or extraterrestrial visitation—no biases necessary.

Prospective Experimental Tests of (Potential) Survival

It's not much of a surprise that Braude et al. would speculate—*after* generating no clear hits—that well-controlled direct tests of survival (or of mind–body separation) might not "even [be] appropriate to the phenomena." That bare possibility doesn't speak to the concerns of most BICS contestants, though, since *had the exact opposite results been obtained*, they surely would've paraded them ahead of everything else as rigorous scientific evidence of discarnate personal survival (or at least as alternatively explicable only in terms of empirically indistinguishable paranormal competitors like all-knowing clairvoyance, psychic reservoirs, or what have you). In such experiments we have simple prospective tests that, if successful, would be collectively lauded by nearly all survival researchers as all but proof of discarnate personal survival—which is exactly why dozens of survival researchers went out of their way to actually conduct them.

To be clear, in outlining their soul-crushing results I don't in fact purport "to know what we should expect to find if the phenomena under investigation are real" (except, perhaps, partially). My point was simply that *if we grant survival researchers their own assumptions*, then by *their* standards of what to expect, absent extenuating circumstances, a medium ought to be able to provide the sort of information that the postmortem tests of survival seek (Thouless, 1984, pp. 24–25). After all, mediumship researchers invariably claim that, outside of experimental settings, mental mediums are *already able to obtain the sort of information sought* (e.g., book titles like Robert Thouless's cracked afterlife code *Black Beauty*). Similarly, near-death researchers claim that NDErs are *already able to provide precise veridical visual information inaccessible to the normal senses* during their experiences—again, just not (so far) under controlled conditions (e.g., Holden, 2009). So what's at issue here is a *historical* question: have survival researchers been able to provide evidence for putative discarnate personal survival that meets the standards of scientific rigor required in, say, pharmaceutical research—or not? My concern is not with "how a parapsychological test or experiment will turn out" (future tense), but rather with how such tests *have in fact turned out* (past tense).

As with any scientific test, Braude et al. are quite right that "[participants might] focus on something more personally meaningful or . . . some minor feature of the target, thereby making it difficult to distinguish near hits from misses." This is undoubtedly another source of frustration, but one that *does not change the requirements of scientific rigor*. If survival researchers are unable to obtain rigorously vetted evidence for their claims, how is that *science's problem*, rather than *survival researchers' problem*? In any case, a practical solution is to *keep trying*, varying the test conditions as much as possible, at least until the accumulated test failures convince survival researchers that this line of research is unlikely to bear fruit whatever the conditions. At the moment, for more than a century such tests have utilized notes in envelopes, audio cassettes, brief musical fragments, encoded messages in various languages, combination locks, static and animated hidden visual targets of various forms, colors, and brightness, thermal, magnetic, and electromagnetic field detectors, visible, infrared, and ultraviolet light sensors, strain gauges, and the behavior of kittens, other animals, and human "sensitives."[13] Is 121 years of varying the test conditions not enough time to work out the kinks?

It's also worth noting that, contra Braude et al., direct tests of survival do not even require their subjects to "elicit psi on demand." Subjects simply have to identify or influence their targets in an apparently anomalous way on *one occasion per test* (though across multiple tests to replicate any effects). For example, the hypothesized discarnate Thouless potentially had countless opportunities in the decades between his death in 1984 and the cracking of his second postmortem cipher in 2019 to supply some living person, *any living person*, with the name of the poem "The Hound of Heaven." Hypothesized discarnates like Stevenson, whose uncracked keys have yet to be revealed (whether through a living person, an electronic device, or a spontaneous assembly of clouds into words), are still "participants" in an ongoing and potentially never-ending experiment *to this day*. Braude et al. raise the initially reasonable possibility that "perhaps they're simply not particularly good at it." But is it reasonable for an *empirical survivalist* taking mental mediumship to provide the best evidence of ostensible survival, say, to also believe that *all* of the several hundred hypothesized discarnates who agreed to such tests during life have been unable or un-

willing to communicate with any living person—and only in the cases when the conditions are this controlled, mind you? Is it reasonable for such an empirical survivalist to alternatively or concurrently believe that all of the living persons who could have been—and still could be—recipients of such a communication (the cumulative human population over several generations, potentially) were unable to receive one? Perhaps "the failure of OBErs and NDErs to succeed in formal or controlled tests" is not "clear enough for us to conclude that the subjects totally lack the ability being tested," but it is certainly strongly suggested by the fact that the purported ability is supposedly able to manifest so long as such tight controls are *not* in place.

It might well be the case that "the demands of the experimental setting tend to frustrate the quest for positive results"—no less for survival researchers than for pharmaceutical company CEOs who are nevertheless expected to take steps to overcome such difficulties and, regardless, demonstrate the safety and efficacy of their drugs by established scientific standards. Indeed, "test conditions might inhibit performance" (not necessarily irreparably), require us to treat "both experimenters and subjects as psychological stick figures," or ignore "the option that the tests were psi-inhibitory"—no less in drug studies than here. But none of this is relevant to whether or not a positive existential claim has been rigorously established in adherence with long-standing scientific principles. The possibility of (likely uncontrollable if real) psi-inhibitory effects not only undermines "whether we can ever confidently assess success or failure in any parapsychological test," but whether we can trust *any* scientific experiment (or series of them).

After all, Braude (2003) hypothesizes a form of psi that has *unlimited* capabilities regardless of its source: living, dead, or inanimate. As my interlocutors note, "if real, [that] could apparently subvert any experimental control" because then "we have no idea what's really going on in a parapsychological experiment"—or *any* scientific experiment, for that matter. For precognition or clairvoyance that can extract information from any time or place whatsoever, or PK that can do anything, would no less sabotage "any experiment . . . in any branch of science." Braude et al. *themselves* thus come "uncomfortably close to" saying that "the results of [science] experiments should be rejected as long as [psi] is possible," substituting their words parapsychology and fraud with my words science and psi, respectively.

Do consistently negative results from direct tests of survival indicate the absence of psi (whatever its source)? No, we're told, because in such cases "Augustine doesn't consider the option that the tests were psi-inhibitory." Braude et al. elaborate: "researchers too often assume, tacitly and naively, that subjects will use only the psychic ability being investigated, that they will use that ability only after the experiment has begun, and that experimenters, judges, and mere bystanders will use no psychic abilities at all to influence the outcomes." Indeed they do, and for good reason—because one cannot do *any* experimental investigation in any science without such assumptions. Braude et al. would have us take seriously the bare possibility that the psi of the hypothesized discarnates might be exactly counterbalanced by the counter-psi of mediums or skeptical experimenters (or any other living persons, near or far, as single individuals or in combination), producing a net zero "amount" of psi displayed. This bare possibility duly noted,[14] if no psi effect is found in a particular test, then we can reasonably presume that no psi was present in it. Otherwise we open the door to "psi-inhibitory" properties of sealed glass (or any other potential control) to explain why PKrs cannot move an object for any distance behind sealed glass, or subconscious counter-psi from the PKr's long-lost high school classmate—or perhaps even that of a demon. The assumptions that Braude et. al eschew here are *necessary for doing science*; without them, we are doing pure metaphysics.

It is notable here that while it is conceivable that "the evidence for a properly-conducted experiment or investigation outweighs the evidence for fraud," the same cannot be said for Braude (2003)'s psi without limits. It could be physically impossible for fraud to have occurred, for example, in the way that it is physically impossible for the suspect to have committed the murder if he was 1,000 miles away at the time—but only so long as one takes for granted that an all-powerful PK does not exist. Otherwise, if we allow for such extravagant possibilities, how could we *ever* conceivably falsify *any* hypothesis? Since surviving actual falsification attempts is what provides positive evidence for a hypothesis in the first place, and unfalsifiable hypotheses cannot be tested—let alone survive tests—allowing psi without limits would render science of any kind (including parapsychology) impossible, since no hypothesis could ever be supported by any data under such a metaphysical scheme, where survivable falsification attempts are not even possible.

The hypothesis that psi without limits exists is little different from the hypothesis that you *could have been* created 5 minutes ago with all of your present memories implanted, or that Descartes' evil demon is consistently trying to deceive you that there's a physical world that other minds partake in when, really, no physical objects or other minds exist at all. But as Karl Popper famously noted, a hypothesis that is compatible with every conceivable piece of evidence—like the hypotheses of Freudian psychoanalysis—doesn't really explain any particular piece of evidence. Taken to its natural conclusion, unlimited-capabilities psi is not a scientific hypothesis *at all*, but a metaphysical one.

And unlike the possibility of fraud, which can be ruled out at least *sometimes*, to various degrees, *unlimited-capabilities psi—like an intervening God—can never be controlled for*, undermining *all* scientific conclusions.

As we've seen, in *every* scientific investigation, scientists regularly assume the nonexistence, powerlessness, or at least absence of influence of Braude's (2003) psi without limits, whether conscious of this or not, because they *must* do so to engage in empirical investigation at all. But those assumptions justify themselves, for in adopting them science has de facto been able to advance our knowledge of the world. They should be no more controversial than the assumption that the future is like the past, that cause–effect relationships exist out there in the world and not just as Humean constant conjunctions in our minds, or indeed that there is any external world outside of our minds at all. All of these assumptions are necessary for us to derive (approximations of) any laws of nature at all, and their usefulness warrants us continuing to assume them. Compared to the possibility that scientific knowledge is not possible simply because such enlisted assumptions can always be questioned, C. D. Broad's basic limiting principles start to look at whole lot better than parapsychologists typically take them to be (1949, pp. 293–296).

The assumptions necessary to do empirical survival research are not nearly as warranted by their usefulness as the ones listed above, whose utility is clear. Indeed, on the face of it, the assumptions needed to do empirical survival research are almost certainly *baseless* (Sudduth, 2016).[15] They are nevertheless quite necessary for survival research to proceed empirically. Without them, one could just as well drop the adjective *empirical* from the term *empirical survivalist* and adopt a faith-based belief in discarnate personal survival.

So it's *undoubtedly* true that "those who originally designed encrypted message or combination-lock tests were making various assumptions about what it's like to survive death" (Braude et al., 2022; cf. Augustine & Fishman, 2015, p. 226). But so what? The aim of my BICS critique overview of such tests was not to attempt the virtually impossible task of *justifying* those evidently unwarranted assumptions (cf. Dodds, 1934, pp. 169–170; Sudduth, 2016), but to make clear what the evidential status of the survival evidence obtained would be *if* we took those assumptions for granted. Why those assumptions, and not others? Because those who claim to have empirical evidence for discarnate personal survival *already make them* whenever they claim, for example, that Mrs. Piper's GP control sometimes provided genuine communications from the deceased Pellew. True, making *other* assumptions might well lead to *different* conclusions about what character of evidence we should expect to find. For example, if persons persist after death discarnate, but are unable to communicate or interact with the living in any way, then we would expect to see *no evidence* of discarnate personal survival *even if it occurs*.[16] Or we might not be able to empirically distinguish between information coming from living persons, inanimate objects, or discarnates given the possibility that *both* LAP and otherworldly psi are examples of psi without limits. (For us to be able to distinguish between them, discarnates would have to be capable of psi feats that living persons cannot pull off.) Attempting to distinguish between them in a world where *all* psi is unlimited would be akin to trying to empirically distinguish various live options in the interpretation of quantum mechanics—potentially something that simply cannot be done.

By empirical survivalists' own standards, then, the rigorous *experimental* evidence for discarnate personal survival is *abysmal*. It does not favor discarnate personal survival even *when we grant empirical survivalists' assumptions*. True, "test failures would at best only disconfirm a particular *model* of personal survival"—indeed, a *testable* model of survival. But that's true for *anyone* evaluating survival research. After all, we don't *know* that discarnates even exist, whether they can interact with our world at all if they do, what capabilities they may or may not have, and so on. We can only *guess* at these things—and there is no empirical evidence that can help us decide between equally possible opposite answers to such questions. Any attempt to appeal to other survival evidence would simply **beg the question**—that is, merely assume what one is trying to show. But *these free parameters are empirical survivalists' problem*, for they are the ones claiming to have positive evidence of ostensible discarnate personal survival, and thus assume the burden of showing what they claim. And even if it is not *possible* for them to meet that burden for their assumptions, they still have the burden to show the next best thing—that *once their assumptions are made* (though we can't justify these guesses), *then* we can have stringent scientific evidence for ostensible discarnate personal survival. And that is a burden that empirical survivalists theoretically *could* meet, but have not in fact met, because the *evidence itself* has thwarted their efforts to meet it.

Given that *no* assumptions can be justified about unavailable discarnates (unlike cryptids that we *could* conceivably get our hands on), why is proceeding from the assumptions of those survival researchers who sought to directly test discarnate personal survival out of bounds?

One final comment about direct tests of survival cries out for reply. Braude et al. ask whether I would concede that replicable positive results from such tests would constitute evidence for discarnate personal survival. Were it not for their prejudice that I would not,[17] the answer would be obvious. For my initial critique was quite explicit that I

thought that such tests were "exactly the sort of controlled experimental research that survival researchers ought to be doing" since "most 'academic scientists and scholars' would surely be satisfied with, say, replicable positive results from Parnia's AWARE II study" (Augustine, 2022). There are several caveats to that answer laid bare above, but perhaps a direct response will ease their anguish about "how open-minded" I am.

It's also worth pointing out the pernicious **misdirection** involved in asking the question. The crux of the debate is not the *psychological disposition* of any particular person or tribe, but *the state of the survival evidence*. What's at issue in the BICS contest is whether or not survival researchers have delivered the kind of *evidence* that would give *the scientific community* reason to think that there is something in this research in need of novel kinds of explanation. What *scientific conclusions* does the evidence warrant? The consensus of Delorme et al. (2021)'s 422 survival-agnostic academics is a better measure of that than the view of any one skeptic, and the consensus of the scientific community as a whole is a better measure of it than that of the coveted survival-agnostic academics.

Thus there's no call for making any of this *personal*. Indeed, if Braude et al. really think that in my critique I've committed as many obvious epistemic "sins" as they accuse me of, it would be counterproductive for them to waste time (and lose face) on pretend or presumed ones. The issue was never about what any particular person believes, but about how discarnate personal survival could move from an item of personal belief to an item of scientific *knowledge* (empirically justified true belief and more).

Until survival researchers produce evidence of the sort that replicable positive results from properly controlled tests of survival *would have* provided, the rest of the world is quite justified in responding: "Call me when a medium gets even *one* hit out of dozens of vetted attempts to get an afterlife code, or when an out-of-body NDEr has actually identified a visual target in the latest installment of the AWARE study. *Then* I'll be keen for replications. Until then, *tend to your own garden*."

Setting the Record Straight

Whether they were intentional or not, it's unwise to leave mischaracterizations of your points uncorrected, otherwise your silence risks further leading readers astray. One big mischaracterization in Braude et al. reads: "This is a good example of how unverified, controversial assumptions can be enlisted when convenient. What's the basis for Augustine's claim italicized above?" The claim in question was: "On the face of it, *if one can really remember aspects of an even older past life, then one should (usually) also be able to remember aspects of a more recent (and perhaps half-a-century-long) intermission period between that life and the current one*, all else held equal (assuming that before-life memories function like those already known to exist, anyway)" [emphasis *theirs*] (Augustine, 2022).

This comment was made in a criticism of Nahm for enlisting *untestable* assumptions to derive his reincarnationist "predictions." The criticism was that one can't predict *anything* from the reincarnation hypothesis simpliciter, and therefore a particular piece of evidence does not constitute evidence for reincarnation. To spin it as *evidence* for such, one has to *amend* the hypothesis with various *uncheckable* auxiliary assumptions to make it count as a "prediction." This would be a legitimate move *if the auxiliaries added were themselves confirmed*—but in Nahm's case they *were not* because *there was no way to confirm (or falsify) them* in principle. The best that one could do was simply assume them *without justification*.

My meaning *should* have been clear from that context, but just to be sure, I added the caveat in parentheses precisely to avoid any misunderstandings. But no matter. To be absolutely clear, the assumption enlisted was merely *floated* as an example of an alternative assumption to that made by Nahm that readers would presumably find more plausible than Nahm's simply because it extrapolates from the known to the unknown, rather than just throwing a dart at the logical space of possible assumptions. Had I used a less plausible *example* of an alternative assumption, my interlocutors surely would have jumped all over *that*. Heads they win, tails you lose.

Absolutely, though: we don't really know *how* discarnate memories work if we don't even know that there *are* discarnates to have memories! Hence *why* the parenthetical comment is structured "assuming that X is true, anyway," which should make clear that the assumption floated indeed *need not be made*. So I was not criticizing Nahm for making *false* assumptions, as *there are no knowable truths* about the characteristics of potentially fictional entities (apart from how authors "paint" them). Insisting that a particular contingent characteristic be taken as fact would be like demanding that a tachyon have a particular mass. Such assumptions cannot be justified precisely because we have no way to confirm their truth, and therefore they obviously can only be stipulated by fiat.

If there is any remaining doubt about my intended meaning, consider this: I'm (obviously) skeptical that discarnate persons *actually exist*. So I'm not invested in demanding that they be characterized in any particular way. From my perspective, any logically possible characterization is just as good as any other for what is, as far as we know, an *imaginary entity*. My interlocutors might as well have scolded me for demanding that we search for the Fountain

of Youth at a particular set of GPS coordinates. At best, such debates are no more than speculative flights of fancy.

So, here at least, it's clearly false that "Augustine once again understands and criticizes the way survivalists often import unstated and undefended assumptions into the debate, but he apparently misses his own frequent deployment of the same strategy." *Not* guilty as charged, your honor. My interlocutors either **miss the point**, interpret me uncharitably, or both. (And for the record, I have never endorsed Paul Edwards' population objection to reincarnation or any variation on it, in either my BICS critique or anywhere else.)

Rising to the Neuroscientific Challenge

Braude et al. (2022) think that I believe that the chiefly neuroscientific evidence "puts survivalists in an awkward position empirically, because they can't explain away a large and respectable body of neuroscientific data suggesting that survival is impossible." First of all, I've *always* characterized this evidence as rendering discarnate personal survival *highly unlikely*, not impossible, since that's the most that any evidence can do for any hypothesis.[18] Second, the issue is not that empirical survivalists *cannot* reinterpret away such evidence—it's that, if they wish to proceed scientifically (rather than pseudoscientifically), they *ought not* reinterpret it away. More on why below.

My interlocutors say that they aim to "respect the evidence" and that I'm "justified in insisting that survivalists confront the challenge posed by the evidence of mind-brain correlations." "But"—which is always interjected when one wants to cling to one's position in the teeth of the evidence—"what are they evidence *of*? Augustine's anti-survivalist position is only an option . . . [one] compelling primarily to those antecedently committed to, or caught in the grip of, a prevailing conventional scientific view of the world." Well, at least *here* they didn't invoke physicalism, or reductionism, or whatever other **red herring** empirical survivalists habitually lean on. If by "prevailing conventional scientific view of the world" they mean the same scientific consensus that rational people grant when they conclude that long-tested vaccines are largely safe and effective, or that anthropogenic climate change is occurring—then yes, I concede that such conclusions might not be compelling to antivaxxers or climate change deniers, and I regard their recalcitrance as *their problem*, not mine. My point was simply that empirical survivalists are no better than other purveyors of pseudoscience in this respect. Little wonder that one can find in their writings the exact same tactics found among young-Earth creationists.

Having self-validated how much they respect the evidence, Braude et al. (2022) go to ask, "who is actually guilty of claiming that neuroscientific evidence is inadmissible?" Survival researchers only need look in a mirror. Nahm, for one, is pretty explicit about dismissing any *conceivable* evidence for the dependence of consciousness on the brain *prior to even looking at the data*: "it is principally impossible to prove that brain chemistry produces consciousness" (2021*, p. 3) since "it is impossible to prove it from a purely logical perspective" (2021*, p. 66). This is just another way of saying that no evidence could ever count in favor of mind–brain dependence *in principle*—rendering one's own independence thesis *unfalsifiable*—which is one way to render unwelcome data inadmissible.

Other survival researchers either ignore or blithely dismiss such data with bumper sticker slogans, or else attempt to consistently reinterpret *all* such evidence away so as to be able to dismiss it *en masse* rather than weigh it. Consider that in Sudduth's (2016) groundbreaking probabilistic evaluation of classical empirical arguments for survival—though this is not his point—*not a single argument from the most discerning empirical survivalists in over a century begins with a less than 50% antecedent probability of discarnate personal survival*. That is, every single one of them begins their evaluation of the *overall* probability of discarnate personal survival in light of the parapsychological evidence *alone*, as if contrary evidence from elsewhere reducing its overall probability doesn't even exist. If that doesn't rig the results of an evidential assessment, then what *does*? A *fair* evaluation would weigh the *total* available evidence relevant to the truth of discarnate personal survival, period. Survivalist presuppositions are so entrenched in this literature that even parapsychologists unsympathetic to survivalist interpretations of the parapsychological evidence (Irwin, 2002) fail to even *mention* the neuroscientific and other evidence *against* discarnate personal survival in their textbook overviews of the subject (Irwin, 1999, pp. 175–277), let alone try to assess it. Such a huge oversight is akin to biology textbooks that fail to mention, in an alternate imaginable universe, independent geological estimates of the planet's age that consistently date it to be too young for biological evolution to have occurred on the planet.

Braude et al. (2022) claim that "its admissibility is precisely why survivalists make the effort to find viable alternative accounts of the data!" I'll get to why the qualifier "viable" makes their reinterpretation efforts self-defeating shortly. But first, let me reiterate what I had already asked in the critique: what *justifies* empirical survivalists' reinterpretation of such evidence, *across the board*, "so that it never counts in one's evaluation" (Augustine, 2022) when one weighs the *total available* evidence? *That question was neither rhetorical nor a straw man*. Conducting an *empirical* investigation in this manner is akin to tipping the scales by simply never adding any opposing items to the opposite

scale. If that's not rendering evidence inadmissible, then nothing is.

For one thing, it *is* possible to weigh evidence that straightforwardly favors a hypothesis against evidence that straightforwardly undermines it without all of this dodgy reinterpretation. The reinterpretation strategy is *particularly* suspect when one has to unfalsifiably invoke auxiliary assumptions that are *untestable in principle* for it to work, such as "disordered brains impair minds *only when persons are incarnate*"—my specific restatement of J. M. E. McTaggart's general auxiliary "the brain is essential to thought *[only] while we have bodies*" [emphasis mine] (1906/1930, p. 106). The more untestable assumptions that one adds to a hypothesis, the more metaphysical (or less empirical) it becomes, inching it closer and closer to becoming purely a matter of faith.

So how might one go about straightforwardly determining whether a particular datum favors (or disfavors) a hypothesis? One can follow C. S. Peirce and Elliott Sober, for one, and make an inference to the best explanation (as I did). Common sense tells us that this is possible. For if there were not sometimes straightforward reads of the evidence—reads where alternative interpretations are *logically possible, but still less probable* than consensus reads—then we would have no reason to favor general relativity over a massively "bulked-up" Newtonian physics unparsimoniously amended with all manner of untestable auxiliary assumptions. Physicists *could have* contorted classical Newtonian physics enough to develop an increasingly clunky neo-Newtonian physics artificially forced to "predict" the relativistic effects that naturally fall out of general relativity. *But they didn't*. Neither should psychical researchers contort the independence thesis so much that it becomes observationally indistinguishable from the dependence thesis simpliciter merely to avoid having to countenance adverse neuroscientific data in their probabilistic assessments:

> For if drastically diminished mental functioning following severe brain damage provides just as good evidence for the independence thesis as subsequently unaffected or considerably enhanced mental functioning would have provided (as predicted by the independence thesis simpliciter and filter theory, respectively), it is hard to see how the independence thesis can stake a claim as an empirical hypothesis at all. It parallels the unfalsifiable Omphalos hypothesis that God created the world to *look like* it had an enormous prehistoric past, but really is less than ten thousand years old. (Augustine & Fishman, 2015, p. 246)

Do Braude et al. really want to say that the consensus of climatologists should not be "privileged" over the alternative beliefs of climate change deniers, since with enough unparsimonious maneuvering, the latter can always be forced to fit the facts, too?[19] If no matter what neither view is any better or worse than the other, why investigate matters empirically at all?

Early on it might be reasonable to try to save one's pet theories[20] from unfavorable evidence in order to avoid their falsification (or at least a reduction in their overall probability). The data themselves might have been bad, for example. But as more unfavorable evidence accumulates—and from a variety of independent, reliable sources—at some point it becomes unreasonable to continue to cling to one's theories in the face of the evidence. All that I ask is that psychical researchers adhere to the same standards that other scientists do.

Braude et al. (2022) posit that empirical survivalists should reinterpret away why mental states "*seem* in so many respects to be bodily dependent" by potentially "arguing that the brain is merely one kind of physical *instrument* for expressing mental activity." Note the **weasel word** "expressing." What does it *mean* to say that the brain is an "instrument for expressing mental activity"? What is the *definition* of the technical term "express"? What hypothesized *relation* between mind and brain is signified by the term? No empirical survivalist ever says.

In normal parlance, to *express* a thing means to show or display it, so *expressing* a person's mental activity usually signifies one's thoughts, desires, and so on being accurately conveyed by one's behavior (e.g., your hand rising because you willed it to rise rather than due to some sort of spasm). The way that former SPR President Broad interpreted Braude et al.'s "instrument for expressing mentality," the instrument theory is inadequate to our own inner experience:

> We will suppose that a man is injured in the head; that before the injury he was of a cheerful and benevolent disposition; and that after the injury he is morose and liable to attacks of homicidal mania. Are we to say that the injury has made no difference to his mind; that this [man] remains cheerful and benevolent; but that the change in his brain compels him to *express* his cheerfulness by scowling and his benevolence by attacking other people with carving-knives? This is scarcely plausible. And, if we accept it, we shall not be able to stop at this point. We shall have to conclude that it is impossible to tell what the character of anyone's mind really is. Lifelong philanthropists may be inwardly boiling with malice which some peculiar

> kink in their brains and nervous systems compels them to *express* by pensioning their poor relations and giving pennies to crossing-sweepers. Once more, *the mind will be reduced to something with no definite traits of its own*, such as benevolence or peevishness, *but merely with certain very general powers* to *express* itself in various ways according to the body with which it is provided. It seems to me that *what is left* of the mind *when we try to square the Instrumental Theory with the known facts* is so abstract and indefinite that it *does not deserve to be called a "mind."* [emphasis mine] (1925, p. 535)

The failure of what he characterizes as "the instrumental theory" led Broad to propose an alternative "compound theory." But unfortunately for empirical survivalists, any "compound of two factors neither of which separately is a mind" stops existing when one of its parts—a functioning brain—no longer exists (1925, p. 536). For the traits that characterize our individual *human consciousness* "depend jointly on [the traits] of the [hypothesized] psychic factor [the separable part of us] and on those of the material organism with which it is united" (Broad, 1925, p. 536). So much for Frederic Myers' "human personality and its survival of bodily death."

Alternative survivalist analogies to (or interpretations of) an "instrument for expressing mental activity" fare worse, for they imply that brained minds are *profoundly different* from brainless minds. Discarnates unencumbered by pesky brains would have to be so radically different from their brained selves that we could not truly say that the *same* mind survived as a discarnate. Worse, given just how many mental capabilities brains de facto contribute to our incarnate minds (Augustine & Fishman, 2015, pp. 274–276), whatever mental remnants might persist once all of our brain-enabled capacities are stripped away hardly deserves to be called a *mind* at all. Whenever empirical survivalists get more specific about their theories on the mind's relation to functioning brains, they are forced by the facts to concede that the functioning brain changes our mental functioning *through and through*. Thus it is as if we are never really *ourselves* when we are incarnate. The corollary of this implication is that who we are *now* in a substantial or "thick" sense will not survive death even if some mere *part* of us becomes discarnate. Some abstract impersonal part (not all that different from our bones) might "survive" biological death—perhaps with the mind of a paramecium—once the brain activity that sustains *human* consciousness during life drops away. But that is not *personal* survival.

By now most empirical survivalists have at least moved from vague talk about "expressing" consciousness to talk about "filtering" it. Unlike the **weasel word** *express*, at least we have some semblance of what a *filter* does that might clarify the role in mental functioning that empirical survivalists hypothesize for a functioning brain. Unfortunately for them, how varying brain functioning de facto affects mental functioning is *not in any way analogous to* what a filter actually does to groundwater (say), but rather *the opposite* of it:

> If the mind is "not generated by the brain but instead focused, limited, and constrained by it" (Kelly et al., 2007), the filter theory entails that a brainless mind will be expanded, less limited, and unrestricted by brain function. Since no brainless minds are available to clinicians for study, this is not a falsifiable prediction in itself. But it does have falsifiable consequences, most obviously that the greater the disruption in brain function, the "freer" the mind will be from its neural confines, and hence the clearer one's cognitive function will be. For example, we would expect the progressive destruction of more and more of the brain's "filter" by Alzheimer's disease to progressively "free" more and more of consciousness, and thus increase Alzheimer's patients' mental proficiency as the disease progresses. Just as removing sections of a dam would increase the flow of water going through it, the degenerating "filter" would become increasingly ineffective in limiting consciousness as more and more neural pathways were destroyed. (Augustine & Fishman, 2015, pp. 230–231)

But Braude et al. (2022) prefer a different analogy: "How, then, can survivalists argue for the superiority—or just the adequacy—of their point of view? According to McTaggart, one strategy would be to offer competing analogies that are [allegedly] at least as weighty as analogies apparently favoring the anti-survivalist." To assess whether their preferred alternative is either superior to, or at least no worse off than, other analogies, we must first discern the apparent analogues in McTaggart's analogy:

> If a man is shut up in a house, the transparency of the windows is an essential condition of his seeing the sky. But it would not be prudent to infer that, if he walked out of the house, he could not see the sky because there was no longer any glass through which he might see it. (McTaggart, 1906/1930, p. 105)

> If a mind is shut up in a body, the functioning of

> the brain is an essential condition of his possessing mental capacities. But it would not be prudent to infer that, if he "left" the body, he could not possess them because there was no longer any brain through which he might possess them.

To be clear, the analogues seem to be: man = mind; house = body; the transparency of = the functioning of; the windows/glass = the brain; perceiving/seeing the sky = possessing (active) mental capacities; and walking out of the house = 'leaving' the body.

Set aside (for now) that this is a *poor* analogy since it does not do justice to the *actual* neuroscientific data (which is what Henry Stapp and Michael Levin were getting at in my initial critique, and what I point out in my critique of the filter theory above). There is a more basic point to underscore. All of these analogies are *illustrative* analogies, not *argumentative* analogies. None of them actually *argue for* empirical survivalists' views on exactly what brains are *for*; rather, they merely *state* or *assert* those views. Thus, contra Braude et al. (2022), they cannot be used to support *either* the superiority or the evidential adequacy of empirical survivalists' analogies.

Consider: A bowling ball–rubber sheet analogy *illustrates* what general relativity *states* is the relationship between massive objects and spacetime, but it is not part of any evidential argument that general relativity is probably (approximately) true. To convey an *argument*, an analogy has to be part of a larger argumentative form:

1. X has (relevantly similar to each other) features a, b, c.
2. Y also has features a, b, c—plus feature d.
3. d is relevantly similar to (not relevantly dissimilar to) a, b, and c.
4. ∴ X probably also has feature d.[21]

The basic idea of an *argument from* analogy is that X is known to be like Y in certain ways, therefore X is probably is like Y in other ways, too. If it's an inductively *strong* argument, then these ways will have many relevant similarities, and few relevant dissimilarities, between them.

If we look at McTaggart's analogy, we can see that it *cannot be* an argument from analogy, for then the basic concept would have to be that windows are like brains in certain ways, brains *also* have this other feature, all these ways are similar to each other, and therefore windows probably have this other feature, too. McTaggart only lists *one* feature of transparently windowed houses that's supposed to exist in functioning-brained bodies (so the former would have to fill in X), leaving any additional features said to be relevantly similar to that one feature to be part of functioning-brained bodies (Y). So, given that McTaggart's final position is that functioning-brained bodies are only necessary for possessing *any* mental capacities (or at least additional non-perceptual ones) when one is incarnate, this is the closest that we could ever come to making his analogy out to be part of an *argument* from analogy (using mathematical comprehension as a specific example of a non-perceptual mental capacity):

1. Transparently windowed houses are required for sky-perception only when a man is housed.
2. Functioning-brained bodies "also" are required for sky-perception only when a mind is incarnate, *plus* they are required for mathematical comprehension only when a mind is incarnate.
3. Being required for mathematical comprehension is relevantly similar to (not relevantly dissimilar to) being required for sky-perception.
4. ∴ Transparently windowed houses probably also are required for mathematical comprehension only when a man is housed.

Of course, conclusion 4 makes no sense and is not McTaggart's view. But suppose that the imagined argument is *not* that functioning-brained bodies are like transparently windowed houses because they both have feature a, functioning-brained bodies also have feature b, and therefore transparently windowed houses probably have feature b, too—but the other way around. That is, suppose that the imagined argument instead is that transparently windowed houses are like functioning-brained bodies because they both have feature a, transparently windowed houses also have feature b, and therefore functioning-brained bodies probably have feature b, too. In *that* case, the imagined *conclusion* would make sense, but not the imagined *premises*:

1. Functioning-brained bodies are required for sky-perception only when a when a mind is incarnate.
2. Transparently windowed houses "also" are required for sky-perception only when a man is housed, *plus* they are required for mathematical comprehension only when a man is housed.
3. Being required for mathematical comprehension is relevantly similar to (not relevantly dissimilar to) being required for sky-perception.
4. ∴ Functioning-brained bodies probably also are required for mathematical comprehension only when a mind is incarnate.

Here conclusion 4 *is* McTaggart's view, but the premises that yield it are not viable. Imagined premise 1 begs the

question since it's contentious that discarnate minds could visually perceive anything without a visual cortex, let alone without eyes—and argumentative analogies start with facts that are not in contention. But more importantly, the second clause in imagined premise 2 is obviously false. The reason that none of this makes any sense is because *McTaggart's analogy is not part of an argument from analogy at all, nor any other evidential or probabilistic argument*.

Thus the appeal to McTaggart's analogy fails in two respects. First, it is not part of any argument that, probably, brain functioning is *not* required for (some or all) mental functioning to exist/occur. Second, it does it not even show that such an independence thesis is no less probable (given the neurophysiological data) than its negation, which is what I take McTaggart's (and my interlocutors') aim to have been. In what follows, I will elucidate how the late great philosopher William Rowe might have argued this second point, putting some meat on the bones of his contention that "against McTaggart, the evidence seems to show that the relation between our bodies and our mental life is enormously more intimate and complex than that between a human being and a room in which she happens to be enclosed" (2007, p. 159).

To show that the dependence and independence theses are evidentially on a par, Braude et al. (2022) would have had to have *shown* (not merely asserted) that either the dependence thesis would not lead us to expect my bulleted agreed-upon facts, or else that the independence thesis would lead us to expect them *just as much*. But they did *neither*. My accessible inference to the best explanation inspired by Peirce and Sober went unrebutted, despite my having indicated exactly *how* one could go about rebutting it. But that's just as well, as it's probably not *possible* for them (or anyone else) to rebut it (correctly) *since the evidence itself constrains one's maneuvers* here, as Rowe recognized. Empirical survivalists would do well to consider whether in neuroscience they have hit a wall rather than simply an obstacle.

Independence thesis proponents may one day come up with a conceivable analogy to the mind–brain relationship that, unlike their attempts thus far, *does justice to the neuroscientific data*. But the fact that it's so difficult to think of a merely illustrative analogy that's not so vague as to be vacuous—*and* is true to the facts—underscores the lengths to which empirical survivalists must go to reinterpret this evidence in some way, *any way*, to force it *not to count* against discarnate personal survival. So much for Braude et al. (2022)'s promise of "competing analogies that are at least as weighty as analogies apparently favoring the anti-survivalist."

With or without analogies[22], one can derive observational consequences from both the independence and dependence theses (in amended or unamended forms). These derived *predictions* either match what neuroscience (plus other science) has in fact uncovered, or they do not. Whether unamended, or amended as it has been thus far, what the independence thesis predicts that we will find contradicts what "neuroscience-plus" has in fact uncovered, whereas what the dependence thesis predicts matches it. We thus have pretty compelling evidence that having a functioning brain almost certainly is necessary for human mental processes to exist/occur. The desperate last resort that a functioning brain is a necessary condition for possessing (nondormant) mental capacities *only when minds are incarnate*[23] is never supported by any arguments or evidence—nor could it be. The italicized auxiliary is *logically possible*, to be sure, but nothing in the body of neuroscientific *data* warrants *adding* this limitation.[24] And since it cannot be scientifically tested even in principle (and thus can never be scientifically confirmed), its addition comes at the expense of lowering the parsimony—and thus (all else held equal) the overall probability—of the independence thesis. For the more unconfirmed auxiliaries that one attaches to one's "bulked-up" hypothesis, the more ways there are for it to be mistaken, and there is no way to compensate for that widening risk by using only confirmed auxiliaries, since untestable auxiliaries can never be confirmed.

For the sake of greater understanding, let me put the point another way. Since my inductive arguments are evidential, the *bare possibility* that the independence thesis could still be true despite the strong evidence against it **misses the point**. For in such cases a *probabilistic* assessment is what's called for. It's nowhere near sufficient, then, to say that alternative interpretations of the neuroscientific data are *possible*. To defeat my arguments, they also have to be *likely* given other things that we know—and *that* is only possible if they are testable and confirmed. It's no less logically possible, after all, that fossils of simpler organisms are found in older geological strata than those of complex ones *because God created the fossils that way all at once 10,000 years ago* (rather than due to biological evolution). Just as no evolutionary biologist takes such "alternative accounts of the data" seriously, no neuroscientist should take a "dependence-looking independence thesis" seriously, either. If all signs from reliable sources of evidence point to existential or functional dependence, then we should tentatively take such evidence to indicate exactly what it seems to indicate (barring forthcoming, comparably reliable bodies of evidence that suggest otherwise—but we are here talking about *available* evidence).

To sum up: McTaggart's analogy, like other ones, merely *illustrates* what the independence thesis *asserts*; it argues neither that the chiefly neuroscientific facts make

the independence thesis more probable than its negation, nor that the independence thesis is just as adequate to the data as the dependence thesis. So it is not part of any evidential argument, nor any adequate critique of one, *at all*. Why, then, do Braude et al. (2022) bring it up in the first place?

Once we substitute McTaggart's illustrations with their analogues, it's plain to see that his "subtle re-framing of the issues" is nothing more than a restatement of the old pilot–vehicle analogy that even René Descartes admitted was inadequate to the known facts *in his day*:

> Nature also teaches me . . . that I am not merely present in my body as a sailor is present in a ship, but that I am very closely joined and, as it were, intermingled with it, so that I and the body form a unit. If this were not so, I . . . would not feel pain when the body was hurt, but would perceive the damage purely by the intellect, just as a sailor perceives by sight if anything in his ship is broken. (1641/2017, p. 64)

Mind you, McTaggart does not *explicitly* advocate anything so naïve as the pilot–vehicle picture of the mind–brain relationship. But it is *implied* by his holding steadfast that *there is nothing essential to individuality that brained minds can do, that brainless minds cannot also do*. If everything essential to our individuality can persist "soul-side" after death, then it *has to be* the soul that possesses—and thus preserves—all of those individualistic mental traits.[25] That is, for discarnate personal survival to occur, individualistic mental traits would have to be "located in [their] own [soul] substance with its own principles of operation" (McGinn, 1996, p. 26) in order to be carried along with the enduring soul—and *not* inhere "in"[26] the brain that dies. But if we really didn't need a functioning brain *at all* in order to possess such traits,[27] then they would be (largely) impervious to such things as the actual effects of brain damage on mental functioning. In matter of fact, such traits are *anything but* impervious to brain damage (Gennaro & Fishman, 2015).

To see this more clearly, note that, definitionally, on the independence thesis simpliciter, mind and brain are simply two independent things that cross-interact. But as personal identity theorist Eric T. Olson points out, on this picture "[w]e should expect the functioning of the body and the soul to be as independent as the functioning of a drone and its operator, contrary to our experience with general anaesthetics and head injuries. Imagine a human being who could not remain conscious even for a moment unless the drone she controls is intact. That would be mysterious" (2021, p. 91)—nonsensical, even. So while mind–brain independence is *conceivable*, absent amending auxiliaries, it predicts mind–brain correlations already known to be false:

> [T]his natural analogy with two independently existing things in two-way interaction makes no sense of *actual* mind–brain correlations. For if a remotely controlled vehicle were captured, no amount of fiddling with the vehicle's circuitry by its captors could debilitate the capacities of its remote operator, miles away. At worst, the vehicle's captors could cut off "sensory data" coming from the vehicle by disconnecting or destroying its camera, microphone, or transmitter, or disable the operator's control by disconnecting or destroying its motor functions or receiver. But the captors would be completely powerless to remotely affect the operator's ability to do math, recognize undistorted faces, or understand language. Yet permanent or transient changes to brain structure or chemistry can produce exactly such results. (Augustine & Fishman, 2015, p. 234)

Bear in mind that the drone analogy is not merely some "preferred" analogy "apparently favoring the anti-survivalist." It is an analogy that simply maps what the independence thesis simpliciter *says* that brains do (or don't do). That is, the analogy is faithfully *derived from* the thesis. Even those sympathetic to discarnate personal survival have at times put their finger on (at least part of) the problem. Since my initial BICS critique already quoted Stapp and Levin realizing it, compare the late survival researcher Hornell Hart:

> The TV actor is affected very little by what happens to any individual receiving set. Even the piano on which the musician learns to perform affects him relatively little as compared with the profound ways in which the growth and development of a given 'I'-thinker is affected by the structure, the chemistry and the functioning of the brain through which he observes and acts. (1959, p. 220)

To be sure, some of these survivalist commentators misidentify the *source* of the problem as faulty analogies. But simple illustrations like the operator–drone or musician–piano analogy often merely *reflect* the actual source of the problem—*the independence thesis itself* (at least when various untestable auxiliaries are not added to unparsimoniously bulk the thesis up, which would exact the toll of lowering its overall probability). The problem doesn't lie with the unfaithfulness of the illustration so much as with *what the analogy illustrates*. That is, the problem is not that

such analogies aren't true to empirical survivalists' theories—*the problem is that empirical survivalists' theories are not true to the facts*. If an analogy illustrates a theory that makes false predictions, then the analogy will make false predictions, too.

Conclusion: Reframing Facts Does Not Change Them

Rather than defend or retract their exposed fallacies, Braude et al. (2022) shift the focus of their reply from *their* BICS contest-winning essays to *my* supposed missteps in evaluating them.[28] I suppose that this should not be surprising given that their own work was on the line, but still I had hoped for better given how systematic my initial critique had been. For in their haste to defensively condemn it, they left the question of the adequacy of the arguments found *in the BICS essay competition* unresolved. Instead of killing the messenger with the ***ad hominem tu quoque*** fallacy, they would do well to step back and reflect on whether *they* should stop making fallacious arguments going forward so that *their* contributions aren't so easily vulnerable to attack.

In redirecting attention to whether *my* position is tenable (by their lights), Braude et al. missed an unparalleled opportunity to groundbreakingly *weigh* the survival evidence on one scale against the chiefly neuroscientific evidence on the other. Many of those skeptical of personal survival—both in academia and in everyday life—regard the neuroscientific evidence as decisive. Thus readers might expect my commentators to give the task of disputing a novel statement of the neuroscientific case against discarnate personal survival the utmost priority, particularly since the parapsychological case for survival has been done to death many times over. Sadly, they don't even *try* to say anything new here. In lieu of lazily regurgitating what they had said on the issue many moons ago, they'd better serve *advancing* the survival debate by meeting my novel criticisms with novel responses. Hopefully future commentators will rise to the challenge.

But perhaps this is asking too much. If one pits the survival evidence *as empirical survivalists construe it* against the neuroscientific evidence as neuroscientists construe it, it's no contest. As those in disciplines *other than* psychical research will attest, the *quality of the evidence* grounding the neuroscientific case against survival, regardless of its conclusiveness, far exceeds that of the data purportedly supporting the survival hypothesis. My initial BICS critique and reply to my interlocutors simply make the reasons *why* explicit.

While it's natural to characterize that case as anti-survivalist because of its *implications*, motivationally it is more accurately characterized as pro-scientific. After all, in this sense the well-supported hypothesis that having a functioning brain is a necessary condition for having a human mind is no more "anti-survivalist" than the well-established descent-with-modification hypothesis is anti-creationist, or heliocentrism is anti-geocentric. Our knowledge of the mind's relationship to the brain, as imperfect as it is, nevertheless renders discarnate personal survival highly unlikely in light of the *totality* of our best evidence. This revelation is not necessarily a *welcome* one to mortal creatures who can contemplate their own extinction, but it is *honest*.

After all, even everyday people notice the clear effects of brain damage (among other things) on mentality first, and *then* hypothesize the dependence of consciousness on the brain to make sense of them. *That* is why Sam Harris' line of reasoning was so persuasive to his applauding audience. Much to our chagrin, the dependence thesis *happens to* severely undercut the prospects for discarnate personal survival. And many hope against all odds to find relief in some evidence, *any* evidence, that might give us some out from Philip Larkin's "sure extinction that we travel to / And shall be lost in always. Not to be here, / Not to be anywhere, / And soon; nothing more terrible, nothing more true" ("Aubade," lines 17–20). That we might be reincarnated as a cork to "stop a beer-barrel" or seal "a hole to keep the wind away" (*Hamlet*, Act 5, Scene 1, lines 209–216) has never been an attractive prospect (#CancelShakespeare). But reality does not bend to our will.

When evaluating the *totality* of the evidence, it's reasonable to presume that *things are exactly as they seem* in the presence of diverse, independent sources of reliable evidence corroborating each other, and in the absence of counteracting reasons to think otherwise. No doubt survival researchers will claim such reasons, but we've seen that their evidence does not really stand up to scrutiny, let alone outweigh our everyday experience of the biological fragility of our own minds. Their evidence is not exactly *nothing*, but neither is it particularly compelling. Perhaps that's because death is exactly what it seems to be—for us no less than for any other living thing.

I thus stand by my original conclusion: given the evidence as a whole, discarnate personal survival is not even minimally more probable than not. For all the ink that my interlocutors spilled, it's notable that they never actually dispute that conclusion—perhaps because it is *indisputable*.

A final caution. Braude et al. accuse me of cherry picking "unsuccessful efforts to get OBErs and NDErs to identify remote targets." If they had some comparable *successful* experimental evidence for ostensible survival to offer, perhaps they would have a point. But none was mentioned (or even cited) because they *do not*. So what specific exper-

imental evidence of survival could I have possibly excluded in cherry-picking the evidence? Accusations of fallacious reasoning are easy to make, but harder to demonstrate, which is why I quoted BICS contestants committing fallacies verbatim and then simply named their fallacies.

Given their financial windfall, if my interlocutors want to fund my (or any other skeptic's) in-depth, paralegal-like document review of the details of Braude's personal hobby-horse—the physical mediumship of D. D. Home and/or Eusapia Palladino—then we can talk about their showmanly challenge. Until then, I'm about as incentivized to do so as Braude et al. are to review a dossier of the last 50 years of testimonial evidence for Bigfoot, which similarly has nothing to do with ostensible evidence for discarnate personal survival.

In their final **tu quoque**, Braude et al. write that "although he charges survivalists with straw-man reasoning, that's something he often does himself, either by describing the opposing survivalist position in perhaps its least plausible form, or by simply charging survivalists with positions they (or at least the best of the lot) don't hold."

So I'll issue a simpler challenge that doesn't require an institutional grant to justify undertaking it: find just one *direct quotation* of a single instance where I explicitly attributed a position to a BICS essay contest winner that the winner did not advocate. Otherwise, mind your accusations.

NOTES

[1] Braude et al. (2022)'s failure to simply cut the reference to "some form of the identity theory or epiphenomenalism" from Braude (2005)'s familiar arguments is particularly egregious. An entire section of my critique made plain *why* these (and other) mind–body theories are irrelevant to the issue at hand. My interlocutors' choice to reserve their commentary for other sections is fair enough, but if you are not going to dispute what I said in that section, you could at least extend the courtesy—and have the wisdom—of not repeating the errors laid bare in it. Contemporary philosophers of mind have been highly critical of *both* the identity theory and epiphenomenalism *for more than half a century*, and my arguments do not require one to assume either anyway, *as I had emphasized*. Anyone who bothered to review this literature would quickly discover the lengths to which the vast majority of contemporary philosophers of mind have gone to *avoid* epiphenomenalism—e.g., as a *nonnegotiable* requirement in both Jaegwon Kim's causal exclusion argument and the critical responses to it. And the threat of epiphenomenalism is *already avoided* if one assumes identity theory, for under it the mental causation that philosophers of mind by far aim to preserve would just *be* physical causation. Adopting *either* reductionist identity theory *or* epiphenomenalism would effortlessly *dissolve* the problem posed by Kim's causal exclusion argument (see Moore, 2022, §g & §i). And yet that problem remains vexing—fueling newfound interest in varieties of Russellian monism as a potential solution to it (ever since Chalmers, 1996, pp. 153–155)—because contemporary philosophers of mind are largely skeptical of *both* identity theory (which already avoids causal exclusion) and epiphenomenalism (which seems to self-stultify by denying mental causation altogether). So why do Braude et al. (2022) insist on *retaining* this **straw man** argument among their **talking points**?

[2] Cf. Tressoldi et al. (2022) to substantiate this assessment.

[3] The "Ranking the Survival Evidence" section was more than twice as long as the next largest section, "What Does the *Total* Available Relevant Evidence Tell Us?"

[4] Of the three most promising contributors that I focused on, only Delorme et al. (2021*) actually systematically ranked the evidentiality of all of the main sources of survival evidence, so I structured my "Ranking the Survival Evidence" section on what they had said first and what others had said second, as is obvious as I go over each source of survival evidence. Since I had to organize a large amount of material on nine sources of survival evidence in some logical way, it made sense to for my progression to mirror that of Delorme et al. (2021*)'s systematic ranking. Typically, I would add what Braude (2021*) or Nahm (2021*) (and later others) said about Delorme et al. (2021*)'s points, or as a contrast to what Delorme et al. (2021*) had said.

[5] Among those skeptical that the GP control was actually the deceased George Pellew were Pellew's mother and brother, particularly after the GP control could not answer a question that the living Pellew could've answered with ease, leading his brother to conclude: "Whoever it was answering that fellow, whether Mrs. Piper or Phenuit [*sic*] or anyone else, it was *not George*" (Gardner, 1992, p. 226).

[6] Richard Hodgson also found evidence of what he took to be the GP control mind-reading the living, such as when "G.P. was factually incorrect in what he described [as having *happened*], [but] his descriptions corresponded to the *intentions or plans of the persons involved*" [emphasis mine] (Sudduth, 2016, p. 81n9).

[7] And I didn't even mention here mental mediums' "contact" with presumed-to-be-deceased fictional characters in experiments designed to "test the spirits" (Rinn, 1950, p. 136; Tanner, 1910/1994, p. 254), or their "communications" with those whom they believed to be deceased, but who turned out to be alive and well when the sittings took place (Holt, 1919, p. 203; Tart, 2009, pp.

266–267).

[8] These examples cast Braude et al. (2022)'s acknowledgement that "belief in survival is easy when one ignores relevant detail" in a rather ironic light.

[9] Though Braude et al. (2022) underscore the GP identifications "repeatedly cited as evidence of [Mrs. Piper's] paranormal abilities by a succession of commentators from Dr. Hodgson onwards" (Coleman, 1998, p. 372), M. H. Coleman points out that conventional explanations of such aspects of her mediumship are too often given short shrift by those who cite them: "[F]or anyone prepared to consider the material objectively, [consider] how much of Mrs. Piper's information could have been obtained from purely mundane sources. Thus it should be remembered that it was not until Professor Hyslop took over the investigation that conversational exchanges taking place in Mrs. Piper's presence were recorded; and even these records did not include significant pauses, changes in facial expression, etc., which convey a good deal of information in normal social intercourse. When these sources are supplemented by subconscious cues provided by her sitters, it is not surprising that she could provide them with personal information, most of it probably obtained by her acknowledged 'fishing'" (Coleman, 1998, p. 372). Others have elaborated on the role of dubious transcription in the GP sittings that so enamor Braude et al. (2022): "At the most important sitting, for example, that at which 'G.P.' made its first appearance (22nd March 1892), Hodgson concealed that he was not present for some 24 minutes, during the one-fourth of the sitting that included the unprecedented spelling of names of several absent friends and of Pellew" (Munves, 1997, p. 143). James Munves also points out that what counted in "identification" was questionable: "Hodgson did not explicitly list failure to recognize as a negative criterion. He did excuse the non-recognition of Sally Fairchild . . . on the grounds of her changed appearance, and of her mother . . . Other non-recognitions, however, were ignored: of Richard Welling, one of Pellew's closest Harvard friends, whom 'G.P.' had repeatedly asked to see. Two other recognitions were dubious: Arthur Carey, and Charles Perkins. 'G.P.' addressed neither by name; but Carey was hailed as 'Arthur' as Piper was coming out of the trance, after 'G.P.' had gone; and 'G.P.' wrote 'Opdyke' and an illegible name before coming up with Perkins, and did not communicate anything to him" (1997, p. 147).

[10] I should make clear that my concern is not so much fraud *among researchers* as it is fraud performed by their subjects. Prior to their deaths, collusion between living mediums and deceased survival researchers cannot be fully ruled out as a source of potential positive results in post-mortem tests of survival, for example—though that possibility can be minimized if such results can be replicated across many different deceased survival researchers. But beyond simply acknowledging that it exists, the possibility that survival researchers *themselves* would be in on perpetrating such a hoax doesn't really concern me.

[11] If we are going by the legal standard requested by BICS, it's worth noting here that in the recent *Depp v. Heard* (2022) defamation trial in the state of Virginia, actress Amber Heard's therapist's notes, though recorded soon after claimed events, were deemed inadmissible because they consisted solely of unverified testimony, unlike her admissible medical records.

[12] Cf. Robert Almeder (1992, p. 249) and Nahm on the much lower "investigability of the most compelling aspects of mental mediumship" (2021*, p. 13) today since survival researchers cannot produce contemporary mediums willing or able to pull off comparably impressive performances.

[13] These details are discussed in the literature *cited* in the "Where Have All the Deceased Survival Researchers Gone?" section of Augustine (2022).

[14] This *bare possibility* is no more a positive reason to believe that *maybe psi was there after all* than is the fact that God *could have* foreseen some overriding good that might emerge in a billion years that would outweigh the evil of the Holocaust, leaving open the bare possibility that apparently gratuitous evil isn't necessarily actually gratuitous evil. Anything's *possible*, but that bare possibility does not change the fact that apparently gratuitous evil constitutes strong evidence against the existence of an all-powerful, all-knowing, and perfectly good God (Draper, 1989, pp. 345–346). For one, we are *justified in tentatively presuming that such forces or entities do not exist until we are given a positive reason to think that they do*, period. More importantly, just as the bare possibility of greater overriding consequent goods is balanced out (or neutralized) by the equally bare possibility of even worse consequent evils, the bare possibility of counter-psi is balanced out (or neutralized) by the equally bare possibility of reinforcing psi doubling rather than neutralizing a *displayed* psi effect.

[15] Making assumptions is a prerequisite for *any* empirical investigation, and the survival hypothesis predicts *nothing* unless it is bulked up with inherently unwarranted, untestable auxiliary assumptions. *In principle*, one cannot claim to have *evidence* favoring ostensible survival without assuming that discarnates have certain unverifiable characteristics. Given that there is no way to *check* whether discarnates have the features that we attribute to them, these assumptions must be *stipulated*, as they can never be *justified*. It's therefore perfectly reasonable to *play along with* empirical survivalists' assumptions and see where that leads empirically/predictively.

[16] In this imaginable world, we could still have *neuroscientific* evidence for *the independence thesis* (the more basic prerequisite of discarnate personal survival): "if greater brain complexity had been found to yield lesser mental acuity, this would have falsified the dependence thesis and confirmed the filter theory. Less obviously, then, the [actual neuroscientific data in our world] constitute potential falsifiers of the dependence thesis that did not in fact falsify it (i.e., its confirmed predictions)" (Augustine & Fishman, 2015, p. 281n33).

[17] Ironically, in the same paragraph where Braude et. al accuse me of committing the same fallacies that I extracted *verbatim* from the BICS essays—which is itself an ***ad hominem tu quoque*** if there ever was one—they add: "Augustine seems to infer not simply that nothing psychic was happening during the tests of OBErs and NDErs, but more likely, given his broad skepticism about things paranormal, that nothing psychic *could* occur." That's an odd thing to say of tests that, as a contingent matter of fact, have *historically* failed to reveal any evidence of psi. Apart from an aversion to the principle of charity, what prompts this attribution—the fact that *other skeptics* have expressed this? (e.g., Alcock & Reber, 2019). The reason why "Augustine never clarifies this" is because it exists in their heads, not in my BICS critique.

[18] Discarnate personal survival may well be nomically or even metaphysically impossible, of course, given the true nature of consciousness (whatever that turns out to be). But the issue here is *what we can know*, in the same sense that we can be said to know things about other scientific matters, about the relationship between our individual mental lives and our brain functioning *in light of the total relevant evidence*. Here we can only speak in probabilities, as with all scientific hypotheses. As far as we can ascertain, personal survival does not seem possible, *given the evidence*, without technological or miraculous intervention. But that conclusion is highly probable, not certain.

[19] Even apart from what common sense implies, presumably the answer is "no." Consider that Braude writes that survivalists' "enlarged ontology . . . would ordinarily place the survivalist position at a theoretical disadvantage compared to ontologically more parsimonious rivals" (2021*, p. 5). Presumably what is "ordinarily" privileged here is not completely arbitrary, else Braude wouldn't have made the point. Moreover, there are clear rationales that *justify* our reliance on various theoretical virtues: theories that make fewer assumptions have fewer ways to be in error (*parsimony*), theories that fit rather than conflict with background knowledge don't require us to reject the large body of evidence grounding that knowledge (*plausibility*), theories that make testable predictions can be *checked* (*testability*), theories supported by a wide *range* of independent sources are more *representative* of the *total* evidence (*scope*), and so on. If what's "ordinarily" used for interpreting evidence isn't warranted, then *any* uncontradicted assumptions could switch out the "privileged" ones when evidential arguments produce unwelcome conclusions. Steering clear of such dodgy reinterpretation is what I mean by *respecting the evidence*. Granted, we *technically* don't have to respect parsimony—but neither do we have to respect the law of noncontradiction (Braude, 2020), and we won't be able to conclude *anything* about the world without some guidelines or heuristics. What *should* be at issue for *empirical* survivalists is a *scientific* question: Can we make a case for (or against) discarnate personal survival *taking for granted the same working assumptions that are made in other successful sciences*? Otherwise, how do we ever *distinguish between* when our understanding "homes in on the most likely candidates" or "leads us astray"? (Braude, 2021*, p. 27n34).

[20] The term "theory" should be understood as a synonym for "hypothesis" throughout—as Braude et al. (2022) also use these terms—following the conventions of philosophers of science.

[21] This formalization is adapted for ease of understanding from Velasquez (2017, pp. 268–269) as informed by Burbidge (1990, pp. 11–20).

[22] Incidentally, since Cartesian dualist Richard Swinburne (1997) already independently made Charles Richet's analogy in my critique, regurgitating Braude (2005) on the matter was superfluous.

[23] Also note how far-reaching the consequences of making such a maneuver would be for *all* science: "The move from independent operation to empirical evidence of such, or from interaction to detectable interactive traces, will always be an inferential leap, even if a small and uncontroversial one (at least in other cases of this kind). Thus, as a last resort it can always be called into question, for nothing is certain in probabilistic reasoning. But if we are barred from deriving such straightforward empirical consequences here, then evidently expecting *any* observational differences to emerge between our rival hypotheses is not permissible, and no facts can ever have a bearing on the likelihood that either of our rival theses are true" (Augustine & Fishman, 2015, p. 245).

[24] Consider how Galileo confirmed the law of falling bodies (free fall) $d = (1/2)gt^2$ from various experiments with vertical drops (and inclined planes that allowed for easier measurement): "Galileo observed several falling metal balls as they dropped a hundred feet and found that each time they were moving at an accelerating rate" (Velasquez, 2017, p. 400). From such observations he conclud-

ed that objects fall at a constant/uniform acceleration. He *could have* unparsimoniously added that objects stop accelerating after 100 feet, or after 1000 feet, or what have you, since any of these alternative reads of his data were logically possible. But scientist that he was, he *did not*, because nothing in his *data* justified *adding* any such ad hoc limitations.

[25] This way of putting things approaches the issue from the perspective of what empirical survivalists *claim is true* about the mind–brain relation. But if we approached the issue from the perspective of what is *known to be true* about mind–brain correlations, the corollary would be: "Once we know *what* the brain does for the mind, we know by process of elimination what the soul *cannot* do for it, given the survivalist requirement that the soul has to be something independent from the brain in order to survive the brain's death" (Augustine & Fishman, 2015, p. 275; cf. Lakoff, 2003, p. 80).

[26] Braude et al. (2022) write that the idea "that memories and mental states generally are *in* the brain or in something else" is mistaken "because memories (and mental states generally) aren't things or objects with distinct spatiotemporal coordinates." Here they'll get no argument from me. Are one's savings in a particular pile of paper bills in a bank vault (or, once upon a time, in stacks of the gold bullion that used to back up paper money)? Or are they somewhere "in" a series of cloud servers? These are questions about stipulated human conventions like highway speed limits, not states of affairs. As my interlocutors note, the word *mind* can serve as a shorthand "for a class of mental events, just as 'the weather' is a general term for the class of meteorological events, and 'the economy' stands for a class of financial transactions." So it's beyond me why they accuse me of "reifying the mind" when they could have more charitably read me as using the very same terms that *they* use in the same manner that they use them—as shorthand. So, for the record: a memory is a specific instance of the act of remembering, which is more accurately characterized as a mental *process* or chain of mental *events* spread out over time (just as a particular movie theater projection is a physical process or chain of physical events). A "snapshot" of one's overall mental condition at any particular time is a mental *state*. A mental *property* is a specific aspect of mental states (e.g., being experiential, or exhibiting intentionality). Technically speaking, on the dependence thesis memories are *realized* or *instantiated* by functioning brains, just as a particular implementation of software is realized or instantiated by some running hardware. One could eliminate such figurative language altogether by talking about the *necessary conditions* for possessing mental traits—as my definition of the dependence thesis does—with the understanding that a "trait" is not a spatiotemporal object, either! The additional comment that "thinking of the mind as an entity—a piece of ontological furniture—surreptitiously tilts the discussion in favor of the anti-survivalist" is odd, too, since traditional Cartesian dualists tend to be the ones who reify minds as irreducible simple substances, and few dependence thesis proponents are Cartesians. In any case, neither survivalists nor mortalists *need* reify the mind, making the whole discussion a giant **red herring**.

[27] Contra my interlocutors, Karl Lashley's futile search for localized memory engrams does not constitute evidence against the dependence thesis since one cannot either deductively or inductively *derive* the existence of localized engrams from the dependence thesis simpliciter. Their existence is thus *not* a prediction of the dependence thesis—at least when it has not been amended with additional auxiliary assumptions that might generate that prediction. Regardless of whether mental functions like memories can be neurologically destroyed (or suppressed) altogether, or merely neurologically degraded, either evidences their instantiation by functioning brains. This is clear because both functional disruptions are paralleled in the disruption of computational processes (running software) by damage to the underlying hardware that everyone grants instantiates running software. Indeed, we would *expect* mere degradation with hardware using parallel distributed processing (Parks et al., 1991). Science reporter Roger Lewin also notes the role of neuroplasticity when brains *are given time to recover* in such examples: "Gross surgical lesions in rat brains are known to inflict severe functional disruption, but if the same damage is done bit by bit over a long period of time, the dysfunction can be minimal. Just as the rat brains appear to cope with a stepwise reduction of available hardware, so too do the human brains in some cases of hydrocephalus" (1980, p. 1233). And however sound Braude (2006)'s critique of trace theories of memory, its "hardware-independent" upshot also makes the critique hardware-irrelevant, for if it suggested that functioning brains could not instantiate memories, then by parity of reasoning, astral bodies or nonphysical substances could not instantiate memories, either. Perhaps trace theories of memory *are* hopeless, but if so, there must nevertheless be *some* substrate-independent account of how memories are laid down, accessed, altered, degraded, and eliminated/suppressed as long as human minds *have* memories.

[28] The bluster that "Augustine's critique could have been written in the 1950s and 60s" was a particularly nice touch for researchers leaning exclusively on Lashley's neuroscientific research from the 1920s.

REFERENCES

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Anomalistics and Frontier Science

COMMENTARY

Beyond the BICS Essay Contest: Envisioning a More Rigorous Preregistered Survival Study

Etienne P. LeBel

Keith Augustine

Adam J. Rock

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HIGHLIGHTS

Working from common assumptions in parapsychology, a group of survival skeptics and an agnostic jointly propose an experiment that is more rigorous than typically used. A successful result here would give compelling evidence consistent with "life after death."

ABSTRACT

Prior experimental studies of anomalous information reception (AIR) have been touted as strong evidence for postmortem survival of consciousness yet are plagued by several methodological weaknesses that preclude clear evidence of positive results. The present team provides an adversarial collaboration to identify and compensate for the major limitations of these previous approaches. We outline a more rigorous preregistered study design that eliminates or minimizes researcher bias in (a) data cleaning and (b) statistical analysis. Obtaining positive results with our recommended design would arguably yield data that skeptics and sympathetic researchers would agree is more clearly interpretable and offers stronger support for a survivalist interpretation. However, this proposed study is not intended to be definitive but rather only a next step in a research program that aims to improve on earlier published efforts. It would also admittedly be time-consuming and expensive to implement, as well as raise ethical considerations in utilizing vulnerable research populations. However, these costs are required to achieve the rigor necessary to advance scientific knowledge in survival research.

KEYWORDS

Anomalous information reception; content–source problem; discarnate; living agent psi; mediumship; postmortem survival

Introduction

Although the preceding exchange in this special subsection of the *Journal* (Augustine, 2022a, 2022b; Braude et al., 2022) has highlighted the differences between skeptics and proponents of discarnate personal survival, there is much more in common between us that often goes unsaid, such as a common respect for sound reasoning and for investigating matters empirically whenever possible. We also agree that this topic warrants further empirical investigation, and of a quality superior to that found in the extant survival literature. While we could further delineate our similarities and differences, a more fruitful avenue for research is to collaborate on a design for an "ideal" prospective test of *potential* survival that, if successful and replicable, would complement and corroborate previous attempts at rigorous experimental survival research.

Working with Braude et al.'s (2022) team of survival proponents would have been optimal, but given time and logistical constraints, we have alternatively joined forces with the last author, who has published several methodological papers in this domain from an agnostic perspective (e.g., Jamieson & Rock, 2014; Rock & Storm, 2015). By developing some of the proponents' own published proposals, we have agreed on an experimental design that would provide substantiating evidence consistent with an

anomalous effect by shielding any attainable replicable positive results, as much as feasibly possible, from normal or conventional explanations. Such explanations run the gamut from simple cueing to researcher degrees of freedom or p-hacking, i.e., researchers inadvertently or deliberately collecting or selecting data or analyses until nonsignificant results are rendered statistically significant (Head et al., 2015).

Given the difficulties attending attempts to discern the *source* of any putative psi effects that might emerge from implementing the present design, our proposal aims only to test for effects consistent with Anomalous Information Reception (AIR). Whether or not any potential evidence of AIR obtained through it would also constitute evidence of discarnate personal survival is an independent question requiring a separate discussion (see the Appendix). This protocol is simply a pragmatic first step, and if it becomes warranted, a catalyst for further experimental survival-related research. Moreover, while the protocol outlined here does not improve upon every aspect of previous AIR research designs, if successful, it would provide strong convergent validity[1] of an AIR effect. Indeed, using mixed or multiple methods to study a phenomenon is proposed to produce results that are more robust and compelling than single-method studies (see e.g., Morse, 2003).

This paper thus does not close the exchanges between Augustine (2022a, 2022b) and Braude et al. (2022) in this special subsection with a polite but feeble statement to the effect that the disputants simply "agree to disagree." Rather, we present an adversarial collaboration that favors practical next steps over endless conceptual debates. Despite our different takes on the current state of the survival evidence, the present team of survival skeptics [EL & KA] and survival agnostics [AJR] concur that (a) the presence of anomalous/paranormal information or influence among phenomena potentially indicative of discarnate personal survival is amenable to experimental testing and (b) such testing must meet the most rigorous evidential standards possible so that any phenomena potentially indicative of discarnate personal survival cannot be reasonably explained in exclusively conventional or normal terms.

Accordingly, we outline below the parameters of a proposed series of studies that, if successful and replicable, would satisfy the present collaborators of the presence of an effect consistent with AIR among phenomena potentially indicative of discarnate personal survival.[2] Thus our recommended research design aims to fulfill the spirit of the Bigelow Institute for Consciousness Studies (BICS) essay competition by offering one approach to seeking what would provide significantly better evidence than that currently heralded for the idea of postmortem survival of human consciousness.

Laying a Working Foundation

There is growing interest in survival research to use the more rigorous standards that have emerged from the global open science movement.[3] One such approach is preregistration, which is a method to increase research transparency by documenting research decisions on a public, third-party repository prior to data collection. If done correctly, preregistration can prevent the cherry-picking of analyses and/or data transformations/cleaning choices that yield more desirable results, a behavior known as "p-hacking" (Simmons et al., 2020; see also Moore, 2016). Given that such behaviors are often done *unintentionally*, we prefer framing the key benefit of preregistration as reducing the number of "researcher degrees of freedom" that could unintentionally or intentionally be exploited to achieve spurious positive—or more desirable—results.

Public preregistration, however, can only achieve such goals if a preregistered protocol contains sufficient details regarding how a study will be conducted and how data will be cleaned and statistically analyzed (Claesen et al., 2021).[4] But even with preregistration, one must be careful because researchers could still cherry-pick evidence from a specific *subset* of preregistered studies within a larger set of preregistered studies (see Laitin et al., 2021). To avoid this problem, we need:

1. All involved researchers' pledge to abide by principles of research integrity/honesty (e.g., the Netherlands Code of Conduct for Research Integrity [NOW, 2018]).
2. All studies meet minimum transparency standards (e.g., disclosing all financial and nonfinancial conflicts of interest, meeting methodological reporting standards, open materials, and open data) so that proper scrutiny of results can be independently checked.

Even then, obtaining a preregistered (and independently) replicable effect is just a minimum requirement for the *perpetual activity* of investigating the generalizability and validity of a (replicable) effect (LeBel et al., 2017; Simmons et al., 2020). Such activities help to establish that one is dealing with a genuine phenomenon rather than just measurement artifacts or method-related artifacts (for example, replicable effects across improved measurement techniques, domains, and approaches to studying a phenomenon). In contrast, if a small replicable effect is observed in only one domain/approach (out of dozens/hundreds—e.g., the 10 proposed survival experiments listed in Delorme et al. (2021*, pp. 26–28)—then this would cast doubt on the validity of the supposed anomalous/paranormal nature of the broader phenomena ostensibly favoring discarnate personal survival.[5] In these senses, research-

ers must avoid a "checklist" approach to science—i.e., the notion that one's study meets all of the checkboxes, and hence is trustworthy—which is inconsistent with the spirit of the scientific method. That is, perpetual questioning and ruthless scrutiny, constantly trying to rule out alternative explanations, improving the precision of empirical measurements, and careful calibration of beliefs based on new credible evidence must be maintained.

Indeed, the purpose of the scientific method is to avoid fooling others and ourselves into believing that something is true when it is in fact false (e.g., false positives/Type I errors), or into believing that something is false when it is in fact true (e.g., false negatives/Type II errors). This is achieved via science's nonoptional requirement of sufficient transparency, which maximizes the likelihood of proving ourselves wrong if we are in fact wrong (i.e., ensuring scientific falsifiability; LeBel et al., 2017). We are the easiest people to fool (Feynman, 1974) because of several cognitive (Nickerson, 1998) and motivational (Kunda, 1990) biases, which can be substantially amplified by outside financial interests and the hyper-competitiveness and perverse incentives found in academia (Edwards & Roy, 2017).

Such scientific principles require that hypotheses are tested and reported with sufficient transparency so that independent researchers can thoroughly and ruthlessly scrutinize the evidence in support of a specific claim. Key dimensions of transparency include:

- *Conflict-of-interest disclosures*: Disclosing all financial and nonfinancial conflicts of interests, including all funding sources and the role of the sponsor in a study design (e.g., LeBel, 2021, §3).
- Sufficient details regarding the experimental setup and materials (*open materials*), so that independent researchers have enough information to find flaws or limitations of the study design and conduct diagnostic independent replications (Glasziou & Chalmers, 2017; LeBel et al., 2017) .
- Access to the data (at least a minimal dataset; *open data*), so that researchers can rule out errors and check for fraud (both data falsification and fabrication), but also verify the analytic reproducibility and analytic robustness of reported results (Steegen et al., 2016).
- *Public preregistration*, which minimizes the multitude of "researcher degrees of freedom" that may have been unintentionally or intentionally exploited to get positive results (e.g., analytic and design flexibility, or selective file-drawering/cherry-picking of "failed" studies) (Simmons et al., 2020; see also Moore, 2016).

Higher levels of transparency allow for more thorough scrutiny. The more scrutiny a reported effect survives—including replication and reanalysis attempts—the better. Then, and only then, can evidence for such an effect can be temporarily considered credible/trustworthy, proportional to the amount and nature of the scrutiny that a reported effect has survived (LeBel et al., 2018; Meehl, 1967, 1978).

Choice of "Survival Effect"

Given the above preamble, we strive here to develop a preregistered protocol of an important ostensible survival effect. We aimed to choose an effect whose study design possesses as many methodological strengths from previous studies as possible, while minimizing the number of design weaknesses of previous approaches. For preregistered study designs, it is also important to choose a design that minimizes unforeseen ambiguities or decisions in data cleaning and statistical analyses.

Mediumship studies appear to be a good candidate in these respects (compared to other approaches exploring near-death experiences or cases of the reincarnation type, which appear much less amenable to testing on demand). For example, Delorme et al.'s (2021*) proposed mediumship experiment is a good start:

> Ten people in hospice would be recruited who agreed to contact one or more of five mediums after they passed away. None of those mediums would be aware of this experiment. After each person died, they would request that the mediums contacted the researchers within the next 30 days. Positive results would include at least five mediums contacting the researchers within 30 days for each deceased person, giving that person's name, and saying that the deceased person told them to contact the researchers. (2021*, p. 27)

With some amendments, this is an ideal basic study design for several reasons: (1) its ease of implementation; (2) the ease of empirical measurement within it; (3) its adaptability to the requirements of preregistration, enhancing interpretability/decreasing its openness to spurious statistically significant results; (4) its representativeness of survival researchers' (and BICS contest participants') assessment of which phenomena provide the best extant evidence of discarnate personal survival; and (5) how it serves as a check on the justifiability of the high confidence that survival researchers place on the establishment of the existence of AIR by mediumship research that has been conducted to date.

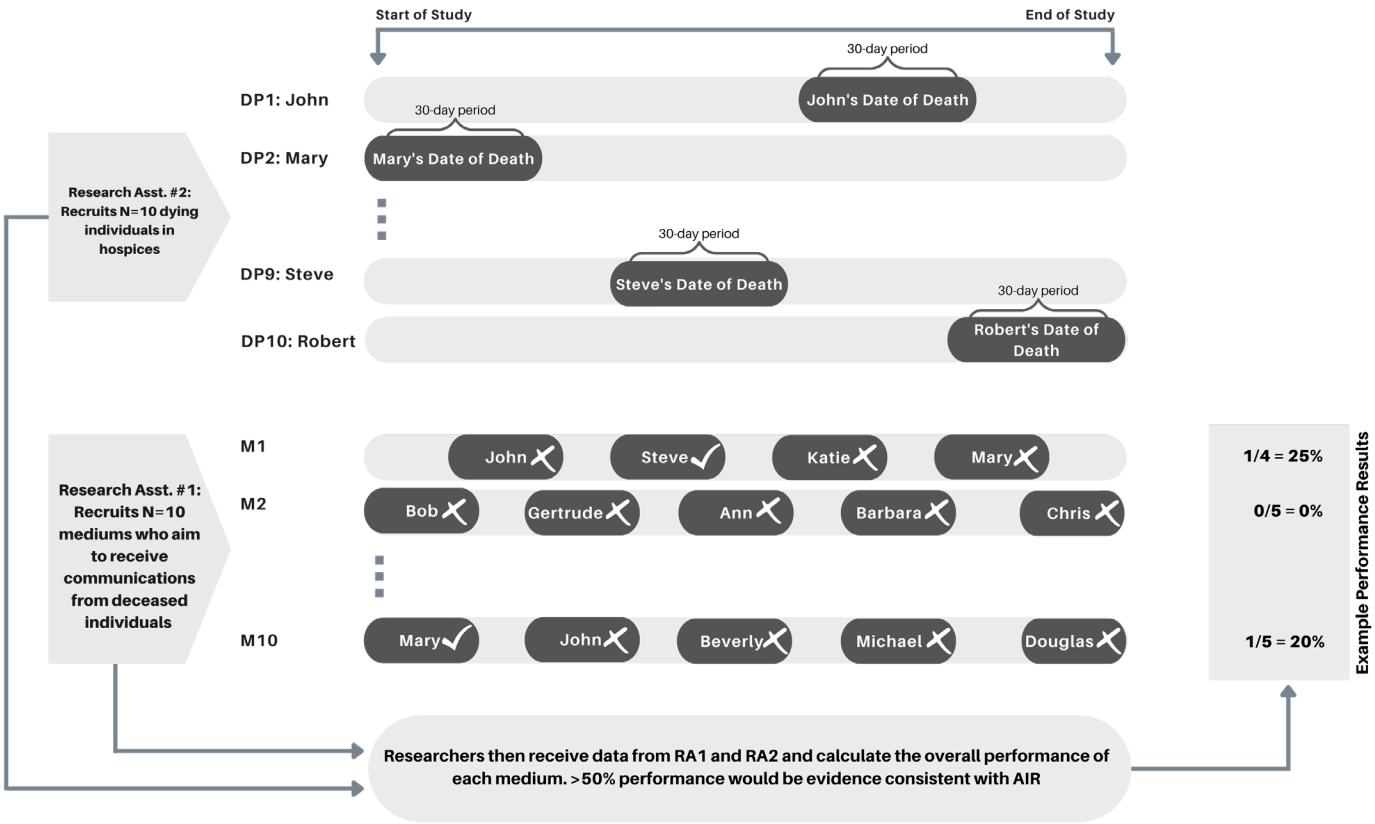
The nascent concept for this design was taken from one of the primary targets of Augustine (2022a) in the spir-

it of collaboration between the participants of the preceding exchange, and between survival skeptics and proponents as a whole. Among the sources of ostensible survival evidence, mental mediumship was selected for multiple reasons. *First*, Julie Beischel has made recent claims—which arguably surpass what the evidence supports[6]—to have proven AIR beyond a reasonable doubt and effectively settled the issue scientifically within both the survival literature and the BICS essay competition (Beischel, 2021*). *Second*, two of the three primary targets (Braude, 2021*; Delorme et al., 2021*) of Augustine's (2022a) critique independently agreed that mental mediumship provided the best evidence for personal survival compared to other sources of ostensible survival evidence. *Third*, while the remaining primary target (Nahm, 2021*) of Augustine (2022a) reserved that distinction instead for "cases of the reincarnation type" (CORT), then-forthcoming survival research cast significant doubt on that assessment (Sudduth, 2021). *Fourth*, that research has underscored what Braude has since dubbed the *problem of investigative intricacy* for CORT in reaction to a draft of Sudduth (2021), showing that mental mediumship is comparatively much easier to investigate (Braude, 2021*, pp. 31–34). *Fifth*, those two sources are widely regarded as the two best sources of ostensible survival evidence among psychical researchers as a whole. *Finally*, both sources are more suggestive of both *discarnate* and *personal* survival than other potential sources of ostensible survival evidence[7], where even ideal evidence would not necessarily suggest the persistence of anything *after* death, or anything that retains individual or personal consciousness.

The Recommended Preregistered Protocol

Given that mediumship research into AIR is generally met with skepticism, a preregistered study design is ideal since it maximizes the rigor of such research. Implementing the following study design protocol in Figure 1 (adapted from Delorme et al., 2021*, p. 27) could provide a rigorous and falsifiable empirical test of mediumistic AIR. The protocol aims to be as clear, specific, and detailed as possible, following recommendations by Simmons et al. (2020) and Claesen et al. (2021)[8]:

1. Research assistant (RA) #1 recruits ten[9] (10) qualified mediums,[10] who are instructed to contact the



Note: DP=deceased person, M=medium, RA = research assistant , AIR = anomalous information reception

Figure 1. Visual diagram of the proposed preregistered survival study (Courtesy: Beth M. Houran).

RA within 30 days of receiving the putative deceased's communications with the name of any deceased persons who ostensibly communicates with them.

1.1. Mediums and RA #1 are blinded to the identity and date of death of the dying/deceased individuals (to prevent bias in classifying correctness of the name of deceased persons as reported by the mediums).

2. A different RA #2 recruits 10 dying individuals in hospices, instructing them to "contact" one or more of five mediums (of the 10 recruited in step 1 above) after they've passed away (showing them just the name and profile picture of each medium). To test for sheep/goat or psi-conductive/psi-inhibitory participant effects (Storm & Tressoldi, 2017), the *Australian Sheep-Goat Scale* (ASGS) (Thalbourne, 1995; Lange & Thalbourne, 2002) will be administered to the individuals in hospices.
   2.1. RA #2 ensures that each dying individual has a unique name.
   2.2. RA #2 ensures that the dying person agrees to provide RA #2 with the contact information of a loved one (so that RA #2 can record the date of death of the dying person).
   2.3. Dying individuals are blinded from the mediums.
   2.4. As an additional control, the two RAs are blinded from each other.
3. After the death of each dying individual (as notified by the designated loved one to RA #2), RA #2 waits 30 days to see if any of the 5 mediums actually contact the experimenter with the correct name of the deceased person.
   3.1. Specific instructions to deal with edge/ambiguous cases: Suppose a medium reports to RA #2 that she received a communication from "John," when in fact the deceased person's name is "Jean." A solution to such ambiguous cases is to be generous in the coding (e.g., "John" and "Johnny" would count as correct; "Rob," "Robert," or "Bob" would count as correct). Alternatively, or in addition, two independent coders could be used, and only names that the two coders agree upon would be used for the primary/secondary analyses.
   3.2. As an additional control, the two coders are blinded from each other, and the two RAs are blinded from the identities of the two coders (and vice versa). This level of blinding requires a fifth member of the research team. The fifth member would be the only person who knows the identities of the other four researchers and works with each of those four researchers directly. Furthermore, to test for sheep/goat or psi-conductive/psi-inhibitory experimenter effects (Parker & Millar, 2014), the ASGS will also be administered to all members of the research team.
4. After all persons have died, researchers tabulate the data (as received independently from RA #1 and RA #2) and calculate the overall performance of each medium in terms of the correctness of the deceased person's name *and* within the correct time period (i.e., *not* prior to a person's death, but also *not after* the 30-day period).
   4.1. A medium needs >50% performance to constitute evidence consistent with AIR (a generous threshold).
   4.2. Secondary analysis: >50% average performance across all 10 mediums could be considered even stronger evidence consistent with AIR.

Considerations and Caveats

Strengths of Such Protocol:

- **The lack of the need for sitters**. The inclusion of sitters would require an additional level of blinding, may provide an additional source of psi for mediums (i.e., the medium could use LAP to telepathically scan the mind of the sitter rather than communicate with a putative discarnate), may produce sheep/goat sitter effects that would need to be tested, and may encourage drop-in communicators.
- **The lack of the need for time-consuming and difficult codings** of subjective interpretations of readings from mediums (though still a minor issue with edge cases of deceased persons' names—see above).
- **The research design is potentially scalable** across investigators or laboratories for independent replications.

Challenges with Such Protocol:

- **Administrative challenges**: This design would re-

quire ample funding, and would have to successfully address the complexities and sensitivities with Institutional Review Board (IRB) approval of end-of-life and palliative care research, particularly with vulnerable populations (see, e.g., Abernethy et al., 2014; IRB Advisor, 2005).

- **Study duration**: Such a study may take over a year to complete, given the length of time required for all of the participating hospice patients to die.
- **Ecological validity/mundane realism issue**: Arguably, a deceased person may be more inclined/able to communicate with a loved one rather than an unrelated medium whom the deceased has never met (i.e., there are suboptimal conditions in the relationship between the deceased persons and the mediums).
    - But if loved ones are used, then such loved ones cannot be sufficiently "trained" to receive the deceased's communications (though one might try to provide each loved one with some sort of basic training).
- **Mediums may just submit common names**: One issue that may need to be accounted for is that mediums might actively search for, or subconsciously infer, the most common names for elderly populations in the United States and simply submit such names to the RA.
    - But given the blinding, mediums will *not* know the location/state of the hospices, so this complication is unlikely to help them.
    - "Qualified" mediums are presumed to be ethical and professional, and so should not attempt such trickery, but a design that avoids requiring such an assumption is preferred.
- **Issues that survival proponents may have**:
    - It could be argued that this design requires suboptimal conditions or context in which the medium operates.
    - Likewise, the protocol could potentially induce or involve suboptimal motivation of the medium(s).
    - There are certainly timing-related issues to be considered: 30 days might be deemed too narrow of a window in which to receive ostensible communications from putative discarnates.
    - And most importantly, parapsychologists are expected to argue that the proposed design suffers from the so-called "content–source problem," whereby hypothesized psi processes (e.g., precognition or telepathy) cannot be ruled out as alternative explanations for positive evidence consistent with an AIR effect (for an overview and discussion, see Appendix). This issue puts "the cart before the horse," as arguably no credible and independently replicable evidence exists for such psi effects or confounds. But assuming that credible and replicable evidence is established via a protocol such as the one proposed here, then, and only then, would it logically follow that a preregistered design should aim to rule out putative psi effects as alternative explanations to positive evidence consistent with an AIR effect.

CONCLUDING THOUGHTS

Our preregistered experimental design for investigating AIR among mediums is intended to provide a more rigorous test of the AIR hypothesis than has been conducted to date. This design is more robust because it mitigates researcher bias in (1) data cleaning and (2) the analysis of any findings. The broader goal is to provide the means by which mental-mediumship researchers can improve the quality of the ostensible evidence for the postmortem survival of human consciousness should discarnate personal survival occur.

That said, one should *not* view the results of this or a related study as a definitive *experimentum crucis* test of AIR. Indeed, any such "checklist" approach to science should be avoided at all costs. Progress in this field can only be made by fully embracing the spirit of the scientific process: subjecting hypotheses to perpetual questioning and ruthless scrutiny, constantly striving to rule out alternative explanations, improving the precision of empirical measurements, and carefully calibrating conclusions based on new, credible, and reliable information.

Obtaining stronger evidence consistent with AIR would not, of course, necessarily explain that evidence in terms of living agent psi, discarnate personal survival, or any other parapsychological hypothesis. Nevertheless, it would rationally oblige all parties to acknowledge that a replicable AIR effect might be in need of a new—and potentially disruptive—explanation in other than known conventional terms. The repeated failure to obtain such evidence, on the other hand, would suggest yet another research avenue to avoid pursuing in the future. Of course, to our thinking, the possibility of assembling a growing body

of null results by itself might be telling and should perhaps factor into the scientific calculus.

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NOTES

[1] In scientific measurement, convergent validity refers to the degree to which different tests ostensibly measure the same or similar constructs.

[2] Multiple studies are necessary to distinguish between a method-related artifact and a veritable substantive phenomenon.

[3] Extant quantitative research often fails to meet such standards, but it does not follow from that failure that less rigorous quantitative research is thereby adequate.

[4] Not all decisions can be prespecified, but archived/time-stamped "standard operating procedures" can be within a preregistered protocol that can aid in these regards (Lin & Green, 2016).

[5] Similarly, if discarnate personal survival was responsible for any replicable positive results obtained from the implementation of this design, then such results should replicate for researchers with a wide range of views on discarnate personal survival (while statistically controlling for "sheep–goat" effects and other potentially "psi-modifying" variables by partialling them out as covariates).

[6] For one, the meta-analytic results regarding mediums' purported AIR are mixed (see Rock et al., 2021, and Sarraf et al., 2021).

[7] With perhaps the exception of veridical near-death experiences (NDEs) that occurred under controlled conditions and whose veridical "out-of-body" elements can be definitively timestamped to a period of complete brain inactivity—though finding and verifying the existence of such NDEs experimentally may not be feasible in practice.

[8] In the event of an actual empirical test, the protocol may have to be adjusted in minor ways to accommodate any specific geographical or cultural realities.

[9] While a larger number could provide a higher-powered test at the group level, in this case the primary analysis is of the individual-level analysis of each medium. Nevertheless, a secondary dependent variable analysis could be performed at the aggregate/group level. Either way, the protocol should include sufficiently high statistical power, e.g., >80%, to detect a small AIR effect size (e.g., Cohen's d = .20; Cohen, 1988).

[10] The "extensive screening of prospective mediums helps ensure a subject population that is reliable, skilled, trained, dedicated, ethical, and professional" (Beischel & Schwartz, 2007, p. 11). Extant Windbridge Certified Research Mediums (WRC, 2022) might be recruited for this purpose.

[11] Parts of this sub-section have been adapted from Rock and Storm (2015) with feedback from Augustine, and are used here with permission from the *Journal*.

[12] However, we argue that if no capacity limits are stipulated for psi, then both LAP and discarnate personal survival are untestable relative to each other. Hypothetically, whatever the deceased can do, the living can do, and vice versa. Consequently, there is no scientific way to operationally distinguish LAP from survival unless the dead possess psi capacities that the living do not.

[13] It may be argued that once one posits the existence of psi, the number of sources of psi are irrelevant with regard to parsimony. For instance, once one posits the existence of stars, it is not more parsimonious to say that there is only one star rather than many trillions. The leading concept of parsimony in the philosophy of science and epistemology pertains to how many untestable auxiliary assumptions one must posit to account for the data, which speaks more to the number of kinds of things that exist than how many instances of that kind are realized. Consequently, the number of sources of psi may be regarded as irrelevant in this context. Otherwise, all psi between living persons, and psi between the living and the deceased, could be rejected as unparsimonious since there are many instances of living persons and many instances of deceased persons to potentially access, but only one instance of a psychic reservoir.

[14] We argue that all the evidence is equally compatible with all of these constructs (e.g., the psychic reservoir, super-psi, LAP). For example, one can interpret clairvoyance as psychic-reservoir-accessing, or remote viewing inanimate signatures, or subconscious telepathy with living or deceased persons, and there is perhaps no method to distinguish between them, except when the living or deceased lack access to information.

[15] If the psychic reservoir exists, then it does not necessarily follow that is has an organizing principle. Alternatively, the psychic reservoir itself could be an organizing principle, and one that (like Plato's realm of forms) is devoid of mentality.

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APPENDIX

Overview of the "Content–Source" Problem in Survival Research[11]

We reiterate that our proposed research design is expected to comprise merely the initial step of an iterative research process. The existence of a reliable and well-controlled AIR effect that is consistent with the survival hypothesis is therefore the fundamental question that our protocol addresses. This issue arguably must be settled first to warrant additional studies of mediating, moderating, or causal variables. Given positive results from our protocol, a second stage of research would likely require innovative research designs to address important nuances or complexities that can obscure a clear interpretation for any observed effects.

Beyond the various conventional explanations for mediumistic or otherwise anomalous "entity" communications (see, e.g., Caputo et al., 2021; Cunningham, 2012; O'Keeffe & Wiseman, 2005), parapsychologists have used four models to explain ostensible AIR by claimant mediums: (a) the *survival* hypothesis, (b) the *living agent psi* (LAP) hypothesis, (c) the *super-ESP* (also *super-psi*) hypothesis, and (d) the *psychic reservoir* (also *cosmic reservoir* and *cosmic psychic reservoir*) hypothesis (Rock, 2014). However, we note that (c) is a more specific kind of (b), and arguably so is (d) (since living agents would need to access said reservoir, i.e., use clairvoyance). Readers should see Augustine (2022b, pp. 420–421) for a defense of the distinction between (b) and (c). The epistemological difficulties associated with identifying the source of veridical information from mediums is called the *source-of-psi* problem. In the interests of completeness and balance, we will summarize each of these ideas below. Note that our discussion of these concepts does not imply any endorsement of their scientific validity.

According to robust versions of the survival hypothesis (per Sudduth, 2016, pp. 16–17), "the existence of discarnate persons provides the best explanation of the data associated with physical and mental mediumship" (Sudduth, 2009, p. 167). One limitation of the survival model is that it is potentially less parsimonious than non-survivalist explanations since it implies that: (a) postmortem consciousness exists, suggestive of an additional dimension or dimensions to Einsteinian space–time; and (b) such entities are distinct, ontologically, from the brains of incarnate minds. Perhaps the "strength" or ubiquity of the belief in survival resides primarily in its historical, religious, and phenomenological origins.

Those who argue that empirical survivalists have thus far failed to make a probabilistic case in favor of the survival hypothesis (e.g., Sudduth 2014, 2016) often employ LAP and the super-ESP hypothesis as alternative, and possibly superior, explanations of survival-related data. Simply put, LAP denotes psi [extrasensory perception (ESP) and psychokinesis (PK)] that originates either consciously or unconsciously from living biological entities. In contrast, super-ESP refers to:

> The hypothesis that since there are no known limits to the scope of psi, extrasensory perception on the part of the living could in principle be used to produce such complex phenomena as ostensible spirit communication, and that therefore the spirit hypothesis is unnecessary and unparsimonious. (Thalbourne, 2003, p. 121)

Consequently, the possibility of super-psi (psi without any known limits) may be interpreted as an extension, conceptually, of the methodological challenges introduced by invoking LAP. While some scholars conceptualize super-psi as LAP "pushed to its limits" (Gauld, 1982, p. 15), others (e.g., Braude, 2014; Sudduth, 2014) do not seem to consider super-psi an *extension of* LAP, but rather recommend that the term "super-psi hypothesis" be supplanted "with the more accurate and neutral 'living agent psi hypothesis'" (Braude, 2014) since any information or influence mediated by psi would be *equally* available to either living or deceased persons.

Braude (2003) further suggested that, according to the literature, the LAP hypothesis appears to consist of two versions. First, the *multiple-process* hypothesis regards LAP "as an organized collection of refined psychic tasks" (Braude, 2003, p. 11). It posits the medium's purported ability to respond successfully to the task complexity in-

volved in putative discarnate communication (e.g., ESP that "reads" the thoughts of the sitter and other salient individuals, or ESP that "intuits" pertinent physical objects and/or events). Second, the *magic-wand* hypothesis asserts that even the most complex ESP occurs simply as a result of the wishes or desires of percipients. Consequently, this hypothesis disregards (a) task complexity and (b) effort exerted by the percipient (Braude, 2003). The central quandary posed by the LAP hypothesis can be described as follows:

> If a piece of putative evidence for survival is to be of use, it must be verifiable—we must be able to check by consulting records or surviving friends that the information given by the ostensible communicator is correct. But if the sources for checking are extant, they might in theory be telepathically or clairvoyantly accessible to the medium or percipient. Since we do not know the limits of ESP we can never say for certain that ESP of the extraordinary extent that would be necessary . . . is actually impossible. (Gauld, 1982, p. 15)

Thus, it is, perhaps, not surprising that it has been contended that the LAP hypothesis is not empirically testable because "it postulates an omniscient and omnipotent capacity that cannot be falsified by the scientific method" (Martinez-Taboas, 1983, p. 58). However, this would only be true of super-psi or unlimited-capacity psi. A limited capacity LAP hypothesis is eminently testable. Recall that LAP is just psi [extrasensory perception (ESP) and psychokinesis (PK)] that originates either consciously or unconsciously from *living* biological entities. Nevertheless, while we may not know the limits of psi (e.g., Braude, 2003), it does not necessarily follow that psi is unlimited. Still, if the case for survival is dependent on eliminating some subset or subsets of counter-explanations, then the former claim is sufficient to generate problems for survivalist interpretations of ostensible survival evidence.

Scholars who argue that the survival hypothesis is untestable often appeal to the LAP hypothesis.[12] For instance, Irwin (2002) reviewed séance phenomena, OBEs, NDEs, reincarnation experiences, and apparitional and poltergeist experiences, concluding that "the operation of such processes" as LAP are "impossible to exclude" and, thus, the aforesaid phenomena "cannot be conclusive for the survival hypothesis" (p. 20). However, others have argued that the LAP hypothesis has less explanatory power than the survival hypothesis:

> I accept that the evidence from mediumistic communications for survival of consciousness is not conclusive; but it is the only viable alternative to [a LAP explanation] which for most informed observers would be considered less persuasive. (Keen, 2003, p. 38)

Unfortunately, Keen made little attempt to justify why he regarded the survival hypothesis as a superior explanation to LAP relative to mediumistic "communication." Keen (2003) briefly referred to three cases, contending that all are ". . . in theory susceptible to an explanation which confines a psychic faculty to the living mind, but only by postulating the most improbable, speculative and evidentially unsupported extensions of psi" (p. 38). However, he neglected to expand on these "most improbable, speculative and evidentially unsupported extensions of psi" (p. 38), and did not defend why survival was more probable, less speculative, and evidentially superior to the LAP explanation.

Braude (2003) aimed to progress the LAP versus survival stalemate using his *argument from crippling complexity* (pp. 86–95). Here, the crippling complexity generated by the *psychic traffic* of the totality of incarnate minds might function as an impediment to LAP during the interaction between mediums and sitters. If correct, this contention might provide indirect support for the survival hypothesis. However, Braude found no persuasive reason to conclude that the complexity of the hypothesized mediumship–sitter interaction's underlying causal nexus is fundamentally different from the mediumship–discarnate interaction's causal nexus:

> . . . it should be as difficult for communicator and medium to create (say) a consistent, long-term impersonation as it would be for the medium to accomplish the same thing through clairvoyance and telepathy with the living. Both tasks would encounter inevitable obstacles from the bustling underlying nexus of psychic activity, and that underlying causal network would have to include attempts by the deceased to gather information and influence the living. (Braude, 2003, p. 93)

Consequently, according to Braude (2003), the argument from crippling complexity appears to apply equally to the LAP and discarnate psi. Braude (2003) nevertheless contended that LAP interpretations have less parsimony than survivalist ones because they posit *multiple* sources of information (e.g., the medium telepathically scanning the mind of sitters, other living people, or discarnates, or clairvoyantly accessing pertinent objects such as photos). In contrast, survivalist interpretations of evidence for psi posit a *single* source (i.e., one discarnate). Consequently,

one might grant "an explanatory edge to the survivalist, at least on the grounds of parsimony" (Braude, 2003, p. 93).[13] However, as Storm (2014) highlighted, ". . . the human mental agility implied in one theory [super-psi] is as equally challenging to the emotions and the intellect as the multidimensionality implied in the other [survival]" (pp. 1–2).

An additional alternative to the survival hypothesis is the psychic reservoir hypothesis, which states that "all information since the beginning of time is stored somehow and somewhere in the universe and mediums are accessing that cosmic store rather than communicating with the deceased" (Beischel & Rock 2009, p. 72). Fontana (2005) argued that this hypothesis is weaker compared to the LAP hypothesis because, while there arguably exists experimental evidence that may be interpreted as supportive of clairvoyance and telepathy (e.g., Radin, 1997), there is no scientific evidence that is supportive of a cosmic store of information.[14] Furthermore, Fontana (2005) argued that, in addition to the fact that the hypothesis cannot be falsified, there are several practical problems with this hypothesis. For instance, "What is the organizing principle or intelligence behind the cosmic psychic reservoir?" (p. 114).[15] However, this hypothesis is perhaps useful for the further delineation of *agentive* (e.g., the sitter's mind) versus *nonagentive* (e.g., a cosmic store of information) sources of AIR.

Taken altogether, an accurate interpretation of reliable AIR effects could prove to be an intractable problem even from a sympathetic, parapsychological perspective. Thus, this adversarial collaboration and the prior exchanges about evidence and outlook (Augustine, 2022a, 2022b; Braude et al., 2022) perhaps ultimately unite on two important conclusions, namely that (a) the quest for a conclusive, unambiguous experiment or study to confirm discarnate personal survival might be ill-conceived from the start, or else that (b) researchers from both the survivalist and mortalist camps have a long way to go to settle the matter scientifically once and for all. Perhaps an iterative process of developing and implementing rigorous and innovative mediumship-testing techniques, continuously updated in response to new evidence, will result in a meta-analytic database that indicates a convergence towards one source of psi over others (Jamieson & Rock, 2014).

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Anomalistics and Frontier Science

OBITUARY

For Brenda Dunne, Fond Memories and Deep Respect

Roger Nelson

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In early 1980, I answered a circumspect ad in the *Chronicle of Higher Education* looking for a "cognitive scientist interested in the lesser-known aspects of perception." After some correspondence, I traveled from northern Vermont to Princeton to interview for a job that would, as I learned, touch on truly rarefied aspects of consciousness. By "accident," I encountered Brenda walking down the hallway toward Bob Jahn's office, where I was headed for an interview, recognizing her though we had never met. It was quite a first impression—she was wearing a long flowing green dress and looked magical, and needless to say, obviously memorable. She was then and always a notable presence.

The Princeton Engineering Anomalies Research (PEAR) lab was taking shape in the basement of Princeton University's School of Engineering, and from the beginning it had an unusually human quality because Brenda saw how important being at ease would be for people willing to try our experiments. She made the lab comfortable and home-like, installing the great orange couch with all its stuffed animals in PEAR's living room, and *Comforto the Incredible* chairs to coddle our operators as they attempted improbable tasks like intending that our Random Event Generator (REG) should produce high (or low) numbers on demand, or attempting to add some order (negentropy) to the Random Mechanical Cascade (RMC) or the big and beautiful but randomly arhythmic Native American drum.

More important by far than the furniture was Brenda's presence in the lab. She was warm and genuinely interested in the people who came by, and many of them became long-time friends. Her easy confidence about the phenomena we studied was infectious, and that probably accounted for a large part of the success we had in demonstrating that the improbable could happen, and the impossible, too, though it might take longer.

Brenda was clear that our studies were of phenomena, not people, and she invited the folks we called operators to relax and have fun with the experiments. She set a tone of collaboration, and rather than telling people how to work their will on the REG, she asked what they thought and felt. Some of the lab's most instructive findings come from what they

had to say. Typically, our operators told us it was a matter of developing a relationship with the machine. "I began to feel loving connection." Then the scores would climb.

Princeton University has a thesis requirement for undergraduates, which led a number of students to choose to work in the PEAR lab designing and conducting experiments, helping with analysis, becoming friends of the lab. Again, Brenda's generosity of spirit promoted the best from them, and again they became friends who maintained their connection to PEAR over many years.

An even more striking version of Brenda's mother hen capacity came in the form of 4th grade classes that arranged visits to the PEAR lab. Brenda would give a brilliant, just-right introduction to science and how to learn from experiments, that I suspect those children, now grown, still remember. The kids played with some of our experiments (they especially liked the table top Robot—which they successfully commanded, more than any of our adult operators). Based on their experience, these 10-year-olds went to work designing their own experiments and later brought them in to show us all. It was an amazing episode to watch: Brenda teaching and inspiring young people to feel and understand the combination of creativity and care that is the core of good science.

Her magnetism brought people together, and her leadership led to the creation of the PEAR Tree, a network of people who wanted to keep the PEAR experience alive by networking, maintaining the connection of like minds interested in solving the puzzles of extended human consciousness. Similarly, Brenda and Bob created the International Consciousness Research Laboratories (ICRL) to spread the PEAR attitude and approach beyond the lab and the University. ICRL produced conferences and encouraged researchers from around the world to dig deeper, and to search for ways to understand human consciousness from a striking range of different perspectives. In the last few years a series of "meetup" gatherings has been bringing people together—even during the pandemic—to hear cutting edge work in the fields represented by PEAR and ICRL.

Brenda was one of the founders of the Society for Scientific Exploration, the SSE. She served for many years as the Education Officer. In that capacity she created the Young Investigators branch of SSE, and offered not only help and encouragement to young people (defined to include senior researchers as well as students) but Pizza at the lunchtime meetings during conferences. Those young people are now the mainstay of SSE, and are managing and expanding Brenda's legacy.

As the Laboratory Manager at PEAR, Brenda had a hand in all the research, helping to define the questions we wanted to ask, and contributing insight and energy to the work. She had already defined a new version of remote viewing research that we called Precognitive Remote Perception (PRP) to identify a protocol that had a percipient describing a remote location that would be visited by an agent in the near future. Brenda showed that there was an inverse relationship of the analytical complexity and effect size in PRP data, and she identified gender differences in the databases from most of our experiments. Etc.!

Bob and Brenda worked together on all levels of the PEAR proposition as what they might describe as a complementary pair, bringing the subjective and objective together to achieve deeper insight into theoretical models to help explain the anomalous alterations of probabilities revealed in rigorous experiments. Their book, *Margins of Reality*, presented a rich language of metaphors drawn from physics and philosophy that could credibly accommodate a consciousness linked to and part of the real world. They continued to develop the point that clear understanding of the complexity evident in both mainstream and anomalies research must depend on an integration of subjective and objective aspects.

We have lost a cherished colleague and friend, but the good news is that several things that were important to Brenda came about in the last year or so. She found a home for the PEAR lab at the Broughton Hall Estate in Yorkshire, UK. PEAR's favorite experiments, even the giant "Pinball" machine, are being rebuilt and restored, and the research will go on. She was awarded an honorary doctorate by Unity University. She resolved several private matters. Most importantly, she had received direct and plentiful appreciation from many sources for her contributions and her wisdom. At recent conferences Brenda stepped on stage as a remarkably effective spontaneous speaker able to tell mixed audiences how it all works. A wonderful display of her magic, bringing many life threads to resolution.

Brenda Dunne was an inspirational figure whose passion was to show that consciousness is creative and active, treating chaos and randomness as the raw material for building the world. She was a force of nature, who brought great personal charisma to the tasks she undertook. I think that for Brenda, those tasks all could be mastered by embracing love and connection as the matrix in which anything is possible. Her spirit will remain with us into the future as we continue the work and follow the paths she forged.

Journal of **Scientific Exploration**
Anomalistics and Frontier Science

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