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**Editorial Office:** *Journal of Scientific Exploration*, Society for Scientific Exploration, Kathleen E. Erickson, *JSE* Managing Editor, 151 Petaluma Blvd. So., #301, Petaluma, CA 94952 USA  
EricksonEditorial@gmail.com, 1-415-435-1604, (fax 1-707-559-5030)

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## JOURNAL OF SCIENTIFIC EXPLORATION

A Publication of the Society for Scientific Exploration

**AIMS AND SCOPE:** The *Journal of Scientific Exploration* publishes material consistent with the Society's mission: to provide a professional forum for critical discussion of topics that are for various reasons ignored or studied inadequately within mainstream science, and to promote improved understanding of social and intellectual factors that limit the scope of scientific inquiry. Topics of interest cover a wide spectrum, ranging from apparent anomalies in well-established disciplines to paradoxical phenomena that seem to belong to no established discipline, as well as philosophical issues about the connections among disciplines. The *Journal* publishes research articles, review articles, essays, commentaries, guest editorials, historical perspectives, obituaries, book reviews, and letters or commentaries pertaining to previously published material.



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## EDITORIAL

This issue of the JSE deals with a potent group of controversial topics. We begin with a research paper addressing the so-called source of psi problem—that is, the problem of identifying causal lines in parapsychological research. For example, in a classic PK lab experiment, there’s no way to determine conclusively why the experimental result occurred and who is responsible for that result. In the absence of something like a PK-meter, for all we know the experimenter (or an onlooker), rather than the official subject, might be the principal causal agent. The paper by Rock and Storm considers the vexing version of this problem as it arises in survival research, where the challenge is to determine whether anomalous information in a mediumistic setting comes from a deceased communicator or whether it’s entirely due to various kinds of psi among the living. Rock and Storm present a novel method which they believe may tilt the scales in favor of positing living-agent telepathy rather than communication involving a discarnate.

Next up is a report on a promising macro-PK subject in Buenos Aires. Of course, the topic of macro-PK is guaranteed to raise red flags for many. But as in many of the best cases, the subject in this study works in good light and under conditions in which fraud would be easily detected if it occurred. The subject’s phenomena are quite modest compared to the more exotic phenomena of physical mediumship. But this subject is not a medium, and he produces his effects independently of the confounding trappings of a typical mediumistic séance.

Next, an essay by Etzel Cardeña discusses “examples of blatant attempts to suppress and censor parapsychology research and those who are doing it.” This, of course, is a problem many JSE readers have encountered personally. Cardeña considers why the resistance to psi research is, not simply intense, but also disproportionate to what one would ordinarily expect in response to an empirical inquiry. And in an appendix to his essay, he presents an Editorial he wrote that was censored by the then-editors of the *Journal Frontiers in Human Neuroscience*.

But perhaps the main event for many will be the dialogue in this issue on the subject of climate change. My editorial predecessor Henry Bauer critiques a recent publication on the subject from the Royal Society and the U.S. National Academy of Sciences, arguing that what’s presented as the mainstream science supporting the case for human-caused climate change is instead a form of “dismissive dogmatism” that “distort[s] and misrepresent[s] evidence with the aim of entrenching a mainstream con-

sensus.” This is followed by challenges to Bauer from Peter Bancel and Andrew Foss, with the final word going to Henry.

I’m especially pleased that the JSE is able to publish this debate. The topic of climate change elicits very strong opposing views, often enough from people who (at best) are only casually familiar with the science and data supporting both sides of the debate. I’m sure our readers are an unusually informed lot on many topics, but one can’t be optimally informed on every matter of importance. So my hope is that readers will find this exchange at least to be enlightening and informative, even if their prior opinions remain unchanged.

—**STEPHEN E. BRAUDE**

## RESEARCH ARTICLE

### Testing Telepathy in the Medium/Proxy-Sitter Dyad: A Protocol Focusing on the Source-of-Psi Problem

**ADAM J. ROCK**

Psychology, School of Behavioural, Cognitive and Social Sciences,  
University of New England, New South Wales, Australia  
arock@une.edu.au

**LANCE STORM**

Brain and Cognition Centre, School of Psychology, University of Adelaide, South Australia, Australia

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**Abstract**—Numerous mediumship studies (e.g., Beischel & Schwartz 2007, Kelly & Arcangel 2011, Rock, Beischel, Boccuzzi, & Biuso 2014) have reported statistically significant results, thus suggesting that various contemporary mediums are able to demonstrate anomalous information reception (AIR) under laboratory conditions. Importantly, however, such studies are unable to address the source of mediums' AIR. Indeed, the source-of-psi problem (survival-psi and living agent psi [LAP] being the most likely contenders) cannot be resolved using current methodologies (Beischel 2012). However, innovative mediumship-testing techniques may produce results that indicate a convergence whereby sets of outcomes may evidentially favor one hypothesis over another (e.g., see Jamieson & Rock 2014 for a neurophenomenological approach). We present an innovative methodology focused on investigating whether mediums and well-rehearsed proxy-sitters, working under well-beyond double-blind conditions, create telepathic links that we refer to as *dyad-telepathy*, thereby producing response sets that indicate the psi source is more likely to be dyad-telepathy than a discarnate entity.

**Keywords:** dyad-telepathy—living agent psi—LAP—mediumship—source-of-psi problem—survival—telepathy

#### Introduction

Within the field of parapsychology, *psi* is a generic term used to refer to anomalistic cognition or *extra-sensory perception* (ESP) and anomalous motor action or *psychokinesis* (PK). The acronym ESP denotes three categories of psi communication: telepathy, clairvoyance, and precognition (Rock, Storm, Irwin, & Beischel 2013). An additional category of psi is

concerned with survival and afterlife topics (e.g., apparitional experiences, near-death experiences, out-of-body experiences, and reincarnation experiences). The survival topic referred to as mediumship is the focus of the present article. A medium may be defined as an individual who ostensibly communicates with a deceased person (Kelly & Arcangel 2011).

In a comprehensive historical review, Kelly (2010) traced the trajectory of mediumship research throughout the 19<sup>th</sup> and 20<sup>th</sup> Centuries. Kelly stated that

The study of mediums was part of a larger program of psychical research, begun in the late 19th century, intended to examine specifically whether human personality survives bodily death. (Kelly 2010:247)

Indeed, an abundance of “proxy-sitter” research occurred during the 1920s and 1930s (e.g., Allison 1934, Saltmarsh 1929, Thomas 1937, Walker 1927) in which an individual (e.g., the experimenter) served as a “proxy” for the absent sitter.<sup>1</sup> However, research involving “cross-correspondences” [i.e. “correspondences between the messages received by different mediums” (Irwin & Watt 2007:140)] and “drop-in communicators” [i.e. “an apparently discarnate personality who uninvitedly drops in to a séance yet is not known to either the medium or sitter” (Irwin & Watt 2007: 139)] also occurred (see, for example, Saltmarsh 1938 and Myers 1903, respectively).

Numerous “proxy” studies (e.g., Walker 1935) yielded positive findings. Clearly, proxy sittings have the distinct methodological advantage of allowing the experimenter to safeguard against sensory leakage (e.g., subtle cueing of the medium by the real sitter). Nonetheless, proxy-sitting study protocols are not without critics. For example, Stevenson (1968) argued that a medium’s drive to communicate with a deceased individual might be strengthened by the presence of the deceased’s loved one. Though ostensibly facilitative, the presence of a deceased loved one creates the very problem we seek to resolve.

Kelly (2010) noted that

the study of mediumship was almost completely abandoned during the latter half of the 20th century, primarily because of the impasse reached over whether the phenomena are best-interpreted as attributable to deceased agents or to living agents. (Kelly 2010:247)

This “impasse” is referred to as the *source-of-psi* problem and is discussed in the next subsection **Mediumship and the Source-of-Psi Problem**.

Beischel, Rock, and Krippner (2011) correctly observed that, “During the first decade of the new millennium, the scientific study of mediums



. . . underwent a major resurgence after considerable neglect” (p. 127). For example, several recent single-blind studies (Robertson & Roy 2001, Schwartz & Russek 2001, Schwartz, Russek, Nelson, & Barentsen 2001), double-blind studies (Jensen & Cardeña 2009, Kelly & Arcangel 2011, Roy & Robertson 2001, 2004, Schwartz, Russek, & Barentsen 2002), and triple-blind studies (Beischel & Schwartz 2007) have yielded positive results concerning mediums’ accuracy. We also note that one double-blind study (O’Keeffe & Wiseman 2005) that failed to obtain positive results was published; however, the experimental protocol contained various methodological flaws (discussed in Beischel 2007).

Importantly, we note that the aforementioned studies (e.g., Beischel & Schwartz 2007) tested the *accuracy* of mediumship under laboratory conditions rather than the *source* of mediums’ anomalous information reception (AIR). Indeed, recently Beischel (2012) asserted that, “The source of psi problem seems insurmountable . . . No amount of scoring data and no type of mediumship content can definitively distinguish between these two explanations [somatic psi and survival psi]” (p. 10).<sup>2</sup> However, Jamieson and Rock (2014) argued that, “even if there is no single test for the survival hypothesis, there may be series of tests capable of converging on one alternative or another” (Jamieson & Rock 2014:310; see also Rock 2014). The objective of this paper is to present an innovative methodology aimed at investigating whether mediums can produce response sets that indicate the psi source may not necessarily be a discarnate entity. That is, the methodology we propose may produce results that are contrary to what we would expect if discarnate (D) communication is operative (O), but which is what would be expected if dyad-telepathy (T) among living agents is operative (in *Procedure Phase 2*, we will explain how we intend to control clairvoyance). This can be formalized in the following confirmation measure:  $\Pr(O | T) > (\Pr(O | D))$ , that is the probability (Pr) of O is greater given T than it is given D. This is a standard Likelihood measure for saying evidence favors/confirms one hypothesis over another. We also note that this confirmation measure should be distinguished from the claim that the outcomes would indicate the psi source is unlikely to be a discarnate entity. Specifically, we aim to show that our preferred outcomes are more likely to indicate dyad-telepathy than survival psi. However, before we present our experimental protocol it would be prudent to provide some background into the source-of-psi problem.

### **Mediumship and the Source-of-Psi Problem**

A number of hypotheses have been proposed to explain mediums’ ostensible AIR: the *survival* hypothesis, the *living agent psi* (LAP) hypothesis, the

*super-ESP* (also *super-psi*) hypothesis, and the *psychic reservoir* (also *cosmic psychic reservoir* and *cosmic reservoir*) hypothesis. The difficulty (philosophical, methodological) associated with delineating the source of mediumistic information is termed the *source-of-psi* problem. Each of the aforementioned sources will be briefly considered, in turn.

The survival hypothesis states that, “the existence of discarnate persons provides the best explanation of the data associated with physical and mental mediumship” (Sudduth 2009:167). One shortcoming of this hypothesis is that it arguably lacks parsimony relative to non-survivalist explanations on the grounds that it, of course, posits: (1) the existence of an afterlife and, therefore, a dimension, or perhaps dimensions, that are additional to Einsteinian space–time; and (2) entities that are ontologically distinct from the brains of embodied minds. It might be argued that the ‘strength’ or pervasiveness of survival belief seems to lie in its historical, religious, and phenomenological roots more than anything else.

Counter-advocates of the survival hypothesis (see Sudduth 2014) often invoke LAP and the super-ESP hypothesis as alternatives, and perhaps superior explanations of survival data. LAP quite simply refers to psi (ESP and PK) originating (consciously or unconsciously) from the living. Moreover, super-ESP may be defined as an

expression possibly first used by Hornell Hart to refer 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 2003:121)

Thus, super-ESP is interpretable as a conceptual extension of the methodological challenge posed by LAP, which excludes personal agency (incarnate or discarnate) and postulates psi devoid of any known limits. However, while some scholars conceptualize super-ESP as LAP “pushed to its limits” (Gauld 1982:15), we acknowledge that others (e.g., Braude 2014, Sudduth 2014) do not appear to regard super-ESP as an extension of LAP, but instead propose that the term “super-psi hypothesis” be replaced “with the more accurate and neutral ‘living agent psi hypothesis’” (see Braude 2014). Following Braude (2014) and Sudduth (2014), we will, for the remainder of this paper, replace the term “super-psi” with “living agent psi” (and its acronym: LAP).

Braude (2003) has noted that there appear to be two variants of the LAP hypothesis postulated by researchers. First, the *multiple-process* hypothesis

conceptualizes LAP “as an organized collection of refined psychic tasks” (Braude 2003:11). That is, this process is concerned with the medium’s ability to negotiate successfully the task complexity associated with discarnate communication (e.g., ESP of sitter’s thoughts, the thoughts of other pertinent individuals, or relevant physical objects or events). Second, the *magic-wand* hypothesis states that even the most detailed ESP occurs as merely a result of the percipient’s wish or desire. Thus, this hypothesis deems irrelevant both the effort on the part of the percipient and task complexity (Braude 2003). Gauld (1982) articulated the central dilemma presented by the LAP hypothesis, 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 1982:15)

Perhaps not surprisingly, then, it has been argued that the LAP hypothesis is untestable because “it postulates an omniscient and omnipotent capacity that cannot be falsified by the scientific method” (Martinez-Taboas 1983:58). However, we note that saying we do not know the limits of psi (e.g., Braude 2003) is not to affirm that psi is unlimited, but the former claim is sufficient to create problems for the survival hypothesis, that is, if the case for survival depends on ruling out some subset of counter-explanations.

Scholars seeking to demonstrate that the survival hypothesis is untestable often invoke the LAP hypothesis. For example, Irwin (2002) reviewed séance phenomena, NDEs, OBEs, poltergeist and apparitional experiences, and reincarnation experiences and concluded that “the operation of such processes” as LAP are “impossible to exclude” and, thus, the aforementioned phenomena “cannot be conclusive for the survival hypothesis” (Irwin 2002:20). We note, however, that others (e.g., Keen 2003) have suggested that the survival hypothesis has more explanatory power than the LAP 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 2003:38)

Unfortunately, Keen made little attempt to justify why he considered the survival-of-consciousness explanation to be superior to the LAP explanation in the case of ostensible mediumistic communication. Keen briefly cited three cases, which he stated are all

... 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. (Keen 2003:38)

However, Keen did not elaborate on these “most improbable, speculative and evidentially unsupported extensions of psi” (Keen 2003:38), nor did he explain why survival was more probable, less speculative, and evidentially superior to the LAP alternative.

Braude (2003) has attempted to address the survival versus LAP stalemate via his *Argument from Crippling Complexity* (see Braude 2003:86–95). Braude argued that the crippling complexity of the *psychic traffic* produced by the totality of embodied minds might serve as an obstacle to LAP in the context of the medium–sitter interaction. This contention appears to provide indirect support for the survival hypothesis. However, Braude (2003) concluded that there is no persuasive reason to suppose that the complexity of the causal nexus underlying mediumship–sitter interaction is fundamentally different from the causal network associated with mediumship–discarnate interaction:

... 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 2003:93)

Thus, it appears that the Argument from Crippling Complexity applies equally to the LAP and survival hypotheses (Braude 2003). It is noteworthy that Braude (2003) has suggested that the survival hypothesis is more parsimonious than the LAP hypothesis because it posits a *single* source of mediumistic information (i.e. a discarnate entity).<sup>3</sup> In contrast, the LAP hypothesis considers *multiple sources* (e.g., the medium telepathically scanning the mind of the sitter or other living people, clairvoyantly accessing pertinent objects such as photos). Thus, it might be appropriate to grant “an explanatory edge to the survivalist, at least on the grounds of parsimony” (Braude 2003:93). But, as Storm (2014) points out, “. . . the human mental

agility implied in one theory [super-ESP] is as equally challenging to the emotions and the intellect as the multi-dimensionality implied in the other [i.e. survival]" (Storm 2014:1–2).

A further alternative to the survivalistic explanation is the psychic reservoir hypothesis. This hypothesis 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 2009:72). Fontana (2005) asserted that the psychic reservoir explanation is weaker than the LAP hypothesis because, while there exists laboratory evidence supportive of telepathy and clairvoyance (see, for example, Radin 1997), there is no scientific evidence indicative of a cosmic store of information. Moreover, Fontana (2005) stated that, in addition to the fact that it cannot be falsified,<sup>4</sup> there are numerous practical objections to this hypothesis. For example, "What is the organizing principle or intelligence behind the cosmic psychic reservoir?" (Fontana 2005:114). We note that the psychic reservoir explanation is arguably useful insofar as it allows one to distinguish further between the concepts of non-agentive (e.g., a cosmic store of information) versus agentive (e.g., the mind of the sitter) sources of anomalous information. However, this hypothesis rests on what some claim is an unintelligible notion of (meaningful) information as something that can be stored in a structure, independent of any context.

As previously stated, numerous mediumship studies (e.g., Beischel & Schwartz 2007, Kelly & Arcangel 2011, Rock, Beischel, Boccuzzi, & Biuso 2014, see also Rock, Thorsteinsson, & Tressoldi, in press) have reported statistically significant results, and thus suggest that numerous contemporary mediums are able to demonstrate AIR under laboratory conditions. Importantly, however, such studies are unable to address the source of mediums' AIR. Indeed, the source-of-psi problem (survival-psi and LAP being the most likely contenders) cannot be resolved using current methodologies (Beischel 2012). However, innovative mediumship-testing techniques may produce results that indicate a convergence toward one alternative or another (Jamieson & Rock 2014). Below we present an innovative methodology focused on investigating whether mediums engage with proxy-sitters in a form of psi we call *dyad-telepathy* (explained next).

### **The Protocol**

#### **Objective**

Our proposal describes the procedure for a study on the hypothesized telepathic link in the medium/proxy-sitter dyad. We aim to show that mediums create telepathic links with proxy-sitters only, thus producing

response sets that indicate the psi source is less likely to be a discarnate entity than a proxy-sitter (we also aim to show that we can control clairvoyance). For convenience, we refer to this specific psychic link as ‘dyad-telepathy’.

The typical response set in a conventional mediumship study consists of free-response items to questions such as “What did the discarnate look like in his/her physical life?” and “Describe the personality of the discarnate. What were the discarnate’s hobbies, activities, or interests?” (Rock, Beischel, Boccuzzi, & Biuso 2014:186). However, in our design, the response set consists mainly of two types of item stimulus on a proxy-sitter’s list (in the form of a questionnaire), so that responses to those item-stimuli take two forms: (i) counts of Yes (i.e. True) responses to Facts about a discarnate entity, and (ii) counts of Yes (i.e. True) responses to so-called ‘Counterfactuals’ about a discarnate entity. Counts on both lists are independently tested using One-Sample *t* tests, and both response sets are compared using the Independent-Samples *t* test.

### **Hypotheses**

We propose the following hypotheses concerning the medium/proxy-sitter dyad (all tests are one-tailed):

- H1: In the medium/proxy-sitter dyad, mediums report facts correctly.<sup>5</sup>
- H2: In the medium/proxy-sitter dyad, mediums do not correct false statements (i.e. they do not correct counterfactuals).<sup>6</sup>
- H3: In the medium/proxy-sitter dyad, mediums report counterfactuals *verbatim* more often than they correctly report facts.<sup>7</sup>

In Table 1, we have modeled all preferred hypothetical outcomes, and indicate how certain outcomes *pertinent to our design* may ameliorate the source-of-psi impasse. In our best-case scenario, an independently nonsignificant count of correct facts and an independently significant count of correct counterfactuals evidentially favors dyad-telepathy over discarnate communication, especially if there is a significant difference between number of facts and number of counterfactuals. This outcome would disconfirm in terms of a lower Likelihood the notion of discarnate communication based on the assumption that a deceased relative: (a) would assist the medium in correctly reporting a majority of facts, and (b) would unlikely be in error (i.e. if the medium is channeling a discarnate entity, we expect facts to be endorsed, and counterfactuals to be refuted or denied). It might be argued that we are

**TABLE 1**  
**Source-of-Psi Problem Modeled for Hypothetical Outcomes Most Supportive of Medium/Proxy-Sitter Telepathy (i.e. Dyad Telepathy)**

Hypothesis	H1 (Facts)	H2 (Counter-factuals)	H3 (Difference in Hypothesized Direction)	Findings and Conclusion	Support for Dyad Telepathy*
Outcome 1	not significant	significant	significant	Medium performs at chance identifying <u>facts</u> (H1); identifies most (or all) <u>counterfactuals verbatim</u> (H2); <u>performance gap</u> in favor of counterfactuals (H3); likely source = proxy-sitter.	<b>Strong:</b> <u>Facts</u> performance rules psi-sources beyond the dyad as unlikely; <u>counterfactuals</u> not corrected, rules psi-sources beyond the dyad as unlikely; <u>performance gap</u> is indicated. Most likely to be dyad-telepathy.
Outcome 2	not significant	significant	not significant	Medium performs at chance identifying <u>facts</u> (H1); identifies most (or all) <u>counterfactuals verbatim</u> (H2); but no significant <u>performance gap</u> in favor of counterfactuals; possible source = proxy-sitter.	<b>Moderate:</b> <u>Facts</u> performance rules psi-sources beyond the dyad as unlikely; <u>counterfactuals</u> not corrected, rules psi-sources beyond the dyad as unlikely; However, <u>performance gap</u> is not indicated. Possibly dyad-telepathy.
Outcome 3	significant	significant	significant	Medium correctly identifies sufficient number of <u>facts</u> (H1); identifies most (or all) <u>counterfactuals verbatim</u> (H2); <u>performance gap</u> in favor of counterfactuals (H3); possible source = proxy-sitter.	<b>Weak-to-Moderate:</b> <u>Facts</u> performance does not rule out psi-sources beyond the dyad; <u>counterfactuals</u> not corrected, rules psi-sources beyond the dyad as unlikely; <u>performance gap</u> is indicated. Dyad-telepathy partially indicated.
Outcome 4	significant	significant	not significant	Medium correctly identifies sufficient number of <u>facts</u> (H1); identifies most (or all) <u>counterfactuals verbatim</u> (H2); but <u>performance gap</u> is not in favor of counterfactuals; possible source = proxy-sitter.	<b>Weak:</b> <u>Facts</u> performance does not rule out psi-sources beyond the dyad; <u>counterfactuals</u> not corrected, rules psi-sources beyond the dyad as unlikely; <u>performance gap</u> is not indicated. Dyad-telepathy partially indicated.

\* Assumes medium is not interacting with a trickster or delusional discarnate, or simply falsely attributing the anomalous information to a particular discarnate. There are other possible outcomes, but these may indicate extra-dyadic ESP or chance.

assuming, perhaps unjustifiably, that deceased communicators suffer no or little postmortem confusion, despite many communications suggesting precisely that (from a survivalist point of view), or that communicators are in a kind of dream-like 'spacy' state. (S. E. Braude, personal communication, August, 2015; see also Braude 2003)

However, we argue that any hypothesized confusion or dream-like states in either the hypothesized discarnate entity, and indeed for that matter the medium, might be as much responsible for hits as for misses, so that such states (rare or common) would, in a statistical sense, have minimal overall influence on our results. In the main, that is why we depend on statistical outcomes. Later, in the section **Analysis of Design**, we make a related point, that a number of possible statistical outcomes, each in their own way, provide limited support for the argument that a discarnate entity (or anyone else for that matter) has helped the medium. That kind of support will be an advance on conventional mediumship research.

### ***Procedure Phase 1: Participant Recruitment and Screening***

Participation in the proposed study will involve claimant mediums and 'sitters-in-absentia' (each medium will read two pairs of sitters-in-absentia who will also serve as sitter-raters). We define sitters-in-absentia as living participants interested in hearing from their deceased loved-ones during mediumship readings but who will not be present at the reading. Sitters-in-absentia will be recruited via email lists.

The aim of the screening is to maximize the likelihood that each claimant medium is able to: (1) report relatively specific, accurate, consistent, and scoreable information under various experimental conditions; and (2) convey accurate information while following specific experimental instructions (Beischel 2007, Rock, Beischel, & Schwartz 2008; we acknowledge that much of Phase 1 has its origins in procedures designed by Julie Beischel).

Each sitter-in-absentia reads a Plain-Language Statement (PLS), which is information about the study, and signs a Consent Form before they can participate in the study. They will then be instructed by Experimenter #1 to complete an online pre-screening questionnaire including items in which one discarnate related to the sitter-in-absentia is chosen and is described in terms of personality and physical traits, favorite activities, and cause of death (Beischel 2007). Discarnates will be paired based on an established pairing system (Beischel 2007). Briefly, as a way of maximizing differences between pairs of discarnates and thus increasing effect size, the information about each discarnate provided by the associated sitter-in-absentia will be used to identify pairs of discarnates of the same gender that are most distinct



in age, physical description (e.g., hair color, build, height), personality description (e.g., extraverted or introverted, rational or emotional), favorite activities (e.g., indoor or outdoor, group or solitary), and cause of death (e.g., part of the body affected, sudden or prolonged) (Beischel 2007). This pairing process maximizes sitter-rater blinding and optimizes each blinded rater's ability during scoring to distinguish between two readings (Beischel 2007, Rock, Beischel, Boccuzzi, & Biuso 2014).

During the test readings (there will be two independent readings performed by each medium), for each reading the medium will be given the first name of the discarnate and then asked several questions about the discarnate's physical life (Beischel 2007).<sup>8</sup> Each reading will be transcribed by Experimenter #2, formatted into a list of individual items, and blinded to remove any reference to the discarnates' names in both trials (Beischel 2007, Rock, Beischel, Boccuzzi, & Biuso 2014).

Each of the two formatted readings will be scored for accuracy (or "fit") by each of the two associated blinded sitter-raters; each sitter-rater will score their own reading as well as the reading intended for the other sitter without knowing which was which. Thus, each sitter will serve as a control rater for the other sitter's reading (Beischel 2007). Each sitter-rater will provide a numeric score for the overall reading, estimating the percentage of items he or she feels are accurate, and choosing which of the two readings he or she believes was intended for him or her. Sitters will be provided with the readings for scoring and return their scores by e-mail to Experimenter #1, who will enter data into a database so mediums can be rated (Beischel 2007, Rock, Beischel, Boccuzzi, & Biuso 2014).

To summarize, the screening process for the claimant mediums will consist of two identically formatted scheduled phone readings for two paired discarnates and their respective sitters-in-absentia (Beischel 2007). The test-reading protocol will involve a beyond-double-blinded<sup>9</sup> (Beischel 2007, Rock, Beischel, Boccuzzi, & Biuso 2014) phone reading, in which only the medium and a proxy-sitter (i.e. proxy for the sitter-in-absentia) will be on the phone. Beyond-double-blinded readings include five levels of blinding: (1) the medium is blinded to information about both the sitter-in-absentia and the discarnate before and during the reading; (2) sitters-in-absentia who also score the readings for accuracy (i.e. sitter-raters) are blinded to the origin of the readings during scoring; (3) the experimenter (Experimenter #1) who consents and trains the sitters-in-absentia/sitter-raters is blinded to which mediums read which sitters-in-absentia and which blinded readings were intended for which discarnates; (4) the proxy-sitter who interacts with the mediums during the phone readings and formats the readings into item lists is blinded to any information about the sitter-in-

absentia and the discarnates beyond the discarnates' first names; and (5) the experimenter (Experimenter #2) who interacts with the sitters-raters (i.e. receives by e-mail the readings-scores) is blinded to all information about the discarnates, to which medium performed which readings, and to which readings were intended for which discarnates/sitters-in-absentia (Beischel 2007, Rock, Beischel, Boccuzzi, & Biuso 2014).

### **Procedure Phase 2: The Six-Step Experimental Protocol**

The claimant mediums who pass the screening procedure (see Beischel 2007 for details regarding passing criteria) will participate in our six-step experimental protocol outlined below. The medium, proxy-sitter, and sitter-in-absentia must never be encouraged to meet as far as is humanly possible, because if the medium ever meets the sitter-in-absentia later and reads his/her mind, then they could, for example, retrocausally send back the correct information and use it in the present during the running of the experiment. The medium must not know that there is a sitter-in-absentia and a proxy-sitter. The proxy-sitter will make contact only once with his/her designated medium in order to administer the stimulus set, and the proxy must never reveal that they are a proxy.

**Step 1:** Sitters-in-absentia (i.e. sitters who will never be present during the medium's reading), the proxy-sitter, and the medium are briefed separately about their roles in the experiment. All will read a PLS and sign a Consent Form before they can participate in the study.

**Step 2:** Experimenter #1 will liaise individually with each sitter-in-absentia to create a list of 30 facts about a deceased relative. Subsequently, Experimenter #1 will (i) randomly convert 12 of the facts into counterfactuals (i.e. "what is not the case"; facts NOT about the discarnate entity) by a simple grammatical negation of the truth status of those 12 items (e.g., adding "not" in the appropriate place, syntactically speaking, in the item), and (ii) randomly convert 6 of the remaining 18 facts into factoids by grammatically 'fuzzifying' them up (i.e. making them non-specific). Factoids function as a 'fuzzy' subset of decoys that have only 'degrees' of correctness—they are neither true, nor false. Factoids are trivial but unreliable items of information that are so often repeated that they become accepted as facts). Factoids function as decoys mainly to inhibit the medium from directly identifying the counterfactuals,<sup>10</sup> thus dissuading attempts to engage in LAP telepathy beyond the proxy-sitter to the sitter-in-absentia (i.e. psi sources beyond the dyad).<sup>11</sup>

The remaining 12 items will be untouched facts. We note that a medium's outright confirmation of a fact about the discarnate entity does not eliminate the discarnate entity's involvement, but a genuine discarnate entity would

not repeatedly verify counterfactuals (indeed, a discarnate entity may not see the need for factoids in a stimulus set, but for the purposes of a LAP telepathy test their presence is a necessary control condition that is ‘grist for the mill’ for the medium). We need to be able to limit the medium to the stimulus set by controlling for responses from sources beyond the dyad. That is why we need, in our stimulus set, a range of facts, factoids, and counterfactuals, all of which are well-rehearsed by the proxy-sitter.

**Step 3:** The ‘blind’ proxy-sitter is contacted through email by Experimenter #1 and given two weeks to rehearse the list of 30 items, so as to think (believe) that all the items are true. It is crucial that the items be embedded in memory to optimize the LAP telepathic process, given the nature of telepathy as a form of paranormal mind-reading. At a later date, the proxy-sitter is contacted by email and asked if he/she feels confident that he/she knows the list by heart; then a day, time, and location are arranged for a drill through Skype to confirm the proxy’s knowledge of the items. The proxy-sitter is required to recite the list via Skype wearing a light-proof mask over the eyes to ensure that the list is not covertly being read. All lists and working documents (except the question sheet) are then destroyed by burning to disenable clairvoyance during the sitting.

**Step 4:** Having passed the drill, the proxy-sitter is asked to phone a randomly assigned medium—the medium is given a specific day and time. However, the medium will think the call is from a sitter, not a proxy-sitter.

**Step 5:** At the proxy-sitting sessions, the proxy-sitter will elicit YES/NO (or TRUE/FALSE, where TRUE = YES; FALSE = NO) responses from the medium. These Y/N (or T/F) responses are recorded on the question sheet. Y/T = hit; N/F = miss. As explained in **Step 3**, we cannot have the naïve proxy-sitter poorly rehearsed (i.e. empty-headed) and uncertain about the items in the stimulus set as that may encourage the medium to use LAP beyond dyad-telepathy. Thus, our test will constitute a direct test of dyad-telepathy (i.e. telepathic scanning by the medium of the proxy-sitter’s naïve mental set)—we expect the naïve medium to vouch that all items are true (even the counterfactuals). The proxy-sitter will not reject correspondences that go beyond mere affirmations and negatory responses (i.e. mentation) so that a qualitative analysis of any mentation can be performed as a post hoc analysis to determine whether the quantitative findings are supported by the mentation.

**Step 6:** The proxy-sitter scans the question sheet, which also contains the responses from the medium, and emails it to Experimenter #1, who enters the data into an SPSS datafile for statistical analysis (NB: data entry is only done after responses on the question sheet are cross-checked with the sitter-in-absentia since all other lists were destroyed).

### Analysis of Design

We have described a procedure for a study on the hypothesized telepathic link in the medium/proxy-sitter dyad that limits the likelihood of psi sources outside the dyad. We propose that it can be demonstrated that mediums can limit themselves to dyad-telepathy (i.e. exclusive telepathic links with well-rehearsed but ‘blind’ proxy-sitters). We argue that our methodology can produce response sets that evidentially favor dyad-telepathy over discarnate communication, especially when responses to the stimulus set include verbatim reports of counterfactuals (i.e. “what is not the case,” which are modified facts that are *not* true of the discarnate entity).

The task requires identification of a rich assortment of facts, counterfactuals (as foils), and factoids (as decoys). We state that if all the information needed to answer the proxy-sitter’s questions is available strictly in the context of the medium/proxy-sitter dyad only, recourse by the medium to psi sources beyond the dyad are unnecessary.

It may be argued that we are not justified in our presumption that what would be difficult in a non-psychic task is likewise difficult in a psychic task. However, we appeal to an old concept that “ESP is voluntary in its *dirigibility*” (Rhine, Pratt, Stuart, Smith, & Greenwood 1940/1966:319), of which most psi researchers have a tacit understanding. In other words, the medium has some degree of volition over ESP and can guide and give the ESP process “definite direction” (psi experimentation would be impossible without that); but the medium’s mental state and psychological set can be changed in order to undermine (or strengthen) the psi process. Psychologists can put up all sorts of blocks to impair or ‘canalize’ a medium’s progress. A considerable number of psychological correlates of parapsychological processes exist, and, given the evidence that psi is very much like many other human functions, we are at liberty to assume that interventions that ‘foil’ or facilitate psi processes are possible. For example, there is evidence that psi can be made weaker (Storm, Ertel, & Rock 2013, Storm & Rock 2014) or stronger (Storm, Tressoldi, & Di Risio 2010) purely through psychological manipulations. Thus, we provide, through manipulation at the proxy-sitter priming phase and, as a consequence, during dyad-telepathy, all the information the medium will ever need for our purposes so that the medium would not need access to multiple obscure information sources. The ‘dirigibility’ hypothesis permits that assumption.

Another problem that may be raised here is that the medium cannot be deceived; how do we prevent the deception from being discovered psychically? To put it another way, someone connected with the experiment would know which facts have been converted to counterfactuals, and since

we are hypothesizing that a form of psi, namely dyad-telepathy, is being shown in this study, how can the application of truth-disclosing telepathy beyond the dyad be ruled out?

Naturally, if a medium corrected a sufficient number of counterfactuals so that the count of counterfactuals reported verbatim was significantly low, *and* the medium identified a significantly high number of facts, *and* the difference was significant, then the experiment fails—we could not abandon the argument for telepathy beyond the dyad. However, that is not the only possible arrangement of three binomial outcomes (again, see Table 1). While support for one hypothesis (i.e. a significant *p* value) on its own is meaningless (test results on all three hypotheses must be taken collectively), we can say, generally speaking, that our first priority is to find a significant number of counterfactuals reported *verbatim*, thus indicating *limited* support for the argument that the discarnate entity (or anyone else for that matter) has helped the medium.

As is shown in Table 1, we have modeled the four outcomes that support our theory. Naturally, some outcomes are more persuasive than others. Essentially, however, our preferred outcome (Outcome 1) disconfirms in terms of a lower Likelihood the notion of discarnate communication, based on the opposing premise that *if* mediums channel discarnate entities, *then* we may expect that facts about the discarnate entity would be endorsed by said entity and appropriately channeled by the medium, whereas counterfactuals would be refuted or denied by the discarnate entity and accordingly relayed as such by the medium. And we would also expect a performance gap. If our study yields our preferred outcome, then mediums are either working mind-to-mind with (a) a proxy-sitter only (or a *disinterested* or *confused* deceased relative or loved-one, which is perhaps unlikely), or (b) a malevolent entity (a problem in all medium research).

### Conclusion

Despite contentions that the source-of-psi problem appears insoluble (e.g., Beischel 2012), we contend that, “even if there is no single test for the survival hypothesis, there may be series of tests capable of converging on one alternative or another” (Jamieson & Rock 2014:310). In agreement with Jamieson and Rock, one such test would involve the protocol described for the first time in this paper. We argued that using facts only is a ‘diffuse’ test of AIR (i.e. not a test of AIR with an attempt to identify the source), whereas our relatively more ‘concise’ test draws in, focuses, and aims to limit AIR to so-called dyad-telepathy, while simultaneously restricting or even excluding the hypothesized presence of a ‘responsive’ discarnate

entity because a discarnate entity would not make so many personal errors originating in the ‘counterfactuals’ relevant to the Likelihoods. Thus, this experimental protocol is one of the few to focus on identifying the source of mediums’ AIR rather than merely testing for the occurrence of AIR. We plan, in a subsequent paper, to present the results of an experiment featuring that design.

### Notes

<sup>1</sup> A proxy-sitter may be defined as “a living person who is present for the reading but is not the person for whom the information reported during a reading is intended. A proxy-sitter may or may not have knowledge about the absent sitter or the deceased persons contacted during the reading” (Beischel & Rock 2009:71).

<sup>2</sup> Beischel and Rock (2009) used “the term ‘somatic psi’ to describe telepathy with living persons, clairvoyance (including of a psychic reservoir), and precognition on the part of the medium” (p. 73). In addition, “the term somatic is used in reference to the physical body of the living client in psychic readings as well as the ‘body’ of information described by the psychic reservoir hypothesis” (p. 73). In contrast, Sudduth (2009) defined survival-psi as a process whereby

the medium acquires her knowledge of discarnate minds by telepathically scanning their minds or . . . the discarnate person is telepathically sending information to a medium’s mind. In either case, living agent telepathy is operative. (Sudduth 2009:177)

Sudduth (2009) described survival-psi as “a highly refined and efficacious sort of psi functioning . . . indistinguishable from the degree or kind of psi required by the super-psi hypothesis” (p. 184).

<sup>3</sup> However, Sudduth (2009, 2014) asserted that the survival hypothesis must posit multiple sources when it comes to discarnate persons acquiring states of affairs in the physical world.

<sup>4</sup> We, of course, note that it has been argued that the survival hypothesis and the LAP or super-ESP hypothesis are not falsifiable (see Irwin 2002, Braude 2003, respectively). However, we note that this does not mean that nothing can count against these hypotheses. Various theoretical and pragmatic considerations (e.g., systematicity, conceptual cost, predictive fecundity, explanatory simplicity) can reasonably be taken as rendering one of them more plausible than the other (see Braude 2003).

<sup>5</sup> Operationalized as: The number of facts reported by the medium and confirmed as correct (‘Yes’/‘True’) is *above* MCE (one-tailed).

- <sup>6</sup> Operationalized as: The number of counterfactuals reported by the medium verbatim ('Yes'/'True') is *above* MCE (one-tailed).
- <sup>7</sup> Operationalized as: The number of counterfactuals reported by the medium ('Yes'/'True') is *higher* than the number of facts reported and confirmed as correct ('Yes'/'True') (one-tailed).
- <sup>8</sup> We acknowledge that a sitter asking a medium specific questions is typically inconsistent with a real-life reading whereby sitters are more passive and the medium simply provides information about the discarnate. We note, however, that numerous studies (e.g., Beischel & Schwartz 2007, Beischel, Boccuzzi, Biuso, & Rock 2015, Rock, Beischel, Boccuzzi, & Biuso 2014) have demonstrated that certain mediums are able to answer correctly specific questions from sitters about discarnates. Indeed, Beischel (2007) asserted that:

The ability to obtain information about a specific discarnate led to the hypothesis that specific pieces of information about the discarnate could be obtained through the asking of specific questions. The use of questions during a reading (a) increases the probability of obtaining information related to the identification of the discarnate, (b) further focuses the medium, and perhaps most importantly (c) emulates normal human communication. The asking-questions paradigm is also important during data analysis: Instead of estimating the probability of the medium's potentially general statements being accurate (for example, Saltmarsh & Soal 1930), the medium is simply asked to provide specific information. (Beischel 2007:42)

- <sup>9</sup> By using multiple levels of 'blinding', we maximally eliminate from the psychological set (though not necessarily from the parapsychological set) as many sensory sources as possible that may explain artifacts, thus undermining the psi hypothesis. The other advantage of 'blinding' is that we have to make sure our designs are as operationally superior as possible in order that they meet conventional standards. As for 'Phase 2' (see the section **Procedure Phase 2: The Six-Step Experimental Protocol**), we are claiming that our results can tell us that psi has been restricted to the dyad as telepathy, thereby undermining claims of psi sources outside that dyad.
- <sup>10</sup> We are aware of the possibility that the medium may engage in 'extra-dyadic' telepathy or even clairvoyance. Therefore, we need to minimize suspicion in the medium. We theorize that the medium will not have a need to differentiate counterfactuals from factoids because the latter are not overtly false and therefore do not warrant 'correction' per se. If we did not have factoids, the counterfactuals may be more readily evident among the facts. In a sense we create in the laboratory a 'smoke and



mirrors' scenario—what Braude describes as “inevitable obstacles from the bustling underlying nexus of psychic activity” (Braude 2003:93).

<sup>11</sup> It might be asked what justifies our apparent assumption that these potential psi agents would be so well-behaved unconsciously or subconsciously? That is, does our protocol provide anything more than merely apparent control of the situation? Though this control condition may initially appear to be only ‘apparent’, we argue that it will be validated over a series of studies, given replication of the desired test result (see especially Outcome 1 in Table 1). Our test results can delineate the two different types of responses (see, again, Table 1). We argue that mediums, not being ‘front-loaded’ (i.e. not primed that there are facts, factoids, and counterfactuals in the set), will not know, and *not seek to know*, the difference, thus theoretically limiting psi to the dyad.

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## RESEARCH ARTICLE

### **Shortage of Rabbits or Insufficient Traps? Table-Turning and the Discovery of a Presumably PK-Gifted Person in Argentina**

**JUAN GIMENO**

jgimeno54@yahoo.com.ar

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**Abstract**—The formation and development of a sitter-group in Buenos Aires is described. Fifteen weekly meetings were conducted, between April and July 2013, attended by 5 to 9 people each. Ostensible movements of a table were reported. One ostensibly psychokinetically gifted member was identified, named Ariel. He had witnessed RSPK at home, when age 11. After identifying him, another 10 meetings were conducted with only this gentleman present (and observers). In these meetings he made a table raise a leg at will, even with additional weight added to the table. All the meetings were conducted with normal illumination, most of them recorded in video. It was impossible to reproduce most of the table movements by normal means. He was not able to achieve the total levitation of the table, nor any movements without hand contact. Gifted people with remarkable psi abilities are scarce, and in the field of physical phenomena objective investigations of macro-PK seem to be stagnant, or at least without the possibility of publishing encouraging results. The results obtained in our studies are promising, nevertheless we plan to conduct further experiments focusing on controlled conditions and in good light conditions.

#### **Background**

Since the mid-Nineteenth Century, there have been reports of informal groups, known as sitter-groups, in which after complying with some formulae or rituals and after a variable period of time, the attendees began to hear some creaks or tiny movements of the table they were gathered around and, in the best cases, obtained a complete levitation of the table, raps, apports, and/or lights of unknown origin. Even though there were few reports, there were some similarities, such as the scientific authority of

the organizers, the selection of attendees (those who never before reported other physical phenomena), and the sequence of the phenomena (usually beginning with raps and proceeding to movement, etc.). In these groups there didn't seem to be one special person who was responsible for the phenomena; the entire group took responsibility.

In Valleyres (Switzerland), Agenor Etienne de Gasparin (Gasparin 1854) trained at home a group of 10–12 people. For three months and in more than 30 meetings he observed that a table of 90 kg moved, in some cases with the addition of weights. He proclaimed the existence of a “psychic fluid” which the assistants deployed. Marc Thury (Thury 1855), professor at the Academy of Ginebra, formed a group composed of his friends. He confirmed reports at the Academy of Paris (Chevreul 1882) on the existence of involuntary muscle movements. But he also noted other changes that could not be attributed to that cause. William E. Barrett (1918) witnessed inclinations, movements, and levitation of a table, which increased when participants intoned cheerful songs. William J. Crawford (Crawford 1916, 1919) entered into a family circle where one of its members, Kathleen Goligher, possessed exceptional gifts. However, the discovery during the sessions of weight loss of all attendees, made him reflect on the importance of each of the members in the production of phenomena.

Starting around 1930, the Rhine paradigm of laboratory experiments became popular, and enthusiasm for working with sitter-groups began to decline. Many experiences with similar patterns and results to those mentioned circulated by word of mouth, and were only published by prestigious authors. Among them are the experiences of Jules Eisenbud (Pilkington 1987), and the essays of Stephen Braude (1992), Montague Ullman (1993), and Silvio Ravaldini (1995), who delayed revealing their “secrets” by 54, 24, 60, and 61 years, respectively. Argentina was no exception, and had two groups in La Plata city. In both cases, their members were students or university graduates. One of the groups was directed by the physicist José María Feola (2013) and the other by the mathematician Mischa Cotlar (Gimeno 2008). Both achieved remarkable physical manifestations, but only published as was usual at that time 50 years after their occurrences.

The researcher who included table-turning among the important subjects of modern parapsychology was Kenneth Batchelder (see Batchelder 1966), not only getting results but also moving forward with the theory of induction to PK in small groups (Batchelder 1984), in which he states that PK is a universal human ability, developable by any group that combines serenity, optimism, interest, and persistence, and does away with negative factors such as skepticism and fear. Batchelder's conclusions and success encouraged others to form new groups (Brookes-Smith & Hunt 1970, Owen



**Figure 1. The Red Lights Group in meeting #10.** Standing from left to right: Laura Fernández, Ariel Fariás, Andrea Altamirano, Juan Corbetta. Seated from left to right: Pedro Saglia, Juan Gimeno, Filippa Sotille, Mary Carballo.

& Sparrow 1976, Storm & Mitchell 2003, Williams & Lang 2002, Wilson, Williams, Harte, & Roll 2010).

### **The Red Lights Group**

In the middle of other parapsychological investigations conducted with Juan Corbetta, we were notified that a recently widowed woman reported that some objects in her house moved without apparent cause. She believed that these were messages from her dead husband.

Instead of following the case as a supposed poltergeist, Corbetta and I decided to organize and direct a sitter-group (Figure 1), called Red Lights, in order to channel the woman's expectations. Eleven people attended the presentation meeting, among them a physicist, an anthropologist, a lawyer, and two healers, all recruited due to their prior attendance at courses and seminars in parapsychology. The commitment was to attend a weekly meeting for three months, even in the case of the non-occurrence of any phenomena. Corbetta and I (hereafter "the organizers") explained that PK group phenomena should be developed slowly, with collective cohesion and camaraderie in mind, by joining in reading and discussing related papers, sharing food and drink, and talking about personal situations related to parapsychology. Each of the members assigned their own probability

to the occurrence of phenomena: The physicist said “impossible,” the anthropologist and one of the invited guests, Ariel, admitted that their attendance was due to curiosity only. The organizers thought it would be very difficult to get the table to move, but the rest of the group considered it possible to repeat the success of groups of the past. Moreover, three of them feared they’d be punished if they disturbed the involved spirits. We tried to calm these people down and minimize their fear.

The sessions would begin with short induction periods of 10 or 15 minutes, increasing with each meeting, with breaks, chatting before and afterward for evaluation. We would sit around the table and would ask, e.g., “If there is someone who can move the table, make *raps* or other physical phenomena, we invite you to try as we are here for that,” and then we would wait. One person in the group would be the observer, sitting away from the table but in the same room, selecting a good place for a view of the whole table and all the people. The organizers didn’t believe in the Spiritist belief regarding the possible agents that produce PK, but it was the explanation for most of the attendees. So, after clarifying our point of view and discussing the methodological difficulties in confirming this hypothesis, we agreed to follow this format.

Every meeting was recorded on audio files and from the fifth session on in video as well, with two mid-quality color cameras. Except for short periods, we always worked in a well-illuminated room. We did not detect any differences in the behavior of the group or the phenomena between dim light and a well-lit room. The group’s name did not oblige us to use red lamps; it was a tribute to the film *Red Lights* directed by Rodrigo Cortés (<http://www.youtube.com/watch?v=Y3W2wfVtWYI>), which had made me think about such physical phenomena.

Our meeting place was a room, an empty space of approximately 7 by 4 meters. We began by including incidental classical music, but after a few minutes it was rejected by several attendees, so from that moment on we only listened to the questions, some comments, and a few jokes to generate a friendly environment. We agreed to discourage any kind of subjective demonstration, such as telepathic messages, asking only for movement of the table or raps. Finally, the organizers agreed not to produce any artifact (against Batcheldor’s recommendation of surreptitiously including or permitting a first fraud—conscious or unconscious—to trigger the PK) and to keep a focused but not invasive control that would be easily accomplished with the good illumination. The amount of light was not measured with an instrument, but it was enough to read a book by without difficulty.

The first meeting on April 5, 2013, was attended by nine people. We sat around a little 4-legged round wood table, with a diameter of 40 cm and



**Figure 2. Ariel Farías.**

a weight of 2.5 kg. During the first 20 minutes we heard creaking or raps coming from the table. The raps were recorded on the voice recorder and their vibrations were felt on the fingertips of those of us who had our hands on the table. On continuing, after a rest, we reported three tiny movements of the table (a few mm each). From the second to the eighth meeting, we decided to exchange the table for a bigger, rectangular-shaped one, also of wood, 70 cm × 116 cm × 76 cm (height × length × width), with a weight of 22 kg. The former table was chosen in order to be more comfortable and to inhibit unconscious muscle efforts that could have provoked the movements. However, movements had increased with each meeting. Usually, after half an hour or a bit more of listening to raps, the horizontal movements began, with longer displacements each time, compelling the attendees to stand up and walk following the table. In the video of meeting #6, the table is shown to be moving continuously for 27 seconds. Of course these results surprised us, not only because of the unusual nature of the movements, but also because of the enthusiastic and excited behavior of the group. (Everyone was happy, everyone wanted to give their opinion, those who believed in Spiritism wanted to communicate with their dead relatives, etc.)

We established a communication code assigning “Yes” and “No”



answers to table movements. For example, we asked: Are you dead? Did you die more than 10 years ago? At other times we made requests such as: If you are male, move the table forward, if you're female, move the table back. The answers were inconsistent, and often contradictory. Through this method we received the name of "Cristina," who introduced herself as the deceased aunt of one of the attendees; however, we did not get any objective data that supported that identification. Still, we asked "Cristina" to help us with the investigation by moving and/or levitating the table. On some occasions the movements became uncontrollable, while in others the table "answered" questions and accomplished precise requirements such as "We want you to go to the door," "We want you to corner this guy because he doesn't believe you are Cristina," etc.

In only 3 of the 15 meetings did we not get any positive results. The ninth meeting was the most significant. Five people attended this meeting, witnessing in the first part only tiny, short, and imprecise movements. During the rest of the time, we decided to change the usual table to a 3-legged, round one, with a diameter of 65 cm and a weight of 8 kg, with legs inserted directly into the edge of the tabletop.

Almost immediately after recommencing the session, while attendees joined hands letting them rest on the table, it began to move, at first with tiny displacements, then longer ones. When the table was asked to "levitate," it raised one of the legs and then the second, turning around over the third one. At the same time it was moving all over the room, supported alternatively by one, two, or all three legs. In the meantime, those who maintained our hands over the table perceived a strong but elastic push in the upper direction coming from it. The sensation was as though the legs were supported by a mattress filled with air or water. Of course we could easily pass our hands through the empty space between the leg and the floor.

On one of these tours the table came to a corner of about 4 square m, which was elevated from the rest of the floor about 20 cm. The table stopped for a while, raised one of the legs, moved forward, raised the other 2 legs, and hopped on. Immediately, the table was asked to come back to the initial site. The table came back without any problem, earning general applause by this feat. When the movements became uncontrollable, one of the attendees decided to hop on the table; however, the table continued moving along the room, but with certain difficulty. After that, to satisfy another request the table suddenly raised one of the legs and ejected the improvised raider. Immediately after this, the table behaved exactly the same way with another attendee. To the amazement and collective agitation of all present, the table continued moving around the room with the only condition that two people had their hands on the table. There was no way to convince "Cristina"



to say good-bye until next week, so we decided to interrupt the meeting unilaterally and abruptly by simultaneously removing all of our hands from the table, at which point it ceased moving.

As long as the meetings continued, the organizers analyzed the videos and discussed the strategies to carry on. There were two main issues to solve:

1. Was there any possibility that the phenomena were based in some kind of fraud? We concluded that there were some movements impossible to be simulated, such as those in the ninth meeting. Most of them would only be possible if several attendees secretly agreed to cheat by adding muscular force to the table. To discard this possibility, we decided to introduce barriers between hands and table to minimize the effect of friction.
2. On the other hand, the speed at which phenomena showed up suggested that this was not group-PK, but that there was a special PK-gifted person in the group. Analyzing attendance at the various meetings, we found that one of the participants, Ariel, was absent in two of the three sessions in which nothing had happened and had acted as the external observer in the third one. In addition, we detected several moments in which the table stopped its movements when Ariel removed his hands from it.

These speculations led us to a distinctive and revealing fourteenth meeting. A new wooden table was especially built and used from the fourteenth session on. It weighed 12 kg and was 1 m in diameter. It had three legs inserted into the outer edge of the table board. Once the table began its usual movement, a synthetic material tablecloth, then up to three superimposed cotton tablecloths, and an expanded polyurethane tray were placed, in sequence, between hands and table. In all cases, the table continued moving, decreasing its speed and momentum however. After taking some minutes to rest, a decisive experiment was conducted: Each of the attendees was asked to leave the table for some minutes, to go about 7 m away, and then to come back to it. The table stopped its movements only when Ariel left the room. After that, with all members around the table, one after another left the table and did not come back to it, in this way reducing the number of members from six to one, and all the while we were observing that the table continued moving or raising a leg. The last person who remained with his hands on the table was Ariel. In this way, we confirmed that almost by chance we had found a special person, and that radically changed our immediate objectives.



**Figure 3. Ariel in action #1.**

### **A Change in Perspective**

The unexpected discovery of Ariel's abilities forced us to reevaluate the background information that had guided us which described the behavior of groups without known special people. From that moment on, we had to check the other half of the literature, meaning the reports concerning groups that focused on a special person, such as the one organized by William Crawford with the medium Kathleen Goligher (Crawford 1916, 1919), pay attention to laboratory work done with a single subject, e.g., by William Crookes with Daniel Home (Crookes 1871), and look at some more recent, though precarious and/or incomplete investigations, such as those of Uri Geller (Hasted, Bohm, Bastin, O'Regan, & Taylor 1975), Matthew Manning (Bierman, De Diana, & Houtkooper 1976), and Nina Kulagina (Pratt & Keil 1973).

Moreover, at first Ariel was reluctant to believe that he was responsible for the movements, generating what we understood to be a negation reaction; however, he eventually accepted it due to the overwhelming evidence. At that point, a previous episode in Ariel's life took on significance: In previous conversations he had told us about a poltergeist case in which he was the main witness. A brief description follows:



**Figure 4. Ariel in action #2.**

Ariel Farías was born in Buenos Aires, Argentina, on December 13, 1975. He and his family lived in a first-floor flat, in a middle-class neighborhood. His family was composed of his father, José María, who worked as a doorman in the building where they lived, his mother, Nélide Esther, a housewife, and a sister, 7 years his senior. Ariel was a good student, finishing primary and secondary education in a public school. He worked in several places and now works selling linens and blankets for a textile factory. He is an intelligent and sociable person, interested in parapsychology, Chinese language (he studied for five years to be a translator), martial arts (he teaches them in a private institute), and Buddhism (recognizes it as his religion, though he does not practice the rituals). He has been in a stable relationship for several years, and is awaiting the birth of a child in a few months.<sup>1</sup>

In the middle of 1986, his father, aged 54, died suddenly. Ariel was completely shocked, not only for the loss but also because of a premonitory dream he'd had in which the time and circumstances were exactly those that finally occurred. From that moment on, he became introverted, gained weight, and began to suffer from a nervous gastritis. At the same time, he experienced learning problems at school, where his classmates bullied him. Two years after his father's death, a series of perturbations began to occur in the flat he shared with his mother (his sister had moved away by then).



**Figure 5. Ariel in action #3.**

In the beginning his mother said she was sure she saw a passing “shadow,” while at the same time the dog barked loudly, the keys in a keyring moved by themselves, and some things hanging on the walls fell down. On several occasions, when they were both in the kitchen, some noises were heard coming from the bedroom. When they entered the room, they found not only the wardrobe doors opened but also the wardrobe contents spread over the bed. At first they saw these facts as a sort of consolation, as they interpreted them as messages sent by his dead father. The fear they felt after a particularly disturbing episode finally defeated them. It happened while Ariel was alone in the flat: He felt an inexplicable sensation of anguish, and then he heard a detonation and observed a little mirror that was in front of him disappear instantly. It was found afterward broken in many pieces in an external alley. After that, they asked for help from several religious groups and cults, until gradually the incidents ceased approximately a year after the disturbances had begun.

### **The Second Part**

An unsuspected new development in the sitter-group was that most of the members began to show a lack of enthusiasm, mostly expressed in unjustified absences to the meetings and a breakdown of the cordial environment.



**Figure 6. Ariel in action #4.**

Most of them seemed to be disappointed because they were not the special person. At the end of the three-month time commitment to the sitter-group, we questioned Ariel regarding his feelings. I knew that Ariel was a solitary personality, so we asked him if he was comfortable with the group. He told us that he disagreed with the spiritualistic hypothesis of some members, and that he did not want to be seen as a rare person. After being identified as the special PK subject, some looked on him with envy, others with fear. He wanted to work without pressure, with a group of friends. We had to decide how to proceed. Basically our analysis focused on the two formerly mentioned models to follow: William Crawford's or Sir William Crookes's. William Crawford (Crawford, 1916) stated:

Before we can expect anything worth having in the way of results we must have the following set of conditions as nearly perfect as possible: (a) A very powerful medium. (b) A circle of sitters supporting the medium. (c) The medium and sitters to be imbued with the seriousness and wonder of the phenomena presented to be linked together, as it were, by the one object of getting the most out of the phenomena for the common good. (d) A band of operators who have the same objects in view as those mentioned in (c). Mischievous operators or others who will not or cannot co-operate with

the experimenter are useless. (e) The phenomena must not be produced spontaneously, but must be under command. (Crawford 1916:3)

Later he adds the necessity of working in the dark or with a soft red light. The word “operators” included in (d) is a euphemism for guiding spirits who help the investigator.

On the other hand, William Crookes examined the classic methods of investigation, warning about some erroneous beliefs (Crookes 1874):

One is that darkness is essential to the phenomena. . . . Another common error is that the occurrences can be witnessed only at certain times and places. . . . A third error is that medium must select his own circle of friends and associates at a *séance*; that these friends must be thorough believers in the truth of whatever doctrine the medium enunciates. (Crookes 1874:80-81)

Having in mind that Ariel shared our parapsychological hypothesis about the facts, and knowing how difficult it would be to rebuild the group, we decided to start a new series of meetings with Ariel as the unique person to investigate, avoiding the mistakes enumerated by Crookes and following at least the first and last conditions mentioned by Crawford. Between August and November 2013, a total of 10 sessions were conducted. They were numbered #16 to #25. All of them were held in conditions of good illumination, including some with the natural light that entered through the windows. The amount of light was not measured with an instrument, but it was enough to read a book by without difficulty. The place was the same used by the previous group, with highly polished floor tiles. Also, the table was the same one from the fourteenth meeting. The security conditions to prevent conscious and unconscious fraud were dramatically improved, as there were only three people in the room, two of them observers.

Our main expectations consisted of achieving the total levitation of the table and the contactless movement of tiny objects. We employed diverse techniques, among them suggestion, meditation, and relaxation. None of these allowed us to witness any contactless movement. Nevertheless, the technique that produced the best results was hyperventilation. Ariel accidentally discovered that breathing more deeply and frequently was a shortcut to accelerate the appearance of the phenomenon, so he began to do it when it seemed appropriate.

However, to elicit the phenomena, it was necessary to lessen Ariel’s fear, which stemmed from the terror caused by the “poltergeist” episodes he had experienced as a boy. He told us that with the first creaks of the table previous to the movements, and especially when the table became uncontrollable, he experienced the same anguish that he did then, fearing



that the phenomena would follow him to his house. In this conflict between his rational enthusiasm to go ahead with the work and his fear of repeating scenes from the past, it was impossible for us to have a determining influence on him, considering the scarce available time. Another half-completed objective was the attendance of remarkable people. Only the museologist Fabiana Savall and the psychologist JCA attended meetings #18 and #19, respectively. Ariel performed differently in each session, although in the first one—perhaps because he and the guest knew each other—he was able to obtain the usual results, lifting one leg of the table or moving the table ostensibly at will, with his hands on the board. However, in the second session, with the psychologist JCA, Ariel was anxious about the visitor's opinion of him and he could only produce tiny movements.

In subsequent sessions, however, he made important advances in controlling his PK at will. He could move the table from one place to another when he was asked to do so. One of his big achievements was “learning” how to raise the table leg closest to him, the only one impossible to raise with his muscular force (the other legs could be raised if he exerted a strong downward pressure on the table). Also, but with a minor intensity, he was able to simultaneously levitate 2 of the 3 legs of the table. He repeated these actions several times, with levitations that lasted from a few seconds to more than 7 minutes. During this second part of the experiment, one of us recorded it on video while the other focused on Ariel's feet and legs, asked him to move his hands, to leave only one hand touching the table, and to touch the table only with his fingertips. Lastly, the second investigator also passed his hands through the free space between the raised leg and floor, and other similar alternatives, to rule out any kind of artifice. When necessary, the organizers commanded Ariel to lift the sleeves of his shirt or to move his hands when the table leg was raised, to inhibit any possibility of fraud. Also, the organizers consistently watched Ariel's feet and legs avoiding any contact with the table.

On one occasion the organizers successfully repeated the inclusion of barriers (cotton tablecloth) between his hands and the table. We also repeated raising the table to the stepped place, as had happened in the first part of our investigation. During meeting #21, Ariel tried to levitate a little 4-legged table of wood, set atop a board of 20 by 30 cm, which was placed on the big table. (The reasoning which led us to design this experiment was: If he could raise the corner of a big table, weighing 4 kg, a third of the total weight, then he should be able to completely levitate a little table with a weight of less than 4 kg.) Ariel would also not have to stoop to place his hands on the small table. But he was able only to raise 2 or 3 legs of the little table. Finally, the goal of improving the documentation of

the phenomena, compared to the first sessions, was achieved. We obtained excellent pictures, sometimes with two video cameras simultaneously, from different angles and distances.

### **Final Considerations**

This is only a preliminary report. We still have to analyze in detail the records we obtained. During the 25 meetings, more than 25 hours of good quality video recordings were obtained, as were audio recordings of all of them and some photographs. The methodical and meticulous revision of these documents might give suggestive evidence about the behavior of PK. These records are available to investigators who may want to review or analyze them. I decided not to make them public in order to protect the privacy of some people who appear in them and to avoid someone who might upload the material to the Internet or whimsically edit them to show erroneous effects. On the other hand, the personal interviews with Ariel are also helpful. In these, he details some aspects of his personal life story that may be relevant to other special people. He relates a series of subjective phenomena that he had forgotten about or thought unimportant but realized their value during the past year.

The organizers have decided to complement each other by following two strategies, both generously supported by Ariel: Juan Corbetta is conducting a qualitative investigation, making an ethnography of the table-turning groups, focused on the ritual aspect, in order to establish the reach of these practices and its influence on the beliefs of the attendees. In my case, I gathered a new group at the Instituto de Psicología Paranormal of Buenos Aires (Institute for Paranormal Psychology) with its director Alejandro Parra and Darío Burgo, an electronics engineer. We plan to organize a long-term investigation and reliably document the phenomena, and perhaps replicate some macro PK experiments conducted in the past, adding new strategies, methods, and instruments to elucidate the causes and the variables associated with the phenomena.

I can say that we have developed a program of macro PK without the atavistic suggestions, such as working in the dark, to which others, such as the Scole and Felix groups, adhered. The Scole Group investigators (Keen, Ellison, & Fontana 1999) did not employ adequate controls in order to confirm or deny the phenomena demonstrated in darkness. The investigation of the Felix group by Stephen Braude (2014) and Michael Nahm (2014) has been deterred by accusations of fraud by its “medium” Kai Mügge. We worked with good illumination, and included some skeptical participants. We encouraged productive discussion of ideas. Furthermore, we found remarkable positive correlations in moments of higher hilarity, perhaps



produced as an emotional reaction when faced with the anomalies we were witnessing. I think that laughter has the same function as the religious anthems loudly sung by the Spiritists of the 19<sup>th</sup> century, with the same good results.

Ultimately, I hope the more important moral would be to contradict—or at least challenge—the collective impression about the impossibility of finding special people or completing successful investigations with ostensible phenomena. Probably what really happened is that investigators stopped looking for these people or for these ostensible phenomena. It is important to remember in this respect what Kenneth Batcheldor wrote:

You can set the best trap, but it does not mean that the rabbit will soon appear. You must be patient and wait. (Batcheldor 1979:6)

We lament the extinction of rabbits when in fact we have stopped putting out traps to hunt them, or at least we are using bait intended for another prey.

### Note

<sup>1</sup> Ariel's son was born on February 21, 2015.

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ESSAY

## The Unbearable Fear of Psi: On Scientific Suppression in the 21<sup>st</sup> Century

ETZEL CARDEÑA

Lund University, Lund, Sweden

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**Abstract**—This paper describes various examples of blatant attempts to suppress and censor parapsychology research and those who are doing it. The examples include raising false accusations, barring access to journals, suppressing papers and data, and ostracizing and persecuting scientists interested in the topic. The intensity of fear and vituperation caused by parapsychology research is disproportionate even to the possibility that the psi hypothesis could be completely wrong, so I speculate on the psychological reasons that may give rise to it. There are very few circumstances in which censorship might be appropriate, and the actions by parapsychology censors put them at odds not only with the history of science but with the history of modern liberal societies. Appendix 1 is an Editorial censored by the then-editors of the Journal *Frontiers in Human Neuroscience*.

*. . . the sole end for which mankind are warranted, individually or collectively, in interfering with the liberty of action of any of their number, is self-protection . . . to prevent harm to others. His own good, either physical or moral, is not a sufficient warrant. He cannot rightfully be compelled to do or forbear because it will be better for him to do so, because it will make him happier, because, in the opinions of others, to do so would be wise, or even right.* —John Stuart Mill, *On Liberty* (1869/2010:10)

One can only pray, even if in a secular prayer, that as the great scientist and philosopher Giordano Bruno had his tongue and palate pierced by an iron gag before being burned alive in 1600 by the inquisitors for daring to speak his mind, he could sense the “every human love” in the midst of the “pedantic boring cry” of his executioners, as W. H. Auden would wish us all in his 1937 poem *Lullaby*. In some countries (and the *extraordinary rendition* program instigated by the USA and in which 54 other countries colluded to extrajudicially abduct and sometimes torture detainees suggest how few, cf. Fisher 2013), blissfully, the instruments of torture have rusted and are now only curiosities in morbid museum collections. But the itch

to silence those whose opinions we disagree with, applied centuries ago against scientists of the stature of Bruno, Galileo, and others, has spread, ironically, to scientists themselves, and there are few cases as blatant as those involving the topic of parapsychology. In this paper I will discuss how most cases of scientific censorship ultimately betray a profound distrust of the scientific process, cover briefly a few noticeable cases going into detail about one, and append an Editorial censored by two editors of *Frontiers in Human Neuroscience* (see Appendix 1).

### **To Censor or Not to Censor?**

I will not cover in this paper the various ways in which showing an interest in parapsychology is hazardous to one's professional health, including the almost nonexistent funding opportunities, the hurdles in getting an academic job or, having obtained it, in advancing, or the constant swaying to avoid the constant, and most often uninformed and groundless, barrage of critical darts. There are already general discussions on the intellectual suppression of identified groups and alternative positions by those with power and a vested interest (e.g., Martin, Baker, Manwell, & Pugh 1986), including the specific case of parapsychology (e.g., Hess 1992, McClenon 1984). My aim here is much more modest, to cite some recent examples of attempts to suppress parapsychology and to discuss how these attempts betray the honor of the entity they outwardly seem to want to guard: science. But let me start with the necessary question of whether censoring or suppressing scientific discourse is ever justifiable.

The answer for me is an unequivocal "yes," but it comes with a very strong caveat. There are only two circumstances under which I would endorse censorship. The first one is when scientific knowledge of, say, how to weaponize a virus (cf. Saey 2012) or easily build a weapon of mass destruction could (and most certainly would) be used by those wanting to destroy others. I do not trust governments either with this power, but would not want to multiply the problem by making the capacity to inflict enormous damage as accessible as an Internet connection. In this case, the risks would greatly outweigh the benefits of open knowledge. This argument is just a reiteration of the quotation at the beginning of the paper by that great champion of liberty John Stuart Mill.

The other circumstance I can think of would be when a communication incites others to violence and provides specific information that would likely culminate in someone being injured or worse, as was done in Rwanda in 1994 with radio calls to massacre the Tutsi minority and moderate Hutu. This is not a type of communication that we likely would run across in a scientific publication, but there are exceptions such as the rhetoric by Nazi

eugenicists and doctors to “cleanse” the German body of the “vermin” of Jews, the mentally disabled, and other groups they detested (Lifton 1986).

How does parapsychology fare with respect to these two proposed criteria? With respect to the first, leaving aside fictional movies and books of extraordinary and even deadly psychokinetic powers such as *Scanners* by David Cronenberg or, more gently, *Matilda* by Roald Dahl, there is no evidence that the knowledge we have about psi phenomena would allow anyone to develop nefarious or even deadly powers (but see Braude 2008, who considers that possibility as a trigger for the fear of psi). Psi phenomena were investigated secretly by the US and the USSR governments for bellicose ends (May, Rubel, & Auerbach 2014), but they evidently could not be harnessed in this way (otherwise, I am quite sure, we would have already had some evidence such as political or military leaders of an antagonist country suddenly having their heads explode or their hearts stop without any apparent reason). There is research evidence for a *small* direct effect of intention on living beings (Schmidt 2015), which of course could travel on the wings of nasty intentions (see Dossey 1997), but nothing to make any non-paranoiac lose sleep.

A quaint version of the idea that publishing parapsychology might bring about terrible events is exemplified by the bombastic opinion of cognitive scientist Douglas Hofstadter, who wrote that a peer-reviewed set of studies finding support for precognition (Bem 2011) would have implications that “would necessarily send all of science as we know it crashing to the ground . . . [and] spell the end of science as we know it” (<http://www.nytimes.com/roomfordebate/2011/01/06/the-esp-study-when-science-goes-psyched/a-cutoff-for-craziness>). He also remarked that psi phenomena would go against the “laws of physics” despite not being a physicist, and called parapsychology researchers “crackpots” (the itch to insult may be even more preemptory than that to censor). In contrast, actual physicists including University of London cosmologist Bernard Carr and Lawrence Livermore Lab physicist Henry Stapp have developed models that accommodate psi phenomena within physics, with neither of them claiming that if their proposals are right science will “go crashing in flames” (cf. Kelly, Crabtree, & Marshall 2015). In their support of research on parapsychology, they have followed physicists of the stature of Bohm, Bohr, Einstein, Planck, and Pauli, who either proposed physics models of psi phenomena or were at the very least open to its scientific inquiry.

Not as apocalyptic in their rhetoric, but reminiscent of the deadly extraterrestrial parasite in the film *Alien*, Torbjörn Lundh of the Swedish Chalmers Institute organized a symposium with the title of “Pseudoscience: An innocent game or a serious parasite” (<http://www.chalmers.se/insidan/>

[SV/om-chalmers/moten/fakultetsradet/fakultetesradets](#)) in which Magnus Fontes “debated” a study on telepathy we conducted in Lund (Marcusson-Clavertz & Cardeña 2011) without even informing, much less actually debating, the authors of the paper.

Hofstadter also called to censor outright any study finding support for psi because “you believe deeply in science and this deep belief implies that the article [finding evidence for psi] is necessarily, certainly, undoubtedly wrong.” Along similar lines, David Helfland, an astrophysicist who also commented on the Bem paper, wrote that publishing research on psi “should be seen for what it is: an assault on science and rationality,” and that “A peer-reviewed article must contain sufficient information for another scientist to replicate the experiments. The ESP study fails this test” (<http://www.nytimes.com/roomfordebate/2011/01/06/the-esp-study-when-science-goes-psychic/esp-and-the-assault-on-rationality>). Helfland himself seems not to have any precognitive abilities since a meta-analysis of 90 replications of that study has been conducted (Bem, Tressoldi, Rabeyron, & Duggan 2014). Although not all the replications have been successful, as a whole they have supported the original study. Unfortunately but unsurprisingly (see below), some journal editors have summarily declined to publish it, although it is currently under review.

Let me discuss now some of the implications of the attitude by Helfland and Hofstadter, shared by a number of opponents of parapsychology. First, they seem to assume that science implies a particular metaphysical belief, rather than a method to reduce personal biases, account for likely alternative explanations, and systematically test hypotheses. It might shock them to know that one of the main founders of the scientific method, Francis Bacon, took precognition as a given (1620/1960), and that many Nobel prize-winners and other eminent scientists have held a very different metaphysical view than the current *en vogue* materialist reductionism. And for all of the added knowledge science has brought, throughout history various philosophers have questioned whether we can have an ultimate and definitive knowledge of nature. For instance, one of the most influential philosophers of science, Karl Popper, proposed that science cannot assert something with ultimate authority but advance a model and evaluate whether it can (at that point) be refuted by the evidence proffered (Popper 1963). Helfland and Hofstadter claim a certainty about the nature and interpretation of the “laws of physics” that physicists themselves argue about. From cosmology to quantum mechanics (Gleiser 2014), not to mention the question of how consciousness relates to a putative external reality (Kelly, Crabtree, & Marshall 2015), there are intrinsic limitations to how much we can know given our epistemological limits and the nature of nature. Probably the most

we can say is that models of reality are just descriptions of regularities based on specific ways of measurement (d'Espagnat 2006).

For the sake of argument, let me at this point grant momentarily to the censors of psi their assumption that they are completely and eternally right, and that all people claiming and finding support for psi phenomena are “crackpots,” crazies who lack rationality, even though more than 25 of them have received Nobel prizes, in addition to other equally eminent supporters in philosophy and other disciplines both in the past and in the present (Cardeña 2014a). What would be then the danger of not censoring research on psi? If the critics are right, sooner or later parapsychologists will be shown to have been deluded, idiotic, or part of a nefarious conspiracy whose ultimate goal would seem to be to damage their own professional careers. Would analyzing their results, or even conducting research to ultimately show their misguided ways dry the funding of Hofstadter, Helfand, and company? No, the vast majority of funding agencies will not even consider psi research in their remit (Hess 1992). Would publishing psi research drive Professors Helfand and Hofstadter out of their cushy academic positions? Again, no, no one in the field even remotely believes that they will be taken by the psi mob to be guillotined. Rather the opposite, since the anti-parapsychology “skeptics” (not actually skeptics who question other and their opinions, but who follow their beliefs dogmatically, see Cardeña 2011) have been very active and have, for instance, gained the upper hand at editing wikipedia entries and restricting access to TED.com (Technology, Entertainment, Design) to fully conform to their beliefs (see below). Or would a belief in the validity of psi drive crowds of graduate students into academic suicide? Not so either, since the majority of students who have gotten their advanced degrees from, say, the Koestler Parapsychology Unit, have continued to further academic work, despite the additional hurdles they might have had to endure (Carr 2008). Furthermore, at least one of them became a well-known critic of parapsychology (Richard Wiseman), showing that an education with a concentration on parapsychology allows alternative perspectives. And as I momentarily conceded, since psi phenomena will be shown to be completely false, neither science “as we know it” nor the universe will come crashing down.

So here we come to a crucial point. The problem with the parapsychology censors is not that they believe too much in science, but that they do not believe in it enough. As another commentator to the Bem study, Stanley W. Timble, pointed out, that the way science should work is through critical but “open inquiry . . . [and] Disapproval of an idea does not disprove it” (<http://www.nytimes.com/roomfordebate/2011/01/06/the-esp-study-when-science-goes-psi/how-open-inquiry-works>). Bill McKelvey



also mentioned one of the virtues of science, “A self correcting process” (<http://www.nytimes.com/roomfordebate/2011/01/06/the-esp-study-when-science-goes-psychic/how-open-inquiry-works>), although one in which valid ideas may be excoriated before being accepted as a new discovery.

As for the second circumstance I mentioned in which I would justify censorship, I have not found a single parapsychology article inciting others to engage in violence, although of course dogmatism and nastiness are probably as prevalent among parapsychology researchers as among other groups (cf. Cardeña 2011). If anything, it is parapsychology researchers who have suffered censorship and unjustified persecution. For instance, the editor of the AAAS journal *Science* in 1975, Philip Abelson, and the AAAS executive officer, William Carey, gave Theodore Rockwell the runaround during a few years when the latter inquired about publishing psi research in the journal (McClennon 1984). Getting more personal, physicist John Wheeler falsely stated in a 1979 AAAS meeting that parapsychology researcher J. B. Rhine had committed fraud as a postdoctoral assistant, although he was later forced by the latter to publish a fairly veiled retraction (see Cardeña 2014b).

### Some Recent Examples of Censorship

The itch to suppress parapsychology work was very present at the end of the 20<sup>th</sup> Century and remains unabated in the 21<sup>st</sup> Century. Here are some brief examples followed by a longer discussion of one case.

1) A National Research Council (NRC) report on parapsychology (Druckman & Swets 1988) published a damning conclusion about it, ignoring or suppressing favorable reviews commissioned by the Council, including those by Harvard professor Robert Rosenthal and University of California professor Jessica Utts (Palmer, Honorton, & Utts 1989). The NRC report had an important negative effect on funding for psi research.

2) In 1993, after Lawrence Livermore lab physicist Henry Stapp had a paper accepted in which he discussed a successful parapsychology experiment he had carried out, he was asked by the Acting Editor of *Physical Review* to delete all data from his paper. Benjamin Bederson, Sr., Editor-in-Chief of the Journal, also chastised Dr. Stapp for even having sent his paper (Kaiser 2011).

3) Brian Josephson, Nobel prize-winner in physics, had his invitation by physicists Antony Valentini and Michael Towler to a conference on the work of David Bohm rescinded for a while when they found out about his positive attitude toward parapsychology (Reisz 2010). Ironically, Bohm himself had discussed how his model of reality could be integrated with psi phenomena (Bohm 1986).



4) A paper describing empirical support for precognition by eminent psychologist Daryl Bem (2011), published by a top-notch journal after the usual peer-review process, was immediately attacked on the Opinion page of *The New York Times* by some contributors. They asked psi publications to be suppressed, as I described above (<http://www.nytimes.com/roomfordebate/2011/01/06/the-esp-study-when-science-goes-psyhic>).

5) This is an example of individuals who, lacking themselves the power to censor, nonetheless seek to pressure those who have that authority. The Lund University employee magazine *LUM* published an article in 2012 on one of my peer-reviewed research studies in which we obtained three moderate-to-strong significant correlations between our measure of psi phenomena and 3 other variables (Marcusson-Clavertz & Cardeña 2011). Almost immediately a group of 9 Lund University faculty, most of them in the hard sciences (Bertil Halle, Germund Hesslow, Gunnar Karlström, Sven Lidin, Georg Lindgren, Christer Löfstedt, Dan-Eric Nilsson, Olov Sterner, and Bengt E. Y. Svensson) but none of them, to the best of my knowledge, having ever published a peer-reviewed paper (either for or against) on parapsychology research, wrote a letter to the media. In it, they stated that “paranormal phenomena are a chimera,” misrepresented the goals of our study, contrasted rationality, reasoning, and integrity with our research, and made a not-so-veiled threat in their mention that a researcher in Lund who had made a mistake had to leave his/her post (<http://www.svd.se/pseudovetenskap-sprids-okritiskt>). Mattias Collin, another Lund faculty member who has not done any work in psi either as far as I can tell, later added his voice, showing that he had absolutely no idea either of the experimental controls of the original article’s research or the topic area by criticizing, among other things, our recruitment of participants who believe in psi phenomena (<http://www.sydsvenskan.se/lund/forskare-rasar-mot-kollega/>). Fortunately, the Editor of *LUM* (Maria Lindh; <http://www.sydsvenskan.se/lund/forskare-rasar-mot-kollega/>), then Chair of the Department of Psychology (Per Johnsson; <http://www.sydsvenskan.se/kultur--nojen/ett-decennium-i-vetenskapens-gransland/>), the College Dean (Ann-Katrin Bäcklund; <http://sverigesradio.se/sida/artikel.aspx?programid=1637&artikel=5330277>), and then-President (Per Eriksson; <http://www.svd.se/vi-studerar-tomtar-och-troll-ocksa>) did not take the bait, and all publicly supported our work and our right to publicize it.

6) In 2013, an anonymous (one should always suspect mischief when someone hides behind a curtain) TED science board deleted a talk by psi-proponent Rupert Sheldrake given at the TEDx Whitechapel, and relegated it to a much less frequented TED blog (<http://www.tricycle.com/blog/ban-rupert-sheldrakes-ted-talk>). One of the apparent proponents of the ban, Jerry

Coyne, also tried to have Sheldrake disinvented to an address he was scheduled to give and wrote favorably about a “Guerrilla [sic] Skeptics on Wikipedia (GSoW)” group who “police” wikipedia to delete any positive mention of psi and “pseudoscience” (<http://www.newrepublic.com/article/115533/rupert-sheldrake-fools-bbc-deepak-chopra>). By the way, the webpage for GSoW only provides 3 full names for their 13 contributors, none of whom seem to have advanced degrees or peer-reviewed publications according to the information on their webpages.

### A Case Study

**First Act.** This is a case I followed closely both as an editor and an author affected by censorship. It all started with an invitation by *Frontiers in Human Neuroscience (FHN)* to propose a special topic for the Journal. Enrico Facco, Christian Agrillo, and I proposed the subject of *Non-ordinary Mental Expressions (NOME)*, which we defined as

experiences and procedures that seek to change short- or long-term psychological processes. . . . We aim to reappraise the importance of NOME and its implications for the mind–brain–world relationship. . . . The editors will solicit original research contributions as well as theoretical papers, such as reviews, mini-reviews, and theoretical discussions,

and mentioned that we would invite not only neuroscientists, psychologists, and psychiatrists, but also philosophers, anthropologists, and other professionals (<http://journal.frontiersin.org/researchtopic/1666/non-ordinary-mental-expressions>). Thus, the topic *FHN* accepted included different types of papers from diverse disciplines discoursing on NOME and their implications for mind–brain relations.

As special topic editors, we had been, without a problem, accepting or rejecting proposals, sending submissions to reviewers, accepting some papers and rejecting others, and were at the stage of processing other submissions after authors had sent their abstracts months earlier. Then John J. Foxe became one of the *FHN* Chief Editors and the problems started. We suddenly heard from him, from the other Chief-Editor, Hauke R. Heekeren, and from *FHN*'s office, about four different papers:

1) The “Editorial Office” of *FHN* wrote that a paper that had been reviewed and accepted by two reviewers and a Topic Editor “does not comply to [sic] general ethical standards . . . this manuscript cannot be accepted for publication.” They mentioned that a manuscript with the same name had been submitted and rejected before the NOME call for papers. We replied, to no avail, that the paper that had been rejected before our call had

a different authorship and content than the one that had been accepted by the two reviewers and the topic editor.

Three other papers were rejected by the Chief Editors even before the review process had been completed:

2) A paper on out-of-body experiences was rejected by the Chief Editors because “the findings and interpretations forwarded in this manuscript are flawed and they cannot be relied upon as the basis for future work. The authors have not adequately discussed biologically plausible mechanisms for the effects they report. The interpretation of the effects violates simple principles of parsimony and indeed, the basic laws of physics as they are currently understood.” It bears mentioning that neither of the Editors’ final degree is in physics and that they did not provide any explanation as to why the paper’s proposed findings and interpretations were flawed.

3) A paper on near-death-experiences (NDE) and cardiac arrest was rejected by Dr. Foxe because

The quality of the article is substandard and below the generally accepted standards of the community . . . . Your paper is not within the scope of our journal which is a venue for work reporting data regarding neural function, which this is clearly not.

The accepted call for NOME stipulated that theoretical discussions on mind–brain relations were within its purview, and it would be difficult to come up with a topic that more clearly challenges a reductionist–materialist account of mind–brain interactions than the complex mental experiences of NDE, apparently occurring during the physiologically impaired condition of cardiac arrest.

4) Finally, a hermeneutical analysis of mysticism was rejected by Dr. Foxe who wrote that “I am taking over the editorial process on this paper at this juncture because it is clear to me, as it should have been to you, that this paper has no place in a journal such as ours.” Prima-faciae, however, the topic of the paper was within the remit of the call for papers accepted by *FHN*.

Lucia Brandi, manager of *FHN*, also wrote to us that *Frontiers* had “encountered a number of anomalies related to some of the manuscripts. . . . Some of the manuscripts were found to have received very light reviews,” but did not specify what the anomalies were or which papers had been given light reviews. This is particularly ironic considering that the Chief Editors edited a paper by D. Samuel Schwarzkopf (one as reviewer, one as editor) and accepted within a week of submission (see <http://www.ncbi.nlm.nih.gov/pmc/articles/PMC4034337/>), that criticized a paper in support of psi findings published under the NOME call. In contrast, we did not come even

close to accepting any paper within a week of submission. A couple of short opinion papers were accepted about a month after their submission, and data-driven papers took months before being accepted.

Enrico Facco and I wrote complaining that the fact that *Frontiers* had suspended publication of one paper and review of three others violated its own arbitration guidelines, which stipulated that

Should a dispute arise that threatens to reject an article, the author may trigger arbitration. In the first place, the associate editor will arbitrate and involve all review editors in a discussion aimed at resolving the dispute. If a resolution cannot be agreed upon, the specialty chief editor is alerted and can opt to bring in additional review and associate editors for consultation  
...

The arbitration process was not initiated by *FHN* despite our request nor did they provide any specifics as to how papers had “anomalies” or had received “very light reviews.” We also commented that the Chief Editors had had access to the abstracts of the censored papers for months and should have intervened, if at all, before having the authors waste their time working on a paper they would later reject.

*FHN* Editorial Director Costanza Zucca, who left the journal shortly afterward, replied to us after a number of prompts, the final one involving a lawyer, that

I truly regret that you found the tone of the communication by our editorial staff offensive or inappropriate, and I apologise for any offence, which I assure you was unintended; the intention of our staff was to remain respectful and professional in communicating with you . . . we will certainly review these procedures to avoid any further misunderstandings in the future.

Nonetheless, an arbitration process was never carried out, the originally accepted paper was censored, and the review process of the other three was suspended.

**Second Act.** Despite the censorship just mentioned, we were able to publish 13 papers (which had received more than 140,000 views on August 13, 2015), and I requested that *Frontiers* produce an e-book, as advertised in their special topics information. Dr. Zucca’s successor, Fred Fenter, gave the green light, and I was told that I should write an Editorial presenting the collection of articles.

After I submitted the Editorial (published in the second part of this paper as Appendix 1), Dr. Heekeren asked us to add some references to a statement and to make two other changes. I added the references, but the second change requested showed that he had not even looked at the sets of

papers since he mistook the initial paper (“A call for an informed . . .”) with the general call for papers for the NOME topic. He also asked for a revision of our sentence on a paper about psi research:

The paper produced various responses and counter-responses, some of them illuminating, others, like claiming that “extraordinary claims require extraordinary evidence,” being unhelpful clichés (see Franklin, Baumgart, and Schooler 2014, for some valuable suggestions). What I would deem acceptable is to change it to “Notably, the paper produced various responses and counter-responses” and then give references to these different reactions, in the spirit of Frontiers’ call for openness and transparency. [emphasis added]

We deleted the sentence, but that was not enough. He demanded that the paper by Dr. Schwartzkopf that he and his Chief Editor had edited or reviewed within one week be referenced:

It will be important to qualify this statement by indicating that there is deep skepticism about this work. Please cite the commentary by Schwartzkopf in doing so <http://journal.frontiersin.org/Journal/10.3389/fnhum.2014.00332/full>.

We made the change and referred to the main points made in the paper by Schwartzkopf, along with the response by the authors of the criticized paper, which they had posted on arxiv.org. We did not take any side on that debate but asked the reader to become informed on the issues for him/herself. Dr. Heekeren, however, did not want anything to be added other than the criticism he and his Chief Editor had helped publish and wanted the replies to the Schwarzkopf paper out, writing that “Your revision would turn at least the final part of your piece into a commentary/opinion paper, which is not acceptable for an editorial according to our policy.”

At this impasse I contacted Dr. Fenter since it was obvious that Dr. Heekeren would only accept a gerrymandered Editorial that toed his ideological line. Dr. Fenter (with whom we had no problem) wrote back that “The Editors-in-Chief of the Journal have expressed their clear opposition to the publication of the Editorial in any of its edited versions” and he proceeded to publish the e-book without the Editorial. I think that the actions and words of Drs. Foxe and Heekeren speak more clearly than any additional comment I could make about them, but this time around the censors will not have a complete victory since the *JSE* has generously agreed to publish the original Editorial (with minute wordsmithing in a few phrases) at the end of this article (see Appendix 1).

### Coda

Whence comes the intolerance and vituperation that some authors and editors pour on parapsychology? As Tart (e.g., 1982) has remarked, its level of emotionality hints that this is not merely a matter of lack of knowledge of the field or intellectual disagreement about the evidence. After all, we all read about findings and theories that we likely know nothing or very little about yet intuitively disagree with, but we do not then singly or with our similarly thinking pals write letters to newspapers denouncing the authors and/or try to have them kicked out from their universities, associations, conferences, or whatever. Most likely, we shrug our shoulders and read about something else. This is not what happens with the psi-censors, though. They seek to exile the dissenters from journals or institutions, catastrophizing that unless they do so science or rationality will perish. One part of the explanation, I think, is the replicated finding in parapsychology that people who tend to believe in psi phenomena actually perform significantly better in controlled psi experiments than their counterparts who do not believe in psi (i.e. the “sheep–goat” effect, see Cardeña, Palmer, & Marcusson-Clavertz 2015). Thus, belief in psi is, to an extent, a self-fulfilling prophecy: Those who believe in it are more likely to have valid corroborations than those who do not. The egocentricity of knowledge, which has been likened to a totalitarian system in which one’s perspective is easily seen as the only valid, “rational,” or “reasonable” explanation (Greenwald 1980) may then make the censors assume that their view is the only reasonable one. The scientific method and process, not to mention the history of science, at its best should ameliorate this entrenched bias.

This might explain why some critics may be more likely to assert that psi phenomena are “hogwash,” but it does not explain their vehemence. For that, I think, additional factors must be considered. I think that a contributing factor is that research on parapsychology is seen as so emotionally (and factually) threatening because it suggests that “things are not as they seem,” or at least as the censors believe they are. Even while fully committed to their (limited) view of science, the censors must realize every day that they cannot control, predict, or even come close to fully understanding their lives or even topics of research, no matter how hard they may hold to their scientist toehold. As a mechanism of defense to avoid contemplating that void of understanding, they are then likely to try to “defend” their (uncertain) view of reality against any outside contender. If I am correct, the justification for their censorship is thus not that different from that used by inquisitors to defend a faith whose evidence was also challenged by other opinions or everyday events.

As mentioned earlier, Trimble and McKelvey are not afraid of psi research because they trust that science, if pursued openly, will in the end self-correct. The censors, on the other hand, ultimately lack confidence in the scientific process and assume that they should dictate what can and cannot be researched by others. More generally, they distrust freedom of expression. John Stuart Mill wrote that the truest (or best, by other criteria) ideas come from the free competition of ideas in public discourse. This value has been fundamental not only to the development of science but of liberal societies, and has been endorsed by a plethora of thinkers including Voltaire, Thomas Jefferson, Anton Chekhov, Isaiah Berlin, Karl Popper, Paul Feyerabend, Vaclav Havel, and many others. It is thus ironical that some scientists would rather follow the model of the censors of yore than that of the builders of the freedoms they enjoy in their everyday lives. Have they already forgotten that not so long ago they were on the other side of the gags for not accepting a particular metaphysical account?

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## APPENDIX 1

### Introduction to Non-Ordinary Mental Expressions

BY ETZEL CARDEÑA AND ENRICO FACCO

[Unpublished Editorial written as an Introduction to the ebook, *Non-Ordinary Mental Expressions* edited by Etzel Cardeña and Enrico Facco published by *Frontiers in Human Neuroscience* at <http://journal.frontiersin.org/researchtopic/1666/non-ordinary-mental-expressions>]

The term *non-ordinary mental expressions* (NOME) encompasses unusual or anomalous experiences, and their related neuropsychological processes and induction procedures. Of course what is considered unusual has varied across time and cultures. Our use of non-ordinary does not assume pathology and includes sophisticated and positive mental activities including some forms of creativity, intuition, and spirituality. Foundational figures in

psychology of the stature of William James, Pierre Janet, and Sigmund Freud exemplified how researching NOME was essential to understanding the mind. For instance, James discussed alterations of consciousness as potentially having practical uses and providing alternative epistemological pathways into our understanding of mind and its relation to reality, and he did not consider these phenomena as necessarily odd or pathological (James 1902/1958). That NOME do not necessarily reflect psychosocial or neurological dysfunctional processes has been borne out by research showing that spontaneous and induced NOME can have long-term positive effects (e.g., Cardeña, Lynn, & Krippner 2014, MacLean, Johnson, & Griffiths 2011).

James and like-minded contemporaneous authors would have been dismayed that phenomena so consequential to religion, philosophical thought, social movements, arts, and individual lives (Cardeña & Winkelman 2011) were mostly ignored by academic psychology during much of the 20<sup>th</sup> Century. Nonetheless, the study of NOME seems to have a current resurgence, partly underpinned by studies of correlated brain dynamics. Something to bear in mind is that although neuroscience studies of NOME may illuminate Aristotelian material and formal causes, they often confuse them with efficient (the proximate source of the experience, e.g., a potentially independent or partly independent set of relations in reality) and final (does the experience serve a purpose, evolutionary or otherwise?) causes. Furthermore, some scientists have proscribed *by definition* areas of NOME research because they grate against their metaphysical positions, without due consideration of the relevant empirical research. Among many examples of this attitude are physicist John Wheeler's attempt to eject the Parapsychological Association from the AAAS while falsely claiming that parapsychologist J. B. Rhine had committed scientific fraud (Cardeña 2014) and cognitive scientist Douglas Hofstadter's plea that the Editors of the *Journal of Personality and Social Psychology* should have just ignored a study supporting the psi hypothesis to "prevent the end of science as we know it" (Hofstadter 2011, see also Cardeña 2011). Therefore, we initiate this e-book with an article co-signed by 100 academics calling for an open, informed study of all aspects of consciousness, including the psi hypothesis (see below), followed by a set of articles centered on procedures that may induce NOME.

A reliable finding in *hypnosis* research is that among individuals responsive to hypnotic suggestions the latter will influence brain activity and the experience reported by participants in accord with the specific verbalizations provided (Oakley and Halligan 2013). That is, however, a different question from whether a mere hypnotic induction (which typically

involves instructions to disregard extraneous concerns and enter a state of hypnosis) produces an experiential and neurological distinct state of consciousness (Cardeña, Jönsson, Terhune, & Marcusson-Clavertz 2013). In their article, Jamieson and Burgess describe EEG indicators of a putative hypnotic state independent of specific suggestions. Their results show that among high but not low hypnotizables a hypnotic induction produced an increase in the theta imaginary component of coherence (iCOH), and a greater decrease in beta1 iCOH. The authors conclude that hypnosis produces a qualitative change in the organization of brain control systems in high hypnotizables. These results should be replicated taking also into consideration group differences within those very responsive to hypnosis (Terhune, Cardeña, & Lindgren 2011).

In a study that employed hypnosis to increase the amount of details recalled, Palmieri et al. conclude that memories of *near-death experiences* (NDE) are similar to those of demonstrably real events in terms of detail, self-referentiality, and emotional information, but dissimilar to those of imagined events such as dreams. Their EEG analyses also revealed that NDE memories were associated with theta and delta bands. The authors conclude that, at a phenomenological level, NDE memories are different from imagined ones and are stored as episodic memories of events experienced in a NOME.

In another study, Charland-Verville et al. compared the characteristics of “NDE-like” experiences not related to a life-threatening event with those associated with pathological coma (anoxic, traumatic, or other), or “real NDE.” Overall, the two types of experiences did not differ in NDE features’ intensity or content, with a sense of peacefulness being an almost universal aspect (only 1% of participants mentioned a dysphoric experience).

To further elucidate one of the features of NDE, *out-of-body experiences* (OBE), Greyson et al. evaluated the phenomenology of 100 seizure disorder patients, 55% of whom could describe their seizure-related experiences (including dysphoric emotional states, episodes of déjà vu, confusion, flashing lights, hearing music, smells, paresthesias, and headaches). Seven individuals also recalled sporadic OBE along with time distortion, but without other characteristics of NDE such as a sense of revelation, joy, or enhanced cognition. In the last paper on this phenomenon, Bókkon, Mallick, and Tuszynski propose that the experience of a bright light in NDE is caused by an overproduction of free radicals and excited molecules, which may generate transient enhancement of luminiscent biophotons in retinotopic and other areas of the brain. They conclude that these stimuli are then interpreted as originating in the physical world.

Moving to *meditation*, Thomas, Jamieson, and Cohen conducted an

EEG study on intermediate and advanced Satyananda Yoga practitioners. Intermediate meditators showed greater source activity in low frequencies during the non-meditation (mental calculation), and meditation (body-steadiness and mantra) conditions. Advanced meditators showed greater activity in high frequencies in all conditions, particularly during meditation. The authors conclude that inhibition of a right lateralized network comprising visual, somatosensory, and body–world self-representations reflect sensory withdrawal and ego-diminishment. In contrast, conscious states specific to advanced practitioners require both disengagement from self–world representational systems and the development of widespread gamma synchronization.

Xu et al. employed fMRI to compare nondirective and concentrative ACEM meditation to a rest condition in a group of experienced practitioners. The first modality involves a relaxed focus of attention allowing the non-judgmental occurrence of mental events, without the expectation that mind wandering will decrease. The second type of meditation is geared to decreasing mind wandering. Results suggest that nondirective meditation involves more extensive activation of brain areas associated with episodic memories and emotional processing (parahippocampal gyrus and amygdala), than concentrative meditation or regular rest.

In the last paper on factors that may induce NOME, Roseman et al. describe the effects on cortical functional connectivity of the *psychedelic* drug psilocybin and the stimulant/psychedelic hybrid, MDMA. Both substances produced marked subjective effects (e.g., a sense of motion, geometric images, alterations in the sense of time and space), more pronounced in psilocybin. Between-network connectivity was generally increased under psilocybin, implying that networks became less differentiated from each other in the psychedelic state, whereas decreased connectivity occurred between visual and sensorimotor cortical networks.

In their paper, Hinterberg, Zlabinger, and Blaser explore how different mental perspectives or positions (toward the mental self or intrapersonal, toward the mental outer world or extrapersonal, or in empathic connection with someone else's intrapersonal space) and attentional foci (self vs. object) correlate with brainwave activity. They propose that alpha2 and beta2 bands are good indicators of different perspectival viewpoints, whereas delta power differentiates attentional focus on the self from that on objects.

The final section of the book is devoted to evaluating the *psi hypothesis*, namely that individuals may be affected by stimuli spatially or temporally distant, without the apparent mediation of the sensory systems or logical reasoning. Mossbridge, Tressoldi, and Utts discuss a 2012 meta-analysis that supported the hypothesis that human physiology can discriminate between

randomly delivered stimuli occurring 1–10 s in the future, a phenomenon known in the literature as *presentiment*. This article stirred a number of comments and a paper by Schwarzkopf (2014), who had 6 criticisms of the meta-analysis, namely that: 1) some of the studies included were of questionable quality, 2) it should have included studies not conducted by psi researchers, 3) there was an imbalance between the more frequent calm versus the less frequent emotional trials, 4) the results might have been caused by analytical artifacts such as not correcting for baseline, 5) there was an unproven assumption that physiological effects scale linearly with expectation, and 6) the results are not plausible because they would reverse the arrow of time. Mossbridge, Tressoldi, Utts, Ives, Radin, and Jonas (2015) responded to these points in the following ways, that: 1) the original meta-analysis (2012) had already reported that not including the articles questioned by Schwarzkopf did not make a difference to the results reported, 2) the original 2012 paper had also reported that the data sent from non-psi labs confirmed the meta-analytic result, 3) if anything, the imbalance between calm and emotional stimuli would have gone against the meta-analysis, 4) some studies had indeed corrected for baseline through normalization, and for those that had used other baselining methods such design features as randomization and sampling with replacement make it difficult to see how such methods could have affected the results, 5) a simulation conducted by the authors showed that expectation bias could not explain away the results of the meta-analysis, and 6) that a presentiment effect is consistent with time-symmetric processes, which are well-known and accepted in quantum mechanics (see Millar 2015). Many of these points and counter-points are complex and the reader is advised to read the original papers directly.

Testing the psi hypothesis of retrocausal effects, Rabeyron presents a study in which researchers probed whether reaction time could be affected by a picture *after* (not preceded, as is conventionally tested) the target word. This study followed an earlier one in which strong significant effects had been obtained in post hoc analyses (Rabeyron & Watt 2010). In the current paper there were overall nonsignificant results. A post-hoc analysis with the 10 participants who had a retro-priming effect showed that they tended to report previous putative precognitive experiences. The author discusses potential explanations as to why replication supporting the psi hypothesis has been inconsistent. The book ends with the opinion paper by Acunzo, Evrard, and Rabeyron reviewing neuroimaging research on the psi hypothesis. They mention that 5 out of 6 studies were consistent with

the hypothesis but also note methodological shortcomings that should be solved in future research.

After a long hiatus, research on NOME has barely restarted and has a long way to go. Comparisons and integrations across different experiences, induction procedures, and analytical techniques are badly needed. We consider this investigation essential but would not dare to predict where it may lead us. As a leading theoretical physicist has stated: “The very nature of scientific inquiry always ongoing and always under revision necessarily implies the notion of a changing understanding of reality” (Gleiser 2014:271).

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## ESSAY REVIEW

### **Climate-Change Science or Climate-Change Propaganda?**

**HENRY H. BAUER**

**Climate Change: Evidence & Causes<sup>1</sup>—An Overview from the Royal Society and the U.S. National Academy of Sciences.** Washington, D.C.: National Academies Press for The National Academy of Sciences and The Royal Society, 2014. 36 pp. \$5 (paperback). Free at Amazon Kindle and at [http://www.nap.edu/catalog.php?record\\_id=18730](http://www.nap.edu/catalog.php?record_id=18730). ISBN 978-0-309-30199-2.

#### **Preamble**

Those who take an interest in topics ignored by mainstream science (e.g., reports of UFOs, of apparently psychic phenomena, of creatures thought not to be extant) are quite accustomed to having even tangible evidence dismissed out of hand, dogmatically, sneeringly, by official representatives of mainstream science or by their camp followers.

It is not yet widely recognized that the same dismissive dogmatism is in play on some matters that are squarely within concerns of mainstream science. Even competent, highly informed experts who present evidence and interpretations that run counter to the prevailing consensus are dismissed dogmatically as “deniers” or “denialists”, notably regarding human-caused climate change or about whether HIV causes AIDS (Bauer 2012).

Science is popularly seen as open-minded and evidence-respecting, so it may well seem incredible that competent minority views on such issues of considerable public importance should be suppressed and their proponents vilified. It must seem unbelievable that the most respected scientific institutions could distort and misrepresent evidence with the aim of entrenching a mainstream consensus. Yet that is demonstrably the case over climate change in the booklet under review here.

#### **Introduction**

Governments and international as well as national scientific authorities project certainty that human activity, in particular the generation of carbon dioxide (CO<sub>2</sub>) is primarily responsible for warming of the Earth and for such perceptible changes in climate as rising sea-levels and increasing frequency of

extreme weather events—heat waves, droughts, floods, tsunamis, hurricanes.

On the basis of this certainty, unprecedented changes in modes of production, involving huge expenditures, are being planned and introduced with the aim of decreasing the present rate of generating CO<sub>2</sub>.

The scientific claims of such human-caused climate change (HCCC) or human-caused (anthropogenic) global warming (AGW) are presented in periodic reports from the Intergovernmental Panel on Climate Change (IPCC).<sup>2</sup> These reports are available free as PDF downloads. The 5<sup>th</sup> (and latest) Assessment Report (AR5) comes in 4 parts, in PDF files with sizes listed as 375 MB, 176 Mb, 51.4 MB, and 10 MB, respectively (however, the last one, the “Synthesis Report”, shows up as 14.2 MB on my computer).

Among that mass of material, one might hope to find somewhere a clear statement of the proof that global warming and associated climate change is owing primarily to increasing levels of CO<sub>2</sub>. Any such hope would be dashed despite >600 MB-worth of PDFs, 7,000-plus pages.

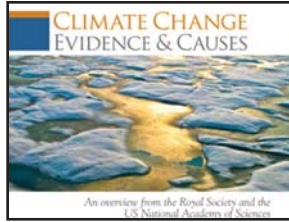
The present mainstream position rests chiefly on two unproven points:

1. Because CO<sub>2</sub> absorbs infrared radiation, which manifests as heat, any heat absorbed in the atmosphere by CO<sub>2</sub> must go into heating the atmosphere and the earth and oceans. Further, computer models based on that view also assume that a feedback mechanism amplifies the heat absorbed by atmospheric CO<sub>2</sub> (e.g., Singer 2014).
2. Misconstruing as evidence of causation the gross overall correlation from about 1850 to the present between CO<sub>2</sub> levels and global temperature. But correlation never proves causation.

The case for CO<sub>2</sub>-caused warming and climate change consists of these and other assumptions built into elaborate computer models. That the models must be exceedingly complex is obvious, given all the variables and interactions among land, sea, and atmosphere, each of those being a combination of different characteristics at different heights or depths, all of that changing dynamically in short-term as well as long-term ways; with local differences to be taken into account; variations in solar radiation striking the Earth; variations in amounts of all the greenhouse gases—water vapor, methane, nitrous oxide (N<sub>2</sub>O), and ozone as well as CO<sub>2</sub>: Water vapor exercises a greenhouse effect several times stronger than that exerted by CO<sub>2</sub>, and methane and other gases together are calculated to be about as important currently as is CO<sub>2</sub>.

It would be rather miraculous if any model were capable of doing this job, since there are innumerable heat-exchange processes occurring all the time in Nature. How well a model performs can only be judged by comparing its output to actual observations. In that respect, all the models

have failed quite dramatically, even though that is not officially acknowledged. It is undisputed that CO<sub>2</sub> levels have increased relentlessly since at least the middle of the 19<sup>th</sup> century. At the same time, from the 1940s into the 1970s global temperatures were going down rather than increasing; and since about 2000 there has been no appreciable warming globally. The Nature-given fact is that there have been 4 or 5 decades out of the last 160 years or so during which temperature did not rise while CO<sub>2</sub> levels did. No model accounts for that, showing that natural influences missing from the models can outweigh any greenhouse warming by CO<sub>2</sub>. It follows that no projections from these models into the future should be taken seriously.



An elephant in the room is the historical record of temperature changes. Direct measurements are available from only about the middle of the 19<sup>th</sup> century, and even these encompass many uncertainties because of different methods of measurement and the fact that each measurement is local or at best regional; and temperatures also vary with height in the atmosphere as well as depth in the earth and oceans. Significant changes over geological time can be estimated with good reliability, but not decade-by-decade<sup>3</sup>; the record for earlier times is not fine-grained enough and nowhere complete enough to compare reliably with what has happened in the last few decades. Ice cores sometimes yield reasonable estimates nearly a million years into the past, but only in one locality. The mainstream claim that rates of temperature rises in the last century-and-a-half are somehow unprecedented cannot be proven because the precedents, the historical records, are neither sufficiently fine-grained nor sufficiently reliable.

The voluminous IPCC reports offer detailed discussions of how the innumerable variables function in the computer models. Different degrees of certainty are assigned to various points; for example, “The period from 1983 to 2012 was *very likely* the warmest 30-year period of the last 800 years in the Northern Hemisphere, where such assessment is possible (*high confidence*)” (italics in the original, p. 56 of *Synthesis Report, AR5, 2014*). Such statements of probabilities and levels of confidence about the probabilities are everywhere. They are described on p. 37:

virtually certain, 99–100% probability; extremely likely, 95–100%; very likely, 90–100%; likely, 66–100%; more likely than not, >50–100%; about as likely as not, 33–66%; unlikely, 0–33%; very unlikely, 0–10%; extremely unlikely, 0–5%; and exceptionally unlikely, 0–1%. . . . Unless otherwise indicated, findings assigned a likelihood term are associated with *high* or *very high* confidence.

Probabilists and statisticians might ask what possible grounds there could be for assigning numbers to these subjective human judgments, let alone including the extreme—and invalid—possibility of 100%; perhaps particularly since these judgments are made with various degrees of confidence, in other words again subjectively.

This internal evidence demonstrates in itself that there is no definitive proof that the models faithfully represent reality. In other words, there is no tangible objective evidence to support the AGW and HCCC scenarios.

That must seem incredible. There is no historical precedent for such a lack of hard evidence for a scientific consensus that has been proclaimed for several decades as trustworthy, as certainly true, by all national and international institutions of government and science, on an issue so pertinent to national and international policies and budgets. Admittedly, minority views in science have always been resisted as a matter of course (Barber 1961, Hook 2003), and sometimes correct minority views had to wait for decades before being accepted (Stent 1972); but never before when huge public expenditures were at stake. The only comparable situation is the contemporary claim that HIV causes AIDS (Bauer 2007, 2012).

National and international institutions do not readily admit error; and large numbers of researchers are vested in the AGW/HCCC scenario. All have staked their credibility and authority and reputations and careers on being right. It follows that every conceivable effort will be made to maintain public belief in AGW/HCCC.

If there were clear, tangible evidence for AGW/HCCC, it would only be necessary to present it. Resorting to computer models that need thousands of pages of justification already reveals the fact that such evidence does not exist.

That is the context in which the Royal Society of London and the National Academy of Sciences of the USA have issued this publication asserting the truth of AGW/HCCC. The pamphlet commits sins of omission and commission in relying on rhetorical trickery and in misrepresenting facts.

*Climate Change: Evidence & Causes* poses and answers 20 questions about climate change, followed by a section on “Basics of Climate Change.” Had it been published by an activist environmentalist organization, it could safely be ignored as a self-confessed piece of propaganda. But it can hardly be ignored when it comes from the top scientific institutions in the United States and Britain and might therefore be presumed to provide the most judicious available assessment of its chosen subject. Nevertheless, it is propaganda, not a scientific assessment. It argues from authority and distorts evidence in doing so.

The very term “climate change” in this context is rhetorical sleight of words. Until a few years ago, “global warming” was the universally used shorthand for human-caused global warming. But since there has been no appreciable warming globally for the last 15 years or so, the critics of carbon emissions have been using the term “climate change,” which cannot be contradicted or falsified: Climate has always changed and always will; global cooling also is climate change.

### **Arguing from Authority with Just-So Stories**

A common tactic when arguing from authority is the Just-So Story, supporting a dogmatic assertion with apparently reasonable statements which, however, have no basis in reality. Rudyard Kipling’s *Just So Stories* are the eponymous icons for this genre, imaginatively whimsical “explanations” for how the leopard got its spots, the giraffe its long neck, the camel its hump, and so on.

“But, Mr. Kipling, how do you know that’s so?”  
“It’s just so . . . Just So.” (Hillerich 1966)

The absolute conviction that human activities are causing global warming and more generally climate change spawns any number of such Just-So tales. Presuming that AGW is “bad,” it follows that its consequences will be bad, for example that it will bring about such catastrophic weather events as hurricanes or tsunamis. But if one thinks about the probability of extreme weather events on first principles, one might equally argue that higher temperatures would bring *fewer* unusual events. After all, heat seeks to even itself out in every possible way, by radiating away and causing material to move (convection) and by transmitting itself to neighboring material (conduction). We have to use elaborate means of insulation like vacuum bottles to discourage heat from averaging itself out. So as the whole globe gets on average warmer, heat should even itself out ever more efficiently: Radiation gets more intense, material moves more quickly, heat transmits itself faster, so that there would be fewer idiosyncratic places or movements to spur extreme events. (Just So!) During much of the ages of the dinosaurs, Earth was between  $\sim 8^\circ\text{C}$  ( $\sim 14^\circ\text{F}$ )<sup>4</sup> and  $\sim 12^\circ\text{C}$  ( $\sim 22^\circ\text{F}$ )<sup>5</sup> hotter than now. No evidence has been presented that “unusual” or “extreme” weather events then were more common than nowadays.

“Why is Arctic sea ice decreasing while Antarctic sea ice is not?” (Question 12). The pamphlet’s Just-So story (no sources or evidence cited) is that the Arctic Sea is sort of enclosed, whereas the Antarctic is open and subject to effects of winds and oceans. To the contrary: Those winds and

oceans would serve to bring heat more efficiently to the Antarctic. That's *my* Just-So story, equally (un)supported by evidence.

### **Is AGW Bad in the Short Term or Only the Long Term?**

There is no empirical evidence for an increase in extreme weather events in the last several decades. Indeed, there hardly could be, given an almost non-existent historical record against which to compare frequencies—not to speak of the problem of even defining what is “extreme.” Official data concerning hurricanes affecting the United States do exist, and those reveal that the total number of hurricanes as well as the number of major ones since about 1960 has been lower and not higher than the average for the period 1851–2000.<sup>6</sup>

Nevertheless, so successful has been the campaign for public acceptance of AGW and its undesirability that pundits and media are wont to ascribe anything undesirable and out of the ordinary to it—bigger tsunamis, more extreme heat waves and cold spells, fiercer and more frequent tornados and hurricanes. Perhaps the prize should be awarded to the chief executive of AirAsia, who commented on the unexplained crash of a plane by “suggesting that climate change was making weather worse and flying riskier, particularly in the tropics” (Bachelard 2015).

In places this booklet indicts human activities for only a long-term warming but not short-term changes: “A short-term slowdown in the warming of Earth’s surface does not invalidate our understanding of long-term changes in global temperature arising from human-induced changes in greenhouse gases” (Question 10); “shorter-term variations are mostly due to natural causes, and do not contradict our fundamental understanding that the long-term warming trend is primarily due to human-induced changes in the atmospheric levels of CO<sub>2</sub> and other greenhouse gases” (Question 9).

At other places the booklet cites recent—in other words short-term—events as resulting from carbon emissions. Thus Question 6 claims that the *current* rate of climate change is more rapid than in the past. Again, “over recent decades heatwaves have increased in frequency in large parts of Europe, Asia and Australia” (Question 11); “Record heatwaves have occurred in Australia (January 2013), USA (July 2012), Russia (summer 2010), and Europe (summer 2003)” (Question 10); “heavy rainfall and snowfall events (which increase the risk of flooding) and heatwaves . . . generally becoming more frequent”(Question 13). In lieu of actual data or logic, graphics serve to spin the message home: p. 13 (Question 11) shows a forlorn, wintry landscape and p. 15 (Question 13) depicts cars on a flooded street.

But data from the past are not fine-grained enough to compare with

what has happened in a period as short as the last hundred years, let alone with the “current rate.” In any case, there are a whole host of natural temperature cycles (Dilley 2012) superposed on the large variations (range of 5–6 °C) owing to the periodic (7 or 8) major Ice Ages of the last million years (Folland et al. 1990:202, figure 7.1); for example, since 800 A.D. there have been 6 warming cycles and cold intervals with durations of roughly a century cycling over a range of about 0.9 °C (about 1.5 °; Dilley 2012:5, figure 2).

Question 14 again asserts a short-term effect: “increased frequency and intensity of occasional storm surges” owing to rising sea levels. It is not often pointed out that glaciers and ice sheets began to melt at the conclusion of the last Ice Age when sea levels were about 400 feet lower than at present; and they were about 15 feet higher during the last interglacial. *On average*, sea level changed by about 5 inches per century from these natural causes, but with pronounced pulses and lulls, for instance “10–15 m in less than 500 years” (Gornitz 2007): 2–3 meters (say 100 inches) in a single century; a whole inch per year. When natural causes can produce so great an effect, how can one be sure that AGW is to be blamed for “0.12 inches per year” (p. 16) in the last few decades, as Question 14 insinuates?

### Unwarranted Claims of Certainty

A copyeditor presented with this pamphlet in draft would read on page 2, “climate change over many decades *will* depend mainly on the total amount of CO<sub>2</sub> and other greenhouse gases emitted as a result of human activities”; and on page B9, “most of the recent change is *almost certainly* due to emissions of greenhouse gases caused by human activities” [emphasis added in both cases]. In the margins of both pages, the editor naturally places a query: “Au: Which is it? Certainly *will* or *almost certainly* will?”

There are innumerable other places where the same query is appropriate. Question 2 asserts certainty in asking how scientists “*know* that recent climate change is largely caused by human activities” [emphasis added].

That “natural causes alone are inadequate to explain the recent observed changes in climate” is quite strikingly misleading: They are inadequate only according to the assumptions fed into the computer models. The problem for AGW is that there has been no significant warming for the last 15–18 years while carbon emissions have continued to increase significantly. Moreover, the speculations by mainstream experts about why their models have failed to account for this admitted “slowdown” invoke precisely such natural causes as oceans acting as heat traps;<sup>7,8,9</sup> and this pamphlet itself in another place (p. 12) seeks to explain away the slowdown as owing to such natural causes as lower solar activity and volcanic eruptions.



Question 8 makes no bones about it: “Is there a point at which adding more CO<sub>2</sub> will not cause further warming?”

No. Adding more CO<sub>2</sub> to the atmosphere will cause surface temperatures to continue to increase. As the atmospheric concentrations of CO<sub>2</sub> increase, the addition of extra CO<sub>2</sub> becomes progressively less effective at trapping Earth’s energy, but surface temperature will still rise.

Just So.

“Global warming of just a few degrees *will* be associated with . . . increases in some types of extreme weather events” (Question 17; emphasis added). A graphic of the Earth (Question 16, p. 19) in light orange (acceptable warmth) for 1986–2005 is side-by-side with a frighteningly hot, dark-red Earth in 2081–2100, offered as an accurate projection. Just So.

Under “Basics of Climate Change,” the pamphlet is again unequivocal: “Greenhouse gases emitted by human activities alter Earth’s energy balance and thus its climate. . . . Scientists have determined that, when all human and natural factors are considered, Earth’s climate balance has been altered towards warming, with the biggest contributor being increases in CO<sub>2</sub>.”

Just So—“Scientists” have spoken.

Even as it exudes such certainty, *Climate Change* attempts to appear scientifically objective by acknowledging uncertainty: “Science is a continual process of observation, understanding, modelling, testing, and prediction. The prediction of a long-term trend in global warming from increasing greenhouse gases is robust and has been confirmed by a growing body of evidence. Nevertheless, understanding (for example, of cloud dynamics, and of climate variations on centennial and decadal timescales and on regional-to-local spatial scales) remains incomplete. All of these are areas of active research” (Question 18). But with those uncertainties, predictions cannot be “robust”; and when “a growing body of evidence” has to be cited as confirmation, evidently certainty has not been attained. Acknowledged uncertainty on “centennial . . . timescales” means uncertainty over the last century or so, which is precisely the timescale for which AGW is being claimed as certain.

This pervasive self-contradicting intermingling of assertions of certainty with admissions of uncertainty, a form of double-speak, recalls attempts to describe anomalous claims as pseudo-science (Bauer 2014). A related rhetorical ploy is to arouse emotion and stimulate fear by innuendo and speculation: “the best available climate models do not predict abrupt changes . . . (often referred to as tipping points) in the near future. However, as warming increases, the possibilities of major abrupt change cannot be ruled out” (p. 21).

Indeed. Given that the future is so hard to predict,<sup>10</sup> there are very few things, if any, that *can* be ruled out, including that carbon emissions have no effect at all on climate.

### Misdirection and Misrepresented Facts

Another tactic of attempted persuasion is misdirection. An example comes already in the introductory Summary: “slowdowns and accelerations in warming lasting a decade or more will continue to occur. However, long-term climate change over many decades will depend *mainly* on the total amount of CO<sub>2</sub> and other greenhouse gases emitted as a result of human activities” (p. 2, emphasis added).

This reads so reasonably—Just So! But the prediction of long-term change resulting *primarily* from steadily increasing atmospheric carbon dioxide comes from computer models that account for neither the “slowdown” of the last decade-and-a-half<sup>11</sup> nor the cooling from about 1940 into the 1970s<sup>12</sup> that had then caused climate scientists to warn about an impending Ice Age.<sup>13</sup> These failures demonstrate unequivocally that the computer models are flawed; since they are wrong even in the short term and for the recent past, they certainly cannot be given credence for the longer term.<sup>14</sup>

Moreover, these data disprove the notion that climate change is “mainly” owing to greenhouse gas: Twice in less than a century, and in each case for some decades, there has been no warming even as atmospheric CO<sub>2</sub> steadily increased. Quite clearly, some natural processes outweigh whatever effect increasing levels of CO<sub>2</sub> might have.

Misdirection in Question 1, “Is the Climate Warming?”, is through rather blatant omission. Temperature data are cited in several graphs, since 1850 in one case and since 1955 in three others. Thereby the unwary reader is not informed about the Medieval Warm Period and the Little Ice Age, which indeed are mentioned nowhere in the whole pamphlet.

The Medieval Warm Period (MWP), about 900–1300 A.D., saw temperatures 1–2 °C (~2–4 °F) higher than at present. Contemporary sources such as *Encyclopedia Britannica* and *Wikipedia* strive mightily to acknowledge the MWP while pulling out all stops to suggest that it might not have been global or even real,<sup>15,16</sup> despite a large body of published peer-reviewed material that attests the MWP,<sup>17</sup> for instance Rosenthal, Linsley, and Oppo (2013). The reality of the MWP was never questioned before AGW became dogma.

The Little Ice Age (LIA), roughly 1350–1850, followed the MWP. Once again, contemporary sources such as *Wikipedia*<sup>18</sup> try to minimize its significance. *Encyclopedia Britannica* weasel-words thus: “the Little Ice

Age, though synonymous with cold temperatures, can also be characterized broadly as a period when there was an increase in temperature and precipitation variability across many parts of the globe<sup>19</sup>—in other words, although it was indeed colder (“synonymous with cold temperatures”), please ignore the plain significance of that. For a less-biased discussion of LIA data, see the Environmental History Resources website.<sup>20</sup> And, again, no one questioned the reality of the LIA before AGW became a pervasive shibboleth of the conventional wisdom.

Question 4 reports that the Sun’s output has not increased appreciably “in recent decades” and therefore warming during this period proves that it is not the Sun that primarily determines global temperatures. This misdirection is nothing short of astonishing: Since there has not been any warming in the last decade-and-a-half at the same time as the Sun’s output has not increased, it seems entirely plausible that the Sun’s output is the primary controller of global temperature. In any case, the Sun is the initial source of energy trapped as heat by greenhouse gases, so variations in the Sun’s output of energy must be taken into account in any model of climate.

That the 11-year solar (sunspot) cycle “may have a *small* effect on surface climate” (p. 7; emphasis added) misleads yet further, for that is not the consensus view of pertinent experts (NASA): The

luminosity of our own sun varies a measly 0.1% over the course of the 11-year solar cycle. . . . [but] even these apparently tiny variations can have a significant effect on terrestrial climate. . . . [They] exceed all other energy sources (such as natural radioactivity in Earth’s core) combined. . . . Within the relatively narrow band of EUV wavelengths, the sun’s output varies not by a minuscule 0.1%, but by whopping factors of 10 or more. This can strongly affect the chemistry and thermal structure of the upper atmosphere.<sup>21</sup>

“Recent estimates” have the temperature “4 to 5 °C” higher than in the last Ice Age, and this increase since the Ice Age is said to have “occurred over a period of about 7,000 years, starting 18,000 years ago. . . . human alteration of the planet’s energy budget . . . has so far warmed Earth by about 0.8 °C” (question 6, p. 9). So between 3.2 °C and 4.2 °C of warming (4 to 5 °C minus 0.8 °C) since the last major Ice Age is *not* owing to human activities since the Industrial Age began. Why then had there been so much and so rapid warming *from natural causes* since the last Ice Age? According to these statements, nearly the whole usual change from an Ice Age to peak warmth, typically over a period of ~100,000 years, had taken place already in the last 10,000 years. Evidently, there is some unknown and very powerful natural cause of warming at work. Yet Question 9 (p. 11) asserts

again that “shorter-term variations are mostly due to natural causes, and do not contradict our fundamental understanding that the long-term warming trend is primarily due to human-induced changes in the atmospheric levels of CO<sub>2</sub> and other greenhouse gases.” Just So.

The present level of atmospheric CO<sub>2</sub> concentration is *almost* certainly unprecedented in the past million years, during which time modern humans evolved and societies developed. The atmospheric CO<sub>2</sub> concentration was however higher in Earth’s more distant past (many millions of years ago), at which time palaeoclimatic and geological data indicate that temperatures and sea levels were also higher than they are today. [emphasis added]

Note once again the insidious “almost,” and the insinuation that modern humans and their societies have not experienced—could not tolerate?—what the Earth experienced before modern humans appeared.

That CO<sub>2</sub>, temperature, and sea level appear to be correlated on very long time scales says nothing about what caused any one of them. In fact, it appears that CO<sub>2</sub> levels rose after temperature increased and not before: Increased temperature appears to cause increased CO<sub>2</sub>, not the other way around (Fischer et al. 1999, Monnin et al. 2001), at least in the Southern Hemisphere (Caillon et al. 2003); however, Parrenin et al. (2013) suggest that the data could be interpreted differently. In any case, there is certainly no clear evidence that increased CO<sub>2</sub> levels preceded increased temperature.

Question 15 introduces another charge against CO<sub>2</sub>: It acidifies the oceans and affects negatively the formation of sea shells. However, sea shells began to form about 500 million years ago<sup>22</sup> when CO<sub>2</sub> levels were >2,000 ppm (parts per million).<sup>4</sup> If sea shells could form in the oceans in those days, there is little to worry about nowadays. Moreover, taken over the whole lifetime of Earth, there is no correlation between CO<sub>2</sub> levels (as high as ~3,000 ppm) and temperature variations over ranges of about 10 °C.<sup>4</sup> That last fact in itself ought to raise strong doubts about current claims of climate change resulting from increased CO<sub>2</sub> levels.

### Scientific Dissent

A striking piece of misdirection and sinning by omission is the pervasive implication that science speaks with a single voice on all this.

Question 16, “How confident are scientists that Earth will warm further over the coming century?: Very confident,” misleads on a central point by implying that all scientists agree. Instead, a large body of largely ignored scientists, meteorologists, and others continues to dispute AGW; see, for example, the Leipzig Declaration<sup>23</sup> and the websites of the Science and Environmental Policy Program,<sup>24</sup> Roger Pielke, Sr.,<sup>25</sup> and Anthony Watts<sup>26</sup>.

### Authorship and Motivation

This pamphlet is blatantly biased, yet published under the auspices of leading scientific institutions, from which one might have expected evenhanded, objective assessments. Is this a deliberate gambit to mislead the public and policymakers? A conspiracy?

I prefer Murphy's Law, which holds that one should never attribute to malice what could be explained by what is much more common, namely incompetence; in this case, the incompetence that accompanies bureaucracy.

Actual authorship is obscured.<sup>27</sup> Twelve individuals are named as "the primary writing team" for *Climate Change: Evidence & Causes*, one being a "UK lead" and another the "US lead." A further thirteen individuals reviewed at least one draft but did not see the final version. Four individuals are named for providing unspecified "staff assistance" (p. B10).

This is absurd. Some one person must have written at least an initial draft. At any rate, this underlines the fact that this is not a scientific publication, where authorship would be unambiguous and all authors would be expected to specify exactly what is attributable to them individually. Here, most or all of the actual writing was surely done by specialists in technical writing, presumably the "staff." Whatever the exact course of events, there is no reason to doubt that all the participants fully believe AGW to be an indisputable fact. Cherry-picking the evidence under the influence of unquestioned belief, together with cognitive dissonance (the inability to appreciate contradicting evidence), could be sufficient explanation for the pamphlet's bias and other flaws.

But what was the need for this publication? Two years earlier, the National Academies Press had published a similar 36-page pamphlet on the same topic: *Climate Change: Evidence, Impacts, and Choices*,<sup>28</sup> "authored" by the National Research Council of the National Academy of Sciences (NRC–NAS). (Perhaps that explains why *Climate Change: Evidence and Causes* [henceforth RS–NAS] is several times labeled *Climate Change: Evidence and Choices*<sup>1</sup>.)

How do these two publications differ?

In most ways, NRC–NAS predicts just as dire future possibilities as does RS–NAS, including similarly scary pictured comparisons (p. 22) of calm green-yellow-orange Earths for 2011–2030 with red-hot Earths for 2080–2099. However,

- NRC–NAS is much more accurate than *Climate Change: Evidence & Causes*, for example in explaining the strong influence of water vapor, which is responsible for *most* of Earth's greenhouse effect: 36–72%, compared to 9–26% for CO<sub>2</sub> and 4–9% for methane (Kiehl & Trenberth

1997). RS–NAS does not even mention water vapor, an extraordinary omission.

- Both booklets show the variations of CO<sub>2</sub> and temperature during the several Ice Age cycles of the last 800,000 years. RS–NAS comments, “changes in CO<sub>2</sub> concentrations . . . track closely with changes in temperature.” NRC–NAS, however, points out (p. 19) that “changes in carbon dioxide concentrations . . . track closely with changes in temperature . . . *with CO<sub>2</sub> lagging behind temperature changes*” (emphasis added). As already noted earlier, this suggests that temperature increase causes CO<sub>2</sub> increase and not the other way around. Admittedly, NRC–NAS then asserts that this might no longer apply under the “relatively rapid release of . . . greenhouse gases since the start of the Industrial Revolution,” but this Just-So story remains pure speculation in the absence of any evidence.
- NRC–NAS explicitly points out that science cannot determine what should be done, since that involves value judgments, including the question of “at what level of warming are risks acceptable given the cost of limiting them” (p. 31). And NRC–NAS also emphasizes irreducible uncertainty: “Further research will never completely eliminate uncertainties about climate change and its risks” (p. 35).

The main difference is that RS–NAS projects certainty where NRC–NAS does not, suggesting that this was the motivation for a new pamphlet two years later and covering the same ground. Another clue pointing in that direction is that RS–NAS spelling follows British rather than American usage, and that Sir Paul Nurse, president of the Royal Society of London, had earlier been the featured narrator of a BBC documentary entitled *Science under Attack* that was broadcast in the UK on January 24, 2011, and which also is AGW propaganda masquerading as science (Bauer 2013).

No matter the history or the motivation, *Climate Change: Evidence & Causes* is a piece of “propaganda science” (Bauer 2012: 64 ff.) to which leading scientific associations have, to their shame, lent their prestige and reputation.

### Notes

<sup>1</sup> Curiously enough, the cover page and last page of the PDF download from the National Academies Press website give the title as *Climate Change: Evidence & Choices* even as the PDF says *Causes* and not *Choices*.

<sup>2</sup> IPCC. [https://www.ipcc.ch/publications\\_and\\_data/publications\\_and\\_data\\_reports.shtml](https://www.ipcc.ch/publications_and_data/publications_and_data_reports.shtml)

<sup>3</sup> The most general method measures differences in the ratio of O<sup>16</sup> to O<sup>18</sup> in

water or ice and in sea-shells, because it is known how that ratio changes with temperature. These isotopes differ in weight and that affects rates of chemical reactions and physical changes like evaporation.

- <sup>4</sup> Dr. Vincent Gray on historical carbon dioxide levels by Anthony Watts (2013). <http://wattsupwiththat.com/2013/06/04/dr-vincent-gray-on-historical-carbon-dioxide-levels>
- <sup>5</sup> Paleomap Project by Christopher R. Scotese. <http://www.scotese.com/climate.htm>
- <sup>6</sup> U.S. Hurricane Strikes by Decade. <http://www.nhc.noaa.gov/pastdec.shtml>
- <sup>7</sup> Davy Jones's Heat Locker (2014). <http://www.economist.com/news/science-and-technology/21613161-mystery-pause-global-warming-may-have-been-solved-answer-seems>
- <sup>8</sup> Solving the Myths of Hiatus in Global Warming by Rob Monroe (2013). <https://scripps.ucsd.edu/programs/keelingcurve/2013/08/28/solving-the-mysteries-of-hiatus-in-global-warming>
- <sup>9</sup> Has the Atlantic Ocean Stalled Global Warming? by Jane J. Lee (2014). <http://news.nationalgeographic.com/news/2014/08/140821-global-warming-hiatus-climate-change-ocean-science>
- <sup>10</sup> Quotes to this effect are often attributed to Yogi Berra, but others cite Niels Bohr and other Danish sources. <http://quoteinvestigator.com/2013/10/20/no-predict>
- <sup>11</sup> Some reports have it as a halt rather than a slowing, or even a decline in global average temperature. <http://isthereglobalcooling.com>  
<http://wattsupwiththat.com/2012/01/11/the-portland-state-university-study-of-shrinking-mt-adams-glaciers-a-good-example-of-bad-science>  
<http://notrickszone.com/2013/09/12/no-warming-left-to-deny-global-cooling-takes-over-cet-annual-mean-temperature-plunges-1c-since-2000/#sthash.mowZKMjF.dpbs>  
<http://www.forbes.com/sites/peterferrara/2013/05/26/to-the-horror-of-global-warming-alarmists-global-cooling-is-here>  
<http://www.globalresearch.ca/global-cooling-is-here/10783>
- <sup>12</sup> National Aeronautics and Space Administration, Goddard Institute for Space Studies. [http://data.giss.nasa.gov/gistemp/graphs\\_v3](http://data.giss.nasa.gov/gistemp/graphs_v3)  
 Global Surface Temperature Change by J. Hansen, R. Ruedy, M. Sato, & K. Lo, NASA Goddard Institute for Space Studies.  
[http://data.giss.nasa.gov/gistemp/paper/gistemp2010\\_draft0803.pdf](http://data.giss.nasa.gov/gistemp/paper/gistemp2010_draft0803.pdf)
- <sup>13</sup> For example, Another Ice Age? *Time*, 24 June 1974, pp. 106–107.
- <sup>14</sup> For a comprehensive discussion of why computer models are inevitably fallible on such complex matters as climate and environment, see Pilkey and Pilkey-Jarvis (2007).



- <sup>15</sup> Medieval Warm Period (MWP)—Climatology by John P. Rafferty.  
<http://www.britannica.com/EBchecked/topic/175842/medieval-warm-period-MWP>
- <sup>16</sup> Medieval Warm Period.  
[http://en.wikipedia.org/wiki/Medieval\\_Warm\\_Period](http://en.wikipedia.org/wiki/Medieval_Warm_Period)
- <sup>17</sup> See for instance links at <http://wattsupwiththat.com/tag/medieval-warm-period> and <http://www.co2science.org/articles/V16/N50/EDIT.php>
- <sup>18</sup> Little Ice Age. [http://en.wikipedia.org/wiki/Little\\_Ice\\_Age](http://en.wikipedia.org/wiki/Little_Ice_Age)
- <sup>19</sup> Little Ice Age (LIA) Geochronology by Stephen T. Jackson.  
<http://www.britannica.com/EBchecked/topic/344106/Little-Ice-Age-LIA>
- <sup>20</sup> The Little Ice Age circa 1300–1870.  
[http://www.eh-resources.org/timeline/timeline\\_lia.html](http://www.eh-resources.org/timeline/timeline_lia.html)
- <sup>21</sup> Solar Variability and Terrestrial Climate (2013). [http://science.nasa.gov/science-news/science-at-nasa/2013/08jan\\_sunclimate](http://science.nasa.gov/science-news/science-at-nasa/2013/08jan_sunclimate)
- <sup>22</sup> Geologic Time Scale.  
<http://www.enchantedlearning.com/subjects/Geologictime.html>
- <sup>23</sup> Science & Environmental Policy Project, Climate Change White Paper, 22 June 2010; Appendix A, Leipzig Declaration.  
[http://henryhbauer.homestead.com/Leipzig\\_DeclarationPontius2005.pdf](http://henryhbauer.homestead.com/Leipzig_DeclarationPontius2005.pdf)
- <sup>24</sup> Science & Environmental Policy Project. <http://www.sepp.org>
- <sup>25</sup> Climate Science: Roger Pielke Sr. [pielkeclimatesci.wordpress.com](http://pielkeclimatesci.wordpress.com)
- <sup>26</sup> WUWT. <http://wattsupwiththat.com>
- <sup>27</sup> For other examples including reports from UNAIDS and the World Bank, see Chapter 8 in Bauer (2012).
- <sup>28</sup> Climate Change: Evidence, Impacts, and Choices (2012).  
[http://www.nap.edu/catalog.php?record\\_id=14673](http://www.nap.edu/catalog.php?record_id=14673)

**HENRY H. BAUER**

Professor Emeritus of Chemistry & Science Studies; Dean Emeritus of Arts & Sciences  
Virginia Polytechnic Institute & State University  
[hhbauer@vt.edu](mailto:hhbauer@vt.edu); [www.henryhbauer.homestead.com](http://www.henryhbauer.homestead.com)

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## COMMENTARY

### Professor Bauer Has It Backwards

**PETER A. BANCEL**

Institut Métapsychique International, Paris, France

In his Essay Review “Climate Change Science or Climate Change Propaganda?” in this issue, Henry Bauer informs us that there is no valid scientific support for anthropogenic global warming (AGW), that predictions of future warming are erroneous, and that a propaganda campaign is being perpetrated by mainstream science to cover up these embarrassing errors. Worse, the campaign is succeeding as pundits and the media buy into the received dogma and the consequences appear dire. The world risks widespread and unnecessary economic disruption by responding to an illusory problem, and “highly informed experts” who challenge the climate change consensus are being hurtfully sneered at as they are dismissed out of hand.

The last point may be familiar to Professor Bauer, who is no stranger to readers of this Journal’s pages. He has long held a contrarian position on the causes of AIDS, and I imagine that the rejection of his views can sting. Still, it’s not clear what exactly has motivated Professor Bauer’s wide-ranging Essay Review, unless it’s just the lure of crusading against the imposed groupthink of mainstream science. That is fine and may well offer some good sparring as long as one prepares one’s case well. Unfortunately, Professor Bauer has not done so, and his arguments against AGW don’t stand up when confronted with the data and research.

To make his case, Exhibit A is the booklet *Climate Change: Evidence and Causes* published by the London Royal Society in collaboration with the U.S. National Academy of Sciences, which he dissects for us in his Essay Review. In a nutshell, he argues that since the science is patently wrong, disseminating a pamphlet affirming it amounts to proof of collaborative propagandizing. Along the way, we learn that comparisons with previous documents, duplicitous choices of wording, and even the use of British spelling provide supporting evidence of the collusion. However, it’s not necessary to debate these points. To undo Professor Bauer’s argument, it suffices to expose the errors in his claim that the science is wrong. The rest of his propaganda argument falls after that.

As the Essay does not describe the basics of climate science, it’s perhaps

helpful to give a brief sketch. Historically, the physics of the greenhouse effect was established and demonstrated in the 1800s. Greenhouse gases warm the Earth by blocking outgoing longwave radiation (heat), thus altering the energy balance between the Sun's warming and the Earth's natural radiative cooling. The first AGW predictions were made at the turn of the twentieth century based on estimates of CO<sub>2</sub> production from industrial sources. For a few decades it seemed that the oceans might safely absorb the excess gas until new calculations showed otherwise. That prompted the first systematic, ongoing measurements of atmospheric CO<sub>2</sub> by Charles Keeling beginning in 1958, and continued monitoring confirms the predicted, inexorable rise. The CO<sub>2</sub> greenhouse effect is thus straightforward physics, and this has been known about for almost two centuries. Today, humans add CO<sub>2</sub> to the atmosphere at 100 times the natural rate, and significantly faster than during the massive volcanic eruptions that led to severe climatic change and mass extinctions seen at several periods in the geologic record.

In the simplest terms, climate is determined by the concentrations of greenhouse gases (GHGs) in the atmosphere and by how sunlight hits and reflects from the Earth. Climate evolves when these change. The primary solar factors are the natural variations in the Earth's orbit and axis that cause the so-called Milankovitch cycles (with periods from 20,000 to 110,000 years) and drive the Ice Ages.<sup>1</sup> Although water vapor contributes a greenhouse effect two to three times larger, CO<sub>2</sub> is the main "control knob" of climatic change because its atmospheric lifetime is long and its presence is necessary to maintain the positive water vapor feedback. CO<sub>2</sub> has fast sources in volcanic eruptions and slow sinks in the weathering of silicate rock. Because the weathering is dependent on temperature, the long-term CO<sub>2</sub> cycle acts like a thermostat: When CO<sub>2</sub> concentrations go up the temperature rises, increasing the rate of chemical weathering which in turn brings CO<sub>2</sub> levels, and temperature, back down. Because this process is slow (it takes about a million years), it is possible for the sources to drive CO<sub>2</sub> levels far from equilibrium and cause excess warming for long periods. This is the basic concern underlying AGW: While some GHGs are relatively short-lived, the long atmospheric lifetime of CO<sub>2</sub> means we can set in motion climatic changes that will continue for many generations. Despite the added complexity of feedbacks and other factors, advances in paleoclimatology during the last 30 years make clear CO<sub>2</sub>'s central role in the Earth's climate. (By the way, Richard Alley's excellent lectures on the subject are available on the Internet and are great fun to watch.)

Understanding things on shorter timescales requires finer data and modeling of the physics, chemistry, and biology that affect GHGs,<sup>2</sup> changes

in the Earth's albedo (the average surface reflectivity, which depends on ice, snow, cloud, and vegetation coverage), and alterations of heat transport mechanisms associated with ocean currents. The dynamics on the timescale of a century, relevant for our current situation, is where computer models come into play. Climate models are important in part because they provide projections of future climatic changes for policymakers and industry leaders. Demonstrating the models' reliability is thus essential if they are to inform policy debates, and this is done by determining how well model simulations of the past agree with historical records and temperature reconstructions from geological and other Earth science datasets. Professor Bauer's main contention is that the models fail to reproduce the temperature records of the last century, which leads him to conclude that pamphlets affirming future climate change are unscientific propaganda.

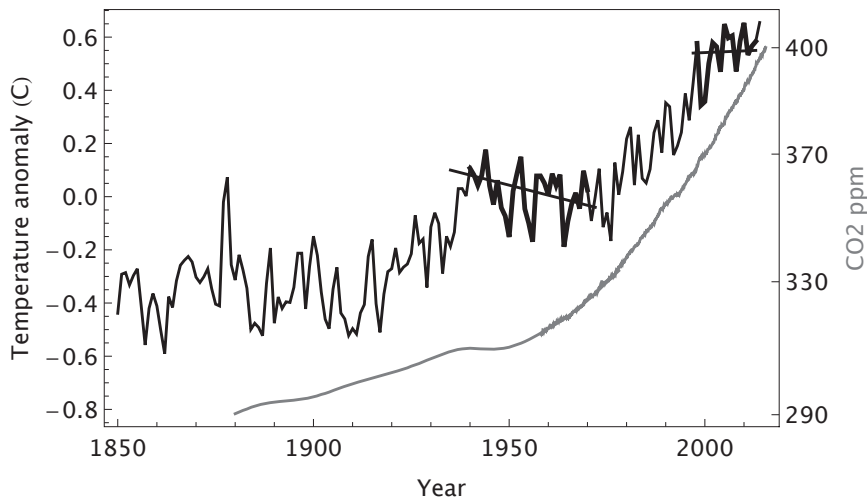
The contention that the models fail is simply wrong. The models not only reproduce recent global temperatures well, down to the scale of a few decades, but more importantly they show that without the presence of extra CO<sub>2</sub> from the burning of fossil fuels, the temperature rise of the last century cannot be explained. Professor Bauer errs because he draws much of his information from dubious sources found on the Internet (as we see from his Notes and References Cited) rather than from the published research. He also confuses how models differentiate between long-term warming trends and natural short-term variability. To understand this, we can indicate where the research contradicts his conclusions. Professor Bauer begins by claiming that the models are based on unfounded assumptions (p. 626):

the mainstream position rests chiefly on two unproven points:

1) . . . any heat absorbed in the atmosphere by CO<sub>2</sub> must go into heating the atmosphere, earth, and oceans. Further, computer models . . . assume that a feedback mechanism amplifies the heat absorbed by atmospheric CO<sub>2</sub> (Singer 2014).

2) Misconstruing as evidence of causation the gross overall correlation from about 1850 to the present between CO<sub>2</sub> levels and global temperature. But correlation never proves causation.

He goes on to argue that the climate is too complex to model, the proof being that models fail to reproduce two periods of the global temperature record: the slight cooling period of 1940–1970 and a “warming slowdown” over the period 1998–2012 (see Figure 1). Claiming, incorrectly, that the models fail, he concludes:



**Figure 1. The Berkeley reconstruction (<http://berkeleyearth.org>) of global surface temperatures showing a rise of about 1 °C over the last century.** The periods in question are indicated by bold traces and trend lines. The smooth curve shows atmospheric measurements of CO<sub>2</sub> concentration in parts per million (ppm) from 1958 to the present, and a reconstruction from ice cores for earlier years. While the temperature rise is seen to track CO<sub>2</sub> over the long term, the relationship is not expected to be linear. Variability in both natural and anthropogenic forcings results in decadal variations of global surface temperatures that are accurately captured by climate models. Fluctuations in the temperature record arise from both climate variability and measurement uncertainty. The latter has improved over time, which explains the decrease in the year-to-year fluctuations in the last 50 years or so.

No model accounts for that, showing that natural influences missing from the models can outweigh any greenhouse warming by CO<sub>2</sub>. It follows that no projections from these models into the future should be taken seriously.

To take his points in order, the CO<sub>2</sub> greenhouse effect is a simple, well-established fact of physics. Similarly for the amplifying effect of atmospheric water vapor (referred to in point 1), which increases with temperature and provides a positive feedback to CO<sub>2</sub> warming. These are anything but “unproven,” and denying them implies we must jettison physics, a move that Professor Bauer might resist upon reflection.

The correlation-is-not-causation argument reveals that Professor Bauer does not contest the last century’s overall increase in global temperature, but it makes for a disingenuous gambit. A main occupation of science is precisely the determination of causes of correlations, and he implies that this has been

collectively forgotten by climate scientists. It is obvious that determining causes requires additional evidence from measurement and experiment. The International Panel on Climate Change (IPCC)<sup>3</sup> uses the term “attribution” to indicate causal determination, and it devotes considerable attention to attribution methodology (Hegerl 2007). The converging lines of evidence for a causal relation between warming and GHG concentrations are clear. Spectroscopic measurements of atmospheric radiation at the Earth’s surface show an increase at precisely the wavelengths corresponding to GHG emission lines, and the spectral intensities track with GHG concentrations (Feldman et al. 2015). Satellite measurements of outgoing radiation find a corresponding deficit at these wavelengths. A key observation is that, as predicted by greenhouse warming, the lower atmosphere is warming faster than the stratosphere (the opposite would occur if warming were due to increased solar activity). These and other measures leave no doubt that the warming is due to increasing GHG concentrations.

The attribution of global warming to anthropogenic causes can also be demonstrated by varying climate-sensitive parameters of the Earth system. Although experimentation on this scale isn’t possible (aside from the one experiment we are currently running by burning fossil fuels), computer models can serve as surrogates for the global climate system. Climate science makes extensive use of modeling to make causal determinations and draw other useful inferences about the Earth’s climate. Professor Bauer’s main objection focuses on model uncertainties, and further on in his Essay (p. 633) he summarizes:

... the prediction of long-term change resulting *primarily* from steadily increasing atmospheric CO<sub>2</sub> comes from computer models that account for neither the “slowdown” of the last decade-and-a-half nor the cooling from about 1940 into the 1970s that had then caused climate scientists to warn about an impending Ice Age. These failures demonstrate unequivocally that the computer models are flawed; since they are wrong even in the short term and for the recent past, they certainly cannot be given credence for the longer term.

Not only is the argument’s premise incorrect, but to reason that the accurate prediction of short-term variability is required for reliably establishing long-term trends misconstrues how models are used. It confuses the detailed dynamics of the climate system, which is complex and requires fine-grained data to model, with the drivers of overall change, for which the variability averages out. Following his reasoning, we should not trust regional weather forecasts—notably accurate these days—because they cannot predict when a thunderstorm will occur in my neighborhood.



The extent of long-term warming predicted by the models of 20 years ago agrees remarkably well with the far more sophisticated models of today. This is precisely because the overall warming trend is a consequence of known physics and chemistry, and these are adequately represented by earlier models. The reason climate science continues to refine its models is not from a need to establish further evidence for AGW (even without models, theory and measurement alone make the case), but rather to provide guidance for solutions to the problem. This includes insight into how climate change may impact different regions of the globe and understanding how climate sensitivity depends on factors we can measure and perhaps control.

Far from calling the reliability of models into question, the two cited periods provide case studies that demonstrate the models' utility. These periods have been studied extensively and the factors responsible for the short-term variability identified. When the data are input to models, there is good agreement between them.

The period of slight cooling from 1940 to 1970 was largely due to an increase in sulphate aerosols from industrial pollution during World War II and the ensuing post-war economic expansion. Aerosols contribute to the Earth's albedo by reflecting sunlight, and the magnitude of this negative forcing accounts for most of the temperature decline. Among the supporting evidence for aerosol cooling are decadal records that show cooling for daytime measurements only, with rising temperatures for nighttime data. This is to be expected if greenhouse warming is concurrent with aerosol cooling (which happens only during daylight hours). The aerosol albedo overwhelmed greenhouse warming, but both effects were active, and this can be seen in the data. Overall warming resumed as CO<sub>2</sub> levels continued to rise and as the aerosol concentrations declined with the passage of the U.S. Clean Air Act of 1970 and similar laws in other developed countries. Models that input the historical aerosol levels yield temperatures consistent with the historical record for the period.

In passing, Professor Bauer references an article in the U.S. news publication *Time* to suggest that climate scientists mistakenly interpreted the cooling as the start of a new Ice Age. There was speculation about whether the cooling could persist, but it was a minority view and the prospect of AGW dominated the discussion even then. From 1965 to 1979 only 7 of 68 papers addressing the topic advanced an Ice Age explanation, while 42 proposed AGW as the most important force shaping the planet's climate on human timescales (Peterson, Connelley, & Fleck 2008). The Ice Age proposal was dropped as data and modeling made clear the interplay between AGW and aerosol cooling. The episode is historically noteworthy since it marks the transition of a fledgling field into a mature interdisciplinary

science as geologists interested in Ice Age dynamics and atmospheric chemists working to measure and model changes in CO<sub>2</sub> concentrations came together to understand the brief period of cooling.

The more recent and much weaker “warming slowdown” has received considerable attention. In the big picture, it is not particularly important since the main concern is how climate will change in the longer term, and not the occurrence of expected short-term variability. The oft-repeated claim that “the warming has stopped” is a misreading of the data that is understood correctly once natural and anthropogenic factors of climate variability are input to the models. These include aerosols from a documented increase in volcanic activity in the 2000s and the occurrence of several strong cooling episodes of the El Niño Southern Oscillation in the Pacific (the ENSO circulation transfers heat between the atmosphere and the subsurface ocean; it is one of the most important drivers of short-term climate variability). The negative ENSO ended in 2013. Not incidentally, 2014 was the hottest year in the global temperature record and 2015 is on track to beat that record.

The 1998–2012 fluctuation has been studied intensely, for two reasons. First, the most recent IPCC assessment<sup>4</sup> highlights that the period’s duration of 15 years corresponds roughly to the prediction horizon of current climate models—the timescale over which uncertainties in the modeled short-term variability grows large. Earth data from the period is the most detailed on record, particularly for ocean currents and temperatures,<sup>5</sup> and this provides an opportunity to sort out different contributions to the variability. Several journals have recently devoted special attention to research on the “slowdown” fluctuation and the lessons it implies for models.<sup>6</sup> Second, although the fluctuation has been widely misrepresented to claim that AGW has “stopped,” it is an instance of the climate variability that is expected to occur even as warming continues, just as was seen for the 1940–1970 period. The distinction between natural variability and model uncertainty is not easy to convey to the public, and new research has addressed this issue as well (Lewandowsky, Risbey, & Oreskes 2015).

By varying parameters during simulations, models can reveal how different factors affect climate change. This flexibility has also been used to test model reliability. Two examples are worth noting. One is that if the human contribution to atmospheric CO<sub>2</sub> is removed during simulations, models do not reproduce the observed temperature rise, even when other parameters are allowed to range freely<sup>7</sup> (Meehl et al. 2004). When the extra CO<sub>2</sub> is included, models track the direction and magnitude of the temperature trend. Second, as reviewed above, short-term temperature trends can be reproduced when models are initiated with real world data. A recent study (Risbey et al. 2014) turns this around to show that when models that allow

for random ENSO variability are run, the simulations that most closely match the observed temperature trends are those that, by chance, selected the actual El Niño record. This further indicates that models reliably track the relation between ENSO and global temperature variability.

Professor Bauer is right to think that variability in ocean currents, volcanic activity, and the like contributes to model uncertainty. This is why many simulations are averaged when estimating the longer trend of AGW. But it is incorrect to state that projections of long-term trends are unreliable, or that models cannot estimate variations in the historic temperature record when initialized with real world data within the prediction horizon.

Professor Bauer's indictment of climate models shows that he largely ignores the extensive research of the past decades, and it is interesting to see where he gets his information. A number of the sources are from people who are not climate scientists and from websites that engage in ideological advocacy. We can look at a few. Fred Singer is a physicist respected for his contributions to the earth and space sciences. However, he has spent much of his career in campaigns to refute the scientific basis of policy responses to the ozone hole, acid rain, and the dangers of second-hand smoke. Not only was the science proven correct in each case, but the cooperation between scientists, government, and industry greatly mitigated adverse impacts and demonstrated that viable solutions to complex problems are possible when institutions work together. On a personal note, I was employed at IBM research in the late 1980s and was impressed by what I saw of IBM's involvement in the effort to reduce CFC emissions and mitigate ozone depletion. Management was not thrilled by the cost and effort it demanded, but as the science was clear, adequate solutions were sought, industry standards were implemented, and things moved on. To my mind, the characteristic objections of capable scientists like Singer and Professor Bauer have more to do with a lack of trust in cooperation and governance, than in the science itself. A profile of Dr. Singer that elaborates on this point is found in the book *Merchants of Doubt* (Oreskes & Conway 2010), which sheds light on the murkier side of the climate debates.

Among the texts cited in Bauer's Essay Review is a self-published handbook by meteorologist David Dilley. Dilley argues that climate scientists are in it for the grant money and that, anyway, climate always changes:

In the Old Testament of the Bible, Genesis I (Verses 9–19) says the cycles of earth's days, seasons, oceans, and atmosphere were created by God the creator of earth and the universe. Does this mean humans have created global warming? Of course not, these are God's natural cycles.

This does not give us much confidence in Professor Bauer's selection of sources, but it does suggest how errors make their way into his Essay Review. The remainder of his text veers between unsubstantiated critiques of the science and supposed evidence of incompetence and propaganda. There is too much to rebut and this is not a surprise. A common rhetorical technique (colloquially known as a Gish gallop) is to overwhelm any rebuttal by presenting a raft of superficially plausible half-truths and strawman arguments that take unreasonable effort to unwind and refute. Indeed, it has taken several pages just to expose the claim that models fail, and we've only advanced a few paragraphs into the Essay Review. But since that is the core of the argument for "propaganda science," it is not necessary to go through it all. Many of Professor Bauer's arguments are standard fare, and there are resources that catalogue and carefully refute the most common myths. The resources also provide copious citations toward the primary literature.<sup>8</sup> Still, it is useful to drive home the point by going through several more of Professor Bauer's objections.

Next in the Essay Review is a claim that the recent warming cannot be "unprecedented" because past temperature records are not accurate or fine-grained enough to support such a claim. However, nowhere does the booklet under review assert that the warming is unprecedented (that qualification is attributed to recent CO<sub>2</sub> concentrations, not temperature). Even if one were to find such a statement somewhere else, and I imagine one could, it is beside the point. The worry about AGW is that the current increases of CO<sub>2</sub> and global temperature are fast by paleoclimatic measures, the rate of warming is increasing, and without action we risk significant levels of warming by the end of the century.

Professor Bauer complains about the use of statistical confidence intervals and their translation to terms such as "likely" or "highly likely." These are used to express degrees of uncertainty in data and analyses and he finds this unacceptably subjective. However, this is standard terminology for statistical uncertainties used across many disciplines and is explained in detail in the research literature, including the IPCC's reports.

Next is a meme found in the blogosphere that aims to cast doubt on climate scientists' integrity: In response to the alleged failure of models to account for the "hiatus" in warming since 2000, climate scientists have conspired to adopt the term "climate change" and drop "global warming" from their lexicon, thereby inoculating themselves against embarrassment due to the "pause" in warming.

Until a few years ago, "global warming" was the universally used shorthand for human-caused global warming. But since there has been no appreciable

warming globally for the last 15 years or so, the critics of carbon emissions have been using the term “climate change,” which cannot be contradicted or falsified.

The two terms mean different things, and both have been used in the scientific literature for more than 40 years. Global warming refers to a global rise in the Earth’s temperature due to increased GHGs, and climate change refers broadly to any alterations of the climate that result. Professor Bauer’s claim that substituting “climate change” for “global warming” is a recent “rhetorical sleight of words” is untrue. The IPCC was formed back in 1988 and I don’t need to remind the reader what CC stands for. A seminal 1956 paper on the topic was entitled “The Carbon Dioxide Theory of Climatic Change” (Plass 1956). In the research literature, “climate change” has always been the more frequent term, its use predating “global warming” by a decade or two.

The use of “global warming” in the press and media did spike sharply in 2007 shortly after the success of Al Gore’s documentary film *An Inconvenient Truth*, which employed the term extensively. After a few years, media usage of “global warming” declined and today both terms are used by the media with roughly equal frequency.<sup>9</sup> This is likely due to journalists gradually adopting climate scientists’ established language as well as an increased awareness that climate change more accurately describes the diverse impacts of warming such as species extinction and ocean acidification.

Professor Bauer makes much ado about extreme weather claims. It is not surprising that the news media may overstate a connection between weather events and climate, but he misrepresents what is said in the booklet, and by climate scientists in general. The chief concern raised by climate scientists is that warming increases evaporation, exacerbating droughts, while the excess water vapor in the atmosphere favors more intense storms. Other factors associated with warming can disrupt weather patterns and cause more severe coastal flooding, but caution is the byword in drawing conclusions about single events, and this is adequately expressed in the Royal Society’s booklet. Recently, a methodology for addressing the climate–weather relation has been developed (Trenberth, Fasullo, & Shepherd 2015), and research into the question is ongoing.<sup>10</sup>

Other objections concerning the Medieval Warm Period, the Little Ice Age, Arctic versus Antarctic sea ice loss, the role of the Sun, or why temperature initially precedes CO<sub>2</sub> rise when exiting the Ice Ages would take far too many pages to elucidate here. The curious reader is invited to refer to the cited resources,<sup>8</sup> where clear and concise rebuttals based on the science and the research can be found.

In conclusion, the claim that AGW skeptics have been treated unfairly by an organized propaganda campaign is a distortion that ignores the scientific research. In fact, Professor Bauer has it backwards: The organized distortion is quite the other way around.<sup>11</sup> Nor is it the case that the assessment of AGW is limited to “mainstream science.” Numerous independent institutions that have the resources and expertise to fully review the evidence concur that the international community needs to address the problem, despite the potential challenges the position implies for their various activities. They include<sup>12</sup> petroleum companies (Shell Royal Dutch, BP, Statoil, and ConocoPhillips, among others), the U.S. Department of Defense (especially the U.S. Navy), not to mention the Chinese government and many more.

### Notes

- <sup>1</sup> On very long time scales, a monotonic increase in the sun’s total energy output also needs to be taken into account. Total solar radiance increases by about 1% every 100 million years.
- <sup>2</sup> This includes a fast cycle of CO<sub>2</sub> exchange between the atmosphere, oceans, and the biosphere.
- <sup>3</sup> The International Panel on Climate Change is the United Nations body charged with synthesizing the evidence for climate change for the world’s governments in order to provide a common basis for policy deliberations.
- <sup>4</sup> AR5, Working group 1, chapter 9, box 9.2.  
<http://www.ipcc.ch/report/ar5/wg1/>
- <sup>5</sup> Monitoring of ocean temperature data was vastly improved in 2000 with the deployment of the Argo array of free-floating instruments. The 4,000 GPS-linked Argo floats relay in real time information on ocean currents, temperature, and salinity down to depths of 2,000 meters.  
<http://www.argo.ucsd.edu/>
- <sup>6</sup> See issues of *Nature Geoscience*, 7, (March 2014); *Nature Climate Change*, 4, (March 2014); *CLIVAR*, 15, (Summer 2015).
- <sup>7</sup> A helpful graphical depiction of the relative contributions of different climate forcings can be found at <http://bloom.bg/1GppERp> (on Bloomberg Business).
- <sup>8</sup> Two helpful resources that explain the science and provide rebuttals to standard counterarguments are <http://skepticalscience.com/argument.php> and <http://climate.nasa.gov/>
- <sup>9</sup> For media usage, the reader can make a comparative search of “global warming” and “climate change” on *GoogleTrends*.
- <sup>10</sup> A list of recent publications that treat the connection between weather and climate change can be found at <https://www.climatecommunication.org/new/features/extreme-weather/>

- <sup>11</sup> Funding by industry front groups that distort the science is well-documented. ExxonMobil and Koch Industries have been particularly active. <http://insideclimatenews.org/content/Exxon-The-Road-Not-Taken> (Brulle 2013). U.S. climate scientists have had their emails stolen, been faced with unfounded threats of legal action by U.S. Senators and District Attorneys, been subjected to abusive Freedom of Information requests and their personal information having been posted on advocacy websites, received threats to their persons and their families.
- <sup>12</sup> 81 major American companies have recently announced their support for a successful outcome to the Paris COP21 accords for international cooperation on climate change. <http://cop21.org/white-house-announces-commitments-from-81-us-businesses/>

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## COMMENTARY

### **Notes on the Essay Review by Henry Bauer of *Climate Change: Evidence and Causes***

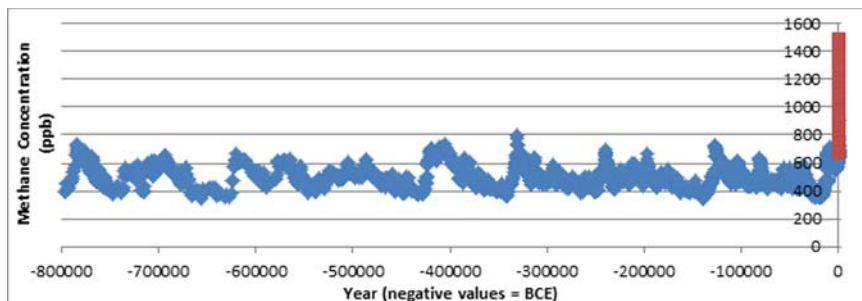
**ANDREW FOSS**

**Abstract**—The National Academy of Sciences and the Royal Society have produced a booklet (the text) for the lay person on the current state-of-the-art understanding of climate change. Our esteemed reviewer of the text has accused the eminent climate scientists of ‘propaganda’. The *Journal of Scientific Exploration* requested a Commentary on the Essay Review. Let us attempt to objectively assess both documents. These Notes start with a few comments on climate change issues that may set some aspects straight, and then go through the Essay Review point by point to discuss its concerns in the light of the text.

#### **Background**

Since there is so much heated debate on climate change, it would serve us by beginning with the facts over which there is no dispute among the learned.

1. Burning fossil fuels releases carbon dioxide.
2. Carbon dioxide is a greenhouse gas and many other greenhouse gases are being released by our modern society as a result of various activities.
3. Greenhouse gases trap heat, so ocean and/or atmospheric temperatures have risen and/or will rise due to their presence.
4. The carbon dioxide levels in the atmosphere have risen since the start of the industrial revolution, after a long period of stability.
5. CO<sub>2</sub>, methane, and nitrous oxide levels are higher now than at any time in the last 800,000 years (see text, also *Wikipedia* Milankovitch Cycles, and Rignot, Fenty, Xu, Cai, & Kemp 2015).
6. The global temperature has risen primarily over the last 100 years coincidental but not exactly correlated with the ‘hockey-stick’ shaped rise in CO<sub>2</sub>, NO, and methane (e.g., see Rignot, Fenty, Xu, Cai, & Kemp 2015, and Figure 1).
7. Insolation, the Sun’s contribution to global heating, is currently at a low. While it oscillates, with three peaks and three troughs in the last 100 years, overall it has been declining for the last 6,000 years.



**Figure 1. Atmospheric methane levels for the last 802,000 years.** Levels first exceeded 850 ppb in 1894 having risen above 800 ppb in 1871 (Rignot, Fenty, Xu, Cai, & Kemp 2015). CO<sub>2</sub> and NO graphs follow the same overall hockey-stick pattern.

Calculations show it is expected to start a short-term cyclical rise about now after a period of about 20 years of decline (*Wikipedia* Milankovitch Cycles).

CO<sub>2</sub> levels have risen unremittingly since around 1800, exhibiting a near-exponential curve and closely tracking our thirst for fossil fuels and our burgeoning population. However, temperature has not exhibited such a satisfyingly simple trajectory. No one can argue that the temperature has not risen, but there is clearly much enthusiasm for arguing about the details and causes. While the text argues that the temperature rise is most likely due to rising greenhouse gas levels, the Essay Review prefers to see these as a coincidence. Several of those holding this view have turned out to be connected in some way to the energy industry, suggesting that this is more about corporate profits than science. However, scientists like to debate, and any rigorous or at least well-founded argument deserves consideration. Below, we look in the present review for such arguments.

If one studies Figure 1a of the text, one can see that the global temperature only started rising around 1910. From about 1945 to 1980 it flat-lined only to track strongly upward until about 2000 when it flat-lined again. Purely from looking at the curve, without testing the significance of the trends, one might assert that there has been a slight downward trend in the periods 1880 to 1910, 1945 to 1980, and 2000 to the present. In short, there appears to be a 30-year cyclic trend between warming and cooling, with a strong overall upward trajectory that started during the industrial revolution. If this is a real and persistent trend, then we can expect potentially devastating warming to set in starting about 2030.

It should be noted that these periods when global temperatures have flat-lined or retreated appear to coincide approximately with declines in insolation (see point 7 above). Similarly, the recent periods in between coincide approximately with increased insolation. However, insolation does not explain the overall sharp rise in temperatures as no such phenomenon was occurring in the 19<sup>th</sup> century and the cyclic nature of insolation is a long-term astronomical phenomenon. Had the temperature changes been driven by insolation as the primary factor, then the overall trend would be downward.

One prime cause for concern, highlighted in the text, is that the ocean heat content anomaly is rising unremittingly, which many scientists think is a prime explanation for where the extra heat is currently going. If there is a cyclic trend of heat exchange with the oceans, then a decadal scale oscillation in the surface temperatures is only to be expected. The steady rise in the ocean levels during the same period (around 1970 to the present) as the rise in the heat anomaly supports this concern. The main contributor to this rise is the fact that water expands with temperature above 4 °C.

### **Reviewing the Essay Review**

Now, let us review the points raised by Dr. Bauer. The first is that he accepts that CO<sub>2</sub> absorbs radiation but doubts that the re-radiation of that heat warms the surrounding atmosphere or the land or ocean below it. The only way that a body subjected to heat does not warm is if it is a super-fluid, a resistanceless heat conductor. Super-fluids can be observed in the lab, but neither the atmosphere nor any part of the Earth's surface are super-fluids.

Second, aside from his doubts that humanity is capable of modeling the climate, he raises the standard concern that temperature is not perfectly tracking CO<sub>2</sub> levels and is sometimes going down when CO<sub>2</sub> is rising. This we have discussed above. Aside from insolation, the text discusses heat exchange between the atmosphere and the oceans. Anyone who follows ocean temperature maps cannot miss the remarkable rise in ocean temperatures.

One of Dr. Bauer's main arguments relates to the difficulty of measuring global temperatures for the time before modern instrumentation, thus casting an element of doubt on all science that relies on historic records such as ice cores, tree rings, etc. No doubt thousands of academics would object strongly to this and argue much more convincingly for their results than is possible in this short piece. However, the reviewer might like to compare the studies that compare astronomical phenomenon with the historical temperature records. Since the work of Milankovitch, it has been widely accepted that long-term climate changes are being driven by varying

levels of solar insolation due to astronomical cycles, and the long-term temperature records are the main evidence (see *Wikipedia* Milankovitch Cycles for references). If these were of little use, why would they agree with such an obvious source of heating? This is not suggesting that we fully understand why some astronomical cycles are more evident than others during different epochs, but to dismiss the temperature records that have been built up by a vast and disciplined effort of scientists from all over the globe using multiple sources is rather too extreme to accept. Besides, the Essay Review makes its own arguments based on historic temperature variations and thus on the temperature records that we have.

Having expressed a lack of confidence in computer models and the underlying historical data, he addresses some of the wording in an IPCC report. He objects to assigning an approximate numerical value to the confidence of a scientific research finding. This is perplexing because virtually all science, certainly including climate science, involves error bars or ranges and/or statistical significance values, both of which do exactly that. In essence, his objection appears to be that climate science is a collection of purely subjective opinions that cannot be quantified. In any case, what this has to do with the text being reviewed is not clear. The text does not use the IPCC system of assigning levels of confidence, while making plain where uncertainties exist.

Having dismissed computer modeling, the underlying data, and the conclusions drawn thereupon, without unpicking even one study to demonstrate this, Professor Bauer arrives at the conclusion that the whole thing is a sham. This is a profound tautology. My question is, how does this advance our understanding? Our knowledge of the human body is far from complete but does that mean that we can have no confidence in modern medicine? Or should we withhold medicines that have shown promise until our knowledge becomes perfect? Some medicines get withdrawn because they prove toxic or ineffective, but, as a society, we accept that for the good that is achieved overall.

In his “Just so” section, Professor Bauer suddenly puts forward a powerful argument for why increased temperatures will give rise to more extreme weather. As he explains, heat tends to even itself out, so more heat means more movement; that means, of course, stronger winds carrying increased water loads due to greater evaporation and so more severe storms, flooding, tornadoes, etc. This is all logical and well-known theory, but what is not clear is how he then concludes that this amounts to *fewer* unusual events (his italics).

His next concern is that we have “no evidence” for extreme weather in the time of the dinosaurs when it was much hotter than today. He has

already asserted that what evidence we have about the climate in the distant past is unreliable and, as he knows, what data we have is not sufficient to tell us about day-to-day climate events. Certainly the text is not making claims about the era of the dinosaurs.

He objects to the arguments about the relative rates of melting at the Earth's poles, but while the text does make a brief attempt to address a complex issue the reviewer declines to argue against the points made. It may be worth noting that the alarm bells about Greenland and the Antarctic appear to have been ringing this year as a batch of new studies indicate that some of the glaciers may be more prone to discharging into the sea under current conditions than even the previous science had indicated (e.g., Rignot, Fenty, Xu, Cai, & Kemp 2015). Melting of ice from the land is much more serious for us because it leads to rising sea levels.

His next point is that there is "no empirical evidence for an increase in extreme weather events in the last several decades." Perhaps he would like to review, for example, the figures published by reinsurance underwriters (e.g., Swiss Re 2014). It might help to quote here from the website of Swiss Re, one of the top global underwriters:

Given Swiss Re's role as an ultimate risk-taker, we are uniquely exposed to the impacts of climate change. We identified climate change as an emerging risk some 20 years ago. . . . If unmitigated, climate change could cost the world economy around 20% of Global GDP by the end of this century. (Swiss Re 2015)

The pullback on flood insurance in the U.S. and elsewhere is well-known. Many homes have lost significant value as they have become uninsurable for flood risks and therefore cannot be mortgaged. Simply quoting the number of hurricanes that have affected the U.S. means little in this regard.

At this point he makes a genuine complaint against the text. He is right that if one says that short-term effects cannot be ascribed to climate change, then one cannot cite individual heat waves, etc., as evidence. However, it is entirely valid for the text to assert that heat waves have become more common over recent decades as the evidence supports that (see *Climate Communication Science and Outreach 2015* for a good summary with many references). It should be noted that it is possible to assess the probability of a single heat wave being due to climate warming.

Dr. Bauer expresses concern about the periods of warming that have occurred over the last few thousand years. The implication is that this is just another one of those warming periods that occurred in the absence of any substantial rise in greenhouse gas levels. For example, the Medieval Warm

Period (MWP) was a little warmer than the surrounding periods (about 0.1 to 0.2 °C, though this depends on which temperature reconstruction series you pick), but never reached the current temperature globally and did not show any of the rapid rise we currently observe (see the references listed in *Wikipedia* Medieval Warm Period). Insolation was higher then, and other regional factors likely played a major role as certain areas were much warmer than others.

It is noteworthy that Bauer cites Dilley in this regard. David Dilley is a meteorologist who developed a theory based on the Milankovitch cycles and a gravitational theory to model the historical temperature fluctuations. He also chose to ignore the effect of rising greenhouse gases. In 2008, he claimed that his model had “a near 100% correlation” to global warming cycles, including the recent warming, and made a firm prediction that 2008 was the beginning of a strong cooling that would see a “climate similar to the 1800s within the next 15 years” (Icecap blog). The complete failure of this prediction, which had been feted by the skeptic community at the time, should have given them pause.

Our nearest neighbor Venus is our best example of a runaway greenhouse effect. One reason why it will be difficult to find advanced life forms in our galaxy is that, within the habitable zone of a solar system, there is a very slim chance of finding a planet that is not in ‘ice box’ or ‘hot house’ mode. Our planet sits right on the cusp and thus oscillates between these modes. Climatic periods as benign as the current climate are rare even for Earth.

Our reviewer objects to the concern about sea level rise because sea levels have changed faster in the geologic past. However, if he lived near the beach, he might retract his words. Being told that the water level was 5 m higher 125,000 years ago does not help when you are facing a rising threat of inundation. His point is that the sea level rise might not be due to anthropogenic causes so let’s not do anything to change our behavior. However, human nature is to try to adapt, so such appeals, presumably on behalf of the energy and related industries, may not succeed despite their deep pockets. It might succeed, however, in delaying action beyond the point that things can be turned around. Perhaps it already has. His reference (Gornitz 2007) states that

Over the past few thousand years, the rate of sea level rise remained fairly low, probably not exceeding a few tenths of a millimetre per year. . . . Twentieth century sea level trends, however, are substantially higher. . . . The current phase of sea level rise appears to have begun in the mid/late 19<sup>th</sup> century to early 20<sup>th</sup> century. . . . (Gornitz 2007)

The next section makes arguments about the use of this or that word in the text and falls back on the 'no recent warming' issue. In every war there are periods of quiet, so are we to unilaterally declare the war is over and lay down our arms? Is this a smart approach to cyclical phenomena when the overall trend is so clear and the likely cause of it is also rather evident? If Dr. Bauer could put down his temperature graph for a moment and study the greenhouse gas graphs, he might have a change of heart.

His section on 'misdirection' repeats his lack of confidence in the computer models and the explanations of the climate science world for the apparently cyclic variations in temperature as already repeatedly discussed. He returns to the issue of the MWP saying it was warmer than the present, which is disputed as the global average was lower (see Climate Communication Science and Outreach 2015). His problems with the Little Ice Age (LIA) are even harder to fathom, as declining insolation can be expected to lead to cooling. According to the studies on the LIA period, the degree of cooling was very varied by locale so there are reasons to even assert that no global phenomenon was in play (*Wikipedia* Little Ice Age). There is also evidence to suggest that the LIA was the result of aerosols due to volcanism (Miller et al. 2012). Aerosols both from increased evaporation due to hotter ocean surfaces and human industrial activity are likely playing a role in moderating recent global temperatures.

Dr. Bauer may be telling us that current climate change is just another fluctuation in a history of fluctuations, but the graphs suggest otherwise. We have not seen such an accelerated rise in temperature at any time over the recent historical period he is discussing. Besides, he is not offering any alternative explanation for the cool and warm periods.

Amazingly, he starts comparing the change in global temperatures to the total change since the peak of the last glacial maximum. Surely he must understand why that makes no sense? Each interglacial starts with a multi-degree rise in temperature followed by a long period of relative stability. This is where we are now, and the current sharp change in the temperatures cannot by any stretch of the imagination be dismissed as negligible on the grounds that larger changes have occurred in the geologic past under entirely different circumstances. People are concerned because we inherited an Earth with a very benign climate that we are seeing sliding away from us.

The same applies to his comment about seashells. Marine biologists are not sounding the alarm for no reason, they are measuring what happens when CO<sub>2</sub> is dissolved in water. This shows that the shells of some creatures thin while others thicken (Ocean Acidification). Thus, finding evidence of marine creatures with shells from periods in the past with higher CO<sub>2</sub> levels is *prima facie* a non-issue.



The notion that there is no correlation between CO<sub>2</sub> and temperature over the history of the Earth is another peculiar suggestion. Long-term plots of the two series show remarkable correlations but skeptics pounce on the lags that can be picked up. In the past, when interglacials start, temperatures rise first for a while before CO<sub>2</sub> starts to lead. This is standard climate theory, where warming leads to the release of reservoirs of greenhouse gas which then accelerate the warming. Skeptics argue that the initial impulse of warming was not caused by CO<sub>2</sub>, which is true at those moments when factors such as peak insolation were driving a major shift in the climate. These occasions have shown us how powerful greenhouse gases are in intensifying climate change, and this is a key reason for the concern of climate scientists.

The reviewer asserts that all authors to a paper have to specify exactly which words or sections they wrote. One might ask the reviewer if he ever did that in any of the papers he published with co-authors?

It is a pity he does not properly develop his point about water vapor as a greenhouse gas. Obviously if water heats up, evaporation increases. However, what role this plays in climate change is not addressed either in the text or the review. The implication is that the scientists are hiding something, but it could be just as possible that the intensified cycle of evaporation and precipitation is not playing a clear role in modifying temperatures. After all, clouds may trap heat but they also reflect incoming radiation back into space.

### Summary

We have sincerely worked through all the points of the reviewer and given his due where earned. However, he has not shown any evidence that the text has done more than state the current view of published peer-reviewed climate science. He is right to say that we do not perfectly understand climate, but his only real argument against the main concern, the consequences of rising greenhouse gases, is that even though he accepts that they trap and re-radiate heat, he believes that there is no evidence that this warmth is being retained by either the atmosphere or the Earth's surface.

The text with its cautious presentation of data without pressing for any particular response hardly fits the definition of 'propaganda'. The reviewer expresses a lot of anger but fails to take up any issue in adequate detail to advance our understanding either of the science or the real source of his concerns.

### Afternote

Both the climate science report and the skeptics such as Dr. Bauer are ignoring the third group that has a point to make about this debate. This group consists of oil industry geologists and analysts who have for many years been telling us that we are approaching the time when half of all Earth's readily extractable fossil fuels will have been consumed. For a while it seemed that their view was becoming widely accepted, and then the shale oil and gas boom started and public attention was diverted (U.S. Energy Information Administration). Shale oil companies, seeking investors, touted the boom as supplying hundreds of years of energy needs. However, the reality has proved quite different as whole shale plays turned out to have a useful life of little more than 10 years with rapidly declining production from all but the newest wells (Hughes 2013). While there is scarcely any talk of peak oil these days, it is obvious that government and industry leaders are fully factoring this in to their plans. They appear to be scaling their announcements about going 'carbon-free' with what the experts are telling them about when there will be very little carbon left to burn. There seems to be a well-coordinated plan to gradually condition public awareness to an electric world powered by windmills without stirring up a hornets' nest of rage about having our SUVs 'taken away' from us.

In 2010, we noticed that no online report considered the run-down of all types of fossil fuels—oil, gas, coal, and uranium. People would write articles saying the 'answer' was more nuclear power without any due consideration about the availability of fuel, not to mention the disposal of the spent fuel. Thus it was decided to collect the best statistics on reserves, additions to reserves, and consumption and apply a business-as-usual model to estimate how long our supplies would last. This simple approach, which assumes that those with resources will sell them to whoever can pay, yielded a date around 2050 when global oil, coal, and natural gas stocks would be largely exhausted, and uranium, assuming no significant increase in consumption, would run out around 2090. Of course, from a local perspective the view is different; for example, if the U.S. keeps its coal exports low then it has more than 200 years' supply.

Since then, the flattening of the global economy led by China, which we predicted, has slowed the growth of consumption, and the reserves of coal and especially natural gas have been revised upward. This pushes these exhaustion dates somewhat further into the future (depending on one's confidence in the reserve figures). However, from a longer-term perspective it makes little difference and the total potential for greenhouse gas emissions is not very substantially increased.

Based on the text (page 20), if we emit as much carbon in the future as we have since 1870, there is a chance, albeit small, that the rise in global average temperatures can be kept to within the target 2 °C. Further, if one studies their graphs on page 22 and applies the apparently inevitable run-down of fossil fuel consumption over the next 30 to 60 years, then CO<sub>2</sub> levels, temperature, and ocean rise will be contained within tolerable limits and return to year 2000 levels by 2400 or so. This won't save us from many years of extreme weather events that we are already starting to experience, but, overall, we have to admit to being quite relieved seeing all the science come together and a somewhat more benign scenario emerge than we had expected.

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## COMMENTARY

### **Response to Commentaries by Peter Bancel and Andrew Foss**

**HENRY H. BAUER**

The chief point my Essay Review makes is that the 2014 booklet *Climate Change: Evidence and Causes* published by the [London] Royal Academy and the [U.S.] National Academy of Sciences gives only one side of the case about carbon dioxide and climate change. The chief assertion from the two commentators is that the mainstream side is right, therefore it's OK to be one-sided.

But there exists undeniably another side, a great number of dissenting voices, many of them from relevantly credentialed, competent, well-informed sources. I cited several including the Leipzig Declaration on Global Climate Change with its many signatories. To ignore them and the evidence and arguments they present is not what one expects from a proper scientific evaluation, which would engage the substance of what critics present. The pamphlet does not do that, and neither Bancel nor Foss claims that it does. They and the pamphlet are one-sided, and one-sidedness is propaganda, not a scientific assessment.

Neither Bancel nor Foss denies that the pamphlet expresses certainty at some places while admitting fallibility at others.

They do not deny that the computer models fail to account for the cooling trend during the 1940s to the 1970s and the lack of warming in the last decade-and-a-half or so. Forget theory and models and look only at the facts. Carbon dioxide was increasing steadily during the period 1940s to 1970s and since about 2000, at the same time as temperatures were falling or remaining unchanged. Obviously, carbon dioxide was not the main determinant of temperature for about 1/3 of the period during which human generation of carbon dioxide is supposed to have become the chief contribution to global warming. Bancel's undocumented assertion that *ad hoc* calculations can account for those non-warming periods, and that this somehow validates the models, makes no sense; until the models incorporate all those factors *inherently* and track temperature changes correctly, they cannot be relied on over any time period, short or long.

Neither Foss or Bancel denies that the pamphlet tries to have it both

ways as to short-term and long-term effects, and they succumb to the same temptation.

Neither commentator denies that the pamphlet fails to mention the Medieval Warm Period and the Little Ice Age. The Little Ice Age ended in the mid-19<sup>th</sup> century. That alone presaged rising temperatures as a rebound from that cold period, during the very same time as levels of carbon dioxide were rising. What makes that natural rebound less good an explanation than the greenhouse one?

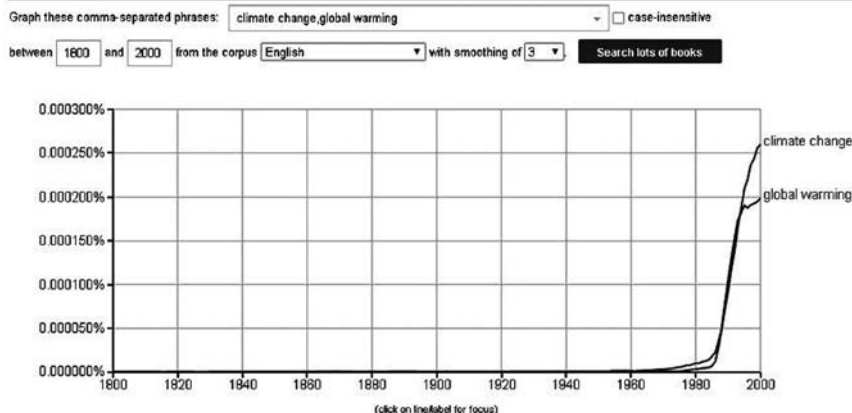
Neither commentator engages my speculation as to the motive for a more dogmatic but otherwise very similar pamphlet only 2 years after publication of the earlier one.

I don't know why Foss refers to me as "esteemed," since he seems not to share that sentiment. Nor do I understand why he would challenge me about the current practice of leading journals requiring authors to state what their specific contributions to an article are. Does he want to dispute that this has been increasingly the practice since concerns about dishonesty became prominent about three decades ago? One need merely look at a recent on-line issue of *Nature* to observe the section "Contributions" in which each author's participation is described. What I may or may not have done is entirely irrelevant. But since I'm asked, I will respond: I have not co-authored articles in any journal that has this requirement, perhaps because I have not been engaged in scientific research since the 1970s and leading journals did not make this a routine requirement until some time after that; dishonesty was much less common in the good old days when I was doing electrochemistry.

Bancel, too, resorts to *ad hominem* statements, including speculation about my motives and my possible reaction to a claimed rejection of my views about AIDS. He makes denigrating references to claimed "ideological advocacy"; questions Fred Singer's credentials, which stand up more than well against Bancel's own. He suggests that Singer and I are more motivated by lack of trust in some institutions than by the science; I might equally say that Bancel is more taken with trust in models than with the actual empirical temperature data that is the only way to test the validity of models. David Dilley is denigrated for citing the Bible, as though religious believers could not also be first-rate scientists (read John Polkinghorne or Francis Collins, for example).

Where Bancel challenges me on a point of fact, he is simply wrong. "Climate change" has indeed become more commonly used than "global

### Google books Ngram Viewer



**Figure 1. Use of the terms *climate change* and *global warming* in books from 1800 to 2000.**

warming” since the mid-to-late 1990s; before making my statement I had checked my impression against Google’s database (Figure 1).

Neither commentator has contradicted my main points about one-sidedness, misleading mixture of asserted certainty and admission of fallibility, and trying to have it both ways as to short-term or long-term influences, together with omission of pertinent data (Little Ice Age and earlier times). The lapses into *ad hominem* are characteristic for polemicists who cannot win an argument on the basis of substance.

## LETTER TO THE EDITOR

### **Is Consensus a Good Thing in Science?**

Henry Bauer's well-written Essay Review (in this issue) on "climate change" brings to mind another attempt to close off debate and pronounce that "Yup, now we know for sure": The Condon Report on UFOs of 1968 (Condon et al. 1969). This was an attempt, on the part of the U.S. Air Force, to discourage interest in UFOs, and to make the issue appear finally resolved. Carried out by the University of Colorado, the project, under the direction of Edward Condon, conducted a number of investigations of UFOs. The Air Force used the resulting report as evidence that UFO research was a waste of time. For five years after its publication, they mostly got away with it. A post-publication review by the National Academy of Sciences had endorsed the Report. The Air Force was able to close Project Bluebook, reporting was discouraged, and what reports were made within the Air Force stayed in highly classified channels. It was a well-orchestrated piece of flim-flammy, and it certainly appeared to be good science, but it wasn't good science (Sturrock 1999, Hall 2001). In spite of the lengthy text of the book (about 1,000 pages), the Report managed to obscure rather than inform.

How this was done is interesting. First, the manager of the project, Edward U. Condon, from the first day, could barely contain his opinion that it was all hooey. He regularly diverted attention from the most serious and interesting cases toward those that were sensational and ridiculous. Second, even though after investigation 30% of the cases remained unexplained, this finding was not emphasized, but rather the casual reader was led to believe that with a little more information these cases too would yield to commonplace explanations. This is unlikely. Third, the report insisted that science had not been advanced by studying UFOs, and that further study was not worth pursuing. Events have demonstrated the reverse (Guerin 2000). Fourth, the report argued that UFOs did not present a threat to national security, and this was and is unequivocally false.

Astronomers and other experts picked apart the report, and were able to demonstrate many of its problems. The 30% figure I used above was developed by a committee of the American Institute of Aeronautics and Astronautics (Sturrock 1999). This committee was highly critical of the Condon Report, but the national media had already bought Condon's conclusions.



The real problem was that the sponsor of the project, the Air Force, had picked a university that was a tame lion, and that could be expected to deliver a negative report, essentially explaining it all away through social science (Saunders & Harkins 1968). But the larger problem was that the Air Force withheld absolutely key data from the project. These data had been gathered through a series of close encounters between the Air Force and the UFO phenomena.

Among the earliest events was the UFO crash at Roswell Air Force Base in 1947. Initially announced as the crash of a “flying disc” by the base commander, the crash was the next day explained away as the fall of a weather balloon. The base commander knew that this was not true. The wreckage was certainly not a weather balloon, and that explanation did not even cover the metal remains found on a field near the Brazel ranch. The debris, samples of which ended up in the car trunk of Jesse Marcel, the base intelligence officer, had some remarkable properties. These included, for some pieces, a stunning ability to return to their shape after being bent. A “memory metal.” Interestingly enough, although Marcel showed the samples to his family, they were returned to the Air Force and never seen afterward. But they were not part of a weather balloon. Moreover, the Brazel ranch debris field was only one of the three crash locations (Carey & Schmitt 2009, Bourdais 2009). The others apparently held the fuselage and the occupants. But the other locations did not become known for decades, except to the Air Force clean-up committee. And then there was a truly strange phenomenon. Many people involved in this event, who knew anything about it in Roswell, were threatened with death, not only for themselves but for their entire family. Meanwhile, the physical evidence was removed and taken somewhere else. So much for the “weather balloon” explanation. On his deathbed, General Blanchard, one of the high officials at Roswell, wrote out a testimony that exposed the hoax the Air Force had played (Bourdais 2009).

However, we know the threats to witnesses were taken very seriously. In the reports of SAC base incursions (see below), virtually all the witnesses waited to report until after they left the Air Force. By contrast, at Roswell, a large percentage of the eyewitness evidence was reported only on someone’s *deathbed*. Witnesses had been scared.

The threats intrigued me, since my father, Edgar Westrum, Jr., worked on the Manhattan Project, pursuing the isolation of plutonium metal at Chicago. Although he discussed his war work with me on several occasions, and although my mother too was involved, no one was threatened with death. In fact, a well-illustrated article on the whole effort appeared in *Life Magazine* shortly after the war (Goro 1946). Whatever happened at Roswell,

it would appear, was much more secret than the atomic bomb. Moreover, there is good reason to believe that after Roswell, there were many more crashes (Wood 2005).<sup>1</sup> In each crash, something would fall out of the sky, then soldiers would appear, cordon off an area, and cart away the debris to somewhere else. So *somewhere else*, there may be a lot of UFO evidence.

But the crashes are only one part of the physical evidence. The other major part is the interaction between UFOs and the atomic weapons programs of the United States. Notably, there have been a number of instances where UFOs flew over Strategic Air Command facilities, sowing havoc and even causing malfunction of the stored nuclear weapons. In some cases, the missiles went “off line” and were unable to be launched. In another case, the launch sequence was triggered, and required a manual shut-down to abort a launch. I gather that the Russians have similar problems with UFOs and their own bases (Hastings 2008).

To give some numbers to these interactions, I asked Robert Hastings to give me some idea of the extent of the “UFOs and Nukes” contacts. He indicated that he had interviewed some 157 people in the course of his research on this, and that his files bore good evidence of some 87 incidents that involved nuclear missiles. In 11 of these cases, the missiles had been deactivated. In 6 of the cases, the launch sequence had been initiated. There were bombers involved in 14 of the cases, of which 2 included adverse effects to the weapons.<sup>2</sup> In some of these cases, there was the usual muting of the witnesses: “Nothing happened. You didn’t experience anything. And don’t talk about it.”

Only one of these events has been investigated in the kind of detail that the Condon Report should have involved, namely the Minot, North Dakota, UFO Event of 24 October 1968, by Tom Tulien of the Sign Oral History Project. This was a very complex event that had several distinct parts. The Sign Project has done 30 interviews, including some with the pilots of an airborne B-52 bomber. While a capsule description of the event is included in the Condon Report, the incompetence of the Air Force investigation of this case by Project Bluebook is shocking. The Bluebook investigators did not even have the Top Secret clearances necessary to interview the pilots. For instance, among the events that took place on 24 October was a low-level flyover of a B-52 bomber of an ostensibly landed UFO. This object was so bright the pilots described it as a “miniature sun.” When they got closer, they found it was an articulated lenticular object, with lights, seemingly at least 200 feet long. This was explained as “ball lightning!” (Tulien). This explanation is obviously ridiculous. There was no storm at the time. We also have another lengthy personal account of a similar “close encounter” at Malmstrom (SAC) Air Force Base in 1967 (Salas & Klotz 2004). And

further witnesses to UFO/SAC base sightings are still constantly coming forward, especially after their retirement, according to author Robert Hastings. Obviously one cannot get more involved with “national security” than events involving the bases of the Strategic Air Command! The Strategic Air Command was very interested in the Minot 1968 case, and repeatedly pressed them for information (Tulien).

Yet the Air Force’s ostensible “conclusion” that there was nothing valuable in the UFO phenomenon also, like the climate change argument, is based on more than simply scientific research. There were and are a lot of “backstage” activities going on. “Scientific” conclusions by committees reflect such maneuvering. Anyone who has been on many government committees tends to recognize some basic facts of life:

- 1) Those put on the committee usually shape the conclusion. Scientists’ points of view and assessments of particular subjects are often known. This was a factor both in the choice of the University of Colorado to do the study and the choice of individuals who went into the later review panel of the National Academy of Sciences (NAS). The NAS panel was apparently handpicked by Condon’s former co-author of the *Handbook of Physics*, Hugh Odishaw. At least this is what Odishaw told his colleague Richard Greenwell at Colorado.
- 2) The amount of time spent on the committee is seldom sufficient to change an already strong opinion. This can allow writing the conclusion before the panel has even met. There is strong evidence that this was the case with Condon. And the committee often does little “research” besides reading the literature. The Condon project was an exception. It actually had time to do fundamental research. Yet even the full conclusions of this research were obscured by the design of the final report.
- 3) In the committee’s report, the relationship between evidence and deduction from it is often other than logical. Condon expected that most of those interested would read the summary rather than the body of the report, and this appears to have been the case. Furthermore, I believe they didn’t even read the summary very closely. Condon’s conclusions do not follow from his data (Sturrock 1999). But few noticed. This may also explain the curious conclusions about climate change and *their* relationship to the data.
- 4) Often, only those who are true experts can see the problems or contradictions of a committee’s report. Given a lack of such

expertise, the temptation for one committee to rubber-stamp what another committee has reported is often strong. And dissenters are often treated to some version of the “bum’s rush” and shown the door.

- 5) Sometimes consulting the “other side” is the best way of finding the problems in the committee’s conclusions. But consulting the other side is a rarity. And if they didn’t get invited to the party, they will seldom be asked to dance.

Truth tends to emerge through conflict in science, rather than through consensus. Yet power is likely to try to engineer consensus. And often its tactics, as Henry Bauer has previously pointed out, are not pretty (Bauer 2012).

### Notes

- <sup>1</sup> Leonard Stringfield made investigation of apparent UFO crashes his specialty. His series of “Status Reports” provides the published part of his researches. His diaries are stored in Cincinnati under control of MUFON. For example, UFO Crash/Retrievals, Status Report VII: Search for Proof in a Hall of Mirrors, privately published, 1994.
- <sup>2</sup> Robert Hastings, personal communication, 9 November 2015.

**RON WESTRUM**

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## BOOK REVIEW

**In Their Right Minds: The Lives and Shared Practices of Poetic Geniuses** by Carol Brooks Platt. Exeter, UK: Imprint Academic, 2015. 220 pp. \$26. ISBN 978-1845407896.

The author introduces her remarkable book with this remark: “My interest in the voices and visions of poets and prophets was precipitated by a dear friend’s claim to channel angels after her mother died.” How was it possible for her friend to undergo such a substantial change in her sense of reality? How could she accept angelic encounters as *really* real?

Witnessing this transformation galvanized Carole Brooks Platt to do twenty years of research, the present book resulting. The book indeed contains a wealth of densely packed ideas, data, and references, drawn from diverse sources and disciplines. Although it would be impossible to cover so much in a brief review, I will say something about the main points.

The question Platt poses is a large one. What kind of world do we inhabit? What is it like to be a human being? Do poets, prophets, mediums, and others break out of their material shells and reach into other dimensions of reality, or is all that illusion, self-deception, escapism? This is a great metaphysical as well as an intensely personal question of our time. Although typically not discussed in the major news media, the issues it entails lurk in the background of all that we think and do.

*In Their Right Minds* has three layers: first, reportage of a fascinating array of alleged facts, experimental and historical; second, an attempt to come up with an explanatory scheme to make intelligible the curious data; and third, the many authorities cited. About this last rather thickly distributed layer, there was so much that I was bewildered, and the explanatory links to the main ideas were sometimes tenuous. I found myself wanting to interact with the author’s ideas more directly instead of through the maze of her sources.

Much of the first layer revolves around the thesis that poetic creativity is in some unstated sense a function of brain laterality, specifically right brain filtering, which many studies purport to establish. Another index of poetic creativity is “bilateral” or equipotent hemispheric functionality, which harks back to Julian Jayne’s ideas (1976) on the “bicameral” mind: the presumed notion of our total mind before it is fractured by the excesses of the analytic left brain.

Central to Platt's research is the observation that various shocks and traumas to the nervous system sometimes open the floodgates of genius. Vico, on the first page of his autobiography (1744), describes how as a boy he had fallen off a ladder and cracked his skull and almost died, to which he ascribed the mobility of his imaginative life and therefore his genius. Genius, according to the implied view, transcends the everyday linear mind and is akin to the daimonic as portrayed in Plato's *Phaedrus*, while more recently Cesare Lombroso's *The Man of Genius* (1891) was rife with accounts of the lunacies and physical traumas of unusually accomplished people. Platt's book covers a wide range of cases in which individuals, for various reasons—congenital, accidental, or voluntary—reveal signs of right-brain creativity.

Apparently, there are many ways of mobilizing these right brain potentials, for example psychoactive chemicals. Another example is the near-death experience, especially if it involves cardiac arrest, which shuts down both hemispheres by instantly curtailing oxygen flow to the brain. During cardiac arrest, subjects sometimes report extraordinary experiences with powerfully creative consequences.

The main idea under study is paradoxical. Attacks on the normal adaptive functions of the brain may free up consciousness to perform in original, unexpected, and seemingly impossible ways—as with the author's friend's alleged conversations with angels. Certain kinds of experience impact one's very sense of reality, and ideas that seemed impossible now seem self-evident. Perhaps we overrate our routine sense of the real and confuse the habitual range of what we experience with the possible range.

The celebrants at Eleusis in ancient Greece emerged from the telesterion with a transformed sense of reality. St. Paul on the road to Damascus, after being knocked off his horse and out of himself by *something*, instantly acquired a new sense of reality, as it turns out to world-historical effect. Inevitably, there are some who cannot abide the notion of alternate realities having any claim to truth, at which point metaphysics and politics clash.

Platt is very careful to trace these expansions of consciousness, which assume various forms, back to the right hemisphere of the brain. We have to ask: How do the chemical and physical events in that right part of the brain relate to the mental experiences associated with the poetry, say, of Yeats or Sylvia Plath? It can't be that the "genius" (the novel words, flashing images, soaring ideas) is hiding somewhere inside those right brain lobes, waiting to be liberated.

The location in one lobe or the other of the brain is contingent, and the relation to creativity is correlative not explanatory. What difference would it make if the creative functions of the brain were located on the left side?

More crucial to creativity is this: the normal adaptive use of the brain as a tool of survival in the material world normally gets in the way of potential genius. Most of us are slaves of the survival-driven left brain.

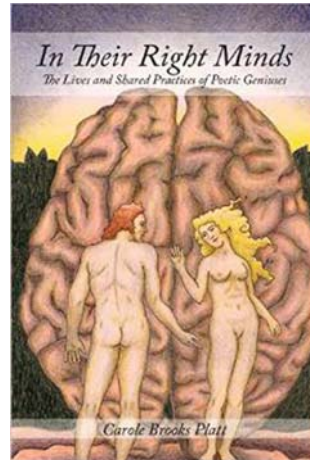
The foregoing are some background concepts shaping this book. Much of it zeroes in on the lives and practices of major poets—Blake, Keats, Hugo, Rilke, Yeats, Merrill, Plath, and Hughes, to mention a few. In her treatment, Platt adds two dimensions to discussing poetic creativity: the roles of group dynamics and of altered states of consciousness.

The poets are discussed in relation to their significant others, their muses, teachers, lovers, and nemeses. So Blake had his wife Catherine; Keats through letters his brother George and sister-in-law Georgina; Hugo his son Charles and mother in their joint mediumistic practices; Rilke his mother and several female muses; Yeats his young wife, Georgie; Merrill his partner David Jackson; while Plath and Hughes had each other.

Each poet is a complicated story. There is, for example, the politics of creativity, in which, as Platt sees it, male genius is sometimes prone to bully and exploit feminine (or other) receptivities. No doubt true in part, I thought she went overboard with Yeats. In Platt's account, Georgie Yeats shines as the feminine ideal incarnate while the poet himself is cast as a kind of intellectual thief obsessed with the occult.

The overarching message of these creative relationships: In surprising ways they often serve to unshackle the poet's intuitive and imaginative powers. One of the techniques used is to exploit automatism. For example, James Merrill's *The Changing Light at Sandover* (1993) was produced by Merrill and Jackson on a Ouija Board. The teacup they used to spell out the words would not move unless Jackson's hand was on it. In a sense, then, they were co-authors of this poem that Harold Bloom has assigned a place in his canon (1995). Platt's discussion of the group dynamics of Merrill shows clearly how two individuals can merge their imaginations, memories, and knowledge and create a greater mind, perhaps with powers that the individual alone could not properly deploy. Jackson, as Platt points out, was himself a writer and scholar of exotic languages.

Yeats and his wife Georgie collaborated in a different way. Georgie had mediumistic talents and part of her self-fashioning involved profound





rapport with her husband's creative work. Everything seems to have been orchestrated around her producing images that Yeats could use in his poetry. Yeats took full advantage of the material given to him by means of these automatisms. Platt calls attention to this notion of creative melding in the other poets she discusses.

As a matter of course, creative artists exploit all sorts of things that come their way, and are bound to step over boundaries. The unique thing about Yeats and Georgie was their rare deliberate partnership in the creative process. The lone genius in creative agony is a well-founded trope; the idea of genius as a product of intimate partnership has yet to fully establish itself.

The second big point is about intuition, the leap beyond left-brain rationality: the release of vision, inspiration, telepathy, clairvoyance, and the like. Again, as in Plato's *Phaedrus*, the poet, like the prophet, goes *ek-static*, out of his everyday-adapted mind, thus opening to the influx of *daimonic* consciousness. The daimonic, we could say, is the ferry between the rational and the subliminal self.

Platt found that the trauma of an aborted mother relationship was conducive to the "atypical" creative mentality. According to this book's thesis, "early trauma predisposes the poet to hear a dissociative Other who can say the unsayable from the vantage of enhanced right-hemispheric processing" (p.111). Clearly, this shattering of the mother archetype activates the imagination, so we find among great poets the quest for a "surrogate" mother that drives the poetic sensibility toward its fullest possible expression.

This, to my mind, suggests a poetics for our time, and Platt has touched a nerve here. Modern materialistic, technological civilization—fuelled by global capitalism and murderous militarism—represents the triumph and deification of the male principle. What's needed, one might say, is a poetics of metaphysical trauma; in short, the poetic remaking of the future will be about the return of the goddess. Along with Carole Platt, we have Henry Adams, Robert Graves, Carl Jung, and others to support the rationale of such a venture.

So, in researching the outer limits of creativity, we are wise not to go where people are happily adjusted and functioning normally. More likely the new miracles of creativity will emerge from global scenes of war, poverty, occupation, mass dislocation and migration, and climate catastrophe. With mother Earth traumatized by technological humanity, new forms of creativity may be looming on the horizon. As a prophylactic against apocalypse, we must be right in our brains—but not I hope in our politics.

**MICHAEL GROSSO**  
grosso.michael@gmail.com

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## BOOK REVIEW

**Observer Effect: The Quantum Mystery Demystified** by Massimiliano Sassoli de Bianchi. Italy: Adea Edizioni, 2013. 94 pp. \$9.58 (e-book). ISBN 978-8897144397.

Quantum mechanics has been a source of interesting analogies for the study of psychical phenomena or consciousness itself. Some thinkers have taken it beyond an analogy, aiming to explain consciousness and psi with quantum physics or to explain certain aspects of quantum mechanics with consciousness (especially the so-called “observer effect” related to the “measurement problem”). Is it a given that consciousness and quantum physics are connected? In his book, *Observer Effect: The Quantum Mystery Demystified* (available as a Kindle ebook on Amazon and iBookstore), physicist Massimiliano Sassoli de Bianchi makes an important educational contribution to both fields. Though the book requires some lay-level background in quantum physics, in his characteristic lucid, engaging, and conversational style, the author breaks down prejudices regarding what an observational process is or is not, on the basis of simple, yet profound, analogies and metaphors.

In quantum physics, a measurement can be described as an experimental situation in which a physical entity undergoes a non-deterministic and irreversible change, which some describe as the collapse of the wave function or reduction of the state vector. What does that mean? It suggests that even if we know all the initial conditions of the entity, that is its state before the measurement, we cannot predict with certainty what its final state will be, that is the state at the completion of the measurement process. The best we can do is to attach probabilities to the different possible final states, by means of a rule of correspondence, called the Born rule.

Solving the (quantum) measurement problem is about explaining what goes on, “behind the scenes,” when a system is subjected to a measurement context. What produces such an abrupt change in the entity’s state? Is this process truly non-deterministic and irreversible and, if so, why? Why is the Born rule so successful in determining the probabilities of the different possible outcomes? It is generally believed that convincing answers to the above questions are yet to be found. Many believe that these answers cannot be found. Consciousness has been proposed as a solution to the problem, though evidence for this is still lacking, and consciousness itself is quite a

riddle of its own—with many concluding it does not exist as such or that it is also an insurmountable enigma. However, due to the impression that some have of quantum mechanics as a complete theory, some are quite convinced of von Neumann's psychophysical interpretation. The book summarizes and points to other scientific interpretations that point to logical and mathematical problems that put into question the completeness of quantum mechanics and the inevitability of the von Neumann interpretation that "the observer or consciousness collapses the wave function."

Perhaps the behavior of quantum physics is truly strange and counter-intuitive and necessarily a product of consciousness. However, one of the problematic conclusions that one might derive from quantum mechanics is that no phenomenon exists before observation. In other words, reality does not exist in the absence of observation. Albert Einstein was, famously, no fan of this measurement problem or observer effect, quipping that the moon continued to exist, undisturbed, even when nobody was watching it! So does our observation create our own reality? Is Schrödinger's cat dead, alive, both, or neither? How can such a sophisticated theory be unable to address such basic questions regarding the moon or a cat? Common sense would say that the moon is there whether you look at it or not and that a cat is either dead or alive, though you may or may not observe it. A theory that cannot explain this is incomplete, is it not?

This book takes Einstein's famous metaphor seriously (and somewhat literally) and shows that we can gain considerable insight into quantum mechanics by performing cleverly designed experiments with everyday classical objects, such as rubber bands, hydraulic presses, and apples, which are described by the author in such a way as to demonstrate that the origin of quantum probabilities can be explained without recourse to psychophysical effects, or to effects that would only be present in the sub-atomic layer of our reality. In other words, the book will show lay and specialist readers alike that the strange properties associated with the observer effect are, surprisingly, not specific to nano-scale systems, as in general the quantum behavior of a macroscopic system can be understood not only as being a consequence of its internal coherence, but also of the way one can decide to actively experiment with it, by means of specific protocols. In other terms, one can show that macroscopic systems can exhibit a quantum (or quantum-like) behavior as a consequence of the fact that one is not conceiving observations (measurements) only as processes of pure discovery, but also as processes of creation, that is processes through which one can create, in an unpredictable manner, the very quantities one is measuring.

The approach employed is known as the hidden-measurement interpretation of quantum mechanics, which was developed in the 1980s

by Belgian physicist Diederik Aerts, and received more recently a comprehensive formulation thanks to Aerts' collaboration with the book's author (see for instance their open access foundational article "The extended Bloch representation of quantum mechanics and the hidden-measurement solution to the measurement problem" [Aerts & deBianchi 2014]).

Contrary to what has been done in the past, in the face of perplexity instead of deriving a formal mathematical structure ("lower your arms, shut up and calculate!") and then trying (unsuccessfully) to understand physical interpretations, the approach taken by Sassoli de Bianchi (which is that of the Geneva–Brussels school on the foundations of physics, of which Aerts is one of the founders) is to try first to identify what the relevant physical concepts are, defining and clarifying them on an operational basis, and then to use them to build a mathematical theory, hopefully with more meaningful and intelligible conceptual and mathematical language.

The book describes Aerts' creation—discovery view, which is able to describe the different entities that comprise our reality—both macroscopic and nano-scale. As we said, the quirky quantum conundrum, under this approach, is elucidated by showing that macroscopic entities can also incorporate the same sort of strangeness as the microscopic ones. Consequently, seemingly simple and conventional objects can be used to offer satisfactory answers to the aforementioned fundamental questions posed by the measurement problem.

The book also reveals an alternative—in a sense deeper—mystery of quantum mechanics: the non-spatial nature of microscopic entities, rather than the role of the observer-consciousness. In *Observer Effect*, Sassoli de Bianchi makes the case that quantum physics seems counter-intuitive because we have tried to understand it within our three-dimensional Euclidean space. He argues effectively for a different interpretation: that reality is not limited to our perceived space–time. The phenomena we observe during quantum measurements appear strange because we may be interacting with a reality that does not entirely fit in this ordinary spatiotemporal theater. A larger view of the physical reality opens up, where measurement apparatuses interact with non-spatial entities through hidden interactions, to create the properties we observe.

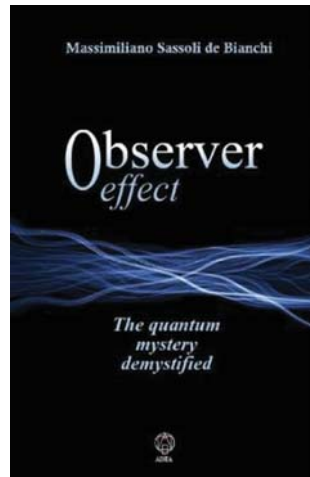
Typically, you would expect someone dismissive of psi phenomena to be the kind of author who would argue that the observer effect may have nothing to do with an observer. You might also expect that this decoupling of both subjects would be used by the author as a way to dismiss psi phenomena or non-reductionist consciousness studies. This is what makes the book *Observer Effect: The Quantum Mystery Demystified* particularly interesting. The author is both a physicist and a consciousness

scholar, practitioner of psi or consciousness practices, open to both a “multidimensional” or “multi-material” physical world and a “multidimensional” world of consciousness, without necessarily conflating the two.

The take-away from the book, for me, is that just as reductionist materialism might not explain consciousness, consciousness may also be excluded as a *sine qua non* concept for modeling quantum phenomena. A solution is revealed that dismisses the need for consciousness–physics interaction without dismissing the possibility that consciousness can affect biophysical systems in other circumstances (e.g., psychokinetic effects, DMILs, engineering anomalies, REG-related phenomena, firing of neurons or mutation of DNA, etc.). If even the foundations of physics, where physical entities increasingly look more like concepts than objects (see Aerts’ conceptuality interpretation of quantum mechanics), are not likely to be limited to Euclidean space (which obviously includes our body and its brain), why insist that consciousness cannot possibly exist beyond our limited, perceived material reality? This does not mean that any other consciousness realities, if they exist, correspond to the multiple dimensions raised by this interpretation of quantum physics. Their relationship or lack thereof would remain a new and even more complex mystery.

This provocative work exposes that we may *not* find in quantum mechanics the long-sought-after bridge between the worlds of consciousness and material reality. We might, instead, find a much more fascinating and expansive physical world. If consciousness or observation is not necessarily behind the measurement problem, quantum physics may not have sufficient explanatory power to explain the brain–consciousness link or phenomena such as psychokinesis, beyond the power of metaphor. Some double-slit interference experiments have aimed to test the possible role of the experimenter’s mind in the collapse of the quantum wave function. However, Sassoli de Bianchi emphasizes that quantum mechanics neither rules out psi nor does it require a psychophysical explanation of physics. The book renews the need for the search for alternative, more convincing, and comprehensive models for consciousness.

To complement this reading, I recommend reading articles published by the author, which are mentioned in the bibliography of the book. Additionally, to further appreciate possible ramifications of the work, I recommend his



article “Quantum dice” (de Bianchi 2013), where the author argues that measurements on a single die can be performed so as to create typical quantum interference effects, and he also shows how to connect (entangle) two identical dice, to maximally violate Bell’s inequality. Bell’s inequality was designed to test whether or not the real world satisfies local realism. If confirmed, Bell’s inequality would show that quantum mechanics must violate either locality or another principle, realism, relating to the value of unmeasured quantities. The two principles are often referred to together as a single principle of local realism. Experimental tests of the Bell inequality, beginning in 1972, seem to show that quantum mechanics disobeys the inequality, and thus must violate either locality or realism, although critics have pointed out various possible “loopholes” in the experiments (consider however that a loophole-free Bell experiment has been reported this year, see Hensen et al. 2015).

Sassoli de Bianchi, echoing previous research by Aerts, is then able to show that the basic mechanism underlying the violation of Bell’s inequality is the *creation* (and not the discovery) of correlations, and that this mechanism can equally operate with microscopic and macroscopic entities. The fundamental difference is that the creation of correlations would be the result of ‘non-spatial connections’ when the entities are microscopic, whereas the connections are necessarily present in three-dimensional space when they are macroscopic.

By consulting the newest publications of the author, which he wrote in collaboration with Aerts, one can observe progress in the investigation of the nature of entanglement in physical systems by means of the hidden-measurement approach. Therefore, I can only hope that he will soon offer us an additional work on this crucial phenomenon, and at the same time pointing to the mystery it also hides, which again, according to the author, would be captured by the notion of “non-spatiality.”

Let me conclude by quoting a few suggestive words by Diederik Aerts:

Reality is not contained within space. Space is a momentaneous crystallization of a theatre for reality where the motions and interactions of the macroscopic material and energetic entities take place. But other entities—like quantum entities for example—“take place” outside space, or—and this would be another way of saying the same thing—within a space that is not the three dimensional Euclidean space. (Aerts 1999:129–183)

Massimiliano Sassoli de Bianchi received a Ph.D. degree in physics from the Federal Institute of Technology in Lausanne (EPFL) in 1995, with a study on time-observables in quantum scattering theory. His research activities are focused on the foundations of physics, quantum theory, and



consciousness. He has written essays, popular science books, children's stories, and has published numerous research articles in international journals, both in physics and in the study of consciousness. He has been a repeat finalist for the IAC Global Award for Scientific Contribution to Consciousness Science. He is a life member of the American Physical Society and the American Association of Physics Teachers, as well as a full member of the Society for Scientific Exploration and the International Academy of Consciousness. He is currently the director of LAB (Laboratory of Basic Self-research), and the editor of the Italian-language journal *AutoRicerca* ([www.massimilianosassolidebianchi.ch](http://www.massimilianosassolidebianchi.ch)).

**NELSON ABREU**

nelson.abreu@iacworld.org

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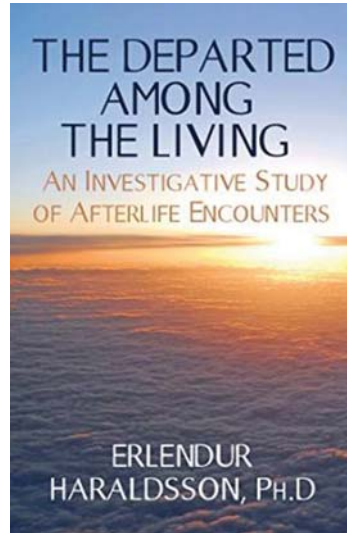
## BOOK REVIEW

**The Departed Among the Living: An Investigative Study of After-life Encounters** by Erlendur Haraldsson. White Crow Books, 2012. 274 pp. \$17.95. ISBN 978-1-908733-29-0.

Reports of encounters with deceased people are as old as humanity and seem to be in the roots of many if not the majority of spiritual traditions. However, scientific studies on this exciting but controversial topic started in Nineteenth Century. The first large survey of apparitions was published by researchers from the British Society for Psychical Research (SPR) in 1886 (Gurney, Myers, & Podmore 1886). Recent authors studying Spiritism, Spiritualism, and psychical research developed the first attempts at systematic and scientific investigations of these experiences (Alvarado 2012, Sharp 2006). The subject has become more relevant given that in the last decades there has been an increasing interest in psychiatry and psychology literature in the study of “psychotic experiences” in the general population, which often involve anomalous sensory experiences. Several large epidemiological data studies have shown that most “psychotic experiences” happen in the nonclinical population and are usually not related to psychotic disorders. In a study performed by the World Health Organization in 52 countries involving more than 250,000 participants, psychotic experiences (when participants were not half asleep, dreaming, or under the influence of alcohol or drugs) occurring the previous year were reported by 12.52% of world’s population, ranging from 1% (Vietnam) to 46% (Nepal). It is remarkable that only 10% of those reporting psychotic symptoms had a diagnosis of schizophrenia (Nuevo et al. 2012). There is a need to investigate and better understand these “psychotic” experiences in non-clinical populations. Since spiritual experiences often involve psychotic-like aspects, they are privileged venues in which to understand psychotic phenomena in the general population (Moreira-Almeida & Cardena 2011). One important sort of hallucinatory experience in the general population, both in terms of impact on the percipient and their potential theoretical implications, is the report of perceiving a deceased person.

This book presents a comprehensive phenomenological analysis of reports of perceptions of deceased people by individuals from the general population in Iceland, where, in 1974–1975, 31% of a national representative sample (n = 902) reported that they “have ever been aware

of the presence of a deceased person.” Data for the book was obtained from 449 Icelanders who reported, in a waking state, direct personal experience of contact with a deceased person (excluding contacts in dreams or through mediums). This sample was obtained from two sources. The first was composed of 128 people selected from the national sample above. Initially, they were recruited from the two largest population centers in Iceland and, later, from the rest of the country. The remaining 321 were obtained through advertisements published in 1980 in five popular magazines that reached a diverse readership.



These two subsamples reported similarities in the distribution of sensory modality involved in the experiences. Comparisons in sociodemographic factors are not provided, so it is hard to evaluate the degree of similarity between the subsamples as well as between the total sample and the general population. It would be useful if the author provided more details about the sampling process and a table with comparisons of sociodemographic characteristics between the two subsamples and the national representative sample of people reporting encounters with the departed.

The best aspect of this book is the detailed descriptions about these experiences, especially regarding phenomenological aspects. Most reports involved only one sensory modality. From the total sample, 67% of encounters involved visual experiences and 28% aural experiences. In addition to quantitative data, the book provides richness in the hundreds of first-hand descriptions of the wide range of experiences of encountering a deceased person.

Chapters are divided to cover different aspects of the experience. The first chapters are devoted to the different sensorial modalities involved in the reports (seeing, hearing, touch, smell, and sensing a presence). The following chapters cover several different aspects, such as: experiences at the moment of death, how long after death, how long they last, how physically real they were, who the deceased persons were, several sorts of violent deaths (accidents, suicide, drowning, murder, etc.), specific types of perceivers (child, widow), unusual behavior of animals, and encounters involving some sort of message or warning. Each chapter usually starts

with one or two brief paragraphs providing a general overview and some quantitative data regarding that specific aspect of the experience and then provides many (often 10 to 20) first-hand reports illustrating the experience.

Most chapters and reports described do not concentrate on whether these experiences are fabrications from perceivers' minds (ordinary hallucinations) or some form of veridical anomalous perception. However, some of the last chapters address this issue more directly: a perceiver obtaining information previously unknown by him/her, and more than one person having the same experience (shared perception). Finally, the last four chapters discuss aspects of an afterlife, from belief in it to empirical evidence of survival of bodily death. While 71% of Icelanders believe in an afterlife, 91% of the sample investigated in the book reported this belief. This difference might be related to the fact that 38% of the sample reported that their experience increased their belief in an afterlife. The author lists some features of afterlife encounters that suggest these reports are not just hallucinations:

- 28% of the appearing deceased died a violent death, the same percentage reported in the pioneer SPR study. This consistency of findings from different countries and different centuries, including that many percipients did not even know that the person had died, would suggest it is a "universal human experience" which may be explained as "due to a high motivation to communicate by the prematurely deceased person" (p. 231).
- Concentration of cases in the first 24 hours after the death of the perceived person, also in line with the SPR study, would also suggest a motivation of the deceased to communicate.
- Collective experiences would make hallucinatory hypothesis less plausible.
- Obtaining veridical information previously unknown by the percipient.
- The intention or purpose of the deceased in several reports, such as those that convey warnings that sometimes rescue the percipient from mortal danger.

In the conclusion, the author states that the data obtained are not compelling, but suggestive of a real human experience that cannot be dismissed as mere illusion. Also along this line, the major characteristics found were also obtained from studies in different countries (China, UK, USA, Germany, Italy, France) and in different centuries. He also briefly discusses the implications of studies of deathbed visions, near-death

experiences, memories of past life, mystical experiences, and terminal lucidity and concludes that “this substantial body of research is increasingly suggestive—if not convincing—evidence for life beyond the physical body” (p. 241).

The book has some limitations. First, it is hard to evaluate the representativeness of the sample and, therefore the generalizability of the findings. However, the results’ similarities with other studies suggest, at least partially, their representativeness. It would be useful to have more information about sampling procedures and a sample description. Second, the author could provide a deeper discussion of the findings and analyze them in the light of the comprehensive literature on the topic. On the other hand, the author seems to have wished to keep his analysis and interpretation at a minimum, focusing on providing direct empirical data for the reader’s own interpretation and conclusions. The major strength of the book is exactly the numerous and rich descriptions of these experiences, providing the reader with direct contact with these reports which allows us a good sense of these fascinating and challenging human experiences. The book may also be a primary source for future in-depth analyses to be performed by researchers who further organize, categorize, and explore this rich material.

**ALEXANDER MOREIRA-ALMEIDA**

Research Center in Spirituality and Health (NUPES)  
School of Medicine, Federal University of Juiz de Fora (UFJF), Juiz de Fora, MG, Brazil  
alex.ma@uff.edu.br

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## BOOK REVIEW

**Indridi Indridason: The Icelandic Physical Medium** by Erlendur Haraldsson and Loftur R. Gissurarson. UK: White Crow Books, 2015. xvii + 264 pp. + Index. \$17.99. ISBN 978-1-910121-50-4.

*Indridi Indridason: The Icelandic Physical Medium* is the second important book to be issued recently about spectacular, relatively unknown physical mediums, the evidence for whom would be inaccessible to English-speaking people if it were not for the translations of the original material by their authors.

The first, Zofia Weaver's excellent *Other Realities? The Enigma of Franek Kluski's Mediumship* (Weaver 2015) was reviewed in the Fall 2015 issue of this journal, and now we have Erlendur Haraldsson and Loftur Gissurarson's definitive book on the short-lived but extraordinary Icelandic medium, Indridi Indridason.

Haraldsson has reported details of his investigation into Indridason's mediumship for years. Those articles have been compiled together with translations of "new" material that the authors have unearthed for this volume.

Indridi Indridason, born in the same year as D. D. Home, 1883, was a farmer's son from a rural area of Iceland. When he was about 21, he went to live in the capital, Reykjavik, to become a printer's apprentice. He lived in the home of a relative whose wife was interested in experimenting with table turning and séances. In early 1905 she asked the young man to participate, and as soon as he sat at the table it trembled, shook, and started to move violently about the room even overturning once. Indridi himself was rather shaken but evidently intrigued enough to continue with the experiments.

The authors compare him to D. D. Home, but whereas we know a great deal about Home's childhood and that his manifestations began literally in the cradle (which would rock itself when he was an infant), the only thing known about Indridi before he was 21 was that he had some "remarkable visions."

The gifted young man agreed to participate in experiments and soon was causing objects to move or levitate, and sometimes produced raps or knocking sounds in response to sitters' requests. In time there were also gusts of cold or warm air, odors or fragrances, the playing of musical instruments as if by invisible hands, various light phenomena from large,

luminous clouds to “fire flashes” or “fire balls,” materializations of human figures or body parts such as hands or fingers, and once an animal that seemed to be a cross between a horse and a calf.

Indridi produced some of the classic phenomena such as sitters feeling touched and pulled, but some of the phenomena became violent at times and he was dragged bodily on the floor or pulled up into the air, once nearly being dragged out a window and another time in danger of being injured by forces almost too strong for his helpers to counteract.

It was said that he had a temper, although I don’t recall anything about his acting angry, but many of his phenomena resembled outbreaks of RSPK or “poltergeisty” in their violence, e.g., chairs being pulled out from beneath people and furniture being tossed about, piled up, or broken.

According to his controllers, his arm dematerialized, and although it was pitch dark they carefully felt his shoulder and upper body and could detect no trace of it.

One of the most remarkable phenomena was his ability to produce direct voice, that is, voices that seem not to come from the medium’s vocal cords but originate in various parts of the room. What is unique I believe to Indridi is that the voices would not only talk and perhaps chastise sitters for breaking rules, but at times the beautiful voice of a woman, a trained voice, would be heard singing a duet with a deep male voice. The two voices were heard simultaneously! (The woman was thought by the sitters to be Maria Malibran, whom they described as a “French lady.” Malibran was born in Paris of Spanish parents and was the most famous singer of the 19<sup>th</sup> century. She died in England in 1836. Indridi might not have known of her, but certainly his investigators, who were educated men, would have.) The voices Indridi produced were many and varied and were heard not only in the experimental room but in Indridi’s vicinity outdoors or in other venues as well. In one case a sitter said he recognized the distinctive voice of a deceased friend whom Indridi had never met.

Indridi also produced direct writing in which a pencil would write on paper left somewhere in the room out of reach of the medium. D. D. Home was partially successful in trying to do this, but that feat was a trademark of Gilbert Roller and the Bindelof group, which produced many such messages (Pilkington 2006). One letter written in large, rounded script was supposedly from Malibran, although this could not be verified, but another purported to be from the composer Edvard Grieg contained a signature similar to his.

The most important of Indridi’s investigators was Dr. Gudmundur Hannesson, a district medical officer and later Professor of Medicine at the University of Iceland and founder of the Icelandic Scientific Society. He was a skeptic, not a spiritualist as were the other observers, was knowledgeable



about fraudulent mediums' tricks, and conducted more tightly controlled investigations of Indridi. In a letter to another investigator he wrote that during a whole winter of séances that he attended, "there was not hardly one at which I did not try to detect fraud in one way or another" (p. 160). But he was never able to ascertain any fraud and was convinced that the bulk of the phenomena were genuine, "whatever their cause may have been."

Hannesson astutely notices that the voices produced commonly are hardly intelligible when the "speakers" appear for the first time, "but gradually become plainer as time goes on" (p. 165). He continues:

These "dead" people are questioned about anything between heaven and earth, but little benefit is derived from their answers, and it is not unusual that they commit themselves to actual mis-statements about things known to persons who are present. They seldom have a clear recollection of their life here. Their answers vary greatly, but most of them are unlike what one would expect from the spirits of eminent personages. (pp. 165–166)

The authors were able to obtain from Hannesson's descendents unpublished notes describing séances held with Indridi in Hannesson's house, where any suspicion of accomplices, hidden devices, etc., could be ruled out and even stricter controls could be exercised. Indridi had never visited the house. He was asked to undress and was provided with special clothes, wound with string fastened to his jacket, which was also sewn closed, and his "watchman" or control held the string that was left over, ensuring that the medium could not reach anything farther from him than the watchman's chair. Phosphorescent tape was put on the watchman's shoulder so that the observers could see his movements in the dark. Despite the precautions, there were many phenomena including touchings, knocks quite far from Indridi, female and male voices, the medium levitating at least a couple of feet in the air, etc. Examination of the medium's bonds afterward showed nothing suspicious and the sewing was undisturbed.

Other séances reported on by Hannesson are included in which Indridi produced a variety of strong phenomena that could not be explained away by normal means.

The authors have added several appendices charting (A) the sequence of séances in chronological order and phenomena reported, (B) a summary of Hannesson's major methods of investigation, (C) a comparison of Indridi with D. D. Home, and (D) a list of séance participants, deceased communicators, witnesses, etc.

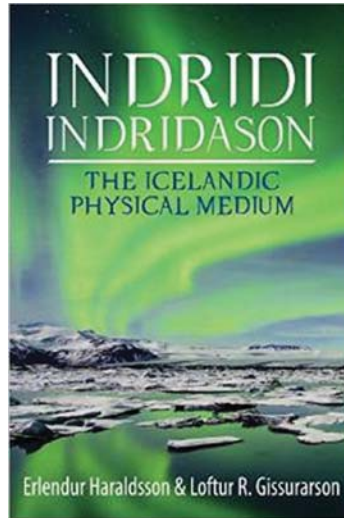
Appendix C tells only part of the story, but is a good start toward a needed in-depth comparison of all the major physical mediums, which I think would be very useful in helping us to understand the similarities and

differences among these rare and unusual people and try to begin to solve the mystery of how these phenomena come about.

What I found lacking in this volume, except for some comments of Hannesson's, is a much-needed psychological analysis of what was going on. Indridi Indridason was a young man with, unfortunately, a very short life: He discovered his gift when he was 22 or 23, died at 28, and wasn't able to hold séances in the last couple of years of his life owing to illness. We are told that he was a pleasant and amiable young man, but he had "a temper." We are not told much about his mental-emotional frame of mind, but from the outward- and self-directed violence he experienced, there must have been anger and/or frustration that had gone unreported. We know very little about his inner life, attitudes, interpersonal relations, etc., which I think is essential to analyzing any mind-body interaction.

The authors acknowledge "the inevitable question" of the origin of these phenomena: "Did they originate solely and exclusively in the medium, or were they beyond him and working through him?" (p. 227). They seem to come down firmly on the latter theory, the survival hypothesis, citing that facts were sometimes revealed that were unknown to the medium, such as reporting on a fire that "should not have interested" him more than any other fire, personifying "communicating entities," who Indridi never knew when they were alive, displaying vocal skills when "No opera singers were living in Iceland at the time" (p. 228), or speaking in languages unknown to him. This last, the authors maintain, "indicates an independence of the communicating entities from the person and capabilities of Indridi and may be interpreted as evidence for their genuineness" (p. 228).

These factors could point, as the authors contend, toward human survival of bodily death, but they could also point to psychic and psychokinetic abilities by the living Indridi. Yes, facts and foreign language may have been "unknown" to him, but not to the others in the room who were eager for evidence of their loved ones' continued existence. Indridi may have been unfamiliar with opera singers, but his sitters and investigators were not, and it is probable that they knew of Maria Malibran, who was world-famous. The same applies to the languages, which, too, may have been unknown to him, but were known to some of the sitters. We do know that, especially



in trance, as Indridi mostly was, the medium is extremely telepathic and clairvoyant and could easily be obtaining the images, sounds, and languages of the deceased from the audience.

As for the fire in Copenhagen, Indridi may or may not have had a personal interest in it, but others in the room may have had: We don't have enough information to know. Many would classify it as classic clairvoyance.

The majority of sitters were believers in an afterlife, as was Indridi. It would have been remarkable if he didn't produce "proof" of spirit existence. In the interest of science, perhaps we should adopt Hannesson's attitude: ". . . the bulk of the phenomena were, as far as I could judge, quite genuine, whatever their cause may have been" (pp. 202–203).

**ROSEMARIE PILKINGTON**

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## BOOK REVIEW

**Knowing the Unknowable: Putting Psi to Work** by Damien Broderick. Vancleave, MS, USA: Surinam Turtle Press, 2015. 274 pp. \$20. ISBN 978-160543861-0.

Damien Broderick provides a lucid, engaging, and challenging description of how we might increase the “signal-to-noise” level of clairvoyance, telepathy, psychokinetic, and precognition effects so that they could be put to practical use to benefit humanity. Anyone working in psi research or applications should own and read this book. For those not working in the field, *Knowing the Unknowable* will be a valuable addition to the library of any statistician, psychologist, science historian, or student of psi who is motivated to learn more about the history of psi or understand how regularities in human responding can be used to help increase effect sizes derived from any behavioral dataset.

In *Knowing the Unknowable*, Broderick starts us off with a clear message that here we will not be concerned with dreamy folktales of psi experiences. Instead he offers to steer us through the details of a series of almost-forgotten psi experiments that elicited large numbers of guesses at targets unknown to the guessers. Before we get there, however, Broderick gives us something rare in the world of scientifically rigorous work. Before we have to do the work of understanding the intricate details of what he is going to tell us, he tells us stories. Sure, he may have an unfair advantage compared to us academics because his Ph.D. is in science and literature, and he has written an intimidatingly large number of science fiction books. But it occurs to me that those of us who write academic prose regularly might still consider imitating Broderick’s methods. After all, most of our readers probably need to be motivated by something other than intellectual curiosity to finish reading our work—our readers need to be emotionally involved to get over the challenging hurdles we invariably give them.

So, in tribute to Broderick, I’ll tell a brief story about a completely fictional psi researcher named Jubulia Mossberg.

<warning: totally fictional story>

After packing her son off to school and unloading the still-dirty dishes from the dishwasher that fails spectacularly every time, Dr. Mossberg set about relaxing on her green chaise longue and reading her new book,

*Knowing the Unknowable*. She was at first struck by the clarity of prose as well as the obvious ability of the author to anticipate and address the likely skepticism of the reader. Upon finishing the Introduction, she realized that the author wrote in a roundabout fashion, in that he first mentioned terms and ideas briefly without explanation, but just when she became concerned, an explanation appeared. The method mirrored the cover of the book, which featured a complex 3-D geometric spiral shape. Once she adapted to this style, the book became a joy to read. By the end of the work, just before her son would return from school, Jubulia was filled with new ideas about how to re-analyze some old datasets to better understand precognition. She launched into this re-analysis and indeed found a critical effect that she had previously ignored. Shifting her point of view, she wondered if the entire book consisted of instructions on how to go on a fishing expedition for psi effects. Just when she was about to resolve this concern, Mossberg's son walked in and announced that he was home and thus the media should be alerted.

<end: totally fictional story>

Hopefully this story worked like Damien's stories in *Knowing the Unknowable*, making you ask how the fictional Jubulia found a new effect in the old datasets and encouraging you to read on. However, I lack Broderick's expertise as a science fiction writer, so it's also possible that you just found yourself asking why this strange character uses a dishwasher that clearly fails at its one and only job. Either way, you're hopefully willing to keep reading because you've got one or more questions to get answered.

Broderick's brief stories all pose the question, "What problems could we solve if we actually had useful psi applications?" They suggest how we could communicate with political detainees locked in isolation, accurately locate underground mineral deposits without expensive equipment, predict a devastating terrorist strike, and more. However, Broderick rightly points out that there is so much variability in psi data that these practical and desirable outcomes seem fantastical. The essential point of the rest of the book is that we ought not to let this variability and "noise" deter us. Instead, we ought to isolate the effect we are interested in by using either pre-tested skilled participants or massive sample sizes, asking each participant to guess multiple times at the same targets, and effectively removing known response biases that contribute to the noise by using normalization procedures.

Broderick describes in detail several large experiments that I had not heard of prior to reading his book. In 1928 a radio experiment in telepathy received 123,295 responses from 24,659 participants who were trying to guess at the objects being considered by a group of sequestered

“senders.” From 1937 to 1938 the Zenith radio network broadcasted an ongoing experiment in telepathy using symbols as targets, receiving more than 150,000 responses. Beginning in 1954 and going on for more than a year, two researchers gathered more than 35,716 responses in a telepathy experiment using clock faces as targets—all of the responses in this last experiment were delivered *by mail*. Today, that seems more fantastical to me than psi.

What do these telepathy experiments have in common beyond a massive number of respondents? Well, they were all dismissed due to inherent biases in the datasets. The data were originally analyzed with an approach that quickly begins to seem simplistic to any reader of Broderick’s book: The researchers looked for either particular participants who performed above chance or a majority vote that predicted the target above chance.

Initially, some experiments revealed psi with this approach. However, further analysis revealed that simple human bias was the explanation for the effects. For instance, in the Zenith network experiments a clear bias emerged, so that participants faced with a binary choice (e.g., square versus circle) were significantly more likely to report a pattern of guesses square–square–circle–square–square (or its inverse) than any other pattern. When the actual pattern of targets matched this pattern, there was apparent evidence for a whopping psi effect, all based on this consistent bias. Yet another psi hypothesis is proven to be the result of response bias. Alas.

But here is where Broderick provides a key insight. If psi exists, it should appear despite any response biases. As he poetically states, “If there is any psi in the data, it will bob up on top of that pattern like a cork on an ocean wave” (p. 79). In the remainder of the book, he shows us, by re-analyzing these old experiments as well as 141,341,969 more recent lottery entries, how to find this cork.

The method is conceptually simple. First, look at the data carefully to observe any clear response biases that produce a pattern that differs consistently from chance in the same way each time the experiment is performed (like the common choice of the number “3” when someone is asked to pick a number between 1 and 10). Second, take this biased response into account by normalizing the data to the expected response rate for each guess. Third, look at the new normalized data to see whether the majority of guesses now correspond to the target.

For instance, let’s assume on a daily “Guess the number I’m thinking of, between 1 and 10” experiment, on average across multiple days “3” receives 20% more guesses than any other number. So if the target is “5” on a particular day, and “5” receives 900 responses on that day even though on average it receives 800 guesses, and “3” receives 960 responses on that

day, and all the remaining numbers between 1 and 10 receive 800 responses on that day, we have potential evidence for psi. This is not because “5” received the majority of guesses (it didn’t), but because normalized to the expected average number of guesses for “5” across days (800), there was a 12.5% increase in guesses for “5” on this day when “5” is the target. This is *potential* evidence for psi because it would have to be repeated multiple times to be impressive. Regardless, performing this normalizing procedure for each target ends up, in many cases, revealing relatively impressive and consistent results where none were thought to exist.

After taking us through this process using multiple datasets, Broderick then speaks to two additional factors that might be taken into account to increase the “signal-to-noise” ratio even further: Individual differences that can conspire to produce null results and phasic environmental conditions, such as geomagnetic effects. Both considerations may be important in winnowing out noise and understanding the mechanisms of psi, but as the examples cited by Broderick make clear, many more experiments need to be performed in this vein before it is clear which individual differences and phasic environmental conditions need to be taken seriously. Of course, one problem with taking the individual differences/environmental conditions results too seriously too soon is that drawing conclusions based on a small number of experiments can send the whole field into a dead end that could have been avoided by performing multiple confirmatory tests for these hypotheses.

*Knowing the Unknowable* ends on a hopeful, fun note: a brief recipe for do-it-yourself applications of psi. The very first suggestion on the list of considerations for such an adventure is to pre-screen participants to find at least one psi-talented person. This suggestion can seem to invalidate the mass-testing approach, but instead Broderick explains it as a best-case-scenario idea: One might get many psi-talented people and ask them to make repeated guesses at targets. The other considerations range from the at-this-point obvious (draw conclusions from normalized rather than absolute numbers of guesses) to the innovative (code the target 10010111 as also 01101000 to help reduce response bias; if a participant is more likely to guess a “1” than a “0”, responses to these two can be compared to find the psi floating above the bias). The entire recipe, as a whole, has yet to be tried. But I am impressed with how it smells, even in print.

I have two minor conceptual concerns with the material in the book. Although open-response remote viewing is discussed at first as a way to give credence to the hypothesis that psi is real and later in more detail as an alternative protocol, most of the book is focused on analysis of forced-choice datasets in which the target is known to the guesser to be one of



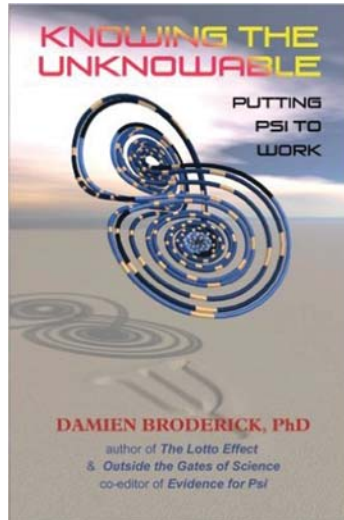
two or more options. Broderick admits that even after using his normalization approach, the results from forced-choice datasets are not likely to be as good as those from remote viewing experiments with highly skilled viewers. This point left me wondering why we don't just put our collective research effort into finding skilled remote viewers.

The second conceptual concern was shared by Jubulia Mossberg, the fictional researcher I described earlier. While reading the book, a fear lingered in my mind. What if it is all just an elaborate discussion of how to mine data to find a result? After all, the normalization method doesn't "work" to show a psi effect in every case, and at certain places in the process Broderick appeals to psi-missing (scoring significantly incorrectly on a psi task) as a legitimate form of psi, while in other places when there is no evidence of psi-missing, psi-hitting is discussed without reference to psi-missing. And almost any of the differentiating factors described in the book, including cognitive style, mood, and environmental changes, could be used post-hoc to explain a pattern of results.

What saves the book, in my view, is that Broderick knows this. He makes it clear that he hopes researchers will take these ideas and perform multiple confirmatory experiments to test them. More than that, it is clear that he hopes that the ideas will lead toward new and helpful practical applications of psi. These are two worthy goals, in my view, and any amount of data mining justifies useful insights that can lead to confirmatory experiments and applications that work.

Oh, speaking of the fictional researcher, here are the answers to those burning questions you should have. I'm not sure what Dr. Mossberg found in her data, because unlike me she is quite shy. But I do know why she uses the dishwasher even though it doesn't work. It's just her human bias. The dishwasher is supposed to work, she expects it to work, and life seems irrationally better when we stick to our biases, no matter how incorrect they are.

Finally, if I were writing a warning label, I might place a stronger caveat on this book than Broderick does in his second chapter. In that chapter, he warns the reader, "You will find some numbers in this book. Tables of them



in many chapters. . . . Don't be alarmed" (p. 17). I might phrase it somewhat differently. Perhaps, "When you read this book, you will start out feeling like a little kid opening a package of sour apple Jolly Rancher candies, and by the end of the book you'll realize that the green dye that caught your eye wasn't 'sour apple' but spinach. You'll eat it anyway, because you already opened the bag. Besides, it doesn't taste that bad. And in the end you'll feel more satisfied than you have felt in a while, and although you might get a bit annoyed at the author for tricking you, you will appreciate his effort in providing a challenging, healthy, and mentally filling treat for a change."

**JULIA MOSSBRIDGE**

Visiting Scholar at Northwestern University  
Visiting Scientist and Director of the Innovation Lab at the Institute of Noetic Sciences  
jmossbridge@gmail.com

## CALL FOR PAPERS

### Joint PA and SSE Meeting, June 2016

#### Accessing the Exceptional, Experiencing the Extraordinary

The 35th Annual Conference of the Society for Scientific Exploration (SSE) and the 59th Annual Convention of the Parapsychological Association (PA) will be held at the newly renovated Millennium Hotel in Boulder, Colorado, from Monday, June 20, 2016, through Thursday June 23, 2016. A welcoming reception and registration is planned for Sunday evening, June 19, and there will be an additional day for workshops, Friday, June 24, 2016. Although each organization has its own purview and style, the program will be fully integrated. Dr. Roger Nelson is the Executive Program Chair, working with his two co-chairs, Dr. Chantal Toporow for the SSE, and Dr. Renaud Evrard for the PA. The program will be a synergetic mix of presentations from PA and SSE members, and there will be no concurrent sessions. We will keep the meeting to 4 days by selecting the best submitted papers, and by using dynamic poster sessions as well as evening sessions for panels and special presentations.

The program theme describes the mission common to both organizations:  
ACCESSING THE EXCEPTIONAL, EXPERIENCING THE EXTRAORDINARY.

**Invited Speakers** will help define thematic topics to be developed further by members of the SSE & PA. The program will include papers assessing progress and issues, both scientific and social/political, in areas of longstanding interest to both societies. All conference sessions will be held at the Millennium Hotel. A poster session is included to accommodate work that requires extended discussion, and to encourage young researchers to present their work.

A **Program Booklet** will be published containing abstracts of all papers and posters. This requires both PA and SSE members to provide a long abstract of 300 to 500 words (about one page of single spaced text), which summarizes

the main points of the paper including its intended goals and conclusions. A link to a template is provided below.

## Submission Deadline

The cutoff date for submissions is March 15, 2016. We expect the program to be full, and submissions received subsequent to that date will likely not be considered. Authors will be notified of the review result (i.e., acceptance or rejection) and any applicable comments, by May 15th, 2016.

## Submission Preparation

For SSE members, Titles and Abstracts for papers and posters should be submitted electronically as an attachment to the SSE co-chair, Dr. Chantal Toporow, [SSEaspiringexplorers@gmail.com](mailto:SSEaspiringexplorers@gmail.com). For PA members, full papers should be submitted electronically as an attachment to the PA co-chair, Dr. Renaud Evrard at [convention\\_program@parapsych.org](mailto:convention_program@parapsych.org). The Title should be short and informative and should be followed by author name and affiliation, email and contact information.

Submissions will be a full paper for PA members, or a long abstract for SSE members. In both cases, we require an abstract of about 500 words or less for inclusion in the convention booklet. Please use this template for creating your abstract: <http://tinyurl.com/pyff9mz>

PA program committees have generally required full papers to encourage later publication, and the combined committee will accommodate this tradition. For convenience and consistency, full papers should be submitted using this template: <http://tinyurl.com/ndfnknk>

SSE program committees require a long, detailed abstract of the submitted paper to review, and for inclusion in the program booklet. The SSE's Journal of Scientific Exploration solicits full papers based on conference presentations. SSE Associate and Student Members must be sponsored by Full Members.



Newly renovated Millennium Hotel in Boulder, Colorado.

## Submission Categories

Floor presentations will include full papers (30 minutes, including 10 minutes for questions and comments) and research briefs (15 minutes, with 5 minutes for comments).

Posters: If a poster is accepted for the dedicated poster session, the available poster board will measure 1m width and 2m height. We recommend that poster pages use sharply focused, concise text, and high quality figures and illustrations. Simple but precise materials work best. The poster pages must be printed beforehand and brought to the meeting. We will supply materials to mount the poster.

Panels discussions may be submitted only by Professional and Full members.

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